# Cash Transfers and Community Participation in Public Affairs: A Village-Level Randomized Controlled Trial in Kenya*

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9th August 2022

**Abstract**

We provide causal evidence on how a large NGO-run cash transfer programme affects household civic and political participation and household requests for resources from local politicians. Between 2014-17, the programme, which provides transfers to poor households meeting a basic means test in treatment villages, was randomly rolled out to over 1,000 villages in western Kenya. We collect survey data from over 10,100 households (both those poor enough to be eligible to receive the transfer and ineligible households) and 1,200 local leaders during the period after the 2017 general election. First, we find voters (correctly) do not attribute the programme to their local politician and politicians do not attempt to claim credit for it. Receiving a cash transfer does not affect self-reported household turnout in the general election or opinions about the candidates in the local (ward-level) race. Treatment intensity is also not associated with increased turnout using polling station data. This allays some concerns that poor citizens may be unable to attribute credit for programmes and that NGO transfer programmes may alter electoral results. Second, we find cash recipient households shift out of patronage relationships with local politicians, attending fewer rallies (for which they receive small payments), making fewer requests for private support, and receiving fewer offers to sell votes. However, ineligible households increase these relationships. This highlights the importance of poverty in perpetuating the prevalence of patronage politics.

## 1 Introduction

Over the last several decades, a large portion of development economics has focused on identifying interventions that can benefit recipient households and their communities, typically with

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the goal of reducing poverty. Direct cash transfers are one form of aid with a strong evidence base of benefits for recipients (Bastagli, Hagen-Zanker, Harman, Barca, Sturge, and with Luca Pellerano, 2016), and a number of studies have found broader effects for local economies (Egger, Haushofer, Miguel, Niehaus, and Walker, 2019; Bracco, Galeano, Juarros, and Riera-Crichton, 2021; Delius and Sterck, 2020). Cash transfers have dramatically grown as a source of aid from both governments and NGOs in recent years, even moreso in response to the COVID-19 padndemic. However, there is less research on how these programs (particularly NGO-run programs) interact with local institutions and civic and political processes.

What are the direct political impacts of NGO-provided cash transfer programmes targeted at poor and often politically excluded citizens? And what are the effects of an increase in income on how these citizens engage with the state and organise collectively outside of elections? Even programs designed to be independent from governments or politicians may still have effects. On the one hand, rising incomes may engender independence from dominant parties, as wealthier citizens may be less likely to sell votes or participate in clientelistic relationships (Larreguy, Marshall, and Trucco, 2018; Bobonis, Gertler, Gonzalez-Navarro, and Nichter, 2017; Blattman, Emeriau, and Fiala, 2018; De Kadt and Lieberman, 2017). Increases in income may even improve the extent to which citizens demand services and hold politicians accountable (Lipset, 1959), if richer people place more emphasis on self-expression or on programmatic politics (Inglehart and Welzel, 2005; Nathan, 2016). On the other hand, NGO programs could also alter the outcomes of local political systems. If citizens misattribute credit for programs to incumbents, or incumbents are able to (falsely) claim credit for the program, this may insulate incumbents and boost incumbent vote shares (Moss, Pettersson Gelander, and Van de Walle, 2006; Cruz and Schneider, 2017).[1] In addition, the manner in which politicians and candidates engage with voters could also change.

This paper provides experimental evidence on the civic and political effects of a large cash transfer programme run by a foreign NGO. We leverage the randomized assignment of a large, one-time unconditional cash transfer program targeted to poor households across 1,066 villages in western Kenya, which distributed USD 21 million to poor households. In treatment villages, households meeting a basic means test threshold (based on housing quality, a measure of asset wealth) receive a large cash transfer from the NGO GiveDirectly. Around 35 percent of households meet this threshold on average across both treatment and control villages ("eligible" households). The transfer amount (US$2,237 PPP) is approximately equal to Kenya's gross national income per capita. The NGO transferred money directly to recipient house-

[1]Indeed, most government conditional cash transfers have this effect, although they bundle the effect of increased income and the effect of changes in perceptions of the state or incumbents when they successfully deliver a programme (Larreguy, Marshall, and Trucco, 2018).

holds via mobile money, cutting out opportunities for misappropriation by politicians in the transfer distribution process. Companion work shows transfers increase asset wealth, enterprise ownership and revenue, and aspirations for the future in the short to medium run (Egger, Haushofer, Miguel, Niehaus, and Walker, 2019; Orkin, Garlick, Mahmud, Sedlmayr, Haushofer, and Dercon, 2020).

We study the period around the April 2017 primaries and August 2017 election in Kenya. We focus on elections for the Member of the County Assembly (MCA), the lowest level of elected official, who serves roughly 10,000 voters. By chance, in part of our study area (covering 413 villages), cash transfers occur between a year and 4 months before the elections, a period when aspirant politicians were travelling through villages holding hustings, meeting citizens and building support. The remaining villages in this study received transfers one to three years prior to the August 2017 election.

We collect household survey data from over 10,000 households across the 413 villages beginning transfers in 2016-17 between nine and fourteen months after the election. Importantly, we survey both households poor enough to be eligible to receive transfers and wealthier, ineligible households, allowing us to at responses for both groups. Surveys included detailed modules on community, civic and political engagement. We combine these household survey data with surveys of over 1,200 local leaders, covering village elders, assistant chiefs, and local politicans (both successful and unsuccessful aspirants for office). Local leader data comes from the same 413 villages, as well as 653 additional villages that started receiving transfers in 2014-15, and are collected in the five months after the election, covering the pre-election period. We also leverage administrative data on registration and turnout for the August 2017 general election from polling stations within the study area.

We begin by documenting strong relationships between wealth and political and civic participation using our household data in villages that did not receive cash. Eligible households are less likely than ineligible households to vote and less interested in public affairs. Campaigning politicians commonly give small gifts (cash and/or in-kind) in the run-up to the election, openly at rallies, or more secretly just before the election. Supporters make public declarations of support by attending rallies, wearing campaign clothing, or singing at rallies and public gatherings. Poorer, transfer-eligible households are more likely to make public declarations of support, for both the dominant party and independent candidates, although they are no more or less likely to be made offers in exchange for their vote. There are no relationships between wealth and vote choice for MCA or ratings of candidates, suggesting these may be driven more by area of residence. Politicians also commonly grant private requests for support with large expenses like school fees and funerals; poorer households more often approach candidates for private support and receive more support. In addition, poorer households are also less involved in community group activities and fundraisers. When the groups they are members of undertake initiatives, they raise less funding. These initiatives by community groups can provide benefits to their own members, as well as to the community at large.

Our first main finding is that, in our household survey data, receiving a cash transfer does not affect voter turnout or vote choice. We also do not see changes in voter registration or party primary participation. We see similar patterns when looking at administrative data on voter turnout at the polling-station level: polling stations with a higher share of treated eligible households do not see higher turnout rates. A range of attitudinal variables are consistent with this null finding. The transfer does not lead to greater political knowledge or a greater sense of political efficacy. It has no effect on the favourability rating of any candidates. Importantly, voters do not attribute credit for the transfer programme to local leaders (potentially because the NGO runs a very clear sensitisation campaign), nor do leaders attempt to claim credit for the program, allowing us to consider this a relatively pure income shock. In any case, they do not seem to punish or reward politicians for the programme in this context.

Second, households which have received a cash transfer directly decrease exchanges of patronage with politicians. Cash recipients are less likely than control households to attend rallies (for either the dominant party or independent candidates) or publicly demonstrate support at meetings, both of which are common. Cash recipients also make fewer requests to candidates for private support. They are less likely to accept an offer of money in exchange for their vote from any candidate or vote for a candidate from a party which made an offer for their vote. They are less likely to receive an offer, they report receiving fewer offers, and the value of offers they receive declines, suggesting that the decrease in direct vote-buying may be driven by politicians' agents (or brokers) in each community, who make offers for votes. Similar patterns are evident among offers from both the dominant party and independent candidates. There is some evidence of spillover effects on ineligible households in cash villages, who increase rally attendance, demonstrations of support, and requests for support more than ineligible households in control villages and than eligible households in cash villages. While there is no change in whether they receive vote-buying offers, the value of offers increases, suggesting the price of votes may increase.

We then turn to a different dimension of citizen interaction with the state: how people organise into groups to organise and fundraise to meet local needs. We complement the household data with village-level data from village elders (VEs), volunteer public servants who oversee villages, across over 1,000 villages. As the welfare state is limited to pensions and grants to vulnerable children and people with disabilities, many people struggle to pay for large expenses like funerals, medical bills or secondary school fees. Villages often organise informal committees (*harambees*) to raise funds for such purposes by conducting door-to-door collections among villagers (Ngau, 1987; Zhang, 2017; Walker, 2018). Households also participate in self-help groups and rotating savings and credit associations (ROSCAs), which often raise funds through "initiatives" (Kremer and Gugerty, 2008). Both groups and harambees often make requests for funds to prominent people including politicians, especially in the pre-election period.[2]

We find the prevalence of group activities increases in cash villages: village elders report increases in the number of groups and harambees for social purposes, as well as the total amount they raise. These increases are driven by harambees for school fees, funeral expenses, collections for orphans and hospital bills. In part, funds are raised from contributions from local leaders. Cash villages make more requests to local leaders for contributions to group initiatives and private harambees and raise more funds. Funds are also raised from households, mainly driven by households who received transfers. They contribute more to social purpose harambees and hold membership of more groups and their group initiatives raise more than eligible households in control villages. There is no evidence of effects on ineligibles' participation. After the transfer there are no significant differences in participation levels between eligible and ineligible households in cash villages.

Taken together, these results suggest that cash households shift out of "subsistence" political engagement into higher value group engagement, as these group fundraisers raise significantly more than private requests. In contrast, ineligible households increase their engagement in these lower-value activities to some extent.

However, these increases in participation do not extend to initiatives to raise funds for larger public goods, such as public springs and wells, local roads or lighting for markets, at least up to three years after transfers. Villages often fund these either through harambees or through making requests to ward and constituency development funds.[3] We measure all stages of the process of raising funds for a public goods project. The transfer programme causes few changes in whether households report attending or speaking at village meetings (*barazas*) about public goods or public consultations held to solicit public opinion about the allocation of ward development funds in each fiscal year. Households do not alter the frequency or amount of

[2]We define local leaders as people who were candidates in the elections for MCA, either in the general election and/or during party primaries, and their spouses. We also consider contributions made on their behalf via representatives or agents. Village elders often assist households in registering new groups and harambees with the assistant chief, and in making requests to local leaders.

[3]Kenya has undertaken an extensive decentralisation programme. Wards, the most local electoral areas, with roughly 10,000 voters, each receive funds for their Ward Development Fund (WDF), which they can allocate to local projects. The ward's elected MCA appoints a committee to allocate of funds to projects. The constitution mandates public consultations, one per roughly ten villages, on which projects are conducted before the county budget each year.

money contributed to public goods harambees for larger public goods, such as school facilities, water points, roads, clinics or market centres, or labour contributed to public goods projects. VEs report no changes at village level in the number of public goods harambees or in the number of actual public goods projects.[4] Null effects are robust across villages who received transfers two to three years before the survey. However, we are least powered for these variables because there are fewer public goods projects than other forms of local organisation.[5]

Finally, we find few responses by different types of local leader to the transfer programme, other than changes in vote-buying activity. We find no effects on the number of meetings held near treatment villages, using a unique GPS technique to map the location of campaign rallies with VEs. We find little evidence of credit claiming by candidates. Incumbent MCAs do not seem to have targeted treatment or control villages for services: we find no difference in the number of public service consultation meetings held in the village, or in the projects provided. Candidates may have other more important reasons for allocating attention to particular areas, such as where their supporters are. This suggests that the unintended political effects of this particular programme are relatively small. However, more localised political actors, such as agents responsible for voter mobilisation, may respond more to the programme.

We present some of the first evidence on effects on turnout and vote choice of large rule-based NGO cash transfer programmes. There are worries that incumbents may reap benefits from large transfers of aid, which may insulate them from accountability or otherwise distort political outcomes (Moss, Pettersson Gelander, and Van de Walle, 2006). Even when not responsible for the program, politicians may be able to falsely claim credit for aid programs (Cruz and Schneider, 2017). These stem in part from literatures on the political effects of unanticipated income shocks and on the political effects of conditional cash transfer programmes instituted by government, which both suggest that voters' perceptions of incumbents may be highly responsive to income shocks. Voters have been found to hold incumbents responsible for events, good and bad, during their tenure over which they have no control (Kinder and Kiewiet, 1981; Healy, Malhotra, and Mo, 2010), including income shocks (Bagues and Esteve-Volart, 2016). Conditional cash transfer programmes increase turnout and incumbent vote share (Manacorda, Miguel, and Vigorito, 2011; Pop-Eleches, Pop-Eleches, et al., 2012; Labonne, 2013).[6] In contrast, we find a large-scale NGO cash transfer programme has no effect on turnout or vote choice or on mechanisms that might change these outcomes, such as perceptions or

[4]We find increases in the total funding raised for public goods, not through local leaders but potentially through requests to local funds, in some specifications, but these are sensitive to the transformation used.

