Do anti-poverty programs sway voters? Experimental evidence from Uganda*

Christopher Blattman Mathilde Emeriau Nathan Fiala†

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Abstract

Policies that change economic and social outcomes for citizens may not always lead to support for the political party that introduced the policy. In 2008, under the Youth Opportunities Program, the Ugandan government encouraged groups of young people to submit proposals to start enterprises. Among the 535 groups whose proposals were deemed eligible, a random 265 received grants of nearly $400 per person. A companion paper showed that, after four years, YOP raised employment by 17% and earnings by 38%. Here, we show that YOP recipients were no more likely to support the ruling party in elections. Rather, recipients slightly increased party membership, campaigning, and voting in favor of the opposition parties. We discuss potential mechanisms for this effect, including misattribution of YOP, group socialization, and financial independence freeing voters from transactional voting.

Keywords. Political behavior, voting, partisanship, employment, labor market programs, poverty, cash transfers, Uganda, field experiment

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†Blattman (corresponding author): University of Chicago, Harris Public Policy, blattman@uchicago.edu; Emeriau: Stanford University, Political Science, memeriau@stanford.edu; Fiala: University of Connecticut, Agricultural and Resource Economics, nathan.fiala@uconn.edu.

1 Introduction

What are the political impacts of development programs? Governments that deliver programs to their constituents hope to be rewarded at the polls. They hope for rewards even when those policies are targeted programmatically — based on need or merit — rather than in a clientelistic way. There are good reasons to think voters reward governments for good policy. In developed democracies, there is evidence that voters punish or reward incumbents for effective policies, for economic conditions, and even for events beyond the government's control.[1] Forward-looking voters may also be swayed by effective government programs. For instance, they could view programmatic policies as a signal that the regime is either competent or taking a policy stance that matches voters' preferences.

There is now a good deal of evidence that voters reward governments for programmatic policies in middle-income democracies, especially from social safety net programs in Latin America. Golden and Min (2013) note that most studies have found that as transfers to a district rise, voter turnout and incumbent vote share tend to rise as well.[2] Nonetheless, it is probably too early to draw firm conclusions. Golden and Min not only suggest exceptions to this pattern, but also raise concerns of publication bias against null findings.[3] Indeed, as this paper will show, transfer programs have sometimes unexpected political consequences.

We know little about the effects of programmatic policies on politics in low-income coun-

[1]A large literature argues that voters reward incumbents for economic conditions because they themselves are doing better or stand to gain (e.g. Kinder and Kiewiet, 1981). Voters also punish politicians for irrelevant events, suggesting voters may follow a form of blind retrospection (e.g. Healy et al., 2010) .

[2]In Uruguay, Manacorda et al. (2011) find that households that benefited from a conditional cash transfer (CCT) are 11–13 percentage points more likely to support the current government than the previous one. In Colombia, Baez et al. (2012) show that recipients of health and education transfers in Colombia were more likely to register, vote and support the government. In Romania, Pop-Eleches and Pop-Eleches (2012) use a discontinuity in a cash transfer program to the poor to show that receipt buys turnout and support for the incumbent. In Mexico, De La O (2013) finds that villages randomized into a CCT have 7% higher turnout and 9% higher incumbent vote share (though Imai et al. (2016) have pointed out that this is driven by increases in registration not turnout, and Schober (2016) argues that the effect is limited to turnout and not incumbent vote share). These populations are wealthier than the target population in Uganda. It is difficult to compare earnings, but we estimate the target population in Uganda earns no more than 40% as much as the target populations in Latin America.

[3]Imai et al. (2016) evaluate a large-scale health policy experiment in Mexico supported by all political parties and find that (perhaps because of this broad support across parties) little effect of the program on vote turnout or shares for the incumbent regime.

tries. Most evidence comes from high and middle-income countries and from more overtly clientelistic programs, where the benefits can easily be withdrawn or tied to political support. Patronage and pork are common and so deservedly get a lot of attention. But parties also compete programmatically, and it is important to understand their political rewards.

Another reason to be interested in the poorest countries is that many of their social programs are funded by foreign aid. The program we study here was financed by the government, but with a concessionary loan and expertise from the World Bank. If poor voters reward incumbents for foreign-funded development programs, then aid could insulate incumbents from competition and accountability to citizens, possibly assisting them to become more authoritarian or extractive (Moss et al., 2006).

The Youth Opportunities Program (YOP) in Uganda offers a chance to investigate this. In 2006–07 Uganda's central government, with assistance from The World Bank, developed the program to help poor and unemployed young adults become self-employed artisans, such as carpenters or tailors. YOP targeted the under-developed northern districts, and invited young people in these districts to form small groups and submit proposals on how they would use a cash grant to start independent trades. Thousands of groups applied. Local bureaucrats nominated proposals for funding. In 2008, they identified 535 eligible groups and awarded grants to 265 of them via lottery. Successful groups received grants of about $382 per person to pay for training and start-up costs. This was roughly the annual income of the average applicant.

YOP, like most government programs, was partisan in the sense that it was designed and supported by the ruling party, and the party hoped to reap electoral support for developing the country. But YOP was still programmatic in the sense that its targeting, advertising, and implementation ignored partisan affiliations. Indeed, we find that most people said YOP was aimed at developing the north rather than at increasing political support.

YOP raised incomes. We experimentally evaluated the economic impacts in 2010 and 2012 in a companion paper (Blattman et al., 2014) and found that people invested grants in training and capital and, four years later, had 38% higher earnings. As a result, YOP is one of the few examples of an employment program with cost-effective impacts (Blattman and Ralston, 2015).

In this paper, we compare successful and unsuccessful applicants to understand the political impacts of YOP. Four years after disbursement of the grant, we collected self-reported data on political preferences, voting behaviors, and other political actions. Did these poor and largely poorly educated recipients reward incumbents at the polls for good policy and programs? If so, this could be a powerful incentive for political parties to compete based on programmatic appeals instead of patronage.

We find an unexpected result: three years after YOP disbursement, beneficiaries were no more likely to vote for the ruling party than the control group, and they were actually more likely to work to get opposition parties elected. This suggests that policies that change economic and social outcomes for citizens may not always lead to support for the party that introduced the policy, at least among young poor voters such as these.

If anything, there was a decrease in support for the ruling party and President. Eighty-eight percent of the control group reported that they voted to reelect the President in 2011, but those who received YOP were 4 percentage points less likely to do so. Given the small opposition vote share (12%), this increased opposition vote share by a quarter. Moreover, those who received YOP were also almost twice as likely to say that they had joined the opposition or actively worked to get opposition parties elected. While small in absolute terms, this is a large relative change: an increase of 3 percentage points on a base of about 4 percentage points. The effects were even larger in more local elections: in electing district counselors, YOP applicants assigned to the program were about 20 percentage points less likely to vote for an incumbent ruling candidate than an opposition one.

