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EAT WIDELY, VOTE WISELY? LESSONS FROM A CAMPAIGN AGAINST VOTE BUYING IN UGANDA

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We estimate the effects of one of the largest anti-vote-buying campaigns ever studied---half a million voters exposed across 1427 villages---in Uganda's 2016 elections. Working with civil society organizations, we designed the study to estimate how voters and candidates responded to their campaign in treatment and spillover villages, and how impacts varied with treatment intensity. Despite its heavy footprint, the intervention did not reduce offers of gifts in exchange for votes. However, it had sizable effects in the polling booth. Votes swung from well-funded incumbents (who buy most votes) towards their poorly-financed challengers. Qualitative and quantitative evidence suggests the swing arose from tactical responses by candidates as well as changes in village norms. Specifically, while the campaign struggled to instill norms of refusing gifts, it convinced some voters to abandon reciprocity---to accept gifts but vote for their preferred candidate. This leveling of the electoral playing field led challengers to buy votes in markets where they had previously been deterred from entering.

JEL Classification: C93, D72, O55

Keywords: Elections, voting behavior, field experiment, Africa

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Christopher Blattman Horacio Larreguy Benjamin Marx Otis Reid †

June 2020

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1 Introduction

Democracy in many developing countries is undermined by widespread vote buying—the provision of cash or goods in exchange for votes (Anderson et al., 2015; Baland and Robinson, 2008; Cruz et al., 2017). Candidates and their intermediaries, commonly known as brokers, use various tactics to buy votes, from giving supporters an incentive to turn out, to targeting the individuals most likely to reciprocate gifts with a vote (Nichter, 2008; Finan and Schechter, 2012; Duarte et al., 2019). Such practices hamper economic development by fostering corruption and by constraining political accountability and the provision of public goods (Stokes, 2005; Robinson and Verdier, 2013; Khemani, 2015).

Policy experiments designed to eradicate vote buying have found that these interventions convince some voters to refuse to sell their vote, which hurts the electoral performance of vote-buying candidates (Vicente, 2014; Hicken et al., 2017; Vasudevan, 2018). Others have shown that reducing economic vulnerability undermines clientelistic exchanges with incumbents, who may lose electoral support as a result (Bobonis et al., 2017; Frey, 2019, 2020). One of the most important and least settled questions, however, is whether these interventions reduce vote buying or merely displace it. In addition, it is unclear if these changes can be durable, whether through changes in social norms or the electoral tactics of politicians.

Interventions against vote buying can affect the behavior of both candidates on voters. On the candidate (or demand) side, politicians and their agents may reallocate effort towards voters not exposed to interventions. On the voter (or supply) side, citizens may start refusing offers of gifts in exchange for their vote. Or they might react by accepting gifts, but voting for their preferred candidate anyway—a behavior that we call a violation of reciprocity. Either reaction would shape the design of anti-vote-buying policies. Because the responses to such policies are complex and potentially differ across buyers and sellers of votes, it is important to track effects among voters as well as candidates, and in both treated and untreated areas. This requires a design tailored to estimate both supply- and demand-side responses, detailed data on vote buying arrangements, and an unusually large sample size. We present experimental results from implementing such a design.

We study the effects of a civil society intervention in Uganda, a low-income East African country where one political party and its leader have held power since 1986. Many experts consider Uganda a "multiparty autocracy" or a "hegemonic party system" at the Presidential level (Tripp, 2010). At other levels, however, polls are fairly competitive. As in many other developing countries, vote buying is endemic in Uganda, and most votes are bought by incumbents (Conroy-Krutz, 2012). Extensive fieldwork we conducted prior to the intervention confirmed the ubiquity of vote buying, while also shedding light on the role that social norms and political machines play in sustaining it.

Ahead of Uganda's 2016 general elections, we partnered with the largest collective of electoral civil society organizations in Uganda, the Alliance for Election Campaign Finance Monitoring (ACFIM), and its international partner the National Democratic Institute (NDI). We studied their village-level campaign, which they designed to undermine norms of accepting and reciprocating gifts with votes, as well as to foster new community strategies against vote selling. Their campaign and its evaluation were unprecedented in scale. The villages in the experimental sample (including control) cover around 1.2 million people registered to vote in the 2016 election, 6% of the country's polling stations, and 12% of

polling stations in the 53 districts we study.

We designed the randomization strategy to measure spatial spillovers of ACFIM's campaign, in particular the effects driven by changes in candidate behavior within treated parishes. Our pre-intervention fieldwork showed that the "parish"—a collection of 3 to 10 villages—is the typical operating unit for the rural vote brokers who work for politicians. Before the campaign, ACFIM activists were based in 2,796 villages across 918 parishes in 53 districts. We call these "eligible villages" because of their potential to receive the campaign. We randomized two thirds of the 918 parishes to treat at least one eligible village in the parish; one third had no villages treated (see Section 3.3). Within treated parishes we then varied treatment saturation by randomizing the share of eligible villages targeted by the campaign. This approach yielded 1,427 treated villages. In treated parishes, saturation varied from 3 to 100% of villages.

Hence, our design gives us three kinds of villages: directly treated; "spillover" (untreated villages in treated parishes); and pure control villages (in control parishes). Due to the geographic dispersion of parishes, electoral behavior across parishes is generally independent. This allows us to estimate average treatment and spillover effects of the ACFIM campaign, and how these effects vary with treatment saturation at the parish level. This approach allows us to identify voter and candidate responses, and builds on a new strand of empirical work designed to uncover spillover effects in experimental settings.

ACFIM's campaign began a month prior to the 2016 election and included five elements: (i) a leaflet drop; (ii) three village meetings organized by local ACFIM activists to build awareness of, and opposition to vote buying; (iii) a public village-wide resolution against vote buying; (iv) posters reminding voters about this resolution; and (v) an automated-call reminder on the eve of the election. These activities were designed building on previous work (Hicken et al., 2017; Vicente, 2014) to generate two kinds of norm and behavior changes. One was to encourage people to refuse gifts offered by politicians. The other was to encourage those who received a gift to still vote their personal preference. In this way, the campaign sought to weaken reciprocity behavior, which is essential to vote-buying arrangements (Finan and Schechter, 2012).

ACFIM aimed to use information and persuasion to change generalized beliefs and individually-held values (internalized norms) around refusal and reciprocity. The leaflets and first informational meetings were designed with this goal in mind; both emphasized the costs of vote selling in terms of corruption and public service delivery. In addition, ACFIM facilitated community discussions, coordinated public commitments, displayed posters, and made robocalls in order to give villagers a shared sense of new standards of behavior in the community—in favor of refusal and against reciprocity. In principle, villagers could change their behavior because of psychic penalties (such as shame, or the fear of social sanctions) caused by deviating from generally accepted standards of conduct. Finally, the public commitments, posters, and robocalls were also a signal to politicians that the electoral playing field had changed, and that their vote-buying transactions might not be welcomed or honored in that village.

Shortly after the elections, we surveyed 28,454 villagers, collecting rich data on people's experience with vote buying. We surveyed all 2,796 eligible villages in the 918 parishes in our experimental sample. To increase our power to estimate spatial spillovers, we also surveyed 1,399 nearby ("ineligible") villages in the same parishes. In addition, we used a combination of administrative data and qualitative accounts

from voters and brokers affected by the intervention to understand how the two sides of the vote-buying market responded to the ACFIM campaign.

Our key finding is that the campaign had substantial effects on electoral outcomes, even though it did *not* reduce the extent of vote buying. In areas exposed to the campaign, vote shares decreased for incumbents and correspondingly rose for challenger (non-incumbent) candidates running in the presidential and parliamentary races.[1] The vote share accruing to incumbents decreased by 0.06 standard deviations (sd) in both treatment and spillover villages, amounting to 0.18 standard deviation (sd) in fully treated parishes. However, contrary to our partners' expectations, as well as our preregistered hypotheses, we see little evidence that the campaign reduced politicians' offers of vote buying, or the extent to which voters accepted them. On the contrary, there was a sizable (but insignificant) *increase* in a standardized index of vote buying in treated parishes. This shift was driven by a significant increase in vote buying by challenger candidates, as we document below.

To unpack mechanisms, we explore the behavior of agents on both sides of the market for votes, through surveys and a large and systematic set of qualitative interviews with voters and brokers. Incumbent candidates, who engaged in the bulk of vote buying prior to the intervention, did not reduce their vote-buying efforts in treated villages. However, challenger candidates increased their attempts to buy votes in both treated and spillover villages. In addition, campaigning efforts by challengers (unlike those of incumbents) intensified in high-saturation parishes.[2] Our quantitative results and interviews with brokers indicate these effects are partly explained by the brokers acting on behalf of challengers entering vote-buying markets where they were previously deterred from operating.

Furthermore, we observe some evidence of changes in social norms among citizens. The ACFIM campaign slightly reduced the perception that others in their village would sell their vote, by about 0.06 sd. Similarly, respondents in treated villages were more aware of the negative consequences vote buying could have for their village, and more likely to expect social sanctions for selling their vote. However, these changes in perceived norms of refusal did not translate into changes in actual refusal behavior.

Overall, neither the change in challenger vote buying nor the shift in social norms seem large enough to explain the sizable swing in vote shares. After the campaign, challengers still bought many fewer votes than incumbents, in absolute terms. In addition, electoral support for incumbents seemed to fall in areas where challengers bought no votes. Overall, the combination of increased vote buying by challenger candidates, changes in village norms, and a shift in reciprocity behavior likely explains our results. Facing offers from multiple candidates, voters still accepted gifts but were less willing to reciprocate them with their votes, and voted instead for their preferred candidate.

A slogan often heard in our qualitative work, "eat widely, but vote wisely," summarizes the new approach taken in villages exposed to the intervention. Villagers told us how the ACFIM campaign inspired them to take cash and gifts from all candidates, and then vote for whom they liked. We find some evidence of voter behavior consistent with this interpretation. The campaign increased the like-

[1] In the parliamentary election, 69% of incumbents were affiliated with the ruling party at the national level. We preregistered the distinction between incumbents and challengers (rather than a distinction between ruling party and opposition candidates) because incumbents are well-funded and buy most votes, as well as to maintain strict confidentiality of the results.

[2] We preregistered that the ACFIM campaign would affect candidates' campaigning tactics, with the sign of this effect depending on whether those tactics are complements or substitutes to vote buying.

lihood that voters accepted gifts from both incumbents and challengers running in the same race, and that voters accepted gifts from a candidate but voted for another.

Systematic qualitative interviews with 438 brokers, conducted after the election, provide more evidence that the campaign weakened the effectiveness of vote buying by well-funded incumbents, and prompted a change in challengers' tactics. Brokers working for incumbents told us that they did not expect the ACFIM campaign to have much of an effect, since voters continued to accept the cash and gifts. In contrast, challengers and their brokers told us that the ACFIM campaign leveled the electoral playing field by weakening reciprocity, which previously disproportionately benefited incumbents. As a result, challengers responded to ACFIM's campaign by entering new markets previously dominated by rich incumbents. This gave voters multiple offers to choose from, often for the first time, and may have contributed to the breakdown of reciprocity. While these qualitative accounts are retrospective and self-reported, they provide useful insights into the response of political machines to an anti-vote-buying campaign that was unprecedented in its scale.

Lastly, we rule out several possible alternative explanations to our results. First, we show that the campaign did not serve as a coordinating platform against incumbents exhibiting poor performance. Second, the campaign did not lead to more honest reporting of vote-buying, increase the salience of the phenomenon, or deter electoral fraud. Finally, we show that agency problems between candidates and their brokers are unlikely to explain our findings on the candidate side.

Related Literature. This paper improves our understanding of the possible effects of large-scale policies designed to promote better governance and political accountability in developing countries. Previous work has shown that changes in social norms induced by large policy experiments can affect electoral behavior and political attitudes (Beaman et al., 2009; Berman et al., 2014; Gerber and Green, 2017). We show that informing voters about a general feature of the political system—vote buying and its social costs—can influence the campaigning strategies of political candidates, shift actual and perceived norms of behavior, and change voting outcomes.

A recent literature specifically explores the effectiveness of programs designed to combat vote buying. In seminal work, Vicente (2014) finds that a voter education campaign in São Tomé and Príncipe reduced the reported influence of money received on candidate choice, decreased voter turnout, and favored the incumbent. Hicken et al. (2017) tackle vote selling as a time-inconsistency problem and show that ex-ante promises can reduce vote selling in the Philippines. Vasudevan (2018) shows that a radio campaign led to a reduction in the vote share of candidates known to buy votes in India. Our experiment differed from these earlier contributions in the following ways. First, we explicitly treat vote buying as a market equilibrium problem. Our experimental design allows us not only to estimate responses on the supply-side (i.e., to what extent information campaigns affect the willingness of voters to sell their vote) but also the demand-side (i.e., to what degree candidates adjust their tactics in the new environment created by the ACFIM campaign). Second, we evaluate a campaign that was sufficiently large and visible to trigger supply-side and demand-side responses of this kind.[3] ACFIM villages in our sample

[3]In contrast, Vasudevan (2018)'s mass mode of delivery limits the ability of candidates to respond by strategically reallocating resources across space.

covered around 1.2 million registered to vote, or 12% of the population in half of the country's districts.[4] Third, the detailed data we collected on attitudes, campaigning tactics, and voting behavior allows us to unpack the mechanisms linking the ACFIM campaign with electoral behavior, including changes in campaigning tactics and social norms.

We also build on recent experimental work that studies whether policy campaigning can successfully substitute for vote buying, and in doing so, foster a transition from clientelistic to programmatic politics. Fujiwara and Wantchekon (2013) compare clientelistic rallies to town hall meetings addressing specific policy platforms in Benin, and find that the meetings reduced reported vote buying and lowered the candidate's vote share in his stronghold. Similarly, Bowles and Larreguy (2019) show that candidate participation in a debate initiative led to a reduction in on-the-ground campaigning. In contrast, Bidwell et al. (2019) find that debate showing increases campaign expenditures by candidates, and Cruz et al. (2019) that information about incumbents' campaign promises increases vote buying. In line with Bidwell et al. (2019), our results indicate that buying votes and campaigning on policy issues might act as complements rather than substitutes.

2 Background

2.1 The 2016 Ugandan general election

The ACFIM campaign was implemented in Uganda, a low-income country of about 40 million people in East Africa. Since 2006, two major political parties and a number of smaller ones have competed in national elections every five years. Despite this, the National Resistance Movement (NRM) and its leader, President Yoweri Museveni, have been in power since 1986.[5] Although politics are fairly competitive at the parliamentary level, Uganda is often regarded as a "multiparty autocracy" with endemic voter intimidation and vote buying in national elections (Tripp, 2010).[6]

We study vote buying and electoral behavior during the 2016 general elections, which were held on February 18. The president was elected in a two-round system, requiring at least 50% of the popular vote to be elected in the first round. Members of Parliament (MPs) were elected in single-member constituencies using first-past-the-post voting. Of the eight candidates who ran for the presidency, two were frontrunners from the outset: the incumbent president, Museveni, and a long-time opposition leader, Kizza Besigye. Museveni's and Besigye's parties, the National Resistance Movement (NRM) and the Forum for Democratic Change (FDC), were also dominant in the campaigns for parliamentary seats. These parliamentary races also involve a large number of smaller parties and independents.[7] A total of 1,743 candidates ran for the country's 290 constituency Representative seats and 112 district Women Representative seats. Election officials set up 28,010 polling stations, 6% (1,603) of which were part of

[4] By comparison, Vicente (2014) treats 40 enumeration areas (out of 50 that composed the experimental sample). Hicken et al. (2017) treat 600 voters (out of 900 that composed the experimental sample) privately.