[5]Walker (2018) finds recipients do not contribute more to local public goods in the short run (1 year after transfers), although control households that earn more income do pay more into public goods.

[6]Of course, effects of such programmes do not only capture income shocks, but may reflect voters rewarding delivery of pure programmatic policies (Golden and Min, 2013) or transfers targeted at supporters (Imai, King, and Velasco Rivera, 2020).

knowledge of candidates. Although this finding is in one context, this suggests programmes may not always have large unintended political consequences. Our related finding that citizens can correctly attribute the programme to the NGO speaks to research on voters' attribution of credit for aid efforts and whether politicians attempt to credit-claim. As in Guiteras and Mobarak (2015) and Larreguy, Marshall, and Trucco (2018), we find that voters are able to correctly attribute credit for the programme (although unlike Guiteras and Mobarak (2015), we do not vary the information level about the programme). Our findings differ from Cruz and Schneider (2017), who find mayors are able to claim credit for bringing programs to their cities, even when they were not involved in the allocation process. In contrast to these papers, we do not find that politicians target more political activities to areas which receive aid to attempt to claim credit.

Second, our work is related to research finding that reducing economic vulnerability via non-cash NGO programmes, such as water tanks, can reduce the prevalence of clientelistic exchanges between households and incumbent politicians (Bobonis, Gertler, Gonzalez-Navarro, and Nichter, 2017; Frey, 2019). Other work finds these effects even from government programmes (Magaloni, 2006; Blattman, Emeriau, and Fiala, 2018; De Kadt and Lieberman, 2017; Larreguy, Marshall, and Trucco, 2018). These findings are important for policy because they suggest further unintended benefits from economic programmes targeted at the poorest.[7] Relative to this research, we offer stronger identification, as only Blattman, Emeriau, and Fiala (2018) and Bobonis, Gertler, Gonzalez-Navarro, and Nichter (2017) are experiments. As in Blattman, Larreguy, Marx, and Reid (2019), our design enables us to test for spillover effects of interventions onto non-recipient households. We find that in this context, cash transfers merely displace offers on to households who do not receive the transfer, rather than reducing overall vote-buying or requests to politicians. This somewhat tempers the conclusion that economic interventions will reduce the prevalence of clientelist practices and lead to more programmatic politics.

Third, we measure many forms of civic engagement, including how households fundraise and organise so they can respond to collective needs and purchase public goods. These measures were based on extensive qualitative work to design questionnaires. In contrast, much research on conditional cash transfers examines participation in formal elections using administrative data on voter turnout and vote choice. Research focused on clientelism and vote-buying tends to capture only individual-level interactions between groups of citizens and politicians. We show that there are two tiers of interaction with politicians. Poorer households make small exchanges of gifts for votes and demonstrations of support. Wealthier households have money to start harambees or to join groups with membership fees. Through these, they request much

[7] These studies find a range of effects on vote choice.

larger donations from politicians for community fundraising efforts or group purposes.[8] Wealth enables households to crowd in more resources. The cash transfer shows that income changes may *cause* households to "upgrade" from one type of exchange with politicians to another, rather than simply decreasing exchanges altogether, providing a more nuanced picture of the effects of the programme.

The paper is structured as follows: Section 2 describes the institutional context. Section 3 covers the treatment and experimental design. Section 4 describes the data. The estimation strategy is discussed in Section 5. Section 6 presents results and Section 7 discusses and concludes.

## 2 Context

The NGO GiveDirectly provides large, one-time unconditional cash transfers of 1,000 USD (nominal) to the poorest 40 percent of households living in treatment villages (described in detail in the next section). This paper combines the sample from two randomised controlled trials which randomly assign villages to this programme. One trial, "Promoting Future Orientation Among Cash Transfer Recipients", examines effects of cross-cutting a goal-setting exercise with the cash transfer (Orkin, Garlick, Mahmud, Sedlmayr, Haushofer, and Dercon, 2020); we have survey data from households and VEs for this sample. A second trial, "General Equilibrium Effects of Cash Transfers in Kenya" (GE), examines effects of cash transfers on prices, wages and economic growth in 653 villages (Egger, Haushofer, Miguel, Niehaus, and Walker, 2019), and Walker (2018) looks at public finance effects in this trial. We use data from VEs for this sample as part of this paper.[9]

### 2.1 Location

This study takes place in Siaya and Homa Bay Counties, Kenya, populous rural areas in western Kenya north and south of Lake Victoria. These areas are predominantly Luo, the second-largest ethnic group in Kenya. In data from the 2009 Kenyan census, both counties are at or below the median on available development indicators.

The trials run in geographical areas in close proximity, as shown in Figure 1. Siaya County, in the left panel, is to the north of Lake Victoria. Two thirds of the aspirations trial villages are in the southern part of Siaya, while GE study villages are in the northern part of Siaya. (Figure 1).[10] The rest of the goal-setting study is in Homa Bay County, in the right panel,

[8]Respondents argued that requests made by groups or harambees are also more likely to succeed than those from individuals, which we can test in future request-level analysis.

[9]Trial registries are at `https://www.socialscienceregistry.org/trials/505` and `https://www.socialscienceregistry.org/trials/996` respectively.

[10]After the aspirations and goal-setting trial, GiveDirectly began running the Universal Basic Income exper-

south of Lake Victoria. GiveDirectly sequentially sent transfers for the GE trial and then the aspirations trial (Appendix Figure 2).

### 2.2 The structure of government

Kenya undertook extensive decentralisation starting after the ratification of its 2010 constitution. Wards, which are the most local electoral areas with roughly 10,000 voters each, were created.[11] Each ward receives unallocated funds for their Ward Development Fund (WDF), which can be allocated to local projects.[12] The ward's voters elect a Member of the County Assembly (MCA), who sits in the County Assembly, appoints a committee to allocate funds to projects, and is expected to consult with their constituents about priorities for development projects.

Since colonial times, the national government has run a highly decentralized network of local officials tasked with administering rural areas. The MCA does not supervise the lower levels of civil servants – chiefs, assistant chiefs (ACs) and village elders (VEs) – who report to the national government. Chiefs manage locations (the next level of administration below counties), while assistant chiefs manage sub-locations. Chiefs and ACs are full-time, appointed positions paid for by the national government. Villages are overseen by a VE, an unsalaried position appointed by the AC (sometimes in consultation with members of the village). There are 36 sub-locations in the aspirations sample (118 across both studies), containing an average of 11 villages in the study (the minimum is 4 villages and the maximum is 27). Villages contain a mean of 100 households in this sample.

ACs and VEs work together to oversee their jurisdictions, including key local public goods, such as public springs and wells, water tanks, public latrines, lighting for markets, cattle dips or terraces to prevent soil erosion. ACs and VEs do not receive a dedicated budget from the government for public goods within their localities, so they must either find external funding or raise money from households within their jurisdiction. To raise external funding, village leaders or groups of villages can solicit funding from government development funds or NGOs.

iment (UBI) in the area between the two studies in Siaya. UBI study sub-locations are effectively untreated at the time of this study's data collection. UBI had started censusing but had not informed villages about the programme or begun distributing transfers during the time of the surveys.

[11] Our sample contains roughly 1,100 villages spread over 25 wards with 25 MCAs.

[12] In addition, each Member of Parliament (MP) has a Constituency Development Fund (CDF). This fund is larger, but tends to fund fewer, larger projects.

### 2.3 Types of civic and community participation

The following sections outline the system of government and how villages engage with local leaders and government structures, both around issues which concern them, and if they wish to improve the level of provision of services or public goods available. They also describe community groups or organisations which fulfil specific purposes or provide specific services for the benefit of members of the community. Our main outcomes capture households' participation in, or interaction with, these various structures and the outcomes of this engagement.

#### 2.3.1 Local democratic participation

Kenyan elections were most recently held in August 2017, shortly after the last villages in our sample had received cash transfers (timelines are shown in Figure 2 and discussed in Section 3). Elections are held at the same time for six different races: the president, senator, governor, women's representative, MP and MCA.

We focus on elections for MCA because these are least likely to be affected by electoral or party dynamics at the national level and are most focused on local issues. MCAs administer the large, ward-level budget and have say over the county budget; therefore, there are strong incentives for voters to hold them accountable and evaluate their performance in office. According to qualitative data collected soon after the election, campaigns for MCA did not centre on political party concerns, but focused on the particular candidates in the race and on service delivery and allocation of funds at the local level.

Voters in this area overwhelmingly vote for a single political party for higher offices. The study takes place across three constituencies and all of these were decisively won by one party's candidates for president, senator and MP. In contrast, there is meaningful variation in competition between wards in MCA races, both during the primary contest for the party nomination, and in the general election between the dominant party candidate and independent (or other party) candidates. While dominant party candidates selected in the primary win all the races for MCA in our sample, victory margins in the 25 wards in the sample range from 2 to 95 points, with a median of 21 points. By contrast, in the presidential race, average ward-level vote share of the leading candidate was 99%, with 82% turnout.

Candidates typically hold numerous hustings, or meetings to address potential voters, in villages in support of their campaign. In addition to registering and voting, citizens can express democratic opinions by attending these meetings, wearing a campaign t-shirt publicly, dancing in support of a candidate at a meeting, or acting as a local agent or representative for a candidate.

Voter registration drives were conducted in January and February 2017, during the period of time in which transfers were beginning to go out to villages. These drives aimed to lower the time and effort costs of registration by bringing mobile units closer to many voters. However, voters could continue registering until May 7, at which point in time all villages had started to receive transfers (Figure 2). Households are not assigned to a specific polling station, but rather than can choose to register at the polling station of their choice.

#### 2.3.2 Individual interactions with local leaders

Households may engage (or attempt to engage) in private exchanges with local leaders. Households facing a shock requiring spending (a funeral, an illness) or requiring a large lump sum (particularly school fees) often request support from prominent local people, including candidates for the office of MCA.

#### 2.3.3 Community groups

Households in rural Kenya are extensively involved in groups (*chamas*) (Kremer and Gugerty, 2008). These include rotating savings and loan groups, merry-go-rounds or table banking groups, and hybrids of these models. Typically, households make contributions to a shared pot each cycle, which is given to each member in turn. But groups can also give loans and serve mutual insurance functions. They often jointly own physical assets or cash savings and share income earned from them. Physical assets such as tractors for ploughing or brick making machines can serve as semi-public goods for the use of group members.

Groups often undertake initiatives, where they gather contributions to purchase assets, undertake projects or build facilities. Funds are raised from group members, and the projects they undertake can be specifically for group members or for the community at-large. They often make requests to prominent people and sources such as the CDF, the WDF or NGOs. Groups also make joint requests to local leaders (Kremer and Gugerty, 2008).

#### 2.3.4 Participation in village meetings

We capture civic participation and whether respondents speak at two types of meetings. First, VEs call regular village meetings (*barazas*). Often, villagers request that barazas are called to discuss issues of concern to the village (such as disputes between village members about grazing rights or the maintenance of public goods like water pumps). They convey information from the government to households. They also provide a forum for NGOs working in the village to meet with villagers. Households that attend these meetings typically have an opportunity to provide opinions and to engage with community issues and priorities.

Second, there are constitutionally mandated consultation meetings between the MCA or ward administrator and villages, to consult on priorities for spending of the Ward Development Fund (WDF). Both counties in our study have established WDFs for all wards through bills.[13] Both bills state that each WDF should be run by a WDF Committee of at least 7 people. Each county must finalise an Annual Development Plan between October and February to pass a budget by the end of June. MCAs, together with ward administrators, must hold meetings in each sub-location in their ward to consult villages on their priorities before the allocation of ward funds. Consultation meetings in every sub-location should be held within the first year of a new assembly, and after that should be held every two years. After these meetings, the WDF Committee should decide on which projects will be implemented. We verified that such committees were constituted and meetings were occurring within each ward in our sample. We thus consider the level of participation in WDF consultations as an indicator of civic engagement outside elections.[14]

Groups of households may also raise money for public goods outside of this process, by making requests, either through their VE or on their own, from local leaders, CDFs and WDFs, other levels of government (such as by approaching the county directly) or NGOs. We capture information on these in the village-level data. (Our module on formal community groups (described in Section 2.3.3) also picks up some of this activity).

### 2.3.5 Local fundraisers (*harambees*)

Citizens also raise funding for public goods or transfers to needy households by collecting money among themselves or asking for transfers from local leaders. We capture household participation in different structures to raise funding, as well as whether these structures make requests to local leaders. We focus on requests to candidates in the elections for MCA, either in the general election and/or during party primaries, and their spouses. We also consider contributions made on their behalf via representatives or agents.