So what then explains the null effect on ruling party support and the increase in political activities in favor of the opposition? We walk through possible mechanisms and the evidence for or against them.

First, our sample could attribute the program to foreign funders and either fail to reward (or punish) the incumbent government.[4] Or, they could see that they were in fact selected by the government, but randomly did not receive the program, and so have no reason to reward the incumbent.[5] As it happens, a majority of both the treatment and control groups gave the incumbent government credit for implementing YOP. Few remembered that groups had been selected randomly. But YOP recipients who did not attribute the program to the government were more likely to support the opposition. While this result is not statistically significant, misattribution could account for some of the effects we observe.

Second, incomes may have brought financial independence, freeing voters from clientelistic networks and allowing them to act on their political preferences. Program evaluations in South Africa, Brazil, Mexico and the Philippines have argued that rising incomes or unconditional transfers weaken a regime's ability to foster clients and buy participation (Magaloni, 2006; Larreguy et al., 2015; De Kadt and Lieberman, 2015; Hite-Rubin, 2015; Bobonis et al., 2017). Vote buying is common in Uganda. These are mainly small cash gifts in the run-up to the election, both openly at rallies and secretly on the eve of the election. Mostly the ruling party buys votes, as the opposition rarely has enough funds. We do not have direct measures of vote buying. But we see some evidence consistent with the hypothesis that moderate income gains from YOP can free opposition supporters to campaign openly for their preferred candidate. For instance, party preferences do not change with YOP or incomes; YOP only affects voting and public actions in support of a candidate. Moreover, support for the opposition is correlated with higher earnings in our sample.[6] And finally, YOP recipients were less likely to be mobilized to turn out by political party operatives.

Income is just one possible mechanism. There are others that we are not able to test.

[4] Using survey experiments in India, Dietrich and Winters (2014) find suggestive evidence that politicians lose reputation when programs are revealed as foreign-funded.

[5] In Bangladesh, Guiteras and Mobarak (2016) find that politicians opportunistically try to associate themselves with foreign-funded projects by non-governmental organizations (NGOs). When the politician's role in program assignment wasn't clear, citizens gave the political partial credit. When the assignment rules and attribution of the projects were clear, however, citizens did not reward the politician at all.

[6] We must take this view with caution, however, as income grew across the board in the sample, and we are not able to directly observe a generalized increase in opposition support.

Groups may have exposed youth to new political ideas or collective action. Or YOP may have increased beneficiaries' exposure to local politicians. The association between income and public opposition support is important and unexpected, however. We believe this calls for more research on the downstream political effects of government and aid programs.

2 Context, intervention, and experiment

Uganda, a small country in east Africa, is extremely poor but with a stable and growing economy.[7] Since 2006, two major parties and a number of smaller ones have competed in national elections every five years. Nonetheless, the National Resistance Movement (NRM) party and its leader, President Yoweri Museveni, have been in power for 30 years.

While there is a high degree of party competition at the local level, the ruling party suppresses political opposition for the Presidency, and cements its position through various forms of patronage. For this reason, most analysts consider Uganda a "hegemonic party system" or "multiparty autocracy" (e.g. Tripp, 2010). Even though the ruling party has a built in advantage, elections are still a fairly competitive affair. Participation rates are high, and election day itself is perceived as free and fair by Ugandans and the international community. The ruling party's advantage comes not from its interference in the actual vote, or extensive fraud, but rather the use of public funds during the campaign, extensive patronage, and the intimidation of opposition candidates.[8]

Both the ruling and opposition parties run extensive rallies, party mobilization effects,

[7] Shortly before the program, in 2007, it had a population of about 30 million and GDP per capita of roughly \$330. Real gross domestic product grew 6.5% per year from 1990 to 2007, inflation was under 5%, and poverty rates were falling (Government of Uganda, 2007). This growth puts Uganda's GDP per capita slightly above the sub-Saharan average.

[8] See Mwenda and Tangri (2005). For instance, vote buying is extensive in the weeks leading up to the election, peaking the evening before. This vote buying is much more common among the ruling party, possibly because of the diversion of public funds and other corruption (Blattman et al., 2016). The forms of patronage are varied and many, but a recent and visible example is the rapid increase in the creation of administrative districts, which allows the national government to expand public employment and central government transfers and use these for political gain . Finally, an example of intimidation were the repeated attempts of the ruling party and military to imprison and try the main opposition candidate for President, sharply curtailing his ability to campaign.

and vote-buying campaigns around the country. The biggest difference is that the ruling party has more resources. Nonetheless, competition is fairly intense for parliamentary seats and powerful district positions. Also, even if the President is unlikely to lose the election, the ruling party has to respond aggressively to opposition support with campaigning, policy change, pork projects, and vote buying. Opposition vote share sends a powerful message.

One of the government's recent priorities has been to develop the north of the country (Government of Uganda, 2007). The north is more distant from trade routes and, as an area of early opposition support, received less public investment from the 1980s onward, especially for power and roads. The north was also held back by insecurity. From 1987 to 2006 a low-level insurgency destabilized north-central Uganda, and wars in Sudan and Democratic Republic of Congo, fostered mild insecurity in the northwest. Cattle rustling and armed banditry were commonplace in the northeast. As a result, in 2006 the government estimated that nearly two-thirds of northern people were unable to meet basic needs, just over half were literate, and most were (under)employed in subsistence agriculture (Government of Uganda, 2007). In 2003, peace came to Uganda's neighbors and Uganda's government increased efforts to pacify and develop the north. By 2006, the military pushed the rebels out of the country and began to disarm cattle-raiders. The government also began to improve northern infrastructure. South Sudan also began to grow rapidly. With this political uncertainty resolved, by 2008 the northern economy began to catch up.

Northern development serves at least two government objectives. One is economic, as the government tries to maximize growth and minimize poverty. The other is political. As multiparty elections become more and more competitive, and as NRM support in the capital has waned, the ruling party appears to be interested in building a broader base of political support in areas such as the north. While pork and patronage around elections is commonplace, the national government has also pursued a set of broad-based and relatively non-politicized programs that serve its broader development objectives.

2.1 The Youth Opportunities Program

From 2003–10, the government's northern development and security strategy centered around the Northern Uganda Social Action Fund, or NUSAF. NUSAF was Uganda's second-largest development program, after the national agricultural extension program. Communities and groups could apply under various NUSAF cash grants components for either community infrastructure construction or livestock for the "ultra-poor".

The government wanted to do more to boost non-agricultural employment. To do so, in 2006 it announced a third NUSAF component: the Youth Opportunities Program, or YOP. YOP invited groups of young adults aged 16 to 35 to apply for grants to start a skilled trade such as carpentry or tailoring. The theory underlying the program was that young unemployed people had high returns to investments in vocational skills and equipment, but had no starting capital and were credit constrained.