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

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

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

our experimental sample.

Several major incidents occurred during the 2016 electoral period. First, the leader of the opposition was arrested twice in the months leading up to the election. Second, security checkpoints were set up on major roads, 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. Lastly, voting materials were delivered late to a large number of polling stations where voters were expected to vote against the NRM (Associated Press, 2016).

On February 20, 2016, Museveni was declared the winner of the presidential election with 60.8% of the vote (against 35.4% for Besigye). Museveni's party, the NRM, also won 200 out of 290 constituency MP seats (69%). Ugandan and international observation missions provided mixed opinions about the fairness and transparency of the election. For example, the EU Observation Mission cited the "intimidating atmosphere for both voters and candidates," and "the orchestrated use of state resources and personnel for campaign purposes" as major obstacles impeding a free and fair election (European Union Election Observation Mission, 2016). We discuss allegations of voter fraud in Appendix A.3.

2.2 Vote buying in Uganda

Like other countries in Sub-Saharan Africa, Uganda has a high prevalence of vote buying. In the 2006 round of the Afrobarometer, 85% of Ugandan respondents reported that politicians "often" or "always" give gifts during political campaigns.[8] Vote buying is "ubiquitous" (Democracy Monitoring Group, 2011), and previous studies have described sizable payment amounts—one such study reported that the median vote price in 2011 was 5 times the daily average income (Conroy-Krutz, 2012). As Table A1 indicates, 40% of respondents in our sample acknowledge receiving cash for their vote, with the bulk of this cash (83%) coming from incumbent candidates.

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

The focus groups highlighted the extent of vote buying and its importance for winning 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 the importance of reciprocity: a large share of voters reciprocate gifts with their vote since money "softens people's hearts." Votes are traded in exchange for cash or basic consumption goods (such as soap, sugar, or salt), which tend to be distributed in the weeks preceding the election.

In practice, these gifts are not handed out by the candidates themselves, but by political brokers who are typically well-known figures in the community. An NDI survey of 185 elected MPs after the inter-

[8]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%).

vention reports that *all* respondents had brokers in the 2016 election. Brokers are not only responsible for handing over gifts to voters, but they also target reciprocity-minded voters and make sure that gift recipients vote as instructed on election day. 73% of the brokers we interviewed reported being confident that voters who accept gifts do vote for the candidate they represent. In addition, 30% of brokers report transporting voters to the polls, and 28% of brokers report casting the ballot on behalf of their clients, who pretend to be illiterate so that the broker can enter the polling booth with them. Candidates hold regular meetings with their brokers, and they monitor performance by assessing electoral outcomes and voter turnout in each broker's area of work. All of these aspects point to the important role of local brokers in carrying out effective vote buying campaigns.

3 Experimental design

3.1 Description of the intervention

We partnered for this experiment with the Alliance for Election Campaign Finance Monitoring (ACFIM), a coalition of 13 Ugandan civil society organizations (CSOs). ACFIM relies on its network of local activists across the country to advocate for greater transparency in the financing of electoral campaigns. ACFIM implemented their anti-vote buying campaign in January-February 2016 in 53 districts, about half the country. The design of the campaign was influenced by ACFIM and NDI's past interventions, by a survey of Ugandan MPs on campaign financing, and by the focus groups described earlier.

The campaign sought to change actual and perceived norms of behavior around vote buying. Information about the negative consequences of vote buying that ACFIM activists disseminated through leaflets, initial village meetings and automated phone calls, as well as resolutions during subsequent village meetings, were meant to establish a refusal norm around gift acceptance. Community discussions and resolutions aimed to foster a sense that vote selling, and thus deviating from the collective commitment, would be socially sanctioned. A central message of the campaign was that vote selling undermined the delivery of public services to the community, and therefore entailed individual and collective costs. With this message, the campaign also tried to weaken reciprocity behavior. Politicians offering gifts in exchange for votes would no longer be perceived as generous caretakers, but rather be associated with bad outcomes for individuals and the community.

The campaign took place in the final five weeks before the election, when most vote-buying transactions take place, and involved several stages in each selected village. First, in January 2016, local ACFIM activists delivered leaflets via door-to-door canvassing to all households in treated villages. The leaflets explained in simple terms the costs and risks of vote buying to their communities in terms of the potential loss of access to public services. In the process of delivering the leaflets, activists also invited households to participate in village meetings to discuss vote buying. The content of the leaflets was approved by the Electoral Commission and was entirely non-partisan. The leaflets contained a cartoon alongside the following message (in the language spoken by the community):[9] "You wouldn't sell your

[9]Eighteen different languages were used in the campaign: Acholi, Alur, Aringa, Ateso, Kumam, Langi, Lubwisi, Luganda, Lugbara, Lusoga, Madi-Moyo, Ngakarimojong, Rufumbira, Rukhozo, Rukiga, Runyankole, Runyoro, and Rutoro.

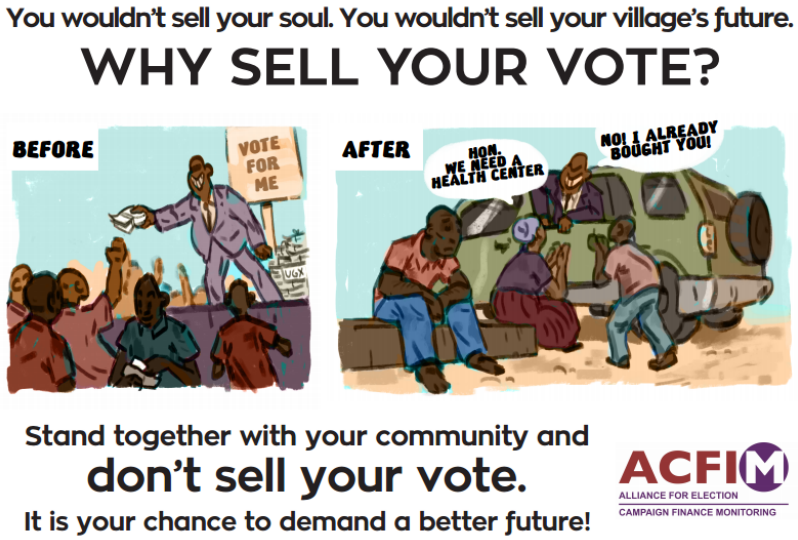

Figure 1: ACFIM leaflet

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, in Figure 1, shows individuals receiving money from a candidate for their votes, and then seeing their request for a health center denied on the ground that the candidate had already bought them off. These illustrations and the caption embody the main messages behind the ACFIM campaign, which were further emphasized during the other components of the intervention. First, individuals who sell their votes are unlikely to be later able to demand public services from vote-buying politicians. Second, community coordination is important to eradicate vote buying.

Following the leaflet distribution, three village meetings were organized by ACFIM. Each meeting was facilitated by a local activist while a second activist took notes. The first meeting introduced the campaign, discussed the leaflet, and gathered participants' thoughts and experiences on vote buying. The second meeting provided an avenue for deliberation about how to collectively tackle 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 their votes—an invitation to enforce the refusal norm. In communities that committed to renouncing vote buying, ACFIM activists then placed posters throughout the village indicating the village is a "no vote-buying village."

Finally, on the eve of election day, individuals who attended the village meetings and provided their phone number received automated phone calls reminding them about the harms associated with 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 goods from politicians during election campaigns, but this will affect the future of your whole community. Do you not want good hospitals, good roads, good schools for your children? When you ask for these services after elections, the politician who wins through buying votes will tell you "I bought your vote, therefore do not bother

me by asking me for more things." Don't let your community down. Don't let your country down. Don't sell your vote!"

3.2 Experimental sample

Our experimental sample includes 918 parishes where ACFIM had some presence before the intervention. This sample is spread across the country, including 110 parliamentary constituencies in 53 Ugandan districts. Within these parishes were 2,796 eligible villages served by 1,603 polling stations. "Eligible" villages are villages where a local ACFIM activist resided or was well known by the villagers prior to the intervention.[10] Thus our experimental sample is not a representative sample of Ugandan villages. Parishes typically have several ineligible villages. These are formally outside our experimental sample, as we performed the randomization among eligible villages only.[11] However, to increase our power to estimate spillover effects of the campaign, we also collected data in an additional 1,399 ineligible villages located in the same 918 parishes. We return to these data below.

3.3 Randomization

To select which villages would be treated with the anti-vote-buying campaign, we used a randomized saturation design similar to the one in Baird et al. (2018). We randomly varied the level of treatment saturation at the level of a parish. 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. The fraction of eligible villages in a parish ranged from 3% to 100%, with an average of 48%. Accounting for the variation in the number of voters registered at each polling station, the fraction of eligible voters ranged from 1% to 100%, with an average of 54%. All our specifications control for the baseline level of ACFIM presence, as described in our pre-analysis plan.

First, we randomly assigned parishes into three roughly equally-sized groups of parishes: a pure control group (no treatment), a partial-saturation treatment group (50% of eligible villages assigned to treatment), and a high-saturation group (100% of eligible villages assigned to treatment). Among the 918 parishes and 2,796 eligible villages, we randomly selected 535 parishes (containing 1,427 eligible villages) for partial- or high-saturation treatment. The remaining 383 parishes were assigned to the pure control group. To illustrate, consider a parish with 8 equally-sized villages, of which 4 have ACFIM activists. If this parish were assigned to the high-saturation group, this would mean that all 4 of the eligible villages would be treated (equivalent to 50% "true" saturation). If assigned to partial saturation, then a randomly selected 2 of the 4 eligible villages would be treated (25% true saturation). We stratified randomization 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.[12]

[10] 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.

[11] Appendix A.5 provides additional details on sampling and external validity.

[12] Specifically, a stratum was defined by the interaction of quartile of ACFIM presence, quartile of the voter population, and quartile of district-level NRM support (64 strata in total).

Second, within the partial-saturation parishes we assigned half the eligible villages to treatment. To maximize our power when looking at electoral results at the polling-station level, we randomized villages to treatment or control status as a function of the randomized status of their polling stations—Appendix A.1 describes the procedure used to map villages to polling stations. All eligible villages that voted at polling stations assigned to treatment status were selected to receive the ACFIM campaign. None of the villages falling under polling stations assigned to control status were selected to receive the campaign. This creates an integer problem if all eligible villages vote at a single polling station. If only one polling station was eligible for treatment in a parish assigned to partial-saturation status, it was either fully treated (with 50% probability) or it was a control (with 50% probability).[13]

In addition to standard intent-to-treat estimates of treatment assignment, this design also allows us to identify spillover effects on the untreated villages in treated parishes. This category of spillover villages includes untreated eligible villages in the partial-saturation parishes as well as ineligible villages in both the partial- and high-saturation parishes (we control for village eligibility in all specifications). We estimate spillover effects by comparing spillover villages to villages in control parishes.

Importantly, our design rests on the assumption that spillovers are limited across parishes. There are several reasons to believe this is reasonable. First, the intervention took place only a few weeks before the election, leaving little time for candidates to reallocate resources across brokers in different parishes. There were no such instances reported in our interviews with brokers. Second, brokers that received funds to buy votes had no incentive to communicate to candidates that they should expect a lower electoral return on those funds. Consistent with this lack of reporting incentives, brokers working for incumbent candidates reported that they expected the ACFIM campaign to have no effect.

3.4 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 estimates 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).[14] Following the leaflet drop, an estimated 62,566 households participated in at least one meeting, which averaged 30 participants. ACFIM also sent 21,390 posters (15 per village) to 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

[13]To fix this concept clearly, we can return to our 8 village (4 with ACFIM presence) parish example from before. Imagine that there are 4 polling stations in this parish, each covering 2 villages. If that parish was assigned to the partial saturation 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 4 of the ACFIM villages, 1 with none), then this parish would either be assigned to have its 1 eligible polling station treated (which is equivalent to high saturation treatment) or its 1 eligible polling station untreated (which is equivalent to being in the control).

[14]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 estimate using our survey, which found that 37 percent of individuals in treatment villages said they received a leaflet.

to ACFIM at one of the meetings. According to administrative data provided by the implementing company, 18,451 (56%) of these calls were answered.

In general, ACFIM's administrative notes suggest that activists implemented the meetings in accordance with their training and the meeting scripts. For the first meeting, note takers indicate that 73% introduced the campaign and discussed the leaflet content, while 51% also involved sharing of participants' views about vote buying and selling. The second meeting was a transition meeting designed to provide an avenue for a collective deliberation on vote buying. There is more variation in what note takers indicated, but all meetings are consistent with the intended purpose. For the third meeting, in 65% of cases the community deliberated on a collective resolution against vote buying, as intended.

The survey data we collected from citizens after the campaign also points to a successful campaign implementation. Table 1 reports control means and treatment effects on various implementation measures, including treatment effects, spillover effects, and the effects of parish-level saturation. We provide a detailed discussion of these effects and the corresponding specifications in section 5, but we highlight a few basic compliance statistics here. Respondents in treatment villages were 34 percentage points more likely to report observing representatives from an NGO conducting an anti-vote-selling campaign and receiving leaflets with an anti-vote-selling message, 29 percentage points more likely to have attended a community meeting to discuss vote buying, and 3 percentage points more likely to have received a automated phone call against vote selling.[15] Table A21 provides further evidence on treatment compliance using instead survey data collected from key informants in each village.

The control means in Table 1 and Table A21 are nonzero, suggesting that representatives from other organizations were active, as one would expect. However, the absence of any sizable or statistically significant effects on spillover villages suggest that these villages were generally not experiencing the ACFIM campaign directly. All this is consistent with ACFIM administrative notes, which indicate that participants at the village meetings were almost all from that village, with an average of fewer than 2 in 30 attendees being from another village.[16]

Activists' notes highlight that the meetings were relatively successful at guiding communities to arrive at a village-wide resolution against vote buying, which occurred in 65% of treatment villages. Their notes further indicate that, in 27% of these cases, the village resolved to "eat widely and vote wisely"—i.e., to take the money offered for their votes but to ultimately vote for the candidate they deemed best. While there was not perfect compliance, these numbers suggest that the intervention might have been successful at facilitating community coordination against vote selling.

Lastly, activists' notes also suggest that in 70% of the village meetings there was at least one influential individual present who was likely to engage in or mediate vote-buying activities, namely a local official, an MP, an MP candidate, or a political broker. In 74% of the cases where at least one such individual was present, note takers reported that he or she tried to influence the meeting by making arguments in favor of vote buying. Such high participation rates by those with a vested interest in vote buying

[15] We expected a smaller effect on calls received, since calls were only made to individuals who voluntarily shared their phone numbers during the meetings organized by ACFIM in treated villages, and the overall pick-up rate was low.

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

Table 1: Quality of Implementation

	NGO visit		Received leaflet		Meetings Attended		Received call		Posters	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment	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		0.004	
	[0.011]		[0.006]		[0.013]		[0.004]		[0.006]	
Treatment Saturation		0.416***		0.432***		0.360***		0.036***		0.250***
		[0.021]		[0.017]		[0.026]		[0.007]		[0.016]
Outside Sampling Frame	-0.018	-0.216***	-0.007	-0.216***	-0.022*	-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	0.025	-0.216***	0.005	-0.246***	-0.019	-0.228***	0.005	-0.016**	0.029**	-0.115***
	[0.018]	[0.024]	[0.015]	[0.019]	[0.027]	[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	0.20	0.20	0.05	0.05	0.11	0.11	0.04	0.04	0.06	0.06
Bonferroni p-value	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	27756	27756	28007	28007	27693	27693	28454	28454	28081	28081

Note: All regressions include a dummy for out-of-sample villages and control for the parish-level ACFIM presence. Odd-numbered columns report estimates from equation (1) and even-numbered columns report estimates from equation (2), described in section 5. Dependent variables in this table are indicators of program implementation: whether the NGO visited (cols. 1-2), distributed leaflets (cols. 3-4), held meetings (cols. 5-6), conducted robocalls (cols. 7-8), or posted signs in the village (cols. 9-10), as reported by respondents in the voter survey. The bottom panel reports the p-value from a Bonferroni correction for multiple hypothesis testing across all outcomes in this table. This p-value corresponds to the coefficient on Treatment Village in odd-numbered columns, and to the coefficient on Treatment Saturation in even-numbered columns.

* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

indicate that these individuals were aware of the ACFIM campaign and potentially felt threatened by it.

4 Data

4.1 Administrative Data

We use official electoral results obtained from the Ugandan Electoral Commission at the lowest possible level, the polling station. We use this data for the presidential and the parliamentary elections conducted in February 2016 for 1,585 of the 1,603 (99%) polling stations in our experimental sample.[17] We also use data on turnout and the incumbent's vote share in the previous general election, conducted in 2011. We discuss the reliability of the electoral data in Appendix A.3.

4.2 Survey data

We conducted an endline survey of 28,454 Ugandan citizens following the ACFIM campaign and the general election. The survey started on March 2, 2016, and ended on July 19, 2016. The survey involved three different questionnaires: one for registered voters, one for a "key informant"—an influential individual—in each village, and a local market survey of the prices of goods commonly used for vote buying as well as other goods. The entire data collection was conducted by a separate research organization with no connections to ACFIM.

The survey of registered voters, which contains many of the outcomes we examine in Section 6, collected detailed data on vote selling behavior, electoral behavior, and attitudes towards vote buying. The data on cash and gifts received was collected across different candidates (and the brokers who operated for them), allowing us to separately examine vote selling behavior by these different candidates. 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 18 or over, registered to vote, and lived in the village.

4.3 Qualitative data

To provide insights into the content of community meetings held as part of the campaign, we use data collected by ACFIM during the three village meetings. The ACFIM note-taker filled in basic information about the meeting, which included the start time, end time, location, estimate of the number of participants from the village and from outside the village, and the presence of influential individuals likely to engage in or mediate vote-buying activities (local officials, MPs, candidates or brokers). The note taker also described 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 to

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

[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 from the 2011 election. For villages with fewer than 40 individuals listed in the voter register, we included all individuals, irrespective of age or gender.

eliminate the practice, and their perceptions of how likely communities were to vote on a resolution against vote buying and whether they ultimately did.

After the campaign, we also conducted structured interviews with a non-representative sample of 438 brokers across 11 districts and 62 parishes. Sampling was restricted to accessible locations and stratified by a measure of vote buying in the village, since we were interested in understanding the response of brokers across areas of different vote-buying intensity. 57% of the brokers we interviewed served incumbent candidates while 43% served challengers. We use this data for descriptive purposes only.

5 Empirical framework

5.1 Estimation

Our experimental design estimates the treatment effects, spillover effects, and saturation effects of the ACFIM campaign. Our baseline equation is the following intent-to-treat (ITT) specification:

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

where $Treatment_{vp}$ is an indicator for assignment to the intervention in village v in parish p; $Spillover_{vp}$ indicates that village v is untreated but located in parish p that is treated; $ACFIM_{vp}$ indicates that village v is an eligible village; $ACFIM\ Presence_p$ is the baseline presence of ACFIM activists in the parish;[19] and X_{ivp} is a vector of pre-specified individual-level controls from the survey and parish-level controls from the electoral data.[20] The $ACFIM_{vp}$ term is an indicator for being part of our experimental sample, which we include in all specifications since the dataset includes 1,399 out-of-sample villages in treated parishes (ineligible for treatment). We focus on the treatment and spillover estimates, α_1 and α_2. All our tables also report coefficients on $ACFIM_{vp}$ and $ACFIM\ Presence_p$, but these are not causally identified. We use the same specification for regressions conducted using the polling station-level data. In this case, observations are at the level of polling station j within parish p.

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

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

where $Saturation_p$ is defined as the fraction of voters in parish p that are being treated (i.e the intensity of the treatment at the parish level). As in equation (1), the terms $ACFIM_{vp}$ and $ACFIM\ Presence_p$ account for variation in $Saturation_p$ that comes from non-randomly assigned baseline ACFIM presence in a parish, giving us causal estimates for saturation. γ_1 then measures the average effect of random

[19] We measure $ACFIM\ Presence_p$ as the fraction of voters in parish p that live in an eligible village.

[20] 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 A.2), as well as occupation, ethnicity, and religion dummies. From the electoral data, we include the 2011 turnout, the NRM and FDC vote shares in the 2011 presidential election, the 2011 fraction of the vote received by the winning parliamentary candidate, and the number of registered voters in 2016.

treatment saturation across treatment and spillover villages.

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

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

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

The two main coefficients of interest here are β_3 and β_4, indicating how the treatment and spillover effects, respectively, change with treatment saturation at the parish level.[21] Note there is no main effect of $Saturation_p$ in this specification since all control parishes have zero saturation by design. β_1 recovers the Treatment on the Uniquely Treated or TUT, which is the intent-to-treat effect of the campaign on a theoretical sole individual offered treatment within a treated parish (Baird et al., 2018). In other words, β_1 measures the direct effect of the campaign measured at the theoretical point of zero saturation. β_1 and β_3 together account for the total treatment effect of the campaign: β_1 captures the TUT while β_3 captures spillovers on the treated.[22] Estimates from equation (3) are reported in Appendix Tables A24 through A29 for our main outcomes of interest.

5.2 Addressing multiple outcomes and comparisons

We sought to reduce the risks of false discovery or cherry-picking results in a number of ways. First, we pre-specified our hypotheses, estimation framework, and outcomes in a pre-analysis plan.[23] Second, we singled out one primary set of outcomes of interest: survey-based reports that candidates gave cash or goods to the respondent or other villagers, which we use to analyze the direct treatment and the spillover effects of the ACFIM campaign on vote buying. In addition, we pre-specified a number of secondary outcomes of particular policy relevance, including vote shares and turnout, measures of the aggregate supply and demand for votes at the village level, attitudinal outcomes, and measures of campaigning. Third, we reduced the number of primary hypotheses to test by combining them into mean

[21] Equation (3) includes a minor deviation from pre-specified equation (2) in our pre-analysis plan, which had two additional right-hand side terms ($ACFIM\ Presence_p \times Treatment_{vp}$ and $ACFIM\ Presence_p \times Spillover_{vp}$) but did not include the $ACFIM\ Presence_p \times \beta_6 ACFIM_{vp}$ interaction. The results obtained from both specifications are similar, but equation (3) above is the correct specification since the previously included terms captured some of the relevant (exogenous) variation and thus should not be included as controls.

[22] β_2 in this specification does not have a meaningful interpretation. Since the corresponding structural parameter is zero by definition, as Baird et al. (2018) explain, a test of $H_0 : \hat{\beta}_2 = 0$ provides a test of the assumption that spillovers are linear. Across Tables A24-A29, we cannot reject that $\hat{\beta}_2 = 0$ in 23 out of 25 specifications.

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

effects indexes of all outcomes in that family.[24] Finally, we adjust for multiple testing by reporting the results from a Bonferroni correction in all our main tables (see the bottom panel of Tables 1 to 6). The correction is implemented across all outcomes reported for the same specification within each table.

5.3 Randomization balance

Treatment is balanced along covariates. We present randomization checks in Appendix Tables A2 through A7. We use a range of baseline or time-invariant variables from the voter survey, key informant survey, and official electoral data. Appendix A.2 provides a detailed description of these variables. We regress these variables on our two main specifications, namely equations (1) and (2) from Section 5.1, 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, our main results include and are thus robust to adding controls for baseline covariates.

6 Results

We start by summarizing qualitative accounts from our debriefing of ACFIM activists and interviews with brokers. While this qualitative evidence must be interpreted with caution, we believe that it provides useful context for our quantitative results. Section 6.2 provides an overview of the main experimental results. We then turn to a detailed description of treatment effects of the campaign on electoral outcomes, vote buying, social norms, and campaigning by candidates.

6.1 Qualitative results

Reports from ACFIM activists point to the intervention's inability to establish a refusal norm around gift acceptance, as well as its apparent success at weakening reciprocity. This undermined the electoral prospects of incumbents, who engaged in the bulk of vote buying at baseline, and incentivized challenger candidates to compete in areas previously dominated by incumbents.

While there are isolated stories of voters refusing gifts for their vote, or even taking cash from the incumbent and handing it over to a challenger candidate, most accounts indicate that treated voters were likely to take the money but then vote for the candidate that they thought would best represent their views. One broker interviewed after the campaign stated that "voters didn't stop accepting gifts, (but) voted for the candidate of their choice," while many mentioned that voters started to "eat widely and vote wisely." Brokers operating for incumbent candidates did not perceive this change in norms, and widely expected that the intervention would have little effect since voters continued to accept gifts.

On the other hand, brokers serving challenger candidates perceived the intervention as an opportunity since existing vote buying arrangements tended to benefit incumbents. In total, 30% of the brokers

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

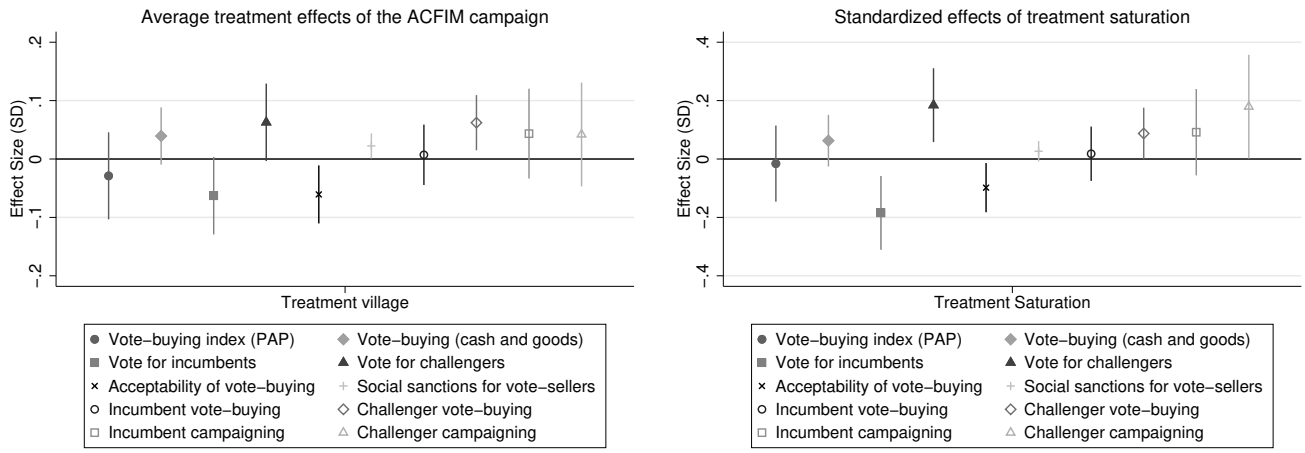

Figure 2: Treatment Effects of the ACFIM Campaign

Note: This figure reports average treatment effects of the ACFIM campaign. The left panel reports estimates of $\hat{\alpha}_1$ from equation (1). The right panel reports estimates of $\hat{\gamma}_1$ from equation (2). The bars indicate 95% confidence intervals. The corresponding coefficients are reported in Table A8. Standard errors are clustered by parish.

we interviewed expected voters would stop reciprocating gifts for their votes and start voting for their preferred candidate. 18% of brokers mentioned that challengers started operating in villages where they previously did not operate, conducting visits and distributing leaflets with campaign promises. For example, one broker explained that "brokers, mostly from opposition parties, went to villages to start campaigning after getting information about the anti-vote-buying campaign."

Overall, 50% of the brokers we interviewed reported that they started to campaign on policies after the ACFIM campaign. Such campaigning covered a range of topics, including improved roads, local public service delivery, education, health, and better economic opportunities. For example, one broker mentioned that "candidates switched to campaigning on policy-based issues like improving roads and availing the community with boreholes." However, even for challengers, policy campaigning still appeared to be tied to vote buying. As clearly explained by a broker, "we were looking for votes addressing policy issues but at the same time buying votes because nowadays, in Uganda, people are not really used to talking to them without giving them something." Brokers indicate that challenger candidates then also commenced to buy votes in areas where they were not operating prior to the intervention.

6.2 Overview of quantitative results

Consistent with these qualitative accounts, Figure 2 summarizes the treatments effect of the intervention on our main outcomes of interest. These coefficients are also reported in Table A8. The left panel of Figure 2 reports average treatment effects of the ACFIM campaign (i.e., estimates of $\hat{\alpha}_1$ in equation (1)). The right panel reports treatment effects of parish saturation ($\hat{\gamma}_1$ from equation (2)).

The first estimate in each panel of Figure 2 is the coefficient on our pre-specified index of vote buying. This includes cash and gift acceptances as well as several outcomes capturing perceptions of vote buying in the village. The second estimate looks only at an index of reported acceptance of cash and goods.

Neither estimate suggests that the campaign achieved its goal of reducing vote buying. Both effects are close to zero and indistinguishable from each other. In the remainder of the analysis, we focus on the second index or its constituent elements since this allows us to disaggregate vote buying across different types of candidates.

Subsequent estimates in Figure 2 examine voting outcomes in the 2016 elections. The third and fourth coefficients capture the average treatment effect (left panel) or the effect of treatment saturation (right panel) on self-reported electoral support for incumbent and challenger candidates, respectively. These results show that the campaign significantly reduced electoral support for incumbents while increasing support for challengers. Results obtained using the official electoral data, which we present in Section 6.3, deliver similar albeit noisier findings.

The next two coefficients report changes in social norms among voters exposed to the ACFIM campaign. The fifth coefficient looks at a pre-specified index of the perceived fraction of village residents who would sell their vote at given price points, and of the perceived acceptability of selling one's vote in a vignette experiment. The campaign had a negative, significant effect on this outcome, consistent with a change in perceptions of the acceptability of vote buying. The sixth coefficient measures the treatment effect on voters' perceptions of the likelihood that vote selling would result in social sanctions. This coefficient is positive in both panels. We describe this set of results in more detail in Section 6.5.

The remaining coefficients examine how changes in the (vote buying and campaigning) tactics of candidates also contributed to the intervention's impacts on voting. The seventh and eighth coefficients reported in each panel of Figure 2 are treatment effects on vote buying (offers of cash or goods) by incumbents and challengers. The coefficient on vote buying by incumbents is a fairly precise zero, while the coefficient on vote buying by challengers shows a significant increase in vote buying by those candidates. Consistent with the qualitative accounts discussed above, the last two coefficients provide evidence for an increase in campaigning, particularly by challengers, in heavily treated parishes. The campaign's effects on vote buying and campaigning are discussed in ections 6.4 and 6.6, respectively.

We next describe our results in more detail, discussing treatment effects for treated and spillover villages. We start by assessing the ultimate impact of the intervention on voting outcomes. We then examine responses of agents on both sides of the market for votes: citizens and candidates.

6.3 Effects on voting

Table 2 reports treatment effects on the vote share accruing to incumbent candidates in the presidential and parliamentary races.[25] Regressions conducted using the survey data are run at the voter level, while regressions using the electoral data are run at the polling-station level. We report the coefficients from equation (1) in odd-numbered columns and those from equation (2) in even-numbered columns. Indices of incumbent support are constructed using the self-reported survey data in columns 1 and 2, and using the electoral administrative data in columns 3 and 4. All outcomes are standardized to have mean zero in the control group, and pooled across the presidential and parliamentary races.