Households frequently participate in a particular institution of informal taxation known as *harambees*, described in detail in Ngau (1987) and Walker (2018). These public fundraising ceremonies have played a central role in development policy since independence. Harambees are typically held to support a particular project or cause. They can collect funds for public

[13] For Siaya, the bill is at `http://kenyalaw.org/kl/fileadmin/pdfdownloads/bills/2014/SIAYACOUNTYWARDSDEVELOPMENTFUNDBILL2014.pdf`. For Homa Bay, the bill is at `http://www.homabayassembly.go.ke/wp-content/uploads/bsk-pdf-manager/2018/03/Ward-Development-Fund-Bill.pdf`. Siaya provides for the Fund to have 5 percent or more of all the ordinary revenue of the County in every financial year; Homa Bay provides for a number less than 20 percent.

[14] During data collection from October 2017 to March 2018, within each ward in our sample, an average of 88% of ACs in each ward report that a Ward Development Fund and committee exists within their wards and is holding meetings (there are 3 to 11 ACs per ward).

goods, such as sinking a borehole or building a school classroom, or for more private purposes, such as paying school fees (often for several children from the community to attend secondary school) or helping a household facing a shock. Harambees are organised by committees and committee members usually have to make contributions to join them. All harambee attendees are expected to contribute, and invited "guests of honour" are expected to make especially large contributions (Zhang, 2017). Committees may also go door to door to collect contributions. Prominent figures receive many requests to contribute to harambees. In the household survey, we captured the frequency and size of households' contributions to harambees. In the village elder survey, we capture whether harambees organised in a village make requests to local leaders.

# 3 Experimental Design

## 3.1 Cash transfer intervention

In both trials in this study, the programme is randomised at the village level. All households within treatment villages meeting a poverty targeting criterion ("eligible households'') are enrolled in the cash transfer programme, while no households in control villages are enrolled. The magnitude of the transfer is large: at US$1,000 (nominal) this is similar in magnitude to Kenya's Gross National Income per capita ($1,280) and corresponds to roughly 75 percent of annual household expenditure for recipient households. In the year they receive the transfer, households move from the bottom third to the top decile of the income distribution in the village. Direct cash transfer programs have been shown to have positive effects on recipient households on a wide variety of economic measures, such as expenditure, asset holdings and food security, (e.g. Arnold, Conway, and Greenslade, 2011; Evans and Popova, 2014; Banerjee, Hanna, Kreindler, and Olken, 2017), including in part of this county (Haushofer and Shapiro, 2016).

In both studies approximately 30 to 40 percent of households in a village meet the eligibility criteria, although the eligibility criteria used by GiveDirectly differ slightly between the two studies.[15] Prior to working in a village, GiveDirectly informs assistant chiefs and village elders, and holds a village meeting (*baraza*) with all households within the village to introduce their program and organisation. GiveDirectly emphasises that the transfers are from an independent NGO, and not the result of any government program, likely limiting the ability for credit-claiming by other sources. The eligibility criteria were not disclosed, although households were

[15]For the GE trial, every household with a thatched-roof living in one of the treatment villages is eligible for the cash transfers. For the goal-setting trial, households have to fulfil at least one of the following criteria: the household's per capita housing space is less than 62,000cm$^2$, it has no telephone AND has a mud floor, the households head is a widow AND the housing has a mud floor, or the household has an orphan child or it is homeless. The household data used in this paper is mainly for households in the goal-setting sample.

told that poorer households will be targeted.

A team of GiveDirectly field officers then returns to conduct a census to collect variables on targeting criteria to determine eligible households. On a different day, another GiveDirectly team is given a list of eligible households for the village. They confirm the household is eligible. If they are, they give the household information on the programme (including the transfer size and timing and that no conditions are attached to the transfer use). They then registered the household for the programme, if the household consented. Transfers were offered to the household as a whole, although whichever household member is at home usually signed up to receive the transfer via M-Pesa. In roughly 86% of households in the goal-setting sample, the woman is the recipient. Households were asked to register for M-Pesa, a mobile money transfer service used to send the transfers. Registration can be done at a network of agents in most small stores. They can receive a mobile phone if they do not have one, with the cost taken off from the transfer amount.

All registered households were backchecked to confirm eligibility in advance of the transfers going out.[16] Households were sent three mobile money transfers, made in intervals of approximately two months: a small transfer ("Token") of approximately USD100 (nominal 2016 dollars); a large transfer ("Lump Sum A") of approximately USD500; and a second large transfer ("Lump Sum B") of USD500 minus the price of the mobile phone. Transfers were typically sent at one time per month to all households scheduled to receive transfers. There is a GiveDirectly helpline that recipients can contact in case of problems.

The initial transfers were rolled out between September 2014 to August 2015 for the GE trial and from November 2016 to June 2017 for the goal-setting trial. For the transfers that went out in 2014-2015, six months elapsed between the two major lump-sum payments (A and B), while for the transfers made in 2016-2017, GiveDirectly shortened the period between the two instalments to two months.

### 3.2 Assignment of villages to treatment

Randomization of treatment at village level was conducted separately by the two study teams. Using GiveDirectly's administrative data, we can verify that in all villages assigned to treatment, households received cash transfers. Eligible villages in both studies have a roughly 50% probability of being assigned to receive cash transfers. In the goal-setting villages, Orkin, Garlick, Mahmud, Sedlmayr, Haushofer, and Dercon (2016) evaluate another, psychological, intervention and its effect when combined with the cash transfer. For the purposes of this study,

[16] These 'backchecks' were conducted on everyone to confirm eligibility. In addition, another audit on a sub-sample flagged for checks was conducted to confirm eligibility.

we focus only on the cash transfer treatment, controlling for assignment to the pychological intervention. Because of this, at times we make comparisons within villages that received a "placebo" psychological intervention (placebo villages).

In Egger, Haushofer, Miguel, Niehaus, and Walker (2019), ex-ante there is a roughly 50 percent probability of a village being assigned to treatment across the sample, but the probability that villages are assigned to receive treatment varies by sub-location.[17]

GiveDirectly did not census control villages. Thus, in both studies, an independently-administered census questionnaire (conducted by Innovations for Poverty Action (IPA) enumerators) replicated GiveDirectly's census process to identify households whom GiveDirectly would identify as eligible in control villages. The questionnaire and method of administration were replicated exactly. GiveDirectly administered training to the IPA enumerators and IPA enumerators shadowed the GiveDirectly team during their training so that they knew how GiveDirectly field officers made judgements in difficult cases. IPA census data form the basis of village-level measures.[18] The duplicate censuses in the same village show that GiveDirectly criteria are objective and replicable. In the goal-setting study, household eligibility status was the same for over 98% of households. This suggests there is no opportunity for outside interference in allocation of transfers.

In some cases, individual households whom our enumerators classified as eligible for treatment are not treated. Households may be delineated differently by GiveDirectly and the IPA survey team, as is discussed in PAPs and papers for both studies, or criteria may be interpreted differently. Second, in the GE villages, there are some cases where village boundaries reported by local leaders are different for GE and GiveDirectly, so there are sometimes households within a control village who were treated by GiveDirectly. Due to this issue, in Aspirations, village boundaries were mapped by the IPA survey team and these boundaries were used by both IPA and GiveDirectly. But this occurs very rarely. As outlined in Section 5, our primary specification is an intent to treat (ITT) analysis based on a village's assigned treatment status.

## 4 Data

This paper primarily makes use of two sources of original survey data collection. First, we utilize a household survey module administered to both cash transfer eligible and ineligible

[17] Haushofer, Miguel, Niehaus, and Walker (2014) generate variation to identify spillover effects by randomly assigning sub-locations (or in some cases, groups of sub-locations) to high or low saturation status.Within high saturation groups, 2/3 of villages are randomly assigned to treatment status, while within low saturation groups, 1/3 of villages are randomly assigned to treatment status.

[18] Haushofer, Miguel, Niehaus, and Walker (2014), Walker (2018) and Orkin, Garlick, Mahmud, Sedlmayr, Haushofer, and Dercon (2016) describe the process of enrolment in the cash transfer programme in detail.

households in the goal-setting trial sample of 413 villages. Second, we make use of survey data from village elders (VEs) across both the goal-setting and GE trials. We also outline several additional datasets that we combine with these main survey measures.

### 4.1 Household surveys

Our main data source for outcome variables covered in this plan is baseline and endline household surveys. We partnered with the aspirations trial (Orkin, Garlick, Mahmud, Sedlmayr, Haushofer, and Dercon, 2019) and integrated a module on civic engagement and group participation in their endline household survey with all eligible and ineligible households surveyed. We also use data on demographic and economic characteristics from the baseline survey. Respondents were surveyed in their homes.

Overall, the household sample includes 7,298 eligible households and 2,816 ineligible households at endline. We tracked respondents who had moved according to a detailed protocol covered in Orkin, Garlick, Mahmud, Sedlmayr, Haushofer, and Dercon (2019). If households had split or the original respondent was living away for the duration of the survey period, we attempted to survey both the original respondent (by tracking them) and a proxy respondent from the remaining household (often the respondents' spouse). As both respondents were eligible for the cash treatment and now constitute separate households, we use both respondents' data as separate observations. If the respondent had died, we identify the most "knowledgeable person" who could serve as a proxy and use their data.

### 4.2 Village elder data

We conducted a separate round of surveys with VEs, described in detail in Orkin and Walker (2018). We invited VEs to a central location, the assistant chief's compound, and followed up with those that were not able to attend. Overall we had high tracking rates, surveying 1,095 from a sample frame of 1,111 village elders.

In addition to village-level outcomes, we collected general information used for constructing the household survey from a group of VEs in each sublocation. VEs helped the field officers to obtain a list of names of MCA candidates who ran in the party primaries and the general election. We further collected information about each individual candidate's party affiliation, whether they were successful, and if possible the number of votes they received. Combining this information with administrative data enabled us to construct a list of MCA candidates in each electoral ward, which we used to provide choice lists for the questions about candidates in the household survey.

### 4.3 Additional data sources

The design and development of our surveys was informed by both administrative data, as well as data from successful and unsuccessful MCA candidates.

Administrative data sources were used in several ways for the household survey. First, we use administrative data to match the respondent's location to their electoral ward. The National Assembly Constituencies and County Assembly Wards Order (IEBC, 2012), which outlined the constituency and ward boundaries, provides a mapping of sublocations to electoral wards. If we had doubts whether particular sublocations remained part of the ward they were allocated to, we double checked with local representatives. Second, having obtained a list of relevant wards, we were able to use electoral information at the ward level in the survey. We used the following resources:

1. Full results from the 2013 general elections. These enabled us to identify the former MCA in each ward.[19]
2. List of candidates who competed in the 2017 general election for the position of MCA and their parties (Kenya Government Press, 2017a). This enabled us to use the official lists for survey questions which used choice lists of local candidates.
3. Winning MCA name and vote share in the 2017 general election (full results including losing candidates were not available at time of surveying) (Kenya Government Press, 2017b). These enabled us to match respondents to the MCA heading their ward, which we use in some survey questions.
4. Finally, we were able to ask respondents to name their polling station from a choice list consisting of all polling stations in their ward (Kenya Government Press, 2017c).

We also used the list of candidates to determine a sampling frame to interview candidates for the MCA position to collect data on their knowledge about GD transfers, the financial support for private and community causes they provide and their election campaigning activities. The full list consisted of 213 candidates across the 25 wards in the study: 195 in the GE sample and 118 in the aspirations and goal-setting sample.

From this complete list, we identified 74 candidates for our sampling frame by including the current MCA (elected in 2017), the former MCA (elected in 2013 or a bi-election since) and the main challenger in the 2017 race.[20] In some wards, where the 2017 election was particularly

[19]The results can be obtained from the Independent Electoral and Boundaries Commission (IEBC). The results can be found at this link: https://www.iebc.or.ke/resources/?Publications

[20]We identified this candidate with the support of Village Elders. We found no publicly available official records to consult on either the primary or the general election result at the time of sampling. We consider a candidate to be the main challenger if they were the closest competitor to the winner in either the primary or the general election. This could be in terms of votes gained (as recalled by the VEs) or their activeness in

close, additional notable candidates were also sampled. In total, 65 of the approached candidates completed a full survey. In some cases, we are also able to glean public information about candidate demographics, which we use to fill in data that would otherwise be missing.

We also make use of election returns for the August 2017 general election and from data scraped from the IEBC website at the polling station level.[21] We can use these to compare with turnout rates from our self-reported data, and to explore whether we see effects by treatment intensity around polling stations or within wards.

### 4.4 Timeline of activities

Figure 2 shows the calendar timeline of household and VE fieldwork as part of this project, as well as the timeline of transfer starts, primary elections, and the August 2017 general election. We designed our surveys to include recall periods that cover the primary and general election period (April to August 2017). Note that, in the specifications that follow, our main focus is on estimating average effects during these periods; future work will look into timing effects in more detail.