YOP had five key elements:

1. People had to apply as a group. One reason was administrative convenience: it was easier to verify and disburse to a few hundred groups rather than thousands of people. Another reason is that, in the absence of formal monitoring, officials hoped groups would be more likely to implement proposals. The YOP groups in our sample ranged from 10 to 40 people, averaging 22. They are mostly from the same village and typically represent less than 1% of the local population.[9] In our sample, most groups are mixed (about one-third female on average), 5% of groups are all female and 12% all male.

2. Groups had to submit a written proposal. The proposal described how they would use the grant for non-agricultural skills training and enterprise start-up costs. They could request up to $10,000.[10] Groups selected their own trainers, typically a local artisan or

[9] Half the groups existed already, often for several years, as farm cooperatives, or sports, drama, or micro-finance clubs. New groups formed specifically for YOP were often initiated by a respected community member (e.g. teachers, local leaders, or existing tradespersons) and sought members through social networks.

[10] The proposal specified member names, a management committee of five, the proposed trade(s), and the assets to purchase. Decisions were made by member vote, and nearly all members report they had a voice. Most groups proposed a single trade for all. One third proposed that different members would train two

small institute. These are commonplace in Uganda and there is a tradition of artisans taking on paid students as apprentices.

3. Groups had to receive formal advising. Many applicants were functionally illiterate, so YOP also required "facilitators" (usually a local government employee, teacher, or community leader) to meet with the group several times, advise them on program rules, and help prepare the written proposals. Groups chose their own facilitators, and the NUSAF office paid facilitators 2% of funded proposals (up to $200).

4. YOP applicants were screened at several levels of government. Villages typically submitted one application, and that privilege may have gone to the groups with the most initiative, need, or connections. Village officials passed applications up to district-level bureaucrats, who verified the minimum technical criteria (such as group size and a complete proposal) and were supposed to visit projects they planned to fund. Districts said they prioritized early applications and disqualified incomplete ones, and while this is in line with our observations, unobserved quality and political calculations could have played a role. A central government NUSAF office—an executive bureaucratic agency created specifically for the implementation of the program—had final responsibility for validating and approving the list of district projects and disbursing funds. Local elected politicians generally had little active role in the project.

5. Successful groups received a large lump sum cash transfer to a bank account in the names of the management committee, with no government monitoring thereafter. In our sample, the average grant was UGX 12.9 million Ugandan shillings (UGX) per group, or $7,497 in 2008 market exchange rates. Per capita grant size varied across groups due to variation in group size and amounts requested, but 80% of grants were between $200 and $600 per capita, and they averaged $382 per person (or $955 in PPP

to three different trades. Females and mixed groups often chose trades common to both genders, such as tailoring or hairstyling. Males and a small number of females often chose building trades.

terms). Unless otherwise noted, all UGX amounts reported in this paper are 2008 UGX, and all USD are converted at market exchange rates.[11]

2.2 Was NUSAF a patronage program?

Government patronage is commonplace in Uganda (Green, 2011). New district creation and public employment are prime examples of how the Ugandan government has sought to build rural support. Nonetheless, our assessment is that the central government did not use NUSAF, including the YOP component, for patronage purposes with individual voters.

The World Bank was closely involved in the design of the program, and monitored impropriety. This limited the program's ability to reward supporters. Also, unelected local bureaucrats nominated projects for funding. These bureaucrats undoubtedly received pressure from politicians of all stripes, but (as we will see in Section 4.1) the program did not have a reputation of being manipulated for electoral gain. Ugandan activists and press made frequent (and subsequently proven) allegations of corruption and impropriety in NUSAF, especially at the district level. But accusations of mass patronage or vote-buying were uncommon.[12] Corruption in NUSAF (including "ghost projects" and irregular procurement contracts) may have transferred funds from the government to local party machines, or strengthened other patron-client relations. But we are not aware of systematic targeting of villages or people for the grants.

We also see no evidence that YOP targeted supportive villages, party members, or swing voters. For example, there is no significant correlation between percent of vote going to the incumbent party in the 2004 election and the per capita NUSAF funds received between 2004 and 2007 at the subcounty level (see Appendix A.1). Indeed, the nomination process sought to avoid this kind of patronage by design. Targeting was highly decentralized, with groups

[11] We use a 2008 market exchange rate of 1,720 UGX per USD and a PPP rate of 688 UGX per USD.

[12] Allegations of misuse concentrated on decisions prior to project nomination and selection, such as the invention of "ghost projects" which transferred money directly to politicians or other insiders, or and the awarding of construction contracts for the NUSAF components that involved local building projects.

nominated by local leaders who may or may not be affiliated with the NRM.[13] We observed the selection, deliberation, and auditing process firsthand and the choosing of groups seemed to be a mix of first-come-first-serve, meritocratic, and ad hoc priorities and procedures.

Rather, our discussions with government and World Bank officials suggest that the national government viewed NUSAF as a way to build support for the ruling party through programmatic effectiveness. This is consistent with scholars who argue that the return of multi-party politics to Uganda in 2005, coupled with the President controversially securing the right to run for a third term, increased the ruling party's incentives to use development policy to mobilize electoral support (Hickey, 2013).

In terms of taking credit, the government did not make explicit efforts to market this as coming from the NRM or central government. In fact, most marketing of the program was done at the district level. District level officials were the ones that spent most of their time going from community to community to advertise and take photos with funded projects. Nonetheless, it is common knowledge that public finance is highly centralized in Uganda, and all revenues and major expenditures come from the national government. Simply put, the Office of the President is responsible for virtually every major development program in the country.

2.3 Experimental design

YOP was oversubscribed, and we worked with the national NUSAF office to randomize funding among screened and eligible proposals. Thousands of groups submitted proposals in 2006. The NUSAF office funded hundreds in 2006-07, prior to our study. By 2008, 14 NUSAF-eligible districts had funds remaining. Figure 1 maps these study districts.[14]

The study population was only moderately affected by war and political instability. None

[13] Participatory nomination processes that involved the whole village were commonplace. Facilitators helped groups organize and write their proposals, particularly teachers and local bureaucrats, and to our knowledge facilitators were not typically political operators or organizers at election time.

[14] By 2008, a national program of decentralization had subdivided these 14 districts into 22, as depicted in the map, but YOP was organized, disbursed, and randomized using the original 14 districts from 2003.

of the most war-affected districts (Gulu, Kitgum, and Pader) had the funds to participate in the final round. Thus the districts in our study were either on the margins of the conflict (center north), more vulnerable to banditry and cattle raiding than conflict (northeast) or relatively secure but underdeveloped (northwest). There are almost no ex-combatants in the study groups. Little distinguishes our sample from other poor Ugandan youth.