[25]The corresponding estimates for challenger candidates (not reported) are identical but oppositely signed to the incumbent effects reported in Table 2.

Table 2: Effects of the ACFIM Campaign on Voting

	Standardized index: Incumbent Support			
	(1)	(2)	(3)	(4)
	Survey Data		Electoral Data	
Treatment	-0.063*		-0.071	
	[0.034]		[0.049]	
Spillover	-0.066*		-0.005	
	[0.037]		[0.052]	
Saturation		-0.184***		-0.171*
		[0.064]		[0.096]
Outside Sampling Frame	0.007	0.008	-0.026	0.007
	[0.032]	[0.017]	[0.031]	[0.023]
ACFIM Presence	-0.097	0.001	-0.256***	-0.175*
	[0.062]	[0.067]	[0.090]	[0.100]
R^2	0.09	0.09	0.49	0.49
Control Mean	0.00	0.00	0.00	0.00
Bonferroni p-value	0.18	0.01	0.43	0.22
Controls	Yes	Yes	Yes	Yes
Observations	27065	27065	3657	3657

Note: This table reports estimates from equations (1) and (2). The dependent variable is a standardized index of electoral support at the Presidential and MP level, self-reported from the survey data (columns 1-2) or collected from the official electoral data (columns 3-4). All indices have mean zero in the control group. The bottom panel reports the p-value from a Bonferroni correction for multiple hypothesis testing across all outcomes in this table. This p-value corresponds to the coefficient on Treatment Village in odd-numbered columns and the coefficient on Saturation in even-numbered columns.
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

As a cautionary note, whether one should put more weight on the (self-reported) survey data or the administrative data is a priori unclear. The survey data could be subject to social desirability bias. However, if anything, this bias should be directed towards incumbents, and thus is not a source of major concern given our findings. Moreover, since there is some measurement error in the administrative data because of treated polling stations potentially containing some voters from non-treated villages, the survey data may yield more precise estimates of actual voting behavior. Lastly, there were allegations of vote fraud in the 2016 elections. We discuss this possibility in Appendix A.3.

Overall, the estimates across both data sources suggest that the ACFIM campaign negatively affected the electoral performance of incumbent candidates, to the benefit of their opponents. The survey data yields statistically significant effects in treated and spillover villages as well as in high-saturation parishes relative to control parishes. Incumbent candidates suffered a loss of around 0.06 sd in treated and spillover villages, and of 0.18 sd in fully treated parishes. These coefficients are similar in magnitude, but less precisely estimated when using the administrative data. In addition, the estimate of the average spillover effect becomes close to zero and statistically insignificant.

Appendix Table A9 presents treatment effects on voter turnout, measured using administrative data at the polling-station level. Turnout in the administrative data was 67% for the presidential election and 69% for the parliamentary elections.[26] Results suggest a moderate positive effect of the ACFIM campaign on voter turnout. The effect size is approximately 0.07 sd for treated villages but falls short of statistical significance (column 1), and is 0.15 sd and statistically significant for fully treated parishes (column 3). In columns 2 and 4, we interact the terms of interest from equations (1) and (2) with voter turnout in the 2011 presidential election. The interaction of treatment with 2011 turnout is, if anything, negative in these specifications. While the ACFIM campaign did not visibly depress turnout among incumbent supporters, it may have contributed to mobilize voters who were previously passive or disillusioned. This could have resulted from an increase in vote buying and campaigning activities observed among challengers, which we document below.

6.4 Effects on vote buying

Next, we analyze the effect of the ACFIM campaign on vote-buying offers reported by survey respondents, and the way in which voters acted in response to these offers.

Main results. Table 3 reports the campaign's effects on a pre-specified index of offers of cash and goods in exchange for votes, measured across all candidates running in the presidential and the parliamentary elections (columns 1-2), and disaggregated across incumbents and challenger candidates (columns 3-6).[27] Specifically, we focus on a standardized index of 4 variables to capture the prevalence of vote buying: whether the survey respondent reported being offered any gift in cash in exchange for votes, the log of the reported amount of cash offered (plus 1 UGX, to avoid dropping zeros), whether the respondent reported being offered goods, and the log of the value of these goods (plus 1 UGX).[28]

As anticipated in Section 6.2, in columns 1 and 2 of Table 3, we do not see evidence of a change in vote buying when pooling all races and candidates. Overall, reports of vote-buying transactions increase by just 0.04 standard deviations in treated villages. The confidence intervals rule out increases of 0.1 standard deviations or greater. These results, however, mask substantial heterogeneity across candidates. While there are no significant treatment or spillover effects on vote buying by incumbents, we observe significantly positive effects for challengers. Reports of vote-buying transactions by challengers increase by 0.06 and 0.05 sd in treated and spillover villages, while fully treated parishes experience a significant 0.09 sd increase in reports of vote buying.

In Appendix Table A10, we test whether this increase in vote-buying by challengers can explain the entire swing in vote shares reported in Table 2. Specifically, we compare our baseline estimates of treatment effects on voting with estimates obtained in the selected subsample of villages where no vote-buying by challengers took place (columns 3 and 4). Of course, this evidence is only suggestive since

[26] 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).

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

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

Table 3: Effects of the ACFIM Campaign on Vote Buying (Cash or Goods)

	Vote buying by any candidate		Vote buying by incumbents		Vote buying by challengers	
	(1)	(2)	(3)	(4)	(5)	(6)
	Index		Index		Index	
Treatment	0.039		0.007		0.062***	
	[0.025]		[0.026]		[0.024]	
Spillover	0.018		-0.010		0.048*	
	[0.025]		[0.029]		[0.028]	
Treatment Saturation		0.063		0.018		0.087*
		[0.045]		[0.047]		[0.045]
Outside Sampling Frame	-0.010	-0.023	0.001	-0.012	-0.018	-0.023
	[0.025]	[0.018]	[0.028]	[0.019]	[0.028]	[0.017]
ACFIM Presence	-0.017	-0.051	-0.066	-0.077	0.066	0.020
	[0.044]	[0.048]	[0.049]	[0.053]	[0.046]	[0.048]
R^2	0.06	0.06	0.06	0.06	0.04	0.04
Control Mean	0.00	0.00	0.00	0.00	0.00	0.00
Bonferroni p-value	0.34	0.49	1.00	1.00	0.03	0.16
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28454	28454	28454	28454	28454	28454

Note: This table reports estimates from equations (1) and (2). The dependent variable is a 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, measured for any candidate running in the presidential and parliamentary races (columns 1-2), or separately for incumbent candidates (columns 3-4) and challenger candidates (columns 5-6). All indices have mean zero in the control group. The bottom panel reports the p-value from a Bonferroni correction for multiple hypothesis testing across all outcomes in this table. This p-value corresponds to the coefficient on Treatment Village in odd-numbered columns and the coefficient on Treatment Saturation in even-numbered columns.

* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

challenger vote-buying was itself affected by the campaign. While we acknowledge the endogeneity of this sample split, we report it nonetheless since we find it relevant for interpreting our main results. While spillover villages experience an increase in support for incumbents (column 3), saturated parishes experience the same decline in support for incumbents as in the full sample (columns 2 and 4). We interpret this as suggestive evidence that changes in the challengers' vote-buying tactics, while substantial, can only be one of several factors behind the shift in vote shares towards these candidates.

Heterogeneity. To further explore heterogeneity across candidates and to better comprehend the magnitudes of estimates in Table 3, Appendix Tables A11 and A12 assess the effect of the ACFIM campaign on the two main components of the index: an indicator of whether the respondent reported being offered cash, and the log amount of cash (plus 1 UGX) offered.[29] The results are very similar to those in Table 3. Worth noting, however, is the magnitude of the effects on challengers. Challengers offered cash

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

Table 4: Vote Buying and Reciprocity

	Gifts from Competing Candidates		Did Not Reciprocate	
	(1)	(2)	(3)	(4)
	Index		Index	
Treatment	0.038		0.040*	
	[0.025]		[0.023]	
Spillover	0.004		0.027	
	[0.028]		[0.024]	
Treatment Saturation		0.080		0.060
		[0.050]		[0.041]
Outside Sampling Frame	0.006	-0.017	-0.001	-0.007
	[0.027]	[0.016]	[0.024]	[0.016]
ACFIM Presence	0.058	0.013	0.051	0.019
	[0.051]	[0.049]	[0.044]	[0.043]
R^2	0.04	0.04	0.06	0.06
Control Mean	0.00	0.00	0.00	0.00
Bonferroni p-value	0.54	0.45	0.31	0.60
Controls	Yes	Yes	Yes	Yes
Observations	28454	28454	28454	28454

Note: This table reports estimates from equations (1) and (2). The dependent variable in columns 1-2 is an indicator for respondents receiving cash from both incumbents and challengers running in the same electoral race (presidential or parliamentary, summing across both races). The dependent variable in columns 3-4 is an indicator for respondents accepting cash from a candidate but voted for a different candidate in a given race. All outcomes are standardized with mean zero in the control group. The bottom panel reports the p-value from a Bonferroni correction for multiple hypothesis testing across all outcomes in this table. This p-value corresponds to the coefficient on Treatment Village in odd-numbered columns and the coefficient on Treatment Saturation in even-numbered columns.

* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

in exchange for votes to 31% more voters and spent 21% more in fully treated parishes than in control parishes.

Also noticeable is that the effect of the campaign on vote buying by challengers is similar in treatment and spillover villages. This is consistent with accounts by brokers working for challenger candidates. Some of these brokers reported starting to operate in treated villages where they previously did not operate, and possibly due to logistical returns to scale in campaigning, also moved to spillover villages in treated parishes. Thus, a significant amount of the increase in vote buying by challengers took place along the extensive margin, i.e., vote buying in villages and parishes where they previously did not operate. To explore this, Table A13 follows the same baseline specifications and considers as outcomes indicators whether candidates and their brokers bought votes in a village. These estimates sum across the presidential and MP races, so that the outcome variables are counts of candidates.[30] Challenger

[30]Note that there can be at most two incumbents (one president and one MP), but many more potential challengers.

candidates and their brokers, but not incumbents and their operatives, were more likely to start working in villages where they previously did not operate. While these are noisier estimates (sometimes falling short of conventional levels of significance), we observe that, relative to control villages and parishes, in treated and spillover villages and fully treated parishes respectively there is an 8%, 16%, and 16% extensive-margin increase in vote buying.

Reciprocity. Table 4 examines whether vote-selling behavior in villages exposed to the campaign is consistent with a weakening of reciprocity. While Table 3 makes clear that the campaign did not reduce vote selling, it may have encouraged some voters to accept gifts from several politicians, or to vote for their preferred candidate regardless of gifts received. In columns 1 and 2 of Table 4, we look at treatment effects on a (standardized) indicator that voters received cash from both incumbents and challengers running in the same electoral race (presidential or parliamentary). On average, the campaign led to a 0.04 sd increase in gifts received from competing candidates (incumbents and challengers), as well as a 0.08 sd increase in fully saturated parishes. Both of these coefficients fall just short of statistical significance at conventional levels. The campaign's effect on this outcome in spillover villages is close to zero and statistically insignificant. In columns 3 and 4, we look at an indicator for respondents reporting they accepted cash from a candidate but voted for a different candidate in a given electoral race. These reports significantly increase by 0.04 sd in treatment villages and by 0.03 sd (not significant) in spillover villages. The effect of treatment saturation is 0.06 sd but falls short of statistical significance. Overall, the results in Table 4 provide suggestive evidence that the campaign convinced some voters to "eat widely, and vote wisely"—namely to accept gifts from multiple candidates, without feeling compelled to vote for any given candidate. This change in reciprocity behavior may have convinced challenger candidates to adjust their tactics in response to the ACFIM campaign.

6.5 Effects on perceptions of social norms around vote buying

We next explore the extent to which the ACFIM campaign succeeded in: (i) establishing a refusal norm against vote buying, (ii) weakening the reciprocity norm associated with vote buying, and (iii) increasing expectations of social sanctions for vote selling. We explore these issues using detailed survey data on attitudes towards vote selling and its consequences. Table 5 presents the resulting estimates.

While (by revealed preference) only a change in *actual* vote selling behavior provides sufficient evidence of a change in the refusal norm, our endline survey also collected a set of proxies for perceptions of this norm. Perceptions matter since they should have affected a voter's fear of social sanctions and thus her willingness to sell a vote. In columns 1 and 2, we report treatment effects on a pre-specified index measuring people's perceptions of the community's willingness to accept vote-buying offers. This index has the following components. First, we asked respondents to estimate the fraction of voters in the village whom they expected would sell their vote for prices ranging between 1,000 UGX and 50,000 UGX (roughly USD 26¢ to $13 in nominal terms). Second, we conducted a vignette experiment that asked how acceptable it would be for a hypothetical hard-working individual in financial distress to sell his vote to provide for his household. The dependent variable in columns 1-2 combines standardized answers to these questions. We use this measure as a proxy for the level of perceived compliance with the refusal

Table 5: Effect of the ACFIM Campaign on Perceptions of Social Norms around Vote Buying

	Supply of Votes		Neg Consequences		Services not delivered		Social punishment		Ostracizing	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Index		Dummy		Dummy		Dummy		Dummy	
Treatment	-0.061**		0.013*		0.022*		0.022**		0.022*	
	[0.025]		[0.007]		[0.013]		[0.011]		[0.012]	
Spillover	-0.023		0.010		-0.019		0.016		-0.004	
	[0.029]		[0.008]		[0.016]		[0.013]		[0.014]	
Treatment Saturation		-0.098**		0.028***		0.050**		0.027		0.012
		[0.043]		[0.011]		[0.023]		[0.018]		[0.021]
Outside Sampling Frame	-0.013	0.010	-0.012	-0.014**	0.005	-0.025**	-0.009	-0.011	-0.006	-0.022**
	[0.029]	[0.022]	[0.008]	[0.006]	[0.015]	[0.010]	[0.013]	[0.009]	[0.014]	[0.010]
ACFIM Presence	0.039	0.093*	-0.013	-0.028**	-0.036	-0.066**	0.016	0.001	0.012	0.005
	[0.040]	[0.049]	[0.011]	[0.013]	[0.024]	[0.026]	[0.017]	[0.020]	[0.020]	[0.023]
R^2	0.03	0.03	0.04	0.04	0.03	0.03	0.04	0.04	0.07	0.07
Control Mean	0.00	0.00	0.89	0.89	0.48	0.48	0.74	0.74	0.57	0.57
Bonferroni p-value	0.08	0.11	0.29	0.04	0.43	0.14	0.20	0.68	0.35	1.00
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28454	28454	28454	28454	28454	28454	28454	28454	27680	27680

Note: This table reports estimates from equations (1) and (2). Dependent variables in this table include: an index of the perceived fraction of village residents who would sell their vote at given price points (ranging from 1,000 to 50,000 Ugandan Shillings) and of the perceived acceptability of selling one's vote in the vignette experiment (cols. 1-2, see text for details); an indicator for respondents saying vote buying has negative consequences for the village (cols. 3-4); an indicator for respondents saying vote buying will result in services not being delivered to the community (cols. 5-6); an indicator for beliefs that vote selling would lead to social sanctions (cols. 7-8), and beliefs that fellow villages would ostracize vote-sellers (cols. 9-10). The dependent variable in cols. 1-2 is standardized with mean zero in the control group. The bottom panel reports the p-value from a Bonferroni correction for multiple hypothesis testing across all outcomes in this table. This p-value corresponds to the coefficient on Treatment Village in odd-numbered columns and the coefficient on Treatment Saturation in even-numbered columns.
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

norm.[31] We find that the ACFIM campaign significantly decreased the perceived acceptability of vote selling by 0.06 sd in treated villages and 0.098 in fully treated parishes.