## 5 Estimation

### 5.1 Household data specifications

We make three main comparisons by pooling household data for eligibles and ineligibles:

1. Eligible households in cash villages versus eligible households in placebo villages
2. Ineligible households in cash villages versus ineligible households in placebo villages
3. Eligible households in cash villages versus ineligible households in cash villages

In addition, we also compare eligible to ineligible households in placebo villages, as a measure of differences in absence of treatment

Our main estimating equation for household data pools data across eligible and ineligible households and estimates:

$$Y_{iv} = \text{Cash}_v \cdot \beta_C + \text{Elig}_i \cdot \beta_E + \text{Cash}_v \cdot \text{Elig}_i \cdot \beta_{CE} + \mathbf{W} + \mathbf{X_{iv}} \cdot \Gamma + \epsilon_{iv}. \tag{1}$$

campaigning and community issues. The VE often learned results from notes pinned to polling stations shortly after the election. Where possible, the approximate vote share of each MCA candidate mentioned during the meetings with VEs was calculated based on the vote count of each candidate reported by village elders and the number of voters who registered for the general election according to official figures from the IEBC.

[21] These were provided by Andy Harris, and cover the presidential and governor races for polling stations within our study area.

Here, $i$ and $v$ index individuals and villages; $Y_{iv}$ denotes the outcome of interest measured in the follow-up; $Cash_v$ is an indicator variable equal to one for villages assigned to receive cash, and $\text{Elig}_i$ denotes an indicator for eligible households. $\mathbf{W}$ is a vector of ward fixed effects and $\mathbf{X}_{iv}$ is a vector of additional control variables, including controls for the cross-randomized psychological intervention and additional covariates selected via a LASSO procedure as pre-specified (Orkin and Walker, 2020), specifically household size, years of schooling of primary female, number of females aged 16 and above, age of primary female, village road distance to county seat, and month of the endline survey.[22] Households are weighted via inverse probability weights from sampling. Standard errors are clustered at the village level.

Based on the three effects of interest above, our main household tables report i) the effect of cash minus placebo villages for eligible households ($\beta_C + \beta_{CE}$); ii) the effect of cash minus placebo for ineligible households ($\beta_C$); and the effects of being in a cash village for eligible versus ineligible households ($\beta_{CE}$). In the absence of cross-village spillovers, these terms capture the total treatment effects of being in a cash village on each of these groups.[23] Lastly, we report $\beta_E$ to characterize whether eligibles and ineligible households differ in cash and psych control villages (those that received a "placebo" psych video).

### 5.2 Village elder data specifications

When using village elder data, we pool across the villages used in both trials, giving us over 1,000 villages measured roughly 5 months after the 2017 August election. Our main specification estimates the pooled effect of being a treatment versus control village:

$$y_{vw} = \alpha_0 + \alpha_1 T_v + \mathbf{W} + V_v'\gamma + \varepsilon_{vw}. \tag{2}$$

Here, $y_{vs}$ is the outcome of interest for village $v$ in ward $w$ and $T_v$ is an indicator equal to 1 for villages assigned to treatment status. As in Equation (1), we include a vector of ward fixed effects $\mathbf{W}$ to capture characteristics of the specific geographic area or specific race for MCA (such as competitiveness and levels of spending).[24] We include a vector of village-level controls $V_v$ to improve statistical precision, selected following a LASSO procedure as with the household specification. Village-level covariates are VE characteristics (age, education level, experience) and village characteristics (population, share of eligibles, distance to major town and seat of local government, number of months since the cash transfer). We calculate robust standard

[22] A more detailed investigation into psychological effects will be the subject of future work.

[23] Future work will explore spatial spillovers following the procedure of Egger, Haushofer, Miguel, Niehaus, and Walker (2019), as well as any psych-related effects.

[24] We do not collect outcomes at baseline.

errors, as treatment is randomized at the village level.[25]

### 5.3 Polling station specification

As polling stations encompass multiple villages and are thus not randomized, we need to translate the village-level randomized assignment into a measure of treatment intensity. We begin by doing this in the goal-setting study area.[26] Our household surveys collected information on the polling stations that respondents reported voting at: using administrative records on polling stations in our study area (and surrounding areas), we developed a choice list of potential polling stations that respondents selected from. Almost all respondents were able to provide the polling station at which they voted, creating a mapping between households and polling stations.

We use this to generate a measure of treatment intensity as the fraction of (surveyed) eligible households that are treated that report voting at a particular polling station out of all (surveyed) eligible households that report voting at that polling station. Surveyed eligible households are a random sample of all eligible households, so the voting patterns of surveyed households should be representative of voting patterns of all eligible households in the study area. (We check the robustness of our results to restricting to polling stations with large numbers of eligible households reporting to visit). While the density of eligible households around polling stations may differ, the share of eligible households assigned to treatment is exogenous and random based on the village in which the household resides. The level of variation in treatment intensity at polling station level is high, with mean 54.1% and IQR of 52.6%.

We then estimate:

$$Y_p = \alpha + \beta CashIntensity_p + \varepsilon_p, \tag{3}$$

where $Y_p$ is an outcome at the polling station (here, turnout) and $CashIntensity_p$ is our measure of treatment intensity described above. In some specifications, we also include ward fixed effects. We report heteroskedasticity-robust standard errors.

### 5.4 Multiple testing considerations

In order to reduce the number of hypotheses that we are testing, we frequently consolidate multiple outcomes into summary indices of a number of questions. We hypothesize that variables within an index are likely to respond to a treatment in similar ways, in which case collapsing

[25]This follows from Abadie, Athey, Imbens, and Wooldridge (2017), and assumes (as most studies do) that we may have SUTVA violations within (but not between) villages.

[26]Ongoing work incorporates alternative measures that can be implemented for both the goal-setting and GE trials.

outcomes to an index is useful in reducing the number of outcomes tested. We construct indices as inverse covariance-weighted averages following Anderson (2008).[27] For our main coefficient estimates for our primary outcomes, we will present two sets of p-values. The first set is (standard) "per-comparison" p-values, which are appropriate for readers with a particular interest in a specific outcome.

To account for multiple comparisons, we will present an additional set of p-values by taking the following approach. First, when reporting on components of our summary indices of primary outcomes, we will report FDR q-values over the number of components within an index, to help avoid overinterpreting particular component results. We estimate sharpened $q$-values that control the false discovery rate (FDR) across outcomes within each of the families (Benjamini, Krieger, and Yekutieli, 2006).[28] These are presented in brackets in our main tables.

### 5.5 Balance and attrition

Egger, Haushofer, Miguel, Niehaus, and Walker (2019) and Orkin, Garlick, Mahmud, Sedlmayr, Haushofer, and Dercon (2020) show that treatment and control villages are balanced on a variety of measures, and that there was no differential attrition by village treatment status.

## 6 Results

We organize our results as follows: first, we look at electoral and political participation using data from households. This includes voting, public displays of support, and household attribution of the cash transfers. We begin by investigating responses for eligible households, but also look at spillovers onto ineligible households in cash treatment villages. We then explore the effects of cash transfers on community organizing, including community groups and harambees for private (excludable) projects and public goods. Next, we look at participation by households in public goods processes. We finish by looking at how leaders responded to the transfers.

Throughout, we focus on the effects of the cash transfer programme; in both household and village regressions, we control for but do not report effects of the psychological intervention for

[27] We will first re-code all primary outcomes so that higher values correspond to "better" outcomes. We will then standardize the outcomes to have mean zero and standard deviation one in the placebo intervention group. We will calculate the average of the standardized constituent outcomes, weighted by the inverse covariance matrix, and standardize this weighted average to have mean zero and standard deviation one in the placebo intervention group. We will estimate the covariance matrix and hence the weights using only observations that have non-missing values for all outcomes in the index. Where a specific outcome value is missing for a respondent, we calculate the value of the index for that respondent using the remaining outcomes.

[28] Rather than pre-specifying a single $q$, we report the minimum $q$-value at which each hypothesis is rejected. The FDR controls for the proportion of false positives, which is relevant if one is interested in the proportion of the outcomes within a family affected by treatment.

villages in the goal-setting trial. We frequently report summary indices of a set of outcomes for a particular topic, which we construct following Anderson (2008).

### 6.1 Electoral and political participation

Table 1 presents results of Equation (1) for electoral participation and individual vote choice outcomes. For eligible households, we find no difference in actual (self-reported) voting participation in the general election. While self-reported rates of voting in the August 2017 general election are high for ineligible households (95 percent), eligible households in placebo villages are 2 percentage points (p-value $< 0.05$) less likely to vote (column 2, row 4), highlighting potential wealth gradients in the most important variable for electoral participation. We also do not see changes in voter registration or primary participation (Table A.1). We do not see major changes for eligible households in reported electoral support during the August 2017 general election (columns 3 and 4), nor in favourability measured at the time of the survey (post-election, Table A.2) for local candidates, so it does not seem like transfer is changing much in this dimension.

In addition to analysis at the household level, using self-reported data, we are able to check for the impact of cash on voting behaviour using administrative data at the polling station level. Table 2 presents results for impact of the cash transfers programme on turnout in the area from Equation (3). We analyse the impact of varying treatment intensity on percentage turnout at the polling station level. We focus on turnout in the presidential election, as this is the first ballot in the general election and turnout is usually highest for this vote. The results from polling station analysis align with those observed in self-reported data. Regardless of specification, there is no evidence of a relationship between treatment intensity and turnout in the general election.[29]

Taken together, these null results on electoral participation and incumbent support by households receiving cash transfers are an interesting contrast to numerous studies that find higher turnout and more votes for the incumbent (Manacorda, Miguel, and Vigorito, 2011; Baez, Camacho, Conover, and Zárate, 2012; De La O, 2013).[30] However, these findings are from government transfer programmes, whereas this programme was delivered by a non-state actor. In Section 6.4, we show that households do not attribute transfers to politicians, which may

[29] We tested these results for turnout in the governor race and they did not change. We are seeking to obtain MCA election results at the polling station level, which will help us further investigate the most competitive local elections.

[30] Blattman, Emeriau, and Fiala (2018) in Uganda finds that, in a semi-authoritarian context where there are incentives not to reveal opposition support, receiving a cash transfer increased the likelihood of voting for the opposition or actively working to get them elected (but did not change underlying voter preferences)(Blattman, Emeriau, and Fiala, 2018).

help explain these results. This is suggestive evidence that these other findings might reflect support for an incumbent who is able to deliver a programme, rather than changes in electoral participation or support due to increased wealth.

Turning to ineligible households, we also do not see effects on our measure of electoral participation in cash versus non-cash villages, nor do we see significantly differential effects relative to ineligible households (Table 1, columns 1-3, rows 2 and 3). While we do find that, post-election, ineligible households report less favourable ratings of the dominant party MCA candidate (-0.52 on a scale of 1 to 10, p-value $< 0.1$, Table A.2), they also report slightly less favourable ratings of the main opposition candidate (-0.22), making this single point estimate hard to interpret. These changes in ratings do not seem to affect vote choice.

While we see limited evidence that electoral outcomes changed, did the manner in which households participate politically differ? For background, there are many potential opportunities for engagement with politicians, particularly around elections, as we outline in Section 2.3.1. Based on our data, the main modes seem to be 1) going to rallies and 2) displaying support for politicians e.g. by wearing campaign clothing or singing or dancing in public for a candidate's campaign. Agents of the political candidates within villages mobilise households to take part in these activities; households often receive small tokens of appreciation (in money or in-kind) for going to meetings. Displays of support by many people are seen as evidence of a successful and wealthy campaign. We find that, in placebo villages, eligible households are no more likely to attend meetings, but do show significantly more public support than ineligible households (Table 3, row 4).

It is important to note that the "supply" of these events is not solely determined by households, and relies on candidate engagement in these communities. In Appendix Table A.3, we show that there are no significant differences in the number of campaign meetings close to the village reported by village elders, nor in the number of villagers that they report attending these meetings. This lends support to the idea that any differences we find are driven by household decisions regarding participation, rather than opportunities for engagement.

How does an increase in wealth affect these behaviours? Interestingly, eligible households in cash villages see declines in public expressions of political opinions, while ineligible households get more involved (Table 3, rows 1-3). Eligible households are less likely to attend meetings with candidates or publicly show support, while ineligibles are more likely to engage in these activities (columns 2 and 3). This is across the board for both main party and independents, so doesn't indicate change in party support. Cash transfer recipient households may have less need for the money, and thus do not attend as many meetings to get tokens.

While our data does not shed direct light on the mechanisms by which ineligible households become *more* likely to attend meetings and express support for candidates, there are several potential channels that are consistent with our data. One option is that agents recruit them more as recipients are less interested in participating. Another potential explanation, which we turn to now, is that the nature of patronage relationships changes in a way that encourages greater involvement of ineligible households.

As Zhang (2017) documents, local politicians (including MCAs and aspirants for these positions) receive and often grant numerous requests for private support to households. These can entail assistance with school fees, hospital bills and funeral expenses. We hypothesized that households that receive a positive wealth shock may make fewer requests for financial assistance from local leaders.

Table 4 presents results on private requests for support by eligible and ineligible households. While on the extensive margin, eligible households in cash villages are no less likely to have ever made a request than their counterparts in placebo villages, there is a significant decrease in the number of times that households approach candidates for support (column 2, row 1). This pattern holds for both local MCA candidates, as well as for candidates to higher posts (governor, senator, MP). Interestingly, the reduction in the number of requests does not translate into fewer successful requests or lower value of support received, though private request success rates are generally low. As we would expect, in non-cash villages, eligible households make significantly more requests than ineligible households (column 2, row 4, p-value $< 0.05$).