District governments nominated 2.5 times the number of groups they could fund. The districts submitted roughly 625 proposals to the national NUSAF office, who reviewed them for completeness and validity. To minimize chances of corruption, the central NUSAF office also sent out audit teams to visit and verify each group. They disqualified about 70 applications, mainly for incomplete information or ineligibility.[15]

In January 2008 the NUSAF office provided the research team with a list of 535 remaining groups eligible for randomization, along with district budgets. We randomly assigned 265 of the 535 groups (5,460 individuals) to treatment and 270 groups (5,828 individuals) to control, stratified by district. Control groups were not waitlisted to receive YOP in future. During the baseline survey, before treatment status was known, groups were told they had a 50% chance of funding and that there were no plans to extend the YOP program in the future. Spillovers between study villages are unlikely as the 535 groups were spread across 454 communities in a population of more than five million, and control groups are typically very distant from treatment villages. Figure 1 also maps eligible groups per parish.

2.4 Data and participants

We selected five people from each of the 535 groups to be tracked and interviewed three times over four years—a potential panel of 2,677 people (seven were inadvertently surveyed in one group at baseline). We worked with Uganda's Bureau of Statistics to conduct a baseline survey in February and March 2008, prior to the announcement and funding of treatment groups. This was a survey of demographic and economic information only, as the government

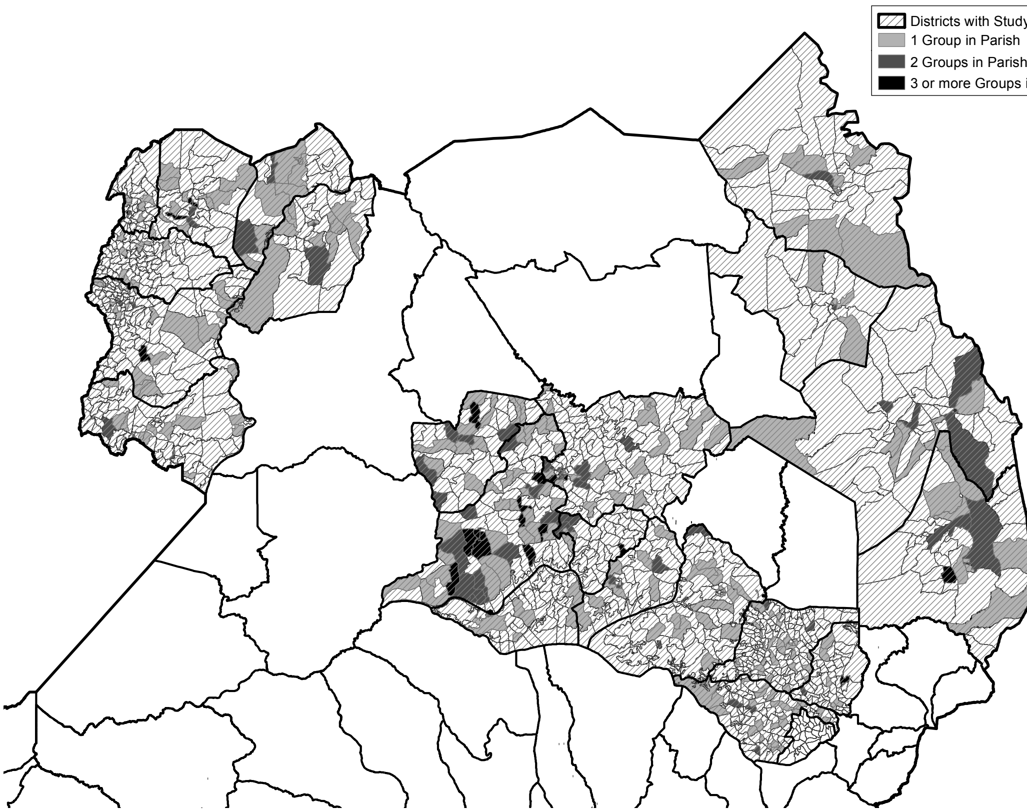
[15]E.g. many group members over 35 years, or a group size more than 40. The government also asked that 22 groups of underserved people (Muslims and orphans) be funded automatically.

Table 1: Selected baseline descriptive statistics and tests of balance

	Baseline (n=2598)			Found in 2010 (n=2005)			Found in 2012 (n=1868)		
	Control	Treatment – Control		Control	Treatment – Control		Control	Treatment – Control	
Select covariates in 2008	Mean	Diff.	p-value	Mean	Diff.	p-value	Mean	Diff.	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Applicant group size	22.53	0.03	0.96						
Grant requested, per group member, USD	363.05	14.09	0.25						
Group existed before application	0.45	0.03	0.42	0.44	0.04	0.36	0.45	0.04	0.36
Individual unfound at baseline	0.06	-0.05	0.00	0.25	0.01	0.47	0.30	-0.01	0.75
Age	24.75	0.17	0.55	24.94	0.20	0.48	25.06	0.06	0.84
Female	0.35	-0.02	0.38	0.36	-0.04	0.15	0.36	-0.05	0.10
Large town or urban area	0.23	-0.02	0.61	0.21	-0.02	0.65	0.18	0.01	0.84
Weekly employment, hours	10.70	0.57	0.48	10.92	0.03	0.97	10.64	1.05	0.24
All non-agricultural work	5.99	-0.45	0.44	6.09	-0.73	0.25	5.82	-0.45	0.49
All agricultural work	4.66	1.04	0.04	4.78	0.81	0.14	4.75	1.54	0.01
Engaged in a skilled trade	0.08	0.00	0.81	0.08	0.01	0.61	0.07	0.01	0.66
Highest grade reached at school	7.95	-0.07	0.62	7.99	-0.06	0.71	7.88	-0.09	0.60
Able to read and write minimally	0.75	-0.03	0.17	0.75	-0.03	0.14	0.74	-0.03	0.19
Received prior vocational training	0.07	0.02	0.07	0.08	0.02	0.16	0.07	0.02	0.14
Wealth index	-0.16	0.07	0.12	-0.16	0.06	0.27	-0.17	0.05	0.40
Monthly gross cash earnings (000s 2008 UGX)	62.19	6.89	0.30	62.11	10.24	0.17	63.96	6.62	0.41
Savings in past 6 mo. (00s 2008 UGX)	19.25	10.89	0.02	19.88	7.11	0.16	16.75	9.68	0.04
Can obtain 100,000 UGX loan	0.33	0.05	0.01	0.36	0.03	0.17	0.34	0.04	0.10
Registered to vote in 2006	0.92	-0.01	0.57	0.93	-0.01	0.61	0.93	-0.01	0.42
Voted in 2006 presidential election	0.73	0.03	0.21	0.73	0.04	0.08	0.75	0.00	0.91
Member of a political party	0.11	0.02	0.06	0.12	0.01	0.36	0.12	0.02	0.13
Currently on a community committee	0.17	0.01	0.60	0.18	0.01	0.77	0.18	0.02	0.36
Parish vote share for Museveni, 2006	0.32	0.00	0.99	0.32	0.00	0.82	0.31	0.00	0.95
Ever member of armed group	0.03	0.00	0.88	0.03	0.00	0.87	0.03	0.00	0.62
p-value from joint F-test		0.00			0.10			0.02	

Notes: Columns (1), (4), and (7) report the mean of control group members. Columns (2), (5), and (8) report the mean difference between the treatment and control groups, calculated using an OLS regression of baseline characteristics on an indicator for random program assignment plus district fixed effects while columns (3), (6), and (9) report p-values. Standard errors robust and clustered at the group level. All USD and Ugandan shilling (UGX)-denominated variables and all hours worked variables were top-censored at the 99th percentile to contain outliers. Baseline refers to all respondents surveyed at baseline, while 2010 and 2012 refer to the respondents located in each year, respectively.