We then turn to the voters' perceptions of the social costs of vote buying. A negative change in these perceptions should have contributed to weakening reciprocity, since politicians who buy votes would subsequently be associated with bad outcomes for the community. In columns 3 and 4 of Table 5, we report treatment effects on beliefs that vote buying had negative consequences for the village. Treated respondents changed their attitude towards vote buying and reported a better understanding of its negative consequences as a result of the ACFIM campaign. As it turns out, 89% of respondents in control village already held the belief that vote selling had negative consequences for their village. Nonetheless, those believing that vote buying is associated with negative consequences for their village increased by 1.5% in treated villages and by 3% in fully treated parishes. In columns 5 and 6, we look specifically at beliefs as to whether vote selling negatively affected services delivered to the community. Here we find substantive effects of 5% in treated villages and 10% in fully treated parishes.

Finally, voters reported greater expectations of social sanctions. This is key to understanding the effects of the ACFIM campaign since social sanctions are an important way in which coordination against reprehensible practices is enforced by communities. The percentage of respondents who believed that vote-sellers would be socially punished and ostracized was already high prior to the intervention, at 74% and 57%, respectively. However, these percentages increased significantly, by 3% and 4%, respectively.

We provide further evidence that the ACFIM campaign changed social norms using data from our survey of key informants. In each village, we asked a key informant whether the village collectively adopted a resolution on vote-buying before the 2016 election. Overall, 33% of these meetings led to the adoption of a collective resolution to declare the village a "no vote-buying village", and 37% led to a resolution that villagers should "eat widely and vote wisely." Appendix Table A22 presents the campaign's treatment effects on the likelihood that a village adopted either one of these two resolutions. We find that the ACFIM campaign increased this likelihood by 8 percentage points for the "no-vote buying" resolution, and by 10 percentage points for the "eat widely and vote wisely" resolution.

6.6 Program impacts on campaigning by candidates

We then explore the effect of the ACFIM campaign on standard campaigning tactics, which we *a priori* expected to be a natural substitute for vote buying. In Table 6, we examine an index of activities conducted by both types of candidates across presidential and parliamentary races. The activities most mentioned by voters were displaying political posters in the village, village visits by candidates, and campaigning through loudspeakers, SMS or phone calls. The outcome we examine in Table 6 is a standardized sum of activities conducted by candidates across the presidential and parliamentary races.

As in all previous tables, we look at program impacts separately for incumbents and challengers. Columns 5 and 6 indicate that challengers campaigned more actively in response to the ACFIM interven-

[31] A benefit of asking about other voters' behavior, as opposed to an individual's own behavior, is to minimize social desirability problems. This behavior captures not only the erosion individual of reciprocity but also social sanctioning resulting from social coordination against vote selling in treated villages.

Table 6: Effects on Candidates' Campaigning Activities

	Campaigning by any candidate		Campaigning by incumbents		Campaigning by challengers	
	(1)	(2)	(3)	(4)	(5)	(6)
	Index		Index		Index	
Treatment	0.050		0.043		0.047	
	[0.041]		[0.040]		[0.041]	
Spillover	-0.036		-0.020		-0.043	
	[0.046]		[0.044]		[0.046]	
Treatment Saturation		0.149*		0.093		0.168**
		[0.079]		[0.077]		[0.080]
Outside Sampling Frame	0.049	-0.018	0.038	-0.007	0.049	-0.023
	[0.038]	[0.023]	[0.039]	[0.024]	[0.037]	[0.022]
ACFIM Presence	-0.033	-0.120	-0.080	-0.135*	0.010	-0.088
	[0.079]	[0.079]	[0.076]	[0.076]	[0.079]	[0.080]
R^2	0.12	0.12	0.13	0.13	0.11	0.11
Control Mean	0.00	0.00	0.00	0.00	0.00	0.00
Bonferroni p-value	0.66	0.18	0.84	0.68	0.78	0.11
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28454	28454	28454	28454	28454	28454

Note: This table reports estimates from equations (1) and (2). The dependent variable is the standardized sum of indicators of campaigning activities: visit to the village, posters, leaflets, advertising over loudspeakers, and merchandise, measured for any candidate running in the presidential and parliamentary races (columns 1-2), or separately for incumbent candidates (columns 3-4) and challenger candidates (columns 5-6). All outcomes are standardized indices with mean zero in the control group. The bottom panel reports the p-value from a Bonferroni correction for multiple hypothesis testing across all outcomes in this table. This p-value corresponds to the coefficient on Treatment Village in odd-numbered columns and the coefficient on Treatment Saturation in even-numbered columns.

* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

tion. For instance, column 6 indicates a 0.17 sd increase in campaigning activities by challengers in fully treated parishes relative to control parishes. This increase is larger in magnitude than the corresponding effect for incumbents (columns 4 and 6). Table A29, which reports treatment effects by parish saturation, suggests that these impacts of the ACFIM campaign are mainly driven by spillovers on the treated (as opposed to direct treatment effects of the intervention), consistent with the notion that this effect mostly took place along the extensive margin—challengers and their brokers entering parishes where they did not previously operate. Overall these estimates suggest that, contrary to our expectations, vote-buying and policy-campaigning strategies are complements rather than substitutes.[32] Together with the increase in vote buying by challenger candidates, this intensification of campaigning activities may be responsible for the moderate increase in turnout in treated parishes documented in Table A9.

[32]This is also supported by the positive correlation (0.14) between the two variables in control parishes.

7 Discussion

Our results show that the ACFIM campaign had a meaningful effect on vote shares, social norms, as well as vote buying and campaigning tactics by candidates and their brokers. Challenger candidates attempted to buy more votes and campaigned more actively in villages exposed to the campaign, while incumbents did not respond in the same way. These results are largely driven by parishes with high treatment saturation, with little difference between the point estimates for treatment and spillover villages. Consequently, any theoretical explanation for these findings should account for effects of the campaign that permeate the entire treated parish, rather than solely treated villages within those parishes.

We believe that one of two complementary explanations can account for these results when focusing on the supply side of the market for votes—the voters. First, the campaign weakened the reciprocity norm around vote-buying arrangements, leading voters to no longer think of them as binding contracts. Instead, voters voted for the candidate they deemed better suited for office. This behavior is described by the Ugandan adage "Eat widely, vote wisely," which was adopted as the official resolution in more than 30% of villages that reached an official resolution. 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 more vote buying.

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 practices on public service delivery. Moreover, both vote buying and poor service delivery are associated with incumbents. As a result, notwithstanding the neutral tone of the campaign with respect to incumbency status, the leaflet message and associated public meetings might have shifted beliefs and attitudes about incumbents in treated villages. However, the results in Table A23 suggest that the ACFIM campaign was not simply interpreted as an invitation to vote incumbents out of office. Here we interact our baseline specifications (1) and (2) with an index of availability of public goods in the village, which we use as a proxy for the perceived past performance of incumbent politicians.[33] None of the relevant interactions in the middle panel seems associated with electoral support for incumbents, and the main coefficients of interest are unchanged. We interpret as evidence that the campaign affected electoral outcomes independently from the perceived past performance of incumbents.

Furthermore, Table A25, which reports treatment and spillover effects by parish saturation, provides evidence at odds with this alternative explanation. In this table, the campaign's effects on the electoral performance of incumbents appears mainly driven by spillovers on the treated, as opposed to direct effects of the intervention. This suggests that electoral effects are likely driven both by a change in social norms (columns 2-3 of Table A26) and by a change in the campaigning tactics of candidates (Table A29), which exhibit a similar pattern of effects and together contributed to a breakdown in reciprocity. The fact that the campaign served as an anti-incumbent platform is unlikely to explain these effects, since if that

[33]The public goods index includes the availability of a school, clinic, electricity, piped water, cell phone coverage, and a paved road in the village as reported in the survey of key informants. The index is standardized (0,1). Note that this index is strongly and negatively correlated with support for incumbents in the electoral data (columns 3 and 4).

were the case, we would observe a direct negative effect of the campaign on incumbent support. Instead, Table A25 shows the opposite: the TUT term in this table is actually positive (statistically insignificant).

Crucially, to account for the effects of the intervention, it is essential to understand the demand-side response to the ACFIM campaign—i.e., the response of candidates and their brokers. We observed a large increase in vote buying and campaigning by challenger candidates due to the expected weakening of reciprocity around vote-selling arrangements, which primarily hurt incumbents and leveled the electoral playing field. Importantly, this increase occurred *throughout* the treated parishes in both treated and spillover villages and is increasing with parish treatment saturation. We also provide anecdotal and quantitative evidence that these effects are driven by challengers starting to campaign in villages where they would have been deterred from operating absent the ACFIM campaign. These effects suggest that there are local returns to scale in vote buying and policy campaigning, possibly due to fixed costs of operating in an area, which our interviews with brokers corroborate.

Lastly, we rule out the possibility 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, as we show in columns 1-2 of Table A30. Since the 2011 election pre-dated the campaign, there should be no relationship between treatment assignment and 2011 vote buying, except through a social desirability or salience channel. In addition, our results on norms about vote buying suggest that the campaign intensified negative feelings about vote buying.

Second, it is possible that the campaign increased the salience of vote buying. However, this alternative explanation cannot account for our findings on the effects of the campaign. In the context of Uganda, well-funded incumbents buy significantly more votes than poorly-financed challengers. Had the campaign only increased the salience of vote buying, then we should have observed an increase in reported voted buying by incumbents relative to that of the challengers. However, our findings indicate the exact opposite. Moreover, we should not have observed the treatment effects in spillover villages that our results indicate across our main outcomes of interest.

Third, the effects of the campaign on electoral results could be due in part to agency problems between candidates and their brokers. Interviews with candidates and brokers indicate that the latter are subject to significant moral hazard and that, despite the efforts invested by candidates to monitor their brokers, these manage to extract significant rents. Candidates often offer brokers performance contracts wherein brokers receive lump sums of resources and are expected to deliver a certain level of electoral support to the candidates. Brokers who fail to reach these targets lose contingent rewards in the form of keeping their position in future elections. Brokers then solve a cost-minimization problem to achieve their target and keep the remaining resources for themselves. If brokers working for incumbent candidates responded to the ACFIM campaign by giving up on the prospect of contingent rewards and reducing the fraction of the money allocated to voters, instead increasing what they kept for themselves, this would have contributed to the effects of the campaign on electoral outcomes. However, we do not

observe a decrease in vote-buying offers or amounts by brokers working for incumbents.[34] In addition, importantly, this alternative explanation does not account for the electoral effects of the campaign in spillover villages, which can only be explained by an increase in vote buying and policy campaigning by challenger candidates and their brokers.

Fourth, the campaign may have deterred electoral fraud that otherwise would have favored the incumbents by engaging citizens in the electoral process. As highlighted by the results in Table A15, there is no evidence that the ACFIM campaign is associated (either positively or negatively) with the presence of markers for electoral irregularities.

8 Conclusion

This paper documents the effect of one of the largest anti-vote-buying campaigns ever evaluated on electoral outcomes, vote buying, and campaigning tactics. We found that the campaign, in spite of its relatively heavy footprint, was not effective at diminishing vote buying. Despite the campaign's inability to introduce a refusal norm, we provide evidence that it was successful at weakening the reciprocity norm associated with vote-buying transactions, and thus freed voters from reciprocal vote-buying relationships with incumbent candidates and their brokers. Anticipating these effects of the campaign on voter behavior, which they saw as leveling the playing field against incumbents, challenger candidates and their brokers intensified their vote-buying and campaigning efforts. As a result, the ACFIM campaign significantly hurt the electoral performance of incumbents while benefiting challengers. These effects are large, enough to reverse the relative electoral performance of the average incumbent and challenger in fully saturated parishes.

Our results on vote buying runs counter to previous experimental evidence on such campaigns, as in Hicken et al. (2017) and Vicente (2014), who find sizable negative impacts on votes sold. The differences between our findings and those of Hicken et al. (2017) and Vicente (2014) are likely explained by the difference in scope between the ACFIM campaign and the interventions they evaluate. The large scale and high degree of publicity of the ACFIM campaign, as well as the fact that local brokers attended the community meetings intended to coordinate citizens' efforts against vote buying, prompted candidates to respond to the campaign. In addition, the commonly held resolution to "eat widely, vote wisely" 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. In contrast to Vasudevan (2018), whose mass mode of delivery and proximity to the election prevented candidates from responding strategically by reallocating resources across localities, our evaluation was crafted to measure such a response.

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

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

we might expect this result to change candidate behavior. In particular, 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 candidates' strategy 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. On the one hand, increased vote buying by challenger candidates might lead to associated corruption and undermine their provision of public services, if elected to office. On the other hand, 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 have leveled the electoral playing field, we might expect it to have positive effects on public service delivery, even if the identity of those elected to office was not affected by the ACFIM campaign. Also note that, since the total amount of cash and goods received by voters did not fall, voters had no short-term costs in foregone vote-buying offers.

Future work should also 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 credibly pledge not to buy votes. 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 side, as opposed to simply the supply side, of the votes' market.

This paper then opens new avenues of research on both vote buying and policy campaigning. This remains a fruitful area for more work, with important policy implications and potential for contributions to our knowledge about candidate, broker and voter behavior and governance.

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A Appendix (For Online Publication)

A.1 Mapping Between Villages and Polling Stations

The randomization procedure described in Section 3.3 involved two steps. In the first step, 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). In the second step, eligible villages were assigned to treatment within the partial-saturation parishes. This section provides additional details on this second step.

First, eligible villages (villages with an ACFIM activist at baseline) were matched to a polling station. This allocation was based on the official voter register provided by the Electoral Commission of Uganda. When an ACFIM village could be matched to multiple polling stations because voters from that village were split across multiple stations in the register, the village was assigned to the polling station with the largest number of voters coming from that village.

Second, villages had to be assigned to treatment or control status within partial-saturation parishes. In practice, this involved conducting the randomization at the polling station level, and then allocating villages to treatment or control based on the treatment status of their corresponding polling station. All eligible villages matched to a treated polling station were assigned to receive the ACFIM campaign. None of the villages falling under control polling stations were selected to receive the campaign. This creates an integer problem if all eligible villages in a parish fall under a single polling station. Thus if only one polling station was eligible for treatment (i.e. had a local ACFIM activist), it was either fully treated with 50% probability or allocated to 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.

A.2 Variables used for Randomization Checks

We present randomization checks in Appendix Tables A2 through A7. From the voter survey, we use the age, years of education, marital status (an indicator variable for married individuals), land ownership (an indicator for households that own any land), the number of adults and children in the household, an index of asset ownership,[35,36] variables indicating the individual belongs to one of Uganda's three largest ethnic groups (Ganda, Nkole and Soga), and three indicator 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 indicator variables for whether the key informant is a local chief or

[35] To construct this index, we simply add up the variables indicating ownership of a TV, radio, motor vehicle, and cell phone four measures of occupational status (indicator variables for individuals working in farming, trade/retail, any high-skill activity, or not actively working)

[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.

elder, a member of a civil society group (a religious, youth, or women's group), a village committee member or a local council member.

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

A.3 Electoral data integrity

Opposition leaders in Uganda and international observers challenged the integrity of the voting data in the aftermath of the election (Agence France Presse, 2016; Gaffey, 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 similar results using self-reported voting outcomes from our voter survey and using the official election data.