In contrast, ineligible households in cash villages are more likely than their counterparts in non-cash villages to make requests for private support, both on the extensive and intensive margin (column 2, row 2). However, relative to both ineligible households in non-cash villages and eligible households in control villages, ineligible households in cash villages are not more likely to have their requests granted or to receive more money from their requests (columns 5 and 6). Despite their lack of increased success, the increased political participation we see in Table 3 (i.e. greater meeting attendance) may be in hopes of these requests being successful, or due to greater demand for tokens.

In Table 5, we present results on offers made by candidates or their agents in exchange for votes. Our qualitative work suggests that these offers are distinct from private requests for support, which are mostly made for particular expenses, such as school fees or health costs. By contrast, exchanges for votes are not targeted by need. We exclude tokens received from rallies in our measures of vote-buying, focusing instead on (relatively) more explicit quid pro quos. Our primary outcome is the end outcome of the process: the value of all offers respondent accepted in exchange for their vote (estimate). As the total value reflects many stages of the vote buying process, we pre-specified secondary analyses on the stages of the process.[31]

In non-cash villages, we find no differences between poorer and richer households in whether they received any offers in exchange for vote or the value of offers received (Table 5, columns 2 and 3, row 4). Together, this suggests that these offers are not necessarily targeted on poverty.

When offers are made, they are almost always accepted (6.7% of respondents received offer, 6% accepted). Conditional on accepting an offer, the value of an offer is USD PPP 15 (though this is driven by a longer right tail of large offers). Cash eligibles are less likely to receive an offer (column 2, row 1), while cash ineligibles are somewhat more likely relative to ineligibles in cash villages. Offer amounts go down for cash eligibles and up for ineligibles (column 3), with a similar pattern for accepted offers (column 5). Ineligible households in cash villages are more likely to accept offers relative to eligible households in cash villages, and eligible households in cash villages are less likely to accept an offer than eligible households in non-cash villages (column 4).

What could be driving these results? One potential mechanism is that agents have detailed knowledge and know who receives a cash transfer, and are targeting offers on the basis of transfer receipt. Since transfer eligibility is on the basis of housing wealth, and agents typically live in these communities, it is likely that they know, or are able to infer with a reasonable degree of confidence, which households are receiving transfers and which are not. Alternatively, this could also be a function of more ineligible households attending rallies. It could also be that agents perceive ineligible households (especially those that missed out on a cash transfer) to be more likely to make an exchange. Thus, while this intervention appears to have decreased clientelistic demands among the recipients of cash transfers, we have not directly restricted the supply side of clientelism in any way, and it appears that ineligible households are thus slihgtly more likely to engage in and/or benefit from clientelism.

### 6.2 Group and private fundraising

We now turn to involvement in other types of community organizations. First, we look at community participation in excludable/private institutions, namely community groups and harambees designed to benefit specific individuals. As outlined above, these are important institutions in our study area, and can serve important social insurance functions.[32]

In placebo villages, eligible households are less likely to be in groups, contribute to groups, or

[31] These questions were introduced after a first set of villages had been surveyed, leading to slightly smaller sample sizes.

[32] We classify harambees for school fees, funeral expenses, orphans, housing and medical bills as excludable. Public goods harambees include purchases of community farming equipment, and spending on school facilities, health clinics, water points, roads, and market centers.

contribute to harambees (Table 7, row 4). This is consistent with Kremer and Gugerty (2008): there are more esteemed, better quality, richer groups which require higher registration fees and contributions, effectively excluding poorer households. As many of these groups have ROSCA components, this also restricts access to capital, and potential access to social insurance, for poor households.

We first look at treatment effects of cash with our village elder data across both studies. Table 6 shows that treatment villages see increased excludable goods fundraising activity (Panel A, columns 1-3), with increases in the number of community groups, the number of group members in these villages, and the number of private harambees and group initiatives.

Appendix Table A.6 shows a breakdown of types of group initiatives taking place in each village. Group initiatives are primarily organised for income generating activities, which can be either directly related to the group's purpose (e.g. purchasing farming equipment) or separate (e.g. purchasing chairs to hire at events). Overall, income generating group initiatives were reported in 30.5% villages, with slightly higher rates in cash villages (see Appendix Table A.6). Groups can also raise funds for private needs, either for members only or for the community as a whole (e.g. school fees for children). These kinds of initiatives were organised in 8% of villages.

Treatment villages also make more requests, and receive more as a result of these requests, from local leaders (Panel B). Together, this suggest that there are more groups forming, and more groups engaging in activities (their motivation for undertaking initiatives). We do not find differences in these outcomes across studies.

In Table 7, we look at the breakdown by household type using data from households in the goal-setting trial. Eligible households in cash villages are contributing more to private harambees (column 3). Eligible households are also more involved in groups, and in particular have increased their number of group memberships since they began receiving transfers (column 5). The groups they are in also raise more for initiatives (column 6), again suggesting that these may be more active, higher-quality groups that they are able to gain access to. We do not find any major effects for ineligibles (though point estimates are generally positive (row 2), suggesting that the increased activity of eligible households is not coming at the expense of ineligible households).

### 6.3 Public good fundraising

We next turn to fundraising for public goods. As VEs do not receive funding from the government, many projects require raising revenue locally, or successfully petitioning for funding from higher levels of government. We study all parts of the process of getting a public good:

1) meetings at start, 2) public goods harambees, as one step on the way to such projects, and 3) actual goods.

While we cannot directly test the direction of causality, we find that poor households participate less in consultations/meetings in general, and also speak less at meetings (Table 8, row 4). VEs do not report increases in the number of public goods harambees, or number of new project starts (Table 9), and we do not find any effects on participation from household data (Table A.7). We should also note that many of these processes are relatively new, and not well understood even by some people that we would expect to be well-informed. This may make it especially challenging for poor households to get more involved.

This lack of participation effects may be part of the story behind lack of effects on fundraising. There are fewer public goods harambees (mean in control villages = 0.185) than social harambees and initiatives (mean in control villages = 2.537) in same period, and this holds true for all types of public goods. The lack of public goods effects are consistent with Walker (2018), which looks at a non-election period; as with that paper, there are also no large negative effects, which is reassuring.

Finally, in general we see very few public goods projects in this period. Given low levels of participation by these households, it may make sense that, even with the transfer, it is less easy for these households to affect change on these margins. In qualitative work, respondents reported that eligible households were very busy with their own affairs after getting a transfer, for instance working on setting up businesses or constructing houses.

Here is another instance where we do not see responses on politician side. There are no more promises of new projects; no more total funding; no more contributions from outside (Table A.8). This differs from Guiteras and Mobarak (2015), where politicians try to credit claim in communities that get a programme, and end up allocating more services to them.

### 6.4 Attribution of the transfers

We check whether households in the study areas attribute credit for the cash transfers programme to any local politicians using the measures from VE and household data reported in Table 10. GiveDirectly conducted a sensitisation campaign in villages to explain the attribution of donations to foreign donors and to make targeting rules clear and transparent. 90 percent of VEs (n=1,097) report, without prompting, that the transfer comes from a foreign NGO. Although the transfers are discussed by MCA candidates in the 2017 election campaigns in 20 percent of villages, VEs report local leaders claiming credit for them in only 2 percent of villages. We find similar patterns in household surveys in the goal-setting study sample, with fewer than 5% of households reporting any local leaders claiming credit for the programme. The rate of credit attribution is slightly higher at 9.3%; however, the majority of these respondents report the VE played a role, instead of local politicians. Given that some VEs did play a role through liaising between households, communities and GiveDirectly, we are not concerned about this level of credit attribution.

# 7 Conclusion

The relationship between cash transfers and civic and community engagement is of significant academic and policy interest. Numerous papers have identified strong relationships at a macro level between higher income, economic growth and the quality of democracy, service delivery and the rule of law and, conversely, that weak institutions may jeopardise economic growth (e.g. Acemoglu, Naidu, Restrepo, and Robinson, 2014). However, programs to build and strengthen political institutions and encourage citizen engagement, particularly at local level, such as community driven development programmes or decentralization, or to combat clientelism, have had mixed success. CDD programmes have very few benefits for the quality of local political institutions (Casey, 2018). Campaigns to reduce vote-buying may result in spillovers of increased vote-buying in non-targeted districts (Larreguy, Marshall, and Trucco, 2018). A large literature has studied how incumbents may benefit from aid and conditional cash transfers (Manacorda, Miguel, and Vigorito, 2011; Pop-Eleches, Pop-Eleches, et al., 2012; Labonne, 2013; Moss, Pettersson Gelander, and Van de Walle, 2006), yet frequently these conflate both program delivery and income receipt.

We contribute to this literature by providing causal evidence how on a large NGO-run cash transfer programme affects household civic and political participation and household requests for resources from local politicians. Receiving a cash transfer does not affect voter registration, turnout, vote choice, or favourability ratings of candidates. Voters mostly attribute credit correctly for the programme. The null effects on electoral participation and incumbent support may be taken as a positive, as it implies that in this context, the cash transfer program is not influencing electoral outcomes. These transfers provide private benefits for recipient households without distorting the political process, a concern some have about these types of programs. This finding may be context-specific, so we do want to be careful about interpretation, but this may serve as a proof-of-concept for similar NGO programs going forward.

Households who have received a cash transfer decrease private exchanges of patronage with politicians: attending fewer rallies (for which they receive small payments), making fewer requests for private support and receiving fewer offers to sell votes. However, ineligible households in cash villages increase such exchanges. This highlights that interventions that may reduce demand for some types of clientelism may not reduce overall clientelism without also focusing on the supply side. This also highlights the importance of collecting data from households that are both eligible and ineligible for development programs, a relatively unique feature of our paper compared to much of the literature.

We find that cash transfer villages set up more informal fundraisers and groups, make more requests to local leaders and receive more funding. Cash households join more groups and increase their contributions to fundraisers. There are no effects on ineligible households. This suggests poor people's involvement in local processes to raise funds to achieve collective purposes may be limited by the time and monetary cost of participation. As well as reducing poverty, transfer programs may enable increased participation in such processes. While we do not find positive effects on public goods fundraising, importantly, we do not find negative effects nor evidence for disengagement in cash villages.

There are a number of limitations to these results. While an advantage of our shorter-run follow-up period is that we are able to capture electoral outcomes, it may take more time for institutional processes, such as public good fundraising to change. Second, most villages make few requests and start few projects, even over a four year period, and because effects may be small on very noisy variables and we may not have enough power, even with a large sample size. The ideal design might look over more dispersed geographic areas. Third, while an advantage of using survey data to collect electoral outcomes means we have a rich set of additional outcomes and covariates, it requires that we rely on self-reports for registration and voting behavior.

In addition to their academic relevance, these results are highly policy-relevant. Direct cash transfers to poor households, long established as a policy tool in in developed countries, are becoming a favoured method of delivering private and bilateral aid in low- and middle-income countries, reaching up to a billion people globally (Arnold, Conway, and Greenslade, 2011). More broadly, this study also sheds light on politician responses to international NGO aid (Guiteras and Mobarak, 2015). Understanding the interaction between foreign aid and political outcomes, particularly how politicians respond to them, is key to determining whether cash transfers complement or detract from processes of democratisation. Our findings provide suggestive evidence that there may be complementarities in these relationships, and this remains an important area for future research.