Figure 1: Eligible districts and study communities (treatment and control) per parish



Notes: The figure shows the distribution of communities participating per parish using 2007 district boundaries. The majority of parishes had either one or two groups apply.

did not want to be seen collecting overtly political data in advance of YOP. Enumerators and local officials mobilized group members to complete a survey of demographic data on all members as well as group characteristics. Virtually all members were mobilized, and we randomly selected five of the members present to be individually surveyed and tracked.

Enumerators could not locate 13 groups (3% of the sample). Unusually, after the survey it was discovered that all 13 were assigned to the control group. We investigated the matter and found no motive for or evidence of foul play. District officials, enumerators, and the groups themselves did not know the treatment status of the groups they were mobilizing. We were only able to find one of the 13 at endline.

Funds were disbursed between July and September 2008. Working with private, independent survey organizations, we conducted the first 2-year endline survey between August 2010 and March 2011, 24 to 30 months after disbursement. We conducted a 4-year survey between April and June 2012, 44 to 47 months after disbursement, and just over a year after the 2011 national elections. The World Bank and Government of Uganda paid for the baseline and 2-year surveys. Both insisted that we ask no political questions. Thus we conducted the 4-year survey with private funds and were able to include political questions, drawing on the Uganda Afrobarometer and similar polls to maximize comparability.

Participants Table 1 reports baseline descriptive statistics for a selection of baseline variables, and we report the full set of 57 variables reported in Appendix B.1. We see that members of the 535 eligible groups were generally young, rural, poor, credit constrained, and underemployed. In 2008 they were 25 years on average, mainly aged 16 to 35. In 2011, 16.1% would have been eligible to vote for the first time, and 34.1% would have been eligible to vote just for the second time. Less than a quarter lived in a town, and most lived in villages of 100 to 2000 households. A quarter did not finish primary school, but on average they reached eighth grade. Given that the three most war-affected districts did not participate in the YOP evaluation, only 3% were involved in an armed group in any fashion.

In 2008 the sample reported 11 hours of work a week. Half these hours were low-skill labor or petty business, while the other half was in agriculture—rudimentary subsistence and cash cropping on small rain-fed plots with little equipment or inputs. Almost half of our sample reported no employment in the past month, and only 8% were engaged in a skilled trade. Cash earnings in the past month averaged a dollar a day. Savings in the past six months were $15 on average, and only 11% reported any savings.[16]

Tracking and attrition YOP applicants were a young, mobile population. Nearly 40% had moved or were away temporarily at each endline. To minimize attrition we used a two-

[16] 33% held loans, but these were small: under $7 at the median among those who have any loans, mainly from friends and family. About 10% reported they could obtain a large loan of 1,000,000 UGX (about $580).

phase tracking approach, outlined in Appendix A.2. In a first phase, we tracked all 2,677 members of the sample, and in a second phase we did intensive tracking of a random sample of unfound. Our response rate was 97% at baseline. Effective response rates (weighted for selection into tracking) were 85% after 2 years and 82% after 4.

Of slightly greater concern is correlation between attrition and treatment, reported in Appendix B.2. The treatment group was 5 percentage points more likely to be found at baseline in 2008, because of the 13 unfound groups (who did not know their treatment status). In 2012, controls were 7 percentage points less likely to be found. Most of these unfound controls were from the 13 "never found" groups.

If unfound controls are particularly successful, we could overstate the impact of the intervention. Such bias is conceivable: baseline covariates are significantly correlated with attrition and the unfound tend to be younger, poorer, less literate farmers from larger communities (see Appendix B.2). For this reason our treatment effects estimates will control for baseline characteristics associated with attrition, and we will test the sensitivity of results to various attrition scenarios.

2.5 Empirical strategy

In designing the experiment, our primary outcomes of interest were the direct economic effects of the business planning and cash on economic performance: investments in training and business assets, levels and type of employment, and incomes.[17] The longer-term political impacts were of interest from the beginning, but we did not identify them as primary outcomes, in part because any political effects were likely to be indirect and a function of successful economic impacts. Thus, as with any set of downstream impacts (and like most other evaluations of the political effects of public programs), the treatment effects on secondary outcomes should be treated with some caution.

[17]The 4-year outcomes were derived from a formal model and pre-specified in the analysis of the 2-year results. As the experiment pre-dated the social science registry, the trial was not formally pre-registered.

We estimate intent-to-treat (ITT) effect on outcomes, Y, via the weighted least squares (WLS) regression:

$$Y_{ij} = \theta_{ITT} T_{ij} + \beta X_{ij} + \gamma_d + \varepsilon_{ij} \tag{1}$$

where T is an indicator for assignment to treatment for person i in group j, X is the vector of baseline covariates displayed in Appendix Table B.1, the γ are district fixed effects (required because the probability of assignment to treatment varies by strata), and ε is an error term clustered by group. We weight observations by their inverse probability of selection into the two-phase endline tracking (see Appendix A.2).

We include covariates in part to account for imbalance in baseline characteristics across arms. Table 1 reports balance tests (for all variables see Appendix B.1). Of 57 covariates, 6 (10.5%) of the treatment-control differences have $p < 0.05$, and 8 (14.0%) have $p < 0.10$. A test of joint significance from an OLS regression on a treatment indicator reveals that baseline characteristics are jointly significant with $p = 0.05$. [18]

Most members of the control group knew that bureaucrats nominated them for the YOP lottery. Hence they know their control status. If this loss translated into resentment of the incumbent, then equation 1 will overstate any increase in incumbent support from treatment. Since we observe the opposite treatment effect (a decline in incumbent support and an increase in opposition activities), resentment at losing the lottery would *understate* the unexpected political effects of YOP. That would not be true if resentment translated into refusing to answer the survey. But the number of control group members both aware of their status and who did not respond is unlikely to be large enough to have this effect.[19]

[18] For instance, at baseline the treatment group report 2 percentage points more vocational training, 0.07 standard deviations greater wealth, 56% greater savings (though only in the linear, not in log form), and 5 percentage points more access to small loans. Group-level balance tests (the level of randomization) yield the same conclusions (not shown). The missing 13 control groups could cause the imbalance. We estimate that if the missing controls had baseline values 0.1 to 0.2 standard deviations above the control mean, it would account for the full imbalance (see Appendix B.3). If so, the observed control group may be poorer than the treatment group, and will overstate true program impacts. Our empirical strategy and sensitivity analysis below explicitly address the concerns that arise from imbalance and potentially selective attrition.