Second, we show that our treatment is uncorrelated with traditional markers of electoral malfeasance (Beber and Scacco, 2012). Specifically, Table A14 indicates that treatment and spillover assignment, and parish saturation are uncorrelated with the last digit of the polling station valid votes and votes in favor of incumbents being rounded off to zero or to 5, which is usually associated with electoral fraud (Beber and Scacco, 2012). Only 1 out of 24 coefficients in this table is significant at 10%. Note that the mean of the dependent variable in columns 5, 6, 13 and 14 highlight an abnormal share of polling stations with valid votes and votes in favor of incumbents rounded off to zero for the parliamentary vote. However, this rounding pattern is uncorrelated with treatment and spillover assignment or saturation, which confirms the validity of our estimates using the official election data.

Furthermore, in Table A15, we show that treatment assignment and saturation do not significantly correlate with "suspicious polling stations," defined by being either at least 2 standard deviations above the sample average in both turnout and support for the incumbent president, the incumbent MP, or both. Overall, there is no evidence that treatment and spillover assignment or parish saturation correlate with electoral malfeasance.

A.4 Results from Pre-Specified Hypotheses

We report treatment effects on the main hypotheses of our pre-analysis plan in Appendix Tables A16 through Table A20. All tables report estimates from four specifications: equation (1) in column 1, equation (2) in column 2, a modified version of equation (3) in column 3 that includes interactions between ACFIM presence and the Treatment and Spillover dummies, and equation (3) in column 4. The specification used in column 3 is the original version of equation (3) that we included in our pre-analysis plan, but the correct specification should not include these interactions since they capture some of the treatment effects of interest.

Our primary hypotheses stated that vote buying should fall in treatment villages (Hypothesis 1) and rise in spillover villages (Hypothesis 2). We expected the intensity of these effects to be increasing in

parish saturation levels (Hypotheses 7 and 8). To test for these hypotheses, the outcome in Table A16 is a preregistered index of self-reported vote buying, knowledge of particular individuals who sold their vote, and perceptions of the frequency of vote buying in the village from the voter survey and the key informant survey. Even if the main coefficients of interest in column 1 have the expected sign, we find little evidence in support of these hypotheses: the main effects of treatment and spillover are statistically insignificant and small in magnitude (column 1, Hypotheses 1 and 2). There is also little evidence that treatment effects vary with saturation levels (column 4, Hypotheses 7 and 8).

In Table A17, we show treatment effects for our Hypothesis 3A: the supply of votes (i.e., the perceived willingness to sell one's vote) should fall in treatment villages. The dependent variable for this hypothesis is an index of the perceived fraction of village residents who would sell their vote at given price points (ranging from 1,000 to 50,000 UShs) and of the perceived acceptability of selling one's vote in the vignette experiment (as in columns 9-10 in Table 5 in the main text). We find that the (perceived) supply of votes fell in treatment villages (see column 1) and in highly saturated parishes (column 2).

Table A18 shows results for our Hypothesis 3B: demand for votes may rise or fall in treatment villages. The dependent variable is an index of total offers received from brokers (accepted or rejected) and of the perceived fraction of village residents who were given a vote-buying offer. Overall, we do not find significant treatment effects supporting this hypothesis, though both the coefficient on saturation (column 2) and the coefficient on the interaction of treatment with saturation (column 4) are positive. This table also provides a test of Hypothesis 5 (demand for votes increases in spillover villages): we find a positive, but statistically insignificant effect on the spillover variable (column 1) and on the interaction between spillover and saturation (column 4). These findings are in line with those in Figure 2 in the main text, but that masks heterogeneity across candidates shown in Table 3 also in the main text.

Finally, Hypotheses 4 and 6 focused on the price of votes, which we argued may increase or decrease in treatment villages depending on the relative magnitude of demand and supply shocks (Hypothesis 4), and increase in spillover villages (Hypothesis 6). Tables A19 and A20 present results from these tests, using the two different outcomes we pre-specified: a measure of the total amount of cash and goods received by the voter from all brokers in Table A19, and an index of typical amounts offered by candidates from the key informant survey in Table A20. The results from these tables are inconclusive: treatment and spillover effects are positive and statistically insignificant in Table A19, and negative and statistically insignificant in Table A20.

A.5 Discussion of external validity

The presence of a local ACFIM activist is clearly non-random. Since our treatment randomization was conducted within the sample of parishes/villages with local ACFIM activists, this is not a problem for internal validity. However, it does require a brief discussion on external validity. From the perspective of civil society organizations (CSOs) 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. As a result, very few CSOs are willing to launch a campaign in locations where they

had never worked before. In particular, this was our experience when we inquired with ACFIM about the possibility of extending the campaign to villages without ACFIM activists. 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. For example, as expected, ACFIM presence is correlated with lower vote-share in 2011 for the incumbent president — in a parish with 100% ACFIM presence the incumbent president got 7 percentage points fewer, on average, than in one with 0% ACFIM presence. Similarly, as expected, 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 for their voters in 2011.

Second, within each parish, we sample every village where an ACFIM activist had the potential to work. However, in addition, we sampled 1,399 additional villages in the eligible parishes that were ineligible for treatment, but could be affected by spillovers. Throughout the analysis, we control for an indicator 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.

Figure A1: Overview of the Randomized Saturation Design

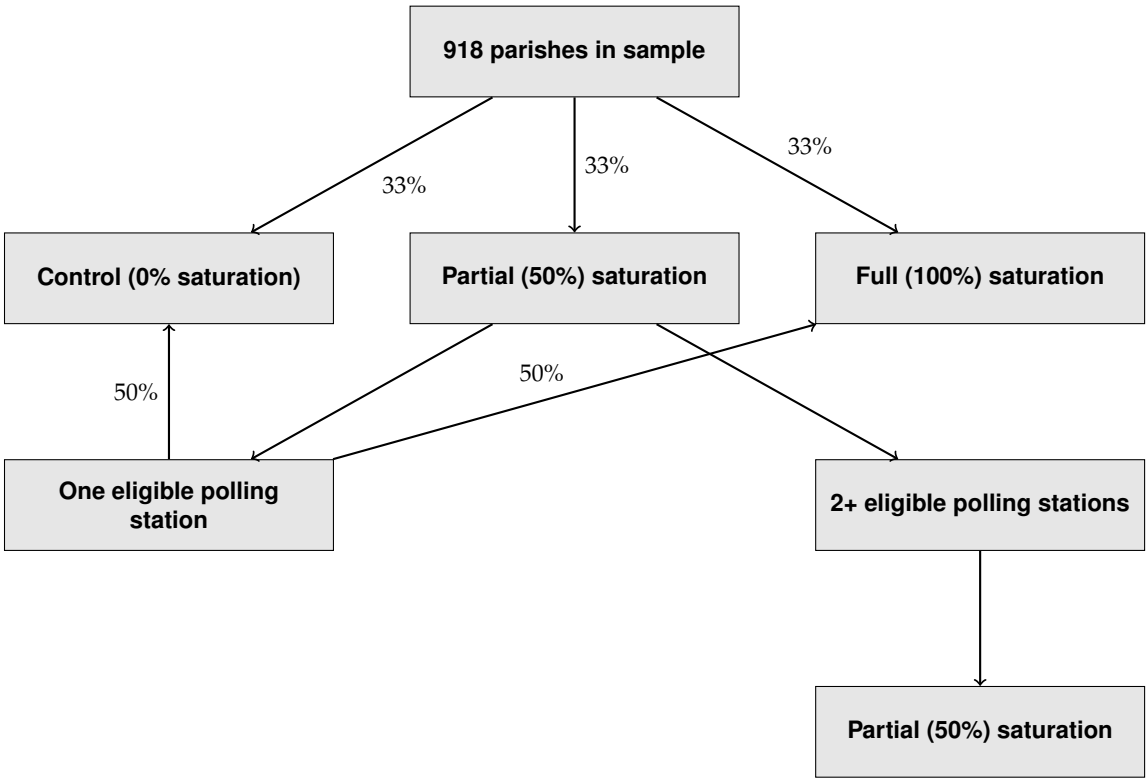

Table A1: Summary Statistics

	Mean	SD	*N*
Survey Data			
Recalls NGO visit in village	0.324	0.468	27,807
Received a leaflet	0.172	0.377	28,060
Recalls meetings took place	0.129	0.335	27,755
Attended meeting	0.207	0.651	27,745
Received a robo-call	0.053	0.224	28,507
Recalls posters	0.129	0.335	28,133
Negative consequences	0.895	0.306	28,507
People angry	0.756	0.43	28,507
Vote sellers ostracized	0.579	0.494	27,732
Vote-buying unacceptable	0.744	0.437	28,501
Any cash received, any candidate	0.4	0.49	28,507
Any cash - Incumbents	0.331	0.578	28,507
Any cash - Challengers	0.111	0.321	28,507
Cash amount received (USh)	1,526.1	4,269.3	28,507
Cash amount - incumbents	1,004.0	2,864.7	28,507
Cash amount - challengers	697.8	2,668.5	28,507
Reported vote for incumbent	0.658	0.349	27,112
Campaign activities, all	5.901	4.246	28,507
Campaign activities, incumbents	3.504	2.536	28,507
Campaign activities, challengers	2.397	2.25	28,507
Electoral Data			
Registered Voters	574.0	202.9	3,659
Turnout 2016, presidential ballot	0.675	0.09	3,659
Turnout 2016, parliamentary election	0.689	0.086	3,112
Incumbent vote share 2016 (pres.)	0.614	0.184	3,654
Challengers vote 2016 (pres.)	0.386	0.184	3,654
Incumbent vote share 2016 (parl.)	0.441	0.246	3,104
Challengers vote share 2016 (parl.)	0.559	0.246	3,104
Turnout 2011 (pres.)	0.601	0.103	3,641
Incumbent vote share 2011 (pres.)	0.678	0.186	3,641

Table A2: Balance on Voter Respondent's Characteristics

	Age		Years Education		Married		Own Land		Adults		Children	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment village	-0.247		0.012		-0.011		-0.002		-0.014		-0.078	
	[0.302]		[0.117]		[0.010]		[0.010]		[0.049]		[0.068]	
Spillover	0.123		-0.120		-0.006		0.002		-0.044		-0.223***	
	[0.338]		[0.146]		[0.011]		[0.011]		[0.057]		[0.075]	
Treatment Saturation		-0.079		-0.004		-0.011		0.008		-0.038		-0.197
		[0.494]		[0.213]		[0.018]		[0.020]		[0.090]		[0.130]
Outside Sampling Frame	-0.843**	-0.650***	0.170	0.083	-0.014	-0.012	-0.001	0.001	0.011	-0.010	0.096	-0.009
	[0.342]	[0.245]	[0.139]	[0.097]	[0.011]	[0.008]	[0.010]	[0.007]	[0.051]	[0.033]	[0.067]	[0.045]
ACFIM Presence	-1.085**	-1.062**	-0.176	-0.185	-0.022	-0.018	-0.044***	-0.048**	0.354***	0.368***	0.696***	0.768***
	[0.467]	[0.518]	[0.209]	[0.223]	[0.016]	[0.018]	[0.017]	[0.020]	[0.082]	[0.096]	[0.114]	[0.138]
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	3.181	3.181	3.551	3.551
Observations	27375	27375	28452	28452	28454	28454	28454	28454	28454	28454	28451	28451

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

Table A3: Balance on Voter Respondent's Characteristics (Continues)

	Assets		Farmer		Trade		High Skill		Not Working	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment village	-0.012		0.025		-0.008		-0.005		-0.008	
	[0.032]		[0.016]		[0.008]		[0.006]		[0.005]	
Spillover	0.011		0.015		-0.006		-0.011		-0.002	
	[0.038]		[0.021]		[0.009]		[0.008]		[0.007]	
Treatment Saturation		-0.030		0.028		-0.012		-0.008		-0.009
		[0.059]		[0.033]		[0.014]		[0.012]		[0.009]
Outside Sampling Frame	-0.020	-0.004	-0.015	-0.018*	-0.006	-0.006	0.013	0.008	0.000	0.003
	[0.033]	[0.022]	[0.017]	[0.010]	[0.008]	[0.005]	[0.008]	[0.005]	[0.006]	[0.004]
ACFIM Presence	-0.186***	-0.170***	0.026	0.016	-0.018	-0.013	0.012	0.015	0.010	0.014
	[0.055]	[0.062]	[0.028]	[0.032]	[0.012]	[0.014]	[0.010]	[0.011]	[0.008]	[0.009]
R^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Control Mean	1.638	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: Odd-numbered columns report estimates from equation (1) and even-numbered columns report estimates from equation (2). All dependent variables come from the voter survey data (see text for details).
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A4: Balance on Voter Respondent's Characteristics (Continues)

	Ganda		Nkole		Soga		Catholic		Protestant		Muslim	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment village	0.015		-0.011		-0.018		0.035*		-0.022		-0.017	
	[0.014]		[0.014]		[0.015]		[0.019]		[0.018]		[0.012]	
Spillover	0.021		-0.009		-0.029		0.049**		-0.022		-0.028**	
	[0.018]		[0.017]		[0.018]		[0.021]		[0.020]		[0.014]	
Treatment Saturation		0.002		-0.011		-0.028		0.082**		-0.039		-0.043*
		[0.029]		[0.018]		[0.031]		[0.036]		[0.035]		[0.023]
Outside Sampling Frame	0.002	0.010	0.004	0.003	0.020	0.009	-0.020	-0.008	-0.003	-0.006	0.016	0.008
	[0.013]	[0.006]	[0.013]	[0.009]	[0.013]	[0.008]	[0.019]	[0.013]	[0.017]	[0.012]	[0.012]	[0.007]
ACFIM Presence	0.057**	0.060**	-0.162***	-0.158***	0.032	0.041	0.014	-0.020	-0.043	-0.028	0.056***	0.074***
	[0.024]	[0.027]	[0.026]	[0.027]	[0.026]	[0.031]	[0.033]	[0.036]	[0.031]	[0.035]	[0.018]	[0.023]
R^2	0.01	0.00	0.03	0.03	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Control Mean	0.075	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: Odd-numbered columns report estimates from equation (1) and even-numbered columns report estimates from equation (2). All dependent variables come from the voter survey data (see text for details).
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A5: Balance on Key Informant Respondent's Characteristics

	Chief or Elder		Civil Society		Village Committee		Local Council	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment village	-0.019		0.009		-0.001		0.026	
	[0.017]		[0.008]		[0.026]		[0.021]	
Spillover	0.023		-0.003		-0.045		0.012	
	[0.023]		[0.008]		[0.031]		[0.025]	
Treatment Saturation		-0.038		0.014		-0.015		0.072*
		[0.031]		[0.011]		[0.047]		[0.039]
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: Odd-numbered columns report estimates from equation (1) and even-numbered columns report estimates from equation (2). All dependent variables come from the key informant survey data (see text for details).

* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A6: Balance on Key Informant Respondent's Characteristics (Continues)

	Ganda		Nkole		Soga		Catholic		Protestant		Muslim	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment village	0.006		-0.004		-0.015		0.005		-0.013		-0.018	
	[0.024]		[0.016]		[0.016]		[0.025]		[0.025]		[0.015]	
Spillover	0.031		-0.005		-0.029*		0.029		-0.001		-0.026	
	[0.030]		[0.017]		[0.016]		[0.029]		[0.028]		[0.016]	
Treatment Saturation		-0.003		0.003		-0.021		0.033		-0.023		-0.043
		[0.055]		[0.029]		[0.035]		[0.046]		[0.045]		[0.030]
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: Odd-numbered columns report estimates from equation (1) and even-numbered columns report estimates from equation (2). All dependent variables come from the key informant survey data (see text for details).

* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A7: Balance on Pre-determined Electoral Data

	Reg'd Voters 2011		Turnout 2011		NRM Vote 2011		FDC Vote 2011		MP Incumbent Vote 2011		Reg'd Voters 2016	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment Polling Station	-284.303		0.004		-0.010		0.005		-0.019		-2.988	
	[196.161]		[0.008]		[0.014]		[0.013]		[0.015]		[10.425]	
Spillover Polling Station	-374.461		0.000		-0.006		-0.006		-0.001		-14.056	
	[375.616]		[0.009]		[0.017]		[0.015]		[0.016]		[9.117]	
Treatment Saturation		-699.026		0.004		-0.024		0.013		-0.035		4.987
		[458.784]		[0.018]		[0.029]		[0.026]		[0.029]		[17.359]
Outside Sampling Frame	550.989***	512.567***	-0.021***	-0.023***	-0.025***	-0.023***	0.015**	0.010**	-0.019**	-0.010*	-81.618***	-86.769***
	[202.723]	[121.113]	[0.005]	[0.004]	[0.008]	[0.006]	[0.007]	[0.005]	[0.008]	[0.005]	[9.420]	[7.363]
ACFIM Presence	-591.868	-298.382	-0.074***	-0.076***	-0.153***	-0.142***	0.036	0.029	-0.076***	-0.059**	-45.561***	-49.449***
	[440.937]	[504.111]	[0.016]	[0.019]	[0.027]	[0.031]	[0.022]	[0.025]	[0.025]	[0.029]	[15.677]	[18.111]
R^2	0.04	0.04	0.03	0.03	0.04	0.04	0.00	0.00	0.01	0.01	0.04	0.04
Control Mean	3007.687	3007.687	0.600	0.600	0.685	0.685	0.262	0.262	0.554	0.554	575.130	575.130
Observations	3641	3641	3641	3641	3641	3641	3641	3641	3214	3214	3659	3659

Note: Odd-numbered columns report estimates from equation (1) and even-numbered columns report estimates from equation (2). All dependent variables come from the official electoral data provided by the Ugandan Electoral Commission (see text for details).

* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A8: Treatment Effects of the ACFIM Campaign

	Vote-buying (PAP Index)	Vote-buying (cash/kind)	Vote for incumbents	Vote for challengers	Acceptability of vote-buying	Social sanctions	Vote-buying by incumbents	Vote-buying by challengers	Campaigning by incumbents	Campaigning by challengers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	(a) Average Treatment Effects									
Treatment Village	-0.029	0.039	-0.063*	0.063*	-0.061**	0.022**	0.007	0.062***	0.043	0.042
	[0.038]	[0.025]	[0.034]	[0.034]	[0.025]	[0.011]	[0.026]	[0.024]	[0.039]	[0.045]
R^2	0.16	0.06	0.09	0.09	0.03	0.04	0.06	0.04	0.13	0.10
Observations	28454	28454	27065	27065	28454	28454	28454	28454	28454	28454
	(b) Treatment Saturation									
Treatment Saturation	-0.016	0.063	-0.184***	0.184***	-0.098**	0.027	0.018	0.087*	0.092	0.179**
	[0.066]	[0.045]	[0.064]	[0.064]	[0.043]	[0.018]	[0.047]	[0.045]	[0.075]	[0.091]
R^2	0.16	0.06	0.09	0.09	0.03	0.04	0.06	0.04	0.13	0.10
Observations	28454	28454	27065	27065	28454	28454	28454	28454	28454	28454

Note: This table reports treatment effects of the ACFIM campaign estimated using equations (1) and (2). Panel (a) reports estimates of $\hat{\alpha}_1$ from equation (1). Panel (b) reports estimates of $\hat{\gamma}_1$ from equation (2). All other terms from each equation are included in the regression but their output is suppressed. The dependent variables in each column are the same as those reported in Figure 2 (see text for details).

* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A9: Effects of the ACFIM Campaign on Turnout

	Standardized index: Turnout			
	(1)	(2)	(3)	(4)
Treatment	0.067	0.071		
	[0.043]	[0.044]		
Spillover	0.026	0.025		
	[0.048]	[0.047]		
Saturation			0.151*	0.154*
			[0.090]	[0.088]
Treatment*Turnout 2011		-0.027		
		[0.050]		
Spillover*Turnout 2011		-0.058		
		[0.056]		
Saturation*Turnout 2011				-0.014
				[0.104]
Outside Sampling Frame	-0.096***	-0.096***	-0.117***	-0.119***
	[0.037]	[0.036]	[0.029]	[0.028]
ACFIM Presence	-0.006	-0.002	-0.075	-0.073
	[0.087]	[0.084]	[0.098]	[0.096]
R^2	0.34	0.34	0.34	0.34
Controls	Yes	Yes	Yes	Yes
Data Source	Official	Official	Official	Official
Observations	3659	3659	3659	3659

Note: This table reports treatment effects of the ACFIM campaign on voter turnout measured in the official electoral data. All regressions include a dummy for out-of-sample villages and control for the parish-level ACFIM presence. The dependent variable is a standardized index of turnout across the presidential and parliamentary races. The index has mean zero in the control group.

* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A10: Electoral Outcomes and Challenger Vote Buying

	Baseline		No Challenger Gifts	
	(1)	(2)	(3)	(4)
Treatment Polling Station	-0.071		-0.022	
	[0.049]		[0.103]	
Spillover Polling Station	-0.005		0.201**	
	[0.052]		[0.095]	
Saturation		-0.171*		-0.169
		[0.096]		[0.217]
Outside Sampling Frame	-0.026	0.007	-0.074	-0.007
	[0.031]	[0.023]	[0.059]	[0.046]
ACFIM Presence	-0.256***	-0.175*	-0.635***	-0.552***
	[0.090]	[0.100]	[0.179]	[0.193]
R^2	0.49	0.49	0.64	0.63
Controls	Yes	Yes	Yes	Yes
Data Source	Official	Official	Official	Official
Observations	3657	3657	899	899

Note: This table reports treatment effects of the ACFIM campaign on incumbent support measured in the official electoral data. All regressions include a dummy for out-of-sample villages and control for the parish-level ACFIM presence. The dependent variable is a standardized index of incumbent support across the presidential and parliamentary races. The index has mean zero in the control group. Columns 1 and 2 replicate our baseline estimates shown in columns 3 and 4 from Table 2. In columns 3 and 4, we report treatment effects in the subsample of parishes with zero vote-buying by challenger candidates (i.e., parishes where zero survey respondent reports receiving gifts from challenger candidates).
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A11: Effects of the ACFIM Campaign on Vote Buying:

Any Cash Received (Individual level)

	All Candidates		Incumbents		All Challengers	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment village	0.020		0.002		0.022*	
	[0.019]		[0.015]		[0.012]	
Spillover	0.028		0.004		0.029**	
	[0.021]		[0.017]		[0.013]	
Treatment Saturation		0.048		0.017		0.040*
		[0.033]		[0.026]		[0.024]
Outside Sampling Frame	-0.013	-0.008	-0.000	0.000	-0.016	-0.009
	[0.020]	[0.014]	[0.015]	[0.011]	[0.012]	[0.007]
ACFIM Presence	-0.035	-0.060	-0.037	-0.046	0.017	-0.003
	[0.034]	[0.037]	[0.026]	[0.029]	[0.025]	[0.023]
R^2	0.13	0.13	0.11	0.11	0.08	0.08
Control Mean	0.43	0.43	0.33	0.33	0.16	0.16
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28454	28454	28454	28454	28454	28454

Note: All regressions include a dummy for out-of-sample villages and control for the parish-level ACFIM presence. The dependent variable is the sum of indicators for any vote buying by candidates in the presidential and parliamentary races. The range of the first outcome is 0-4 and the range of the second and third outcomes is 0 to 2.

* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A12: Effects of the ACFIM Campaign on Vote Buying

Log Cash Received (Individual Level)

	All Candidates		Incumbents		All Challengers	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment village	0.012		-0.009		0.129*	
	[0.095]		[0.088]		[0.076]	
Spillover	0.102		0.026		0.218**	
	[0.107]		[0.101]		[0.086]	
Treatment Saturation		0.083		0.044		0.239*
		[0.161]		[0.149]		[0.140]
Outside Sampling Frame	-0.068	-0.006	0.022	0.041	-0.120	-0.047
	[0.099]	[0.071]	[0.093]	[0.067]	[0.077]	[0.047]
ACFIM Presence	-0.309*	-0.346*	-0.304**	-0.326*	0.071	-0.047
	[0.168]	[0.182]	[0.154]	[0.171]	[0.142]	[0.146]
R^2	0.12	0.12	0.11	0.11	0.07	0.07
Control Mean	2.58	2.58	2.14	2.14	1.12	1.12
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28454	28454	28454	28454	28454	28454

Note: All regressions include a dummy for out-of-sample villages and control for the parish-level ACFIM presence. The dependent variables are equal to the natural log + 1 of the amount of cash received by the respondent from candidates in the presidential and parliamentary races, measured for any candidate (cols. 1-2) and separately for incumbents (cols. 3-4) and challenger candidates (cols. 5-6).

* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A13: Effects of the ACFIM Campaign on Vote Buying

Any Cash Received (Village level)

	All Candidates		Incumbents		Challengers	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment village	0.026		-0.000		0.050	
	[0.056]		[0.043]		[0.041]	
Spillover	0.076		-0.005		0.095**	
	[0.066]		[0.048]		[0.046]	
Treatment Saturation		0.019		-0.018		0.094
		[0.110]		[0.082]		[0.080]
Outside Sampling Frame	-0.113**	-0.072**	-0.040	-0.042*	-0.107***	-0.071***
	[0.057]	[0.031]	[0.040]	[0.025]	[0.040]	[0.022]
ACFIM Presence	-0.398***	-0.402***	-0.361***	-0.352***	-0.149**	-0.194***
	[0.099]	[0.104]	[0.072]	[0.079]	[0.075]	[0.074]
R^2	0.06	0.05	0.05	0.05	0.05	0.05
Control Mean	1.216	1.216	0.919	0.919	0.595	0.595
Observations	4111	4111	4111	4111	4111	4111

Note: All regressions include a dummy for out-of-sample villages and control for the parish-level ACFIM presence. Each dependent variable is the sum of dummies for each individual (presidential or parliamentary) race. All Candidates ranges from 0 to 15, while the other dependent variables range from 0 to 2.

* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A14: Electoral Checks - Rounding

	Presidential (Total Valid)				MP (Total Valid)				Presidential (Incumbent Vote)				MP (Incumbent Vote)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	0	0	5	5	0	0	5	5	0	0	5	5	0	0	5	5
Treatment Polling Station	-0.00		-0.02		0.02		0.01		0.01		-0.00		0.03		-0.01	
	[0.02]		[0.02]		[0.03]		[0.02]		[0.02]		[0.02]		[0.03]		[0.02]	
Spillover Polling Station	-0.02*		-0.00		0.05		-0.01		0.00		-0.02		0.05		-0.00	
	[0.01]		[0.01]		[0.03]		[0.01]		[0.01]		[0.01]		[0.03]		[0.01]	
Saturation		-0.00		-0.04		0.05		-0.00		0.02		0.02		0.10		0.02
		[0.02]		[0.03]		[0.06]		[0.02]		[0.02]		[0.02]		[0.06]		[0.02]
Outside Sampling Frame	-0.01	-0.01	-0.00	0.00	-0.04**	-0.03*	-0.02	-0.03***	-0.01	-0.01	-0.00	-0.01	-0.05**	-0.04***	-0.00	0.00
	[0.01]	[0.01]	[0.02]	[0.01]	[0.02]	[0.02]	[0.01]	[0.01]	[0.01]	[0.01]	[0.02]	[0.01]	[0.02]	[0.02]	[0.01]	[0.01]
ACFIM Presence	-0.02	-0.03	-0.01	0.01	-0.11**	-0.13**	0.02	0.01	-0.01	-0.02	-0.03	-0.04	-0.11**	-0.15***	-0.01	-0.01
	[0.02]	[0.02]	[0.02]	[0.03]	[0.05]	[0.06]	[0.02]	[0.03]	[0.02]	[0.03]	[0.02]	[0.02]	[0.06]	[0.06]	[0.02]	[0.02]
R^2	0.00	0.00	0.00	0.00	0.04	0.04	0.01	0.01	0.00	0.00	0.00	0.00	0.04	0.04	0.00	0.00
Control Mean	0.100	0.100	0.105	0.105	0.202	0.202	0.091	0.091	0.100	0.100	0.093	0.093	0.223	0.223	0.080	0.080
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3192	3192	3192	3192	3192	3192	3192	3192	3192	3192	3192	3192	3192	3192	3192	3192

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

Table A15: Electoral Checks - Abnormal Returns

	Above 2SD, Pres		Above 2SD, MP		Above 2SD, Both	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Polling Station	0.001		0.045		-0.000	
	[0.002]		[0.031]		[0.000]	
Spillover Polling Station	-0.000		0.062**		-0.001	
	[0.002]		[0.030]		[0.001]	
Saturation		0.004		0.090		-0.000
		[0.003]		[0.062]		[0.000]
Outside Sampling Frame	-0.002	-0.002	-0.040**	-0.033***	0.001	0.000
	[0.002]	[0.002]	[0.016]	[0.011]	[0.001]	[0.000]
ACFIM Presence	-0.001	-0.003	-0.106**	-0.143**	-0.001	-0.001
	[0.003]	[0.003]	[0.053]	[0.056]	[0.001]	[0.001]
R^2	0.01	0.01	0.07	0.06	0.00	0.00
Control Mean	0.001	0.001	0.110	0.110	0.001	0.001
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3192	3192	3192	3192	3192	3192

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

Table A16: ACFIM Campaign Effects, Primary Hypotheses (Overall Vote Buying)

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

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

Table A17: ACFIM Campaign Effects, Hypothesis 3A (Supply of Votes)

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

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

Table A18: ACFIM Campaign Effects, Hypotheses 3B & 5 (Demand for Votes)

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

Note: This table reports experimental results for Hypothesis 3B in our pre-analysis plan: the campaign affects the demand for votes in treatment villages. The dependent variable is an index capturing offers made by brokers of votes (accepted and rejected).

* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A19: ACFIM Campaign Effects, Hypotheses 4 & 6 (Price of Votes)

	Index for Hypothesis 4_1			
	(1)	(2)	(3)	(4)
Treatment village	0.012		0.021	0.026
	[0.020]		[0.044]	[0.037]
Spillover	0.029		0.023	0.024
	[0.023]		[0.042]	[0.041]
Treatment Saturation		0.017		
		[0.033]		
Treatment*Saturation			-0.040	-0.029
			[0.094]	[0.064]
Spillover*Saturation			0.006	0.016
			[0.112]	[0.101]
Outside Sampling Frame	-0.018	-0.004	-0.017	-0.013
	[0.021]	[0.014]	[0.024]	[0.044]
ACFIM Presence	-0.052	-0.059	-0.049	-0.051
	[0.037]	[0.042]	[0.058]	[0.097]
ACFIM Presence*Treatment			0.020	
			[0.107]	
ACFIM Presence*Spillover			0.009	
			[0.108]	
ACFIM Village*ACFIM Presence				0.009
				[0.098]
R^2	0.09	0.09	0.09	0.09
Control Mean	0.003	0.003	0.003	0.003
Controls	Yes	Yes	Yes	Yes
Observations	28454	28454	28454	28454

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

Table A20: ACFIM Campaign Effects, Hypotheses 4 & 6 (Price of Votes)

	Index for Hypothesis 4_2			
	(1)	(2)	(3)	(4)
Treatment village	0.002		-0.003	-0.009
	[0.026]		[0.059]	[0.047]
Spillover	-0.041		-0.028	-0.040
	[0.033]		[0.070]	[0.058]
Treatment Saturation		-0.007		
		[0.042]		
Treatment*Saturation			0.130	0.023
			[0.125]	[0.081]
Spillover*Saturation			-0.178	-0.005
			[0.138]	[0.135]
Outside Sampling Frame	0.044	0.013	0.061	0.100
	[0.033]	[0.024]	[0.041]	[0.069]
ACFIM Presence	-0.096**	-0.095*	-0.088	-0.209
	[0.044]	[0.052]	[0.075]	[0.141]
ACFIM Presence*Treatment			-0.101	
			[0.147]	
ACFIM Presence*Spillover			0.081	
			[0.144]	
ACFIM Village*ACFIM Presence				0.140
				[0.148]
R^2	0.07	0.07	0.07	0.07
Control Mean	-0.003	-0.003	-0.003	-0.003
Controls	Yes	Yes	Yes	Yes
Observations	28440	28440	28440	28440

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

Table A21: Quality of Implementation (Key Informants)

	NGO Visit		% Received Leaflet		% Attended Meetings		% Received Calls		Posters	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment village	46.296***		26.748***		12.766***		3.731***		33.399***	
	[1.927]		[0.988]		[0.892]		[0.581]		[1.770]	
Spillover	3.294		1.109		-0.228		-0.292		3.680**	
	[2.218]		[0.682]		[0.860]		[0.545]		[1.701]	
Treatment Saturation		60.292***		35.742***		16.127***		5.003***		46.397***
		[3.543]		[1.688]		[1.458]		[0.959]		[3.293]
Outside Sampling Frame	-0.353	-27.381***	-0.221	-16.472***	-0.809	-9.058***	0.476	-2.108***	1.536	-17.288***
	[2.275]	[1.804]	[0.748]	[0.883]	[0.878]	[0.702]	[0.601]	[0.475]	[1.685]	[1.488]
ACFIM Presence	4.809	-29.053***	3.111*	-16.979***	-0.279	-9.435***	1.415	-1.408	7.754***	-18.189***
	[3.055]	[3.800]	[1.618]	[1.877]	[1.434]	[1.580]	[0.984]	[1.092]	[2.910]	[3.231]
R^2	0.19	0.13	0.26	0.18	0.09	0.06	0.02	0.01	0.13	0.09
Control Mean	23.695	23.695	4.037	4.037	5.554	5.554	2.644	2.644	10.020	10.020
Observations	4068	4068	4021	4021	4058	4058	3884	3884	4071	4071

Note: This table reports experimental results from survey data collected with key informants in every village. Odd-numbered columns report estimates from equation (1) and even-numbered columns report estimates from equation (2), described in section 5. Dependent variables in this table are indicators for: whether an NGO visited the village (cols. 1-2), the proportion of village residents who received leaflets (cols. 3-4), attended meetings (cols. 5-6), and received robocalls (cols. 7-8), and whether signs were posted in the village (cols. 9-10), as reported by the key informant.

* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A22: Village Decision (Key Informants)

	No Vote Buying Village		Eat Widely Vote Wisely	
	(1)	(2)	(3)	(4)
Treatment village	0.083***		0.105***	
	[0.010]		[0.011]	
Spillover	0.004		0.006	
	[0.007]		[0.010]	
Treatment Saturation		0.101***		0.141***
		[0.015]		[0.020]
Outside Sampling Frame	-0.008	-0.057***	-0.008	-0.070***
	[0.008]	[0.007]	[0.010]	[0.009]
ACFIM Presence	0.003	-0.054***	0.022	-0.057***
	[0.014]	[0.017]	[0.018]	[0.020]
R^2	0.03	0.02	0.04	0.03
Control Mean	0.020	0.020	0.042	0.042
Observations	4195	4195	4195	4195

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

Table A23: Electoral Outcomes, Heterogeneity by Public Goods

	Standardized index: Incumbent Support			
	(1)	(2)	(3)	(4)
	Survey Data		Electoral Data	
Treatment	-0.065*		-0.074	
	[0.034]		[0.048]	
Spillover	-0.065*		-0.006	
	[0.037]		[0.051]	
Saturation		-0.189***		-0.178*
		[0.064]		[0.096]
Public Goods Index	0.006	-0.001	-0.122**	-0.126***
	[0.028]	[0.025]	[0.053]	[0.046]
Treatment*Public Goods	-0.007		-0.007	
	[0.022]		[0.045]	
Spillover*Public Goods	-0.028		-0.040	
	[0.029]		[0.047]	
Saturation*Public Goods		-0.068		-0.049
		[0.047]		[0.096]
Outside Sampling Frame	0.006	0.008	-0.019	0.016
	[0.031]	[0.017]	[0.031]	[0.023]
ACFIM Presence	-0.097	0.000	-0.240***	-0.155
	[0.062]	[0.067]	[0.089]	[0.099]
R^2	0.09	0.09	0.49	0.49
Controls	Yes	Yes	Yes	Yes
Data Source	Survey	Survey	Official	Official
Observations	27065	27065	3657	3657

Note: Each dependent variable in cols. 1-4 is a standardized index of electoral support at the Presidential and MP level, self-reported from the survey data (cols. 1-2) or collected from the official electoral data (cols. 3-4). The public goods index is a standardized (0,1) index of availaibility of the following public goods in the village: school, clinic, electricity, piped water, sewage, paved road.

* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A24: Interactions on Key Outcomes - Quality of Implementation (Table 1)

	NGO visit	Received leaflet	Meetings Attended	Received call	Posters
	(1)	(2)	(3)	(4)	(5)
Treatment village	0.374***	0.349***	0.311***	0.031***	0.183***
	[0.021]	[0.019]	[0.031]	[0.008]	[0.016]
Spillover	-0.003	-0.017*	-0.006	-0.003	-0.009
	[0.018]	[0.009]	[0.023]	[0.007]	[0.010]
Treatment*Saturation	-0.084**	-0.025	-0.044	-0.004	0.014
	[0.040]	[0.037]	[0.055]	[0.015]	[0.032]
Spillover*Saturation	0.064	0.080***	0.021	-0.002	0.043
	[0.045]	[0.025]	[0.058]	[0.019]	[0.028]
Outside Sampling Frame	-0.006	-0.003	-0.012	0.001	-0.006
	[0.022]	[0.013]	[0.024]	[0.009]	[0.012]
ACFIM Presence	0.023	-0.013	-0.022	0.016	0.035
	[0.038]	[0.021]	[0.048]	[0.019]	[0.024]
ACFIM Village*ACFIM Presence	0.025	0.015	0.019	-0.012	-0.026
	[0.043]	[0.027]	[0.050]	[0.019]	[0.028]
R^2	0.14	0.20	0.06	0.04	0.09
Control Mean	0.198	0.052	0.113	0.040	0.062
Controls	Yes	Yes	Yes	Yes	Yes
Observations	27756	28007	27693	28454	28081

Note: This table reports estimates from equation (3). All regressions include a dummy for out-of-sample villages, the parish-level ACFIM presence, and their interaction. Dependent variables in this table are indicators of program implementation: whether the NGO visited (col. 1), distributed leaflets (col. 2), held meetings (col. 3), conducted robocalls (col. 4), or posted signs in the village (col. 5), as reported by respondents in the voter survey.
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A25: Interactions on Electoral Outcomes (Table 2)

	Survey Data	Electoral Data	
	Incumbents	Incumbents	Turnout
	(1)	(2)	(3)
Treatment	0.060	0.120	0.023
	[0.056]	[0.081]	[0.077]
Spillover Polling Station	0.020	0.028	-0.051
	[0.064]	[0.093]	[0.080]
Treatment*Saturation	-0.258**	-0.418**	0.102
	[0.112]	[0.171]	[0.160]
Spillover*Saturation	-0.299*	-0.131	0.280
	[0.164]	[0.226]	[0.221]
Outside Sampling Frame	0.043	0.133**	-0.045
	[0.059]	[0.059]	[0.071]
ACFIM Presence	0.051	0.009	-0.017
	[0.136]	[0.116]	[0.111]
ACFIM Village*ACFIM Presence	0.002	-0.316**	-0.166
	[0.133]	[0.144]	[0.166]
R^2	0.09	0.49	0.34
Controls	Yes	Yes	Yes
Observations	27065	3657	3659

Note: This table reports estimates from equation (3). All regressions include a dummy for out-of-sample villages, the parish-level ACFIM presence, and their interaction. Dependent variables are defined as in Table 2. All outcomes are standardized indices with mean zero in the control group.
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A26: Interactions on Key Outcomes - Voter Reciprocity and Social Punishment (Table 5)

	Supply of Votes	Neg Consequences	Services not delivered	Social punishment	Ostracizing
	(1)	(2)	(3)	(4)	(5)
Treatment village	-0.044	-0.007	-0.021	0.039**	0.052**
	[0.046]	[0.013]	[0.023]	[0.019]	[0.022]
Spillover	-0.011	0.008	-0.041	0.008	-0.001
	[0.046]	[0.013]	[0.027]	[0.021]	[0.024]
Treatment*Saturation	-0.036	0.042*	0.091**	-0.035	-0.063
	[0.082]	[0.022]	[0.043]	[0.033]	[0.039]
Spillover*Saturation	-0.044	0.008	0.078	0.024	-0.016
	[0.114]	[0.033]	[0.068]	[0.050]	[0.059]
Outside Sampling Frame	0.032	-0.012	0.024	-0.009	-0.007
	[0.055]	[0.014]	[0.029]	[0.024]	[0.027]
ACFIM Presence	-0.015	-0.037	-0.141**	0.026	0.050
	[0.109]	[0.030]	[0.062]	[0.045]	[0.053]
ACFIM Village*ACFIM Presence	0.099	0.011	0.076	-0.003	-0.016
	[0.113]	[0.031]	[0.064]	[0.048]	[0.057]
R^2	0.03	0.04	0.03	0.04	0.07
Control Mean	0.046	0.888	0.482	0.745	0.567
Controls	Yes	Yes	Yes	Yes	Yes
Observations	28454	28454	28454	28454	27680

Note: This table reports estimates from equation (3). All regressions include a dummy for out-of-sample villages, the parish-level ACFIM presence, and their interaction. Dependent variables in this table include: an index of the perceived fraction of village residents who would sell their vote at given price points (ranging from 1,000 to 50,000 Ugandan Shillings) and of the perceived acceptability of selling one's vote in the vignette experiment (col. 1, see text for details); an indicator for respondents saying vote buying has negative consequences for the village (col. 2); an indicator for respondents saying vote buying will result in services not being delivered to the community (col. 3); an indicator for beliefs that vote selling would lead to social sanctions (col. 4), and beliefs that fellow villages would ostracize vote-sellers (col. 5).

* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A27: Interactions on Key Outcomes - Vote-Buying Index (Table 3)

	All Candidates	Incumbents	All Challengers
	(1)	(2)	(3)
Treatment village	0.050	0.008	0.061
	[0.047]	[0.048]	[0.047]
Spillover	-0.025	-0.054	0.064
	[0.048]	[0.054]	[0.059]
Treatment*Saturation	-0.022	-0.003	0.002
	[0.089]	[0.091]	[0.091]
Spillover*Saturation	0.145	0.145	-0.053
	[0.123]	[0.137]	[0.143]
Outside Sampling Frame	0.000	0.031	-0.048
	[0.048]	[0.054]	[0.055]
ACFIM Presence	-0.068	-0.163	0.137
	[0.100]	[0.111]	[0.107]
ACFIM Village*ACFIM Presence	0.041	0.094	-0.081
	[0.102]	[0.113]	[0.111]
R^2	0.06	0.06	0.04
Controls	Yes	Yes	Yes
Observations	28454	28454	28454

Note: This table reports estimates from equation (3). All regressions include a dummy for out-of-sample villages, the parish-level ACFIM presence, and their interaction. The dependent variable is a 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, measured for any candidate running in the presidential and parliamentary races (col. 1), or separately for incumbent candidates (col. 2) and challenger candidates (col. 3). All outcomes are standardized indices with mean zero in the control group.
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A28: Interactions on Key Outcomes - Vote Buying and Reciprocity (Table 4)

	Gifts from Competing Candidates	Did Not Reciprocate
	(1)	(2)
Treatment village	-0.019	0.046
	[0.046]	[0.043]
Spillover	-0.004	-0.001
	[0.051]	[0.046]
Treatment*Saturation	0.120	-0.012
	[0.095]	[0.083]
Spillover*Saturation	0.035	0.094
	[0.124]	[0.115]
Outside Sampling Frame	-0.006	0.048
	[0.048]	[0.045]
ACFIM Presence	0.011	-0.063
	[0.106]	[0.101]
ACFIM Village*ACFIM Presence	-0.002	0.133
	[0.101]	[0.095]
R^2	0.04	0.06
Controls	Yes	Yes
Observations	28454	28454

Note: This table reports estimates from equation (3). All regressions include a dummy for out-of-sample villages, the parish-level ACFIM presence, and their interaction. The dependent variable in columns 1-2 is an indicator for respondents reporting they received cash from at least two competing candidates in the same electoral race (presidential or parliamentary). The dependent variable in columns 3-4 is an indicator for respondents reporting they accepted cash from a candidate but voted for a different candidate in a given race. All outcomes are standardized indices with mean zero in the control group.
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A29: Interactions on Key Outcomes - Campaigning Index (Table 6)

	All Candidates	Incumbents	All Challengers
	(1)	(2)	(3)
Treatment village	-0.112	-0.060	-0.134*
	[0.071]	[0.068]	[0.071]
Spillover	-0.071	0.005	-0.119
	[0.076]	[0.075]	[0.077]
Treatment*Saturation	0.341**	0.218	0.379**
	[0.147]	[0.139]	[0.147]
Spillover*Saturation	0.140	-0.071	0.283
	[0.180]	[0.177]	[0.184]
Outside Sampling Frame	-0.012	0.024	-0.038
	[0.072]	[0.073]	[0.070]
ACFIM Presence	-0.131	-0.140	-0.101
	[0.148]	[0.145]	[0.152]
ACFIM Village*ACFIM Presence	-0.065	-0.004	-0.102
	[0.148]	[0.146]	[0.148]
R^2	0.12	0.13	0.11
Controls	Yes	Yes	Yes
Observations	28454	28454	28454

Note: This table reports estimates from equation (3). All regressions include a dummy for out-of-sample villages, the parish-level ACFIM presence, and their interaction. The dependent variable is the standardized sum of indicators of campaigning activities: visit to the village, posters, leaflets, advertising over loud-speakers, and merchandise, measured for any candidate running in the presidential and parliamentary races (col. 1), or separately for incumbent candidates (col. 2) and challenger candidates (col. 3). All outcomes are standardized indices with mean zero in the control group.

* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table A30: Test of Social Desirability Bias

	Vote-Buying 2011		NRM Vote 2011	
	(1)	(2)	(3)	(4)
Treatment village	-0.002		-0.004	
	[0.010]		[0.008]	
Spillover	-0.003		-0.008	
	[0.012]		[0.009]	
Treatment Saturation		-0.007		-0.019
		[0.018]		[0.013]
Outside Sampling Frame	-0.011	-0.011	-0.002	-0.005
	[0.011]	[0.008]	[0.009]	[0.006]
ACFIM Presence	-0.001	0.003	0.018	0.028*
	[0.017]	[0.020]	[0.013]	[0.015]
R^2	0.07	0.07	0.09	0.09
Control Mean	0.22	0.22	0.84	0.84
Controls	Yes	Yes	Yes	Yes
Observations	28454	28454	21785	21785

Note: * 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 dependent variables in this table are: whether the respondent reported selling their vote in 2011 (cols. 1-2), and whether they reported voting for the NRM in 2011 (cols. 3-4).