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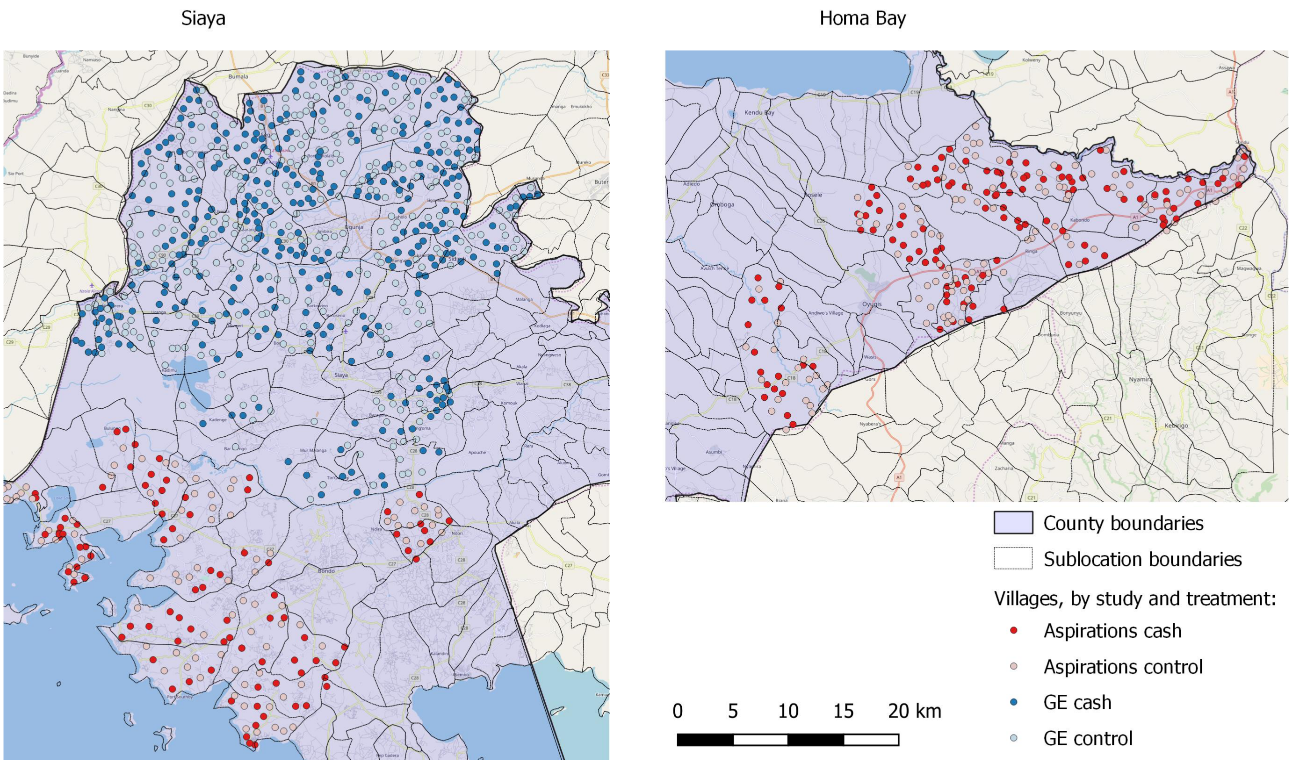
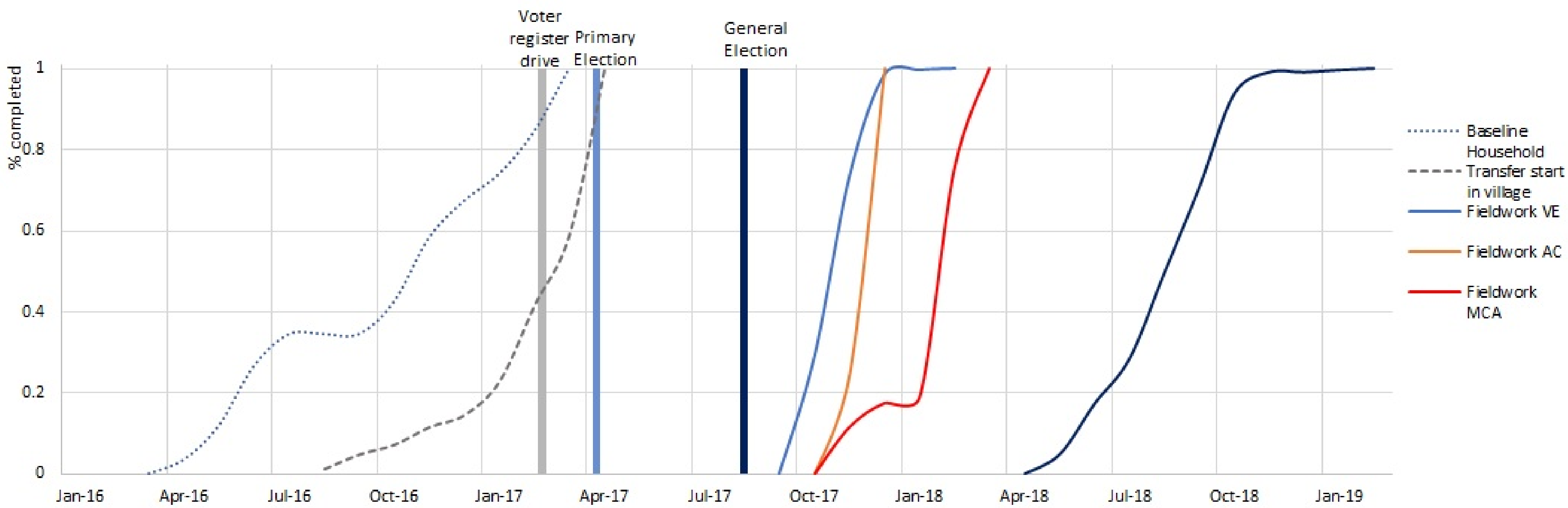
Figure 1: Map of study villages



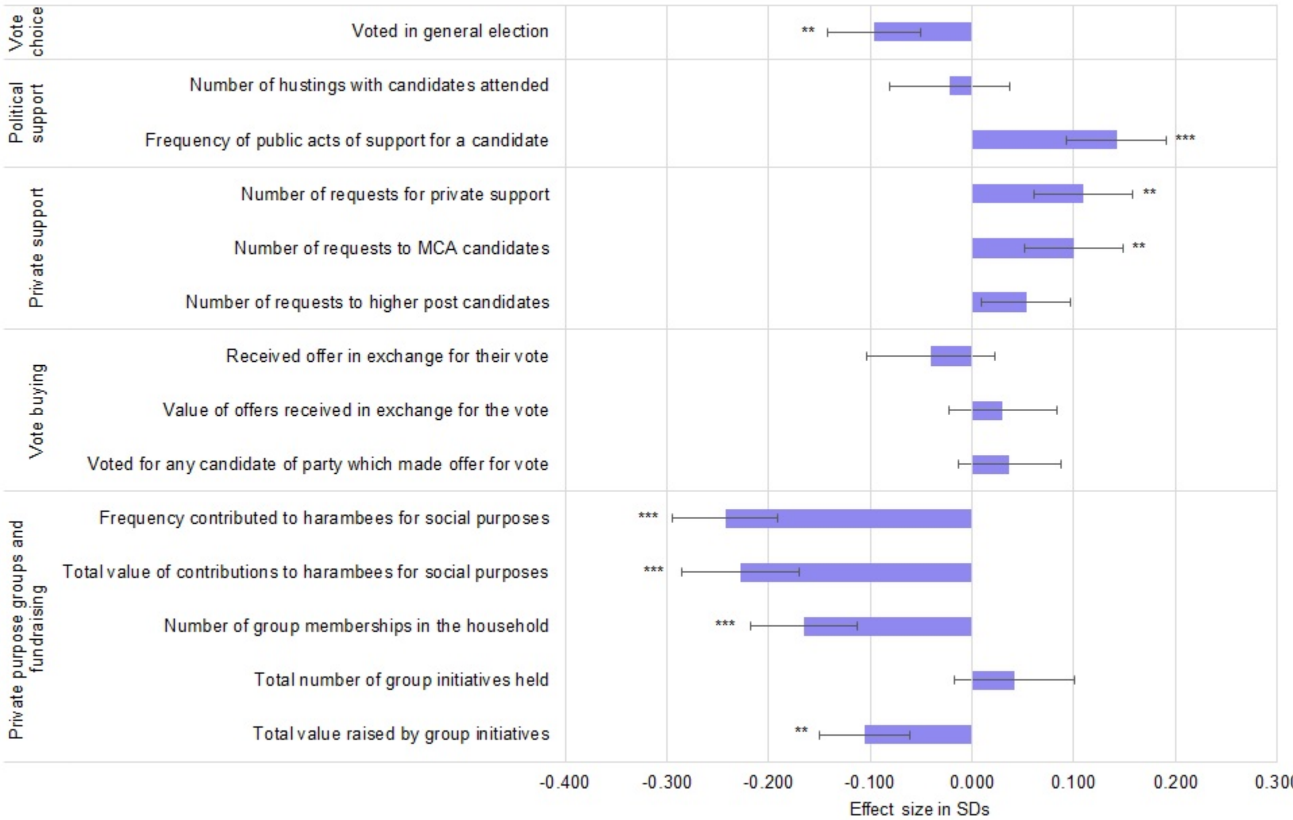

*Notes: This figure plots study villages within Siaya County (left panel) and Homa Bay County (right panel), both of which border Lake Victoria. Villages in the GE study (Egger, Haushofer, Miguel, Niehaus, and Walker, 2019) are all located within Siaya County, and are denoted by blue dots (cash villages) and circles (control villages). Villages in the Aspirations study (Orkin, Garlick, Mahmud, Sedlmayr, Haushofer, and Dercon, 2020) are located across Siaya and Homa Bay counties, and are denoted by red dots (cash villages) and red circles (control villages).*

Figure 2: Project timeline: fieldwork and treatment




*Notes: This figure plots the cumulative distribution functions of fieldwork and transfer start (for the goal-setting trial, transfers for GE villages began in 2014-15), relative to the 2017 Kenyan primary and August general elections. Household fieldwork was conducted in goal-setting villages (413 villages), while village elder (VE), assistant chief (AC) and Member of County Assembly (MCA) surveys were conducted across both goal-setting and GE villages (1,066 villages).*

Figure 3: Political and civic engagement for richer versus poorer households




*Notes: This figure presents differences between outcomes for households that are eligible for the cash transfer (poorer households) versus households that are ineligible for the cash transfer (richer households) living in control villages for the aspirations trial. The solid bars denote coefficient estimates from regressions of an outcome on cash treatment, eligibility status, psychological treatment, and the interactions, a vector of covariates listed in Appendix Table A.9, and ward fixed effects. Whiskers denote standard errors. *** and ** mark 1% and 5% significance.*

Table 1: Cash transfers do not alter turnout or vote choice

| | (1) Summary Index | (2) Voted in general election | (3) Voted for ODM MCA candidate | (4) Voted for ODM governor candidate |
|---|---|---|---|---|
| Cash minus placebo (eligible) | 0.031 | -0.010 | 0.024 | 0.002 |
| | (0.055) | (0.010) | (0.026) | (0.026) |
| | | [1.000] | [1.000] | [1.000] |
| Cash minus placebo (ineligible) | 0.040 | -0.011 | 0.001 | 0.038 |
| | (0.074) | (0.012) | (0.033) | (0.044) |
| | | [1.000] | [1.000] | [1.000] |
| Eligible minus ineligible (cash) | -0.009 | 0.002 | 0.023 | -0.036 |
| | (0.071) | (0.015) | (0.032) | (0.042) |
| | | [1.000] | [1.000] | [1.000] |
| Difference between eligibles and ineligibles in placebo villages | 0.013 | -0.021** | -0.020 | 0.034 |
| | (0.054) | (0.010) | (0.025) | (0.035) |
| | | [0.129] | [0.395] | [0.395] |
| Outcome type | Summary | Primary | Primary | Primary |
| % of households | | 0.935 | 0.692 | 0.629 |
| Villages | 413 | 413 | 411 | 411 |
| Observations | 10084 | 10084 | 8793 | 8963 |

*Notes: This table presents outcomes from household survey data. The summary index in column (1) is an Anderson (2008) index of all primary outcomes in the table. For each outcome, we report the coefficient; the heteroskedasticity-robust standard error, clustered at the village-level, in parentheses; and the sharpened q-values controlling the FDR across all primary outcomes in square brackets. Each regression contains: assignment to cash treatment, eligibility status, assignment to psychological treatment, the interactions of the three; a vector of covariates listed in Appendix Table A.9, and ward fixed effects. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.*

Table 2: Cash transfers into the area do not alter turnout, polling station data

| | (1) | (2) | (3) |
|---|---|---|---|
| | Turnout (% registered voters) | | |
| Treatment intensity | 0.002 | 0.009 | 0.011 |
| | (0.008) | (0.009) | (0.009) |
| Ward FE | No | No | Yes |
| Sample mean | 0.847 | 0.847 | 0.847 |
| No. polling stations | 180 | 164 | 164 |

*Notes: This table presents regressions using a dataset that links administrative data at the polling station level to household survey data. The independent outcome is turnout as a percentage of registered voters, measured for the presidential election. Votes were cast in 5 races overall, but the presidential ballot is the biggest race in terms of turnout. For each specification, we report the coefficient and the heteroskedasticity-robust standard error in parentheses. Each regression contains treatment intensity among the eligible population in the sample. Specification 2 tests robustness to dropping polling stations where the sample size is low. Specification 3 additionally tests for ward fixed effects, as indicated. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.*

Table 3: Recipients reduce displays of support for candidates while ineligibles increase them

| | (1) Summary Index | (2) Number of hustings with candidates attended | (3) Frequency of public acts of support for a candidate |
|---|---|---|---|
| Cash minus placebo (eligible) | -0.122** | -0.246** | -0.152** |
| | (0.047) | (0.121) | (0.066) |
| | | [0.044] | [0.044] |
| Cash minus placebo (ineligible) | 0.141** | 0.321* | 0.157* |
| | (0.065) | (0.184) | (0.085) |
| | | [0.089] | [0.089] |
| Eligible minus ineligible (cash) | -0.263*** | -0.568** | -0.308*** |
| | (0.080) | (0.226) | (0.107) |
| | | [0.009] | [0.009] |
| Difference between eligibles and ineligibles in placebo villages | 0.074 | -0.058 | 0.208*** |
| | (0.053) | (0.153) | (0.072) |
| | | [0.545] | [0.008] |
| Outcome type | Summary | Primary | Primary |
| % of households | | 0.552 | 0.341 |
| Conditional placebo mean | -0.000 | 3.54 | 2.13 |
| Clusters | 413 | 413 | 413 |
| Observations | 10108 | 10087 | 10108 |