[19] As noted above, non-response is 7 percentage points greater in the control group, but most of this are 13

3 Results

3.1 Economic impacts of the program

YOP led to large and persistent increases in investment, work, and income. Blattman et al. (2014) report detailed ITT estimates on economic outcomes two and four years after the interventions. Briefly, 89% of groups assigned to a YOP grant received it.[20] A majority of groups and members invested the funds in skills training and business materials, as planned. Groups reported that they spent about two thirds of the grant on tools and materials, a tenth on training, and the remainder on miscellaneous other things. They focused on skilled trades: 38% in tailoring, 23% in carpentry, 13% in metalwork, and 8% in hairstyling. By 2012, assignment to YOP was associated with UGX 224,986 ($130) greater capital stocks, a 57% increase over the control group.

With these investments, YOP led these young people to shift their occupations toward skilled work and cottage industry, increasing their labor supply overall. After four years, those assigned to YOP were more than twice as likely to practice a skilled trade, and they worked 5.5 more hours per week than the control group — a 17% increase.

YOP's ultimate aim was to increase earned income.[21] An index of consumption, asset, and labor earnings measures of income increased by 0.17 standard deviations with YOP after two years, and by 0.24 standard deviations after four years. Since these people are fairly poor and underemployed, this earnings increase is modest in absolute terms — just under a dollar a day in PPP terms. But relative to the control group's earnings this is a 38% increase

"never found" groups who did not learn their treatment status. Even if these non-responders did resent the government in large numbers, there would have to be an extremely powerful connection between resentment and non-response to overcome the effect of resentful control group members who did respond to the survey (leading us to understate the fall in incumbent support from YOP).

[20] 21 groups could not access funds because of problems with identifying the group leaders and banking details, bank complications, collection delays, or corruption. Only 8 groups reported that they never received funds due to some form of theft or diversion.

[21] Income is notoriously difficult to measure, especially in poor and rural areas where the average person has volatile and seasonal work, multiple sources of income, and both monetary and in-kind remuneration. We measured income in three ways: self-reported earnings, consumption assets owned, and an estimate of total household consumption. The consumption and asset measures are thought to be better measures of stable or "permanent" income.

in cash earnings — a hugely important change for someone earning so little per day.

Both men and women benefited from the program. A third of applicants were women and the program had large and sustained impacts on them: After four years, incomes of treatment women were 73% greater than control women, compared to a 29% gain for men. Over the four years, control men kept pace or caught up with treated men. Women stagnated without the program but took off when funded. These are extremely large impacts, especially considering how few employment programs even pass a simple cost-benefit test.[22]

3.2 Impacts of the program on political behavior

YOP is unlike the sort of clientelistic program most commonly used in transactional politics and vote-buying, such as public sector jobs. It was a large-scale state employment program that was foreign-financed, relatively technocratic and non-politicized in its targeting and implementation, and (unlike a public sector job) the grant was by its nature impossible to revoke once given.[23] Indeed, it transferred resources directly to voters, much like land titling, conditional cash transfers, or skills training or other public programs. These are commonly labeled "programmatic policies" rather than pork programs or traditional patronage.

3.2.1 Theory and predictions

There is a growing base of evidence that voters reward incumbents for programmatic policy, at least in aggregate. For instance, comparing areas with varying exposure to conditional cash transfer programs in Latin America, Manacorda et al. (2011); Zucco (2013); Diaz-Cayeros et al. (2016) argue that retrospective voting could account for the fact that areas that

[22] Blattman and Ralston (2015), in their review of the evidence of the effectiveness of employment programs in poor, middle-income, and high-income countries, identify the YOP program (and cash transfer programs like it) as some of the highest return employment programs with evidence in the world.

[23] One important different between conditional and unconditional transfers is the amount of interaction individuals have with government. In the YOP case, young people interacted with the government, but in a limited way and only during the application process or, in limited cases, briefly after receiving funds. Most conditional cash programs deliver money in tranches over long periods of time, requiring greater interactions with officials and more reliance on the continuation of the distribution. YOP participants neither needed nor expected further interactions with government after receiving the program.

received more assistance rewarded incumbents, sometimes even after the program benefits had finished.[24] Similarly, Casaburi and Troiano (2015) see an increase in incumbent vote share after a successful anti-tax evasion program, and Larreguy et al. (2015) see incumbent vote share rise after a land titling program.

The literature provides several reasons why people assigned to treatment should reward a ruling party for programmatic policies, and together they led us to hypothesize that assignment to treatment would increase partisanship and electoral support for the ruling party.

One is that economically successful voters tend to reward the incumbent. Overall, YOP recipients experienced a large increase in wealth and may have rewarded the incumbent as a consequence, independently of whom they attribute the responsibility of the program to. This idea that voters are naïve and make simple calculations is supported by the literature on how natural or idiosyncratic events can sometimes boost incumbents' popularity (e.g. Healy et al., 2010). One explanation is that poorly informed voters interpret good fortune as plausible new information about an incumbent's quality or characteristics (Ashworth et al., 2016).

A second reason is that voters may reward incumbents if they interpret development programs as a signal that the incumbent is effective, or that the incumbent will work to benefit voters like themselves in the future. Relatedly, some theories emphasize reciprocity in voting — that voters reward incumbents out of a sense of gratitude or perceived obligation—and this would generate similar predictions to retrospective voting: increased vote share for the incumbent, at least when they attribute the program to that party or politician.

The YOP program was one of the largest development program ever run in Uganda. As such YOP could be viewed as a costly signal from the ruling party that it intended to channel more funds in the future to the north of the country, thus changing the expected benefits of keeping the party in power.[25] This led us to predict that YOP beneficiaries might

[24] In one case, that of conditional cash transfers in Mexico, it is contested whether incumbent vote share increased, or whether the effect was purely on turnout (Schober, 2016). Nonetheless, the argument that incumbent vote share responds to programmatic policy extends well beyond Mexico.

[25] Of course, for there to be a differential effect on treated individuals, the actual receipt of YOP would

reciprocate with votes for the ruling party.

Most of these explanations were developed to explain voting in democratic regimes. Yet Uganda is a multiparty autocracy. While it is possible that the voting calculus could be very different in a more autocratic regime, in our view the voting calculus in Uganda has more similarities than differences with more democratic African regimes. As we discussed above, the poll itself is reasonably free and fair in Uganda. Even in advanced democracies, many local, state and national elections feature a dominant party that is almost sure to win. In these cases, two of the most powerful voting explanations are voter's expressive preferences, and the strategic value of signaling opposition support in order to influence the ruling party's policies or patronage. Autocrats get at least as much informational value from elections as democratic leaders (Brancati, 2014). In many ways, Ugandan voting behavior resembles voting behavior in any poor country where politics is highly transactional and based on powerful ethnic or regional organizations.