*Notes: The summary index in column (1) is an Anderson (2008) index of all primary outcomes in the Table. For each outcome, we report the coefficient; the heteroskedasticity-robust standard error, clustered at the village-level, in parentheses; and the sharpened q-values controlling the FDR across all primary outcomes in square brackets. Each regression contains: assignment to cash treatment, eligibility status, assignment to psychological treatment, the interactions of the three; a vector of covariates listed in Appendix Table A.9, and ward fixed effects. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.*

Table 4: Recipients make fewer private requests for support while ineligibles make more

| | (1) Summary Index | (2) Number of requests for private support | (3) Number of requests to MCA candidates | (4) Number of requests to higher post candidates | (5) Any successful request for private support | (6) Total value received from all candidates |
|---|---|---|---|---|---|---|
| Cash minus placebo (eligible) | -0.094 | -0.096*** | -0.076** | -0.023** | -0.005 | -0.387 |
| | (0.067) | (0.036) | (0.033) | (0.011) | (0.009) | (1.018) |
| | | [0.037] | [0.045] | [0.056] | [0.397] | [0.392] |
| Cash minus placebo (ineligible) | -0.015 | 0.094* | 0.036 | 0.056** | -0.003 | 1.449 |
| | (0.046) | (0.048) | (0.039) | (0.024) | (0.014) | (1.496) |
| | | [0.121] | [0.572] | [0.121] | [1.000] | [0.376] |
| Eligible minus ineligible (cash) | -0.078 | -0.190*** | -0.112** | -0.079*** | -0.002 | -1.837 |
| | (0.083) | (0.054) | (0.044) | (0.026) | (0.015) | (1.659) |
| | | [0.003] | [0.012] | [0.005] | [0.551] | [0.156] |
| Difference between eligibles and ineligibles in placebo villages | 0.072 | 0.087** | 0.072** | 0.017 | 0.005 | 0.019 |
| | (0.083) | (0.039) | (0.035) | (0.014) | (0.011) | (0.054) |
| | | [0.111] | [0.111] | [0.287] | [0.592] | [0.592] |
| Outcome type | Summary | Primary | Primary | Primary | Primary | Primary |
| % of households | | 0.161 | 0.138 | 0.037 | 0.060 | 0.066 |
| Conditional placebo mean | 0.001 | 1.72 | 1.59 | 1.65 | 1.00 | 73.1 |
| Clusters | 413 | 413 | 413 | 413 | 413 | 413 |
| Observations | 10107 | 10099 | 10099 | 10099 | 10107 | 10091 |

*Notes: The summary index in column (1) is an Anderson (2008) index of all primary outcomes in the Table. For each outcome, we report the coefficient; the heteroskedasticity-robust standard error, clustered at the village-level, in parentheses; and the sharpened q-values controlling the FDR across all primary outcomes in square brackets. Each regression contains: assignment to cash treatment, eligibility status, assignment to psychological treatment, the interactions of the three; a vector of covariates listed in Appendix Table A.9, and ward fixed effects. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.*

Table 5: Recipients receive fewer offers for their vote

| | (1) Summary Index | (2) Received offer in exchange for their vote | (3) Value of offers received | (4) Accepted offer in exchange for their vote | (5) Value of accepted offers | (6) Voted for party which made offer |
|---|---|---|---|---|---|---|
| Cash minus placebo (eligible) | -0.099** | -0.022** | -0.110** | -0.023** | -0.089* | -0.014* |
| | (0.046) | (0.009) | (0.055) | (0.009) | (0.054) | (0.008) |
| | | [0.052] | [0.052] | [0.052] | [0.063] | [0.052] |
| Cash minus placebo (ineligible) | 0.159* | 0.011 | 0.267* | 0.018 | 0.236** | 0.029* |
| | (0.093) | (0.020) | (0.142) | (0.017) | (0.110) | (0.016) |
| | | [0.313] | [0.128] | [0.172] | [0.128] | [0.128] |
| Eligible minus ineligible (cash) | -0.258** | -0.033 | -0.377** | -0.041** | -0.325*** | -0.043** |
| | (0.105) | (0.021) | (0.156) | (0.019) | (0.126) | (0.017) |
| | | [0.049] | [0.028] | [0.028] | [0.028] | [0.028] |
| Difference between eligibles and ineligibles in placebo villages | 0.026 | -0.009 | 0.043 | 0.003 | 0.065 | 0.007 |
| | (0.055) | (0.014) | (0.076) | (0.013) | (0.066) | (0.009) |
| | | [1.000] | [1.000] | [1.000] | [1.000] | [1.000] |
| Outcome type | Summary | Primary | Primary | Primary | Primary | Primary |
| % of households | | 0.051 | 0.043 | 0.045 | 0.039 | 0.033 |
| Conditional placebo mean | -0.000 | 1.00 | 5.76 | 1.00 | 5.40 | 1.00 |
| Villages | 383 | 383 | 383 | 383 | 383 | 383 |
| Observations | 9278 | 9278 | 9277 | 9278 | 9278 | 9278 |

*Notes: The summary index in column (1) is an Anderson (2008) index of all primary outcomes in the Table. For each outcome, we report the coefficient; the heteroskedasticity-robust standard error, clustered at the village-level, in parentheses; and the sharpened q-values controlling the FDR across all primary outcomes in square brackets. Each regression contains: assignment to cash treatment, eligibility status, assignment to psychological treatment, the interactions of the three; a vector of covariates listed in Appendix Table A.9, and ward fixed effects. 7. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.*

Table 6: Cash villages begin more groups and fundraisers, request and raise more from local leaders

| | Community Groups | | | | Raising External Funding | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Number of community groups | Number of group members across all groups | Number of private purpose harambees and group initiatives | Amount raised by groups and private purpose harambees | Number of requests to local leaders for group initiatives and private harambees | Amount raised from local leaders for groups and private purpose harambees |
| Treatment village | 0.377** | 6.332* | 0.254** | -864.412 | 0.470*** | 3682.861** |
| | (0.177) | (3.440) | (0.120) | (9764.713) | (0.175) | (1484.150) |
| | [0.074] | [0.074] | [0.074] | [0.303] | [0.014] | [0.014] |
| Control group mean | 4.472 | 57.285 | 2.537 | 109087 | 1.711 | 12678.514 |
| No. observations | 1092 | 1009 | 1094 | 1094 | 1094 | 1094 |

*Notes: Outcome variables are listed at the top of each panel. Column 4 coefficient is an IHS transformation. Panel A focuses on group and fundraising activity, while Panel B looks at raising funds from external sources. For each outcome, we report the coefficient; the heteroskedasticity-robust standard error in parentheses; and the sharpened q-values controlling the FDR across outcomes in square brackets. Each regression contains village cash treatment assignment, a vector of covariates and ward fixed effects. Covariates for village-level outcomes are listed in Appendix Table A.10. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.*

Table 7: Recipients participate more in excludable groups and fundraisers and contribute more

| | (1) Summary Index | (2) Number of contributions to harambees for private purposes | (3) Total value of contributions to harambees for private purposes | (4) Number of group memberships in the household | (5) Number of group initiatives held | (6) Value raised by group initiatives | (7) Total value of contributions to group assets and initiatives |
|---|---|---|---|---|---|---|---|
| Cash minus placebo (eligible) | 0.102*** | 0.213 | 3.545*** | 0.081* | 0.029 | 25.363*** | 4.065* |
| | (0.033) | (0.146) | (1.269) | (0.043) | (0.026) | (9.244) | (2.445) |
| | | [0.132] | [0.020] | [0.088] | [0.155] | [0.020] | [0.108] |
| Cash minus placebo (ineligible) | 0.031 | 0.349 | -1.433 | -0.028 | 0.062 | -6.375 | -0.538 |
| | (0.055) | (0.279) | (3.406) | (0.078) | (0.039) | (16.792) | (3.686) |
| | | [1.000] | [1.000] | [1.000] | [1.000] | [1.000] | [1.000] |
| Eligible minus ineligible (cash) | 0.071 | -0.136 | 4.979 | 0.110 | -0.033 | 31.739 | 4.603 |
| | (0.062) | (0.317) | (3.727) | (0.081) | (0.045) | (19.944) | (4.002) |
| | [.] | [0.575] | [0.575] | [0.575] | [0.575] | [0.575] | [0.575] |
| Difference between eligibles and ineligibles in placebo villages | -0.258*** | -0.920*** | -13.593*** | -0.185*** | 0.023 | -35.030** | -6.405** |
| | (0.047) | (0.234) | (2.897) | (0.059) | (0.032) | (14.861) | (2.802) |
| | | [0.001] | [0.001] | [0.003] | [0.087] | [0.014] | [0.014] |
| Outcome type | Summary | Primary | Primary | Primary | Primary | Primary | Primary |
| % of households | | 0.744 | 0.744 | 0.649 | 0.177 | 0.115 | 0.469 |
| Conditional placebo mean | 0.001 | 4.57 | 45.0 | 1.69 | 1.28 | 569 | 56.5 |
| Clusters | 413 | 413 | 413 | 413 | 413 | 413 | 413 |
| Observations | 10110 | 10087 | 10074 | 10104 | 10109 | 9646 | 9818 |

*Notes: The summary index in column (1) is an Anderson (2008) index of all primary outcomes in the Table. For each outcome, we report the coefficient; the heteroskedasticity-robust standard error, clustered at the village-level, in parentheses; and the sharpened q-values controlling the FDR across all primary outcomes in square brackets. Each regression contains: assignment to cash treatment, eligibility status, assignment to psychological treatment, the interactions of the three; a vector of covariates listed in Appendix Table A.9, and ward fixed effects. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.*

Table 8: Cash recipients do not become more active in community decision making or leadership

| | (1) Summary Index | (2) Frequency attended barazas or consultation meetings | (3) Frequency spoke up at any barazas or consultation meetings | (4) Any household member holds a leadership position | (5) Any household member holds a group leadership position |
|---|---|---|---|---|---|
| Cash minus placebo (eligible) | -0.019 | 0.04 | -0.021 | 0.011 | -0.025 |
| | (0.041) | (0.21) | (0.052) | (0.016) | (0.018) |
| | [.] | [1.000] | [1.000] | [1.000] | [1.000] |
| Cash minus placebo (ineligible) | -0.007 | -0.34 | -0.113 | 0.013 | 0.018 |
| | (0.071) | (0.32) | (0.117) | (0.027) | (0.030) |
| | [.] | [1.000] | [1.000] | [1.000] | [1.000] |
| Eligible minus ineligible (cash) | -0.012 | 0.38 | 0.092 | -0.002 | -0.043 |
| | (0.078) | (0.33) | (0.126) | (0.03) | (0.037) |
| | [.] | [1.000] | [1.000] | [1.000] | [1.000] |
| Difference between eligibles and ineligibles in placebo villages | -0.035 | -0.39 | -0.177* | -0.023 | 0.040 |
| | (0.056) | (0.26) | (0.093) | (0.020) | (0.027) |
| | [.] | [0.234] | [0.234] | [0.234] | [0.234] |
| Outcome type | Summary | Primary | Primary | Primary | Primary |
| % of households | | 0.395 | 0.147 | 0.266 | 0.394 |
| Conditional placebo mean | | 4.37 | 2.93 | 1 | 1 |
| Clusters | 413 | 413 | 413 | 413 | 413 |
| Observations | 10114 | 10107 | 10107 | 10108 | 10106 |

*Notes: The summary index in column (1) is an Anderson (2008) index of all primary outcomes in the Table. For each outcome, we report the coefficient; the heteroskedasticity-robust standard error, clustered at the village-level, in parentheses; and the sharpened q-values controlling the FDR across all primary outcomes in square brackets. Each regression contains: assignment to cash treatment, eligibility status, assignment to psychological treatment, the interactions of the three; a vector of covariates listed in Appendix Table A.9, and ward fixed effects. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.*

Table 9: Participation in village organisation of public goods processes

| | (1) Number of public good harambees | (2) Amount raised for public good harambees | (3) Funding for public projects from all sources | (4) Number of new projects started since transfers |
|---|---|---|---|---|
| Treatment village | -0.006 | -986.378 | 183,000** | 0.007 |
| | (0.029) | (3315.748) | (74418.355) | (0.043) |
| | [1.000] | [1.000] | [0.059] | [1.000] |
| Control group mean | 0.185 | 13529.093 | 289198.731 | 0.497 |
| No. observations | 1094 | 1094 | 1094 | 1094 |
| Adjusted R-squared | 0.01 | 0.022 | 0.031 | 0.054 |

*Notes: This table reports OLS estimates of intent-to-treat effects of village assignment to cash transfers using village-level data collected from VEs. For each outcome, we report the coefficient; the heteroskedasticity-robust standard error in parentheses; and the sharpened q-values controlling the FDR across outcomes in square brackets. Each regression also contains a vector of covariates and ward fixed effects. Covariates for village-level outcomes are VE characteristics (age, education level, experience) and village characteristics (population, share of eligibles, distance to major town and seat of local government, number of months since the cash transfer). *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.*