In general, at the outset of the study we were not aware of theories or literature leading us to predict the opposite effect: that YOP could augment support for the opposition. In retrospect, we found a literature suggesting that rising wealth could mitigate the effects of patronage on politics. We return to this theory in the discussion and conclusions section below.

3.2.2 National election outcomes

Three years after the grants, we see no evidence that the program increased general political participation or support for the ruling party. Rather, if anything, young people assigned to the treatment increased their support for the opposition.

have to change these expectations. It is possible that treatment and control group members would see or absorb the signal differently. For instance, NUSAF was widely perceived as corrupt. But those who actually received the grants have direct evidence that it reaches people like them. Also, any element of reciprocity would likely affect the actions of YOP recipients. That said, were non-recipients to reward the incumbent for good policy, this would attenuate the treatment effects in our experiment. This highlights one of the key differences that separates our study from previous ones: we examine variation between treated and non-treated individuals in the same locality, rather than treated and non-treated localities.

Table 2: Impacts on partisan attitudes and actions, by incumbent and opposition party

	2012 sample			
	(1)	(2)	(3)	(4)
	Control	ITT, with controls		
Dependent variable in 2012	Mean	Coeff.	Std. Err.	N
Index of NRM/Presidential support (z-score)	-0.05	-0.04	[.052]	1858
Would vote NRM if election tomorrow	0.75	-0.02	[.022]	1858
Like or strongly like the NRM	0.81	-0.02	[.020]	1845
Feels close to the NRM	0.55	0.01	[.024]	1833
Worked to get the NRM elected	0.29	0.01	[.023]	1844
Member of the NRM	0.40	-0.02	[.026]	1849
Voted or supported the President in 2011	0.88	-0.04	[.018]**	1755
Approve or strongly approve of President	0.85	-0.02	[.018]	1847
Index of opposition support (z-score)	0.00	0.11	[.053]**	1858
Would vote opposition if election tomorrow	0.17	0.01	[.020]	1858
Like or strongly like any opposition party	0.36	0.03	[.023]	1844
Feels close to any opposition party	0.10	0.03	[.016]**	1833
Worked to get the opposition elected	0.04	0.03	[.011]***	1844
Member of an opposition party	0.05	0.02	[.013]**	1849
Voted or supported an opposition party in 2011	0.12	0.04	[.018]**	1755

Notes: Column (1) reports the control group mean, weighted by the inverse probability of selection into each endline sample. Columns (2)-(3) report the estimated intent-to-treat (ITT) coefficient and standard error at endline, using equation 1. Standard errors are heteroskedastic-robust and clustered by group.

Table 2 reports our main results on the impacts of receiving the program on political behavior and attitudes towards the ruling party and opposition parties. To reduce the number of hypotheses being tested, we group outcomes thematically into a small number of families and calculate a standardized mean effects index of all component outcomes.[26] Note that the survey was conducted four years after the grant and a year after the last election. Party and political attitudes (e.g. support for the ruling party) are reported at the time of the survey, while electoral participation and political actions (e.g. attending a rally) are retrospective measures of pre-election and election activities. For causal identification, this requires that recall error is not correlated with treatment status.

First, an index of ruling party support — vote intentions, support for, work for, and membership in the ruling party, plus support for the President in particular — falls by

[26] We standardize the components, average them, and re-standardize.

0.05 standard deviations. This is not statistically significant, but the sign of the coefficient is the opposite of what we expected. Moreover, while 88% of the control group voted for the President, this declined by 4 percentage points with treatment, significant at the 5% level. This latter result would not hold after correcting for multiple hypotheses within the family, and so we must take it cautiously, but it is worth noting that it is probably the most important political indicator for the national government and it runs in the opposite direction of our prediction.[27] We can certainly rule out an increase in support for the ruling party.[28]

Second, support for and actions on behalf of an opposition party increased by 0.11 standard deviations among those assigned to treatment. The vast majority of opposition support is for Kizza Besigye and his party, the FDC, but we pool all opposition candidates for this analysis. Looking at the components of this family index, all treatment effects are positive.[29] The proportionally largest and statistically significant changes are to feeling close to the opposition party, working for the opposition, being a member of the opposition and actual voting for the opposition. In this context, "working to get a candidate elected" can include being a party activist (e.g. organizing events and rallies) but this role is rare, especially among young people. Rather, in most cases this reflects more informal activities, such as persuading friends and family to support your candidate or turn out to vote. Formal get-out-the-vote efforts are actually outlawed on election day in Uganda.

Treatment appears to have increased voting for an opposition candidate from 12% in

[27] If we adjust for seven comparisons within the family, the coefficient on voting for the President has a p-value of 0.24. We use the Westfall and Young (1993) free step-down resampling method for the family-wise error rate (FWER), the probability that at least one of the true null hypotheses will be falsely rejected, using randomization inference.

[28] Parish-level data also supports the view that the program's effect on support for the ruling party was limited. Using parish-level voting returns in 2011, we can examine the impact of having at least one NUSAF group assigned in the parish, to see if local populations reward the President for targeting the parish with any NUSAF project, including a YOP project. Support for the President is 2.2 percentage points higher in these districts, with a standard deviation of 0.015 (not statistically significant.Table not shown, but the regression is analogous to the treatment effects estimated above. There are 420 eligible parishes in the sample.

[29] One feature of our population is that they are mainly under 35, with about a quarter eligible to vote for the first time. As we illustrate in Appendix B.5, the results are not driven by these young and inexperienced voters. There is no statistically significant difference between first time and older voters, and if anything the average treatment effect is slightly higher when we exclude first time voters.

the control group to 16% in the treatment, a one third increase. While we have to take the patterns within any family with some caution, note that stated preferences for the opposition change proportionally less, and are not statistically significant. If we adjust p-values for the two main family comparisons (NRM/Presidential support and opposition support), the p-value on the opposition support family index is 0.07.

Robustness Our results are robust to alternate specifications but are sensitive to extreme attrition scenarios. As noted above, attrition was greater in the control group. If attrition is correlated with treatment in unobserved ways, then our treatment effects could be spurious (see Appendix B.4). We have no reason to believe that unfound members of the sample are any more or less likely to support the NRM or opposition, however, and indeed our estimates correct for some of the observable determinants of attrition already, including many of the demographics (wealth, education, ethnicity) that are predictive of party support (see Appendix B.2).