Table 10: Voters (correctly) do not attribute credit to local leaders

| | **VE surveys** | | **Household surveys** | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | Number of candidates who discussed cash tranfsers during barazas | Number of candidates who claimed credit for transfers | Any local leaders claiming credit for bringing the transfers to the area | Any local leaders were involved in allocating the transfers |
| Treatment Village | 0.001 | -0.028** | -0.008 | 0.014 |
| | (0.037) | (0.011) | (0.006) | (0.014) |
| Control group mean | 0.267 | 0.04 | 0.041 | 0.093 |
| Clusters | | | 413 | 413 |
| Observations | 1094 | 1094 | 10095 | 10095 |

*Notes: For each outcome, we report the coefficient and the heteroskedasticity-robust standard error (clustered at the village-level for household outcomes), in parentheses. The dependent variable vector consists of assignment to cash treatment, a vector of covariates, and ward fixed effects. Covariates for village-level outcomes are listed in Appendix Table A.10, while household covariates are listed in Appendix Table A.9. Households are weighted via inverse probability weights from sampling. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.*

# A Appendix

Table A.1: No effects on participation in the general election, or party primaries

| | (1) Summary Index | (2) Registered to vote | (3) Voted in general election | (4) Voted in primary election |
|---|---|---|---|---|
| Cash minus placebo (eligible) | -0.007 | 0.005 | -0.010 | -0.003 |
| | (0.043) | (0.010) | (0.010) | (0.020) |
| | [.] | [1.000] | [1.000] | [1.000] |
| Cash minus placebo (ineligible) | -0.023 | -0.021 | -0.011 | 0.025 |
| | (0.065) | (0.022) | (0.012) | (0.028) |
| | [.] | [0.591] | [0.591] | [0.591] |
| Eligible minus ineligible (cash) | 0.015 | 0.026 | 0.002 | -0.028 |
| | (0.078) | (0.025) | (0.015) | (0.032) |
| | [.] | [1.000] | [1.000] | [1.000] |
| Difference between eligibles and ineligibles in placebo villages | -0.010 | 0.017 | -0.021** | -0.012 |
| | (0.053) | (0.018) | (0.010) | (0.023) |
| | [.] | [0.554] | [0.129] | [0.683] |
| Outcome type | Summary | Primary | Primary | Primary |
| % of households | | 0.923 | 0.930 | 0.667 |
| Clusters | 413 | 413 | 413 | 413 |
| Observations | 10109 | 10058 | 10084 | 9999 |

*Notes: This table presents outcomes from household survey data. The summary index in column (1) is an Anderson (2008) index of all primary outcomes in the table. For each outcome, we report the coefficient; the heteroskedasticity-robust standard error, clustered at the village-level, in parentheses; and the sharpened q-values controlling the FDR across all primary outcomes in square brackets. Each regression contains: assignment to cash treatment, eligibility status, assignment to psychological treatment, the interactions of the three; a vector of covariates listed in Appendix Table A.9, and ward fixed effects. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.*

Table A.2: Favourability ratings for candidates do not change in cash villages

| | (1) Favourability rating of ODM MCA candidate (1-10) | (2) Favourability rating of main opposition MCA candidate (1-10) | (3) Favourability rating of the incumbent MCA |
|---|---|---|---|
| Cash minus placebo (eligible) | 0.04 | -0.21 | -0.10 |
| | (0.16) | (0.17) | (0.17) |
| Cash minus placebo (ineligible) | -0.52* | -0.22 | 0.01 |
| | (0.27) | (0.23) | (0.24) |
| Eligible minus ineligible (cash) | 0.57** | 0.01 | -0.12 |
| | (0.28) | (0.24) | (0.25) |
| Difference between eligibles and | -0.10 | 0.06 | 0.09 |
| ineligibles in placebo villages | (0.21) | (0.18) | (0.17) |
| Outcome type | Secondary | Secondary | Secondary |
| Ineligible placebo mean | 5.49 | 4.52 | 5.10 |
| Clusters | 413 | 413 | 395 |
| Observations | 10009 | 9909 | 9020 |

*Notes: The summary index in column (1) is an Anderson (2008) index of all primary outcomes in the Table. For each outcome, we report the coefficient; the heteroskedasticity-robust standard error, clustered at the village-level, in parentheses; and the sharpened q-values controlling the FDR across all primary outcomes in square brackets. Each regression contains: assignment to cash treatment, eligibility status, assignment to psychological treatment, the interactions of the three; a vector of covariates listed in Appendix Table A.9 and ward fixed effects. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.*

Table A.3: Opportunities for engagement with local leaders

| | (1) Number of campaign meetings close-by | (2) Number of villagers attending MCA meetings |
|---|---|---|
| Treatment village | -0.102 | -1.585 |
| | (0.134) | (18.33) |
| | [1.000] | [1.000] |
| Control group mean | 3.991 | 308.162 |
| No. observations | 1094 | 1022 |
| Adjusted R-squared | 0.126 | 0.076 |

*Notes: Outcome variables are listed at the top of each panel. For each outcome, we report the coefficient; the heteroskedasticity-robust standard error in parentheses; and the sharpened q-values controlling the FDR across outcomes in square brackets. Each regression contains village assignment to treatment, a vector of covariates listed in Appendix Table A.10, and ward fixed effects. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.*

Table A.4: No change in political knowledge or self-efficacy

| | (1) Index of election knowledge | (2) Political self-efficacy (z-score, increasing in efficacy) |
|---|---|---|
| Cash minus placebo (eligible) | -0.018 | 0.012 |
| | (0.050) | (0.043) |
| Cash minus placebo (ineligible) | 0.006 | 0.024 |
| | (0.066) | (0.075) |
| Eligible minus ineligible (cash) | -0.024 | -0.011 |
| | (0.073) | (0.091) |
| Difference between eligibles and | -0.060 | 0.189** |
| ineligibles in placebo villages | (0.053) | (0.073) |
| Outcome type | Secondary | Secondary |
| Ineligible placebo mean | -0.026 | -0.151 |
| Clusters | 413 | 413 |
| Observations | 10080 | 10079 |

*Notes: The summary index in column (1) is an Anderson (2008) index of all primary outcomes in the Table. For each outcome, we report the coefficient; the heteroskedasticity-robust standard error, clustered at the village-level, in parentheses; and the sharpened q-values controlling the FDR across all primary outcomes in square brackets. Each regression contains: assignment to cash treatment, eligibility status, assignment to psychological treatment, the interactions of the three; a vector of covariates listed in Appendix Table A.9, and ward fixed effects. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.*

Table A.5: Vote buying acceptance rate is higher for placebo eligibles than ineligibles, and lower for cash eligibles than placebo ineligibles

| | Ineligible | Eligible | Eligible - ineligible |
|---|---|---|---|
| Placebo village mean | 0.811 | 0.915 | 0.105*** |
| SE | 0.042 | 0.018 | 0.039 |
| N | 66 | 171 | 237 |
| Cash village mean | 0.858 | 0.836 | -0.022 |
| SE | 0.040 | 0.030 | 0.051 |
| N | 63 | 123 | 186 |
| Cash - placebo | 0.048 | -0.079** | |
| SE | 0.058 | 0.033 | |
| N | 129 | 294 | |

Table A.6: Types of Group Initiatives

| Group initiative type | Control (% villages) | Treatment (% villages) | Diff | Observations |
|---|---|---|---|---|
| Benefit group members, income generating activity related to group purpose | 0.121 | 0.160 | 0.039 * | 1,091 |
| | (0.326) | (0.367) | (0.021) | |
| Benefit group members, other income generating activity | 0.188 | 0.221 | 0.032 | 1,091 |
| | (0.391) | (0.415) | (0.024) | |
| Benefit group members, private benefits | 0.038 | 0.051 | 0.013 | 1,091 |
| | (0.192) | (0.221) | (0.013) | |
| Benefit non-group members | 0.040 | 0.048 | 0.008 | 1,091 |
| | (0.197) | (0.214) | (0.012) | |
| Observations | 547 | 544 | | |

Table A.7: Household participation in village organisation of public goods processes

| | (1) Frequency contributed to harambees for public goods | (2) Total value of contributions to harambees for public goods | (3) Frequency contributed labour to community projects |
|---|---|---|---|
| Any cash v no cash (eligibles) | 0.038 | 0.04 | 0.012 |
| | (0.049) | (0.07) | (0.04) |
| | [0.693] | [0.693] | [0.862] |
| Any cash v no cash (ineligibles) | 0.017 | -0.01 | 0.058 |
| | (0.088) | (0.12) | (0.08) |
| | [1.000] | [1.000] | [1.000] |
| Eligible v ineligible (cash village) | 0.021 | 0.05 | -0.046 |
| | (0.103) | (0.15) | (0.094) |
| | [1.000] | [1.000] | [1.000] |
| Difference between eligibles and ineligibles in placebo villages | -0.191** | -0.28** | -0.084 |
| | (0.082) | (0.11) | (0.073) |
| | [0.016] | [0.016] | [0.053] |
| Outcome type | Primary | Primary | Primary |
| % of households | 0.349 | 0.349 | 0.159 |
| Conditional placebo mean | 2.10 | 23.7 | 3.46 |
| Clusters | 413 | 413 | 413 |
| Observations | 10105 | 10093 | 10100 |

*Notes: This table reports household-level measures of public good involvement and contributions. For each outcome, we report the coefficient; the heteroskedasticity-robust standard error (clustered at the village-level for household outcomes) in parentheses; and the sharpened q-values controlling the FDR across outcomes in square brackets. The correction is done across all outcome variables in the table The dependent variable vector consists of assignment to cash treatment, eligibility status, assignment to psychological treatment, and the interactions of the three. The table presents the interaction coefficients of interest. Each regression also contains a vector of covariates and ward fixed effects. Covariates for village-level outcomes are listed in Appendix Table A.10, while household covariates are listed in Appendix Table A.9. These were selected using the methodology specified in the household-level PAP (Orkin and Walker, 2020). Households are weighted via inverse probability weights from sampling. 8. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.*

Table A.8: Local leader public good responses

| | (1) Number of candidates promising concrete projects | (2) Number of requests for projects | (3) Number of successful requests to outside sources for projects | (4) Number of projects receiving outside support | (5) Amount of funding provided across all external sources | (6) Number of requests to local leaders for public goods harambees | (7) Amount raised from local leaders for public goods harambees |
|---|---|---|---|---|---|---|---|
| Treatment village | 0.051 | 0.13 | 0.011 | 0.046 | 177,000** | -0.018 | -323.93 |
| | (0.049) | (0.124) | (0.09) | (0.047) | (72718.981) | (0.059) | (530.358) |
| | [0.744] | [0.744] | [0.991] | [0.744] | [0.120] | [0.991] | [1.000] |
| Control group mean | 1.868 | 1.664 | 0.989 | 0.59 | 280606.171 | 0.29 | 1833.216 |
| No. observations | 1094 | 1094 | 1094 | 1094 | 1094 | 1094 | 1094 |
| Adjusted R-squared | 0.112 | 0.079 | 0.044 | 0.07 | 0.029 | 0.032 | 0.005 |

*Notes: This table reports village-level outcomes from OLS regressions based on village assignment to cash transfers, as reported by the VE. For each outcome, we report the coefficient; the heteroskedasticity-robust standard error (clustered at the village-level for household outcomes) in parentheses; and the sharpened q-values controlling the FDR across outcomes in square brackets. Each regression also contains a vector of covariates and ward fixed effects. Covariates for village-level outcomes are listed in Appendix Table A.10. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.*

Table A.9: Covariates used in household-level regressions

| Variable name | Variable description |
|---|---|
| hh_size_census | Number of household members resident in the house at census |
| educ_yrs_female | Years of schooling of the primary female (usually the respondent) in the household at census |
| hh_16f_buw | Number of females aged 16 and above in the household at census |
| age | Age of the primary female in the household (usually the respondent) at census |
| roaddistance_countyseat | Distance by road between the village and the seat of the county government |
| month_endline | Month the survey was conducted |

Table A.10: Covariates used in village-level regressions

| Variable name | Variable description |
|---|---|
| VEgender | Village Elder gender |
| VEcontage | Village Elder age |
| VEformalexperience | Indicator equal to 1 if Village Elder had prior formal sector experience |
| VEyearseducation | Village Elder years of education |
| VEyearsinoffice | Years of experience as a Village Elder |
| agentve | Indicator equal to 1 if Village Elder served as agent or committee member for any election candidate |
| v_num_hh | Number of households in the village |
| v_share_elig | Share of cash-eligible households in village |
| majortown_roaddistance | Distance by road between the village and the closest major town |
| roaddistance_countyseat | Distance by road between the village and the seat of the county government |
| months_since_transfer | Number of months since transfers to the area begin |
| qmonths_since_transfer | Number of months since transfers to the area begin, squared |