Unfound respondents would have to be extremely different from found respondents to account for the size of treatment effect we observe. To give a numerical example, in order to make the treatment effect on opposition support go away, then arithmetically 22% of all unfound control group members would have to have voted for the opposition (assuming found and unfound treatment group members vote the same). This seems implausibly large given that found members of the control group voted for the opposition roughly 12% of the time. We do not see remotely this level of selection in any other covariate.

We can also examine exactly how much these observed covariates matter. If we estimate a simple average treatment effect with strata fixed effects but no covariate controls, we get a coefficient [standard error] of 0.1206 [0.053] on opposition support. If we add weights to account for selection into attrition, the estimate changes to 0.1086 [0.054].[30] This is closer to zero, suggesting that attrition did indeed bias the prior treatment effects away from zero.

[30] We use inverse propensity score weights for the propensity to go unfound based on all covariates, using the leave-one-out method to estimate weights for each individual in the sample.

Table 3: Program impacts on general political participation and partisan action, irrespective of party

	2012 sample			
	(1)	(2)	(3)	(4)
	Control	ITT, with controls		
Dependent variable in 2012	Mean	Coeff.	Std. Err.	N
Index of general electoral political action (z-score)	-0.11	0.06	[.053]	1858
Attended voter education meeting	0.48	0.03	[.026]	1858
Got together with other to discuss vote	0.56	-0.03	[.025]	1857
Reported a campaign malpractice	0.10	0.02	[.017]	1857
Voted in the presidential election	0.91	0.00	[.014]	1857
Attended an election rally (0-3)	1.24	0.04	[.050]	1858
Participated in an political primary (0-3)	0.71	0.04	[.049]	1857
Worked to get a candidate/party elected (0-3)	0.64	0.10	[.051]*	1852
Member of a political party (0-3)	0.85	0.02	[.051]	1851

Notes: Column (1) reports the control group mean, weighted by the inverse probability of selection into each endline sample. Columns (2)–(3) report the estimated intent-to-treat (ITT) coefficient and standard error at endline, using equation 1. Standard errors are heteroskedastic-robust and clustered by group.

To believe that attrition drives our results, we would have to believe that a set of unobserved characteristics unrelated to the covariates we observe have an order of magnitude larger association with treatment and voting. For instance, the control group could only appear to like the ruling party more, because opposition supporters in the control group are less likely to answer the survey as they resent being excluded from the program. We regard these as possible but unlikely scenarios.[31]

General political behavior Increased political action seems to be concentrated among opposition supporters, since it is not associated with a similar increase in political participation in the full sample. Table 3 reports impacts on political participation in general, irrespective of party. These include measures from Table 2 where we ignore the ruling party/opposition distinction, but also includes non-partisan political participation (or po-

[31] In particular, as we note above, something such as resentment would have to translate mainly into nonresponse. Because resentful control group members who do respond to the survey will lower the control group incumbent support, moving it in the same direction as the treatment effect. This could overwhelm the effects of nonresponse. Thus it seems at least as plausible that we understate the decrease in incumbent support.

tentially partisan measures where we do not know the party in question, such as attending a rally).

The program had little effect on the general index of political participation or any of the individual components: whether someone attended voter education meetings, met with others to discuss the election, reporting of malpractice or even whether they voted in the presidential election. The family index rises by 0.06 standard deviations but has a p-value of 0.262. 91% of the sample reported voting, perhaps leaving little room for improvement on this metric, but we likewise see no improvement in the other measures of participation.

The program also had no statistically significant effect on general partisan actions—including attending a political rally, participating in a primary, working to get a candidate elected, or being a member of a party. Only one component measure shows any evidence of change: self-reporting working to get a party elected increased from 64% in the control group to 74% in the treatment, significant at the 10% level. These effects are largely driven by the increase in activity on behalf of the opposition.

3.2.3 Local election outcomes

Table 4 displays the program's impact on support for local politicians. The major elected positions include local councilors at the district level (called LC5s), at the subcounty level (called LC3s), and the village level (called LC1s). While LC1s and LC3s may have played some role in nominating projects to the district, the main nomination process was done at the district level by unelected bureaucrats who nominated projects to the central government for funding. In general, LC5s (who have a strong party affiliation) did not have a formal role in project nomination in the districts. In principle they could have had a behind-the-scenes role, or be a convenient local target for people's support for the ruling party or their antipathy (especially since these races are more competitive than the Presidency). Hence we tracked local impacts.

We first consider incumbent LC5s who served during the YOP disbursement period and

Table 4: Program impacts on local political participation and partisanship

	2012 Sample			
	(1)	(2)	(3)	(4)
	Control	ITT, with controls		
Dependent variable in 2012	Mean	Coeff.	Std. Err	N
Races with an incumbent LC5 :				
Voted or support the previous incumbent LC5 (0-1)	0.560	-0.057	[.037]	890
Races where incumbent was from ruling party	0.650	-0.127	[.042]***	601
Races where incumbent was from opposition	0.422	0.028	[.069]	289
All races:				
Voted in the LC5 election (0-1)	0.867	0.014	[.016]	1852
Approve or Strongly Approve the current LC1 (0-1)	0.795	0.002	[.021]	1853
Approve or Strongly Approve the current LC3 (0-1)	0.784	0.002	[.020]	1856
Approve or Strongly Approve the current LC5 (0-1)	0.773	-0.034	[.022]	1852

Notes: Column (1) reports the control group mean, weighted by the inverse probability of selection into each endline sample. Columns (2)-(3) report the estimated intent-to-treat (ITT) coefficient and standard error at endline, using equation 1. Standard errors are heteroskedastic-robust and clustered by group.

re-ran for election. This is about half of all races. The table also displays treatment effects for whether the individual voted in the LC5 election (a measure of local political participation), and also the approval for current local councilors. Treatment led to a 5.7 percentage point decrease in voting for or supporting the incumbent LC5, regardless of party (not significant). But support for NRM incumbents fell dramatically, by 12.7 percentage points (significant at the 1% level), while support for opposition incumbents rose slightly (not statistically significant). We do not have party affiliation data for LC3s, and LC1s are not officially affiliated with a party. But treatment did not lead to increased support for the current LC1, LC3, or LC5, nor did it significantly increase the likelihood of voting in the local elections.

4 Discussion

This section lays out the major possible explanations for our results. We see some evidence that the fall in incumbent support could arise from misattribution, but at least some of the effect of YOP on opposition support seems to operate through higher incomes.

4.1 Misattribution

The fact that beneficiaries did not reward the ruling party as we expected could be due to the fact that respondents simply did not attribute the YOP program or their own selection to the ruling party. We see only limited evidence for this view. Table 5 presents summary statistics and treatment effects on respondents' beliefs about the program. These effects are post-treatment opinions, however, and so should be taken with some caution. Nonetheless, a few messages are clear:

- Most respondents correctly attributed the introduction of NUSAF and YOP to either the central government or a foreign donor (56% and 32% of the control group), typically the World Bank.[32] People assigned to treatment were slightly more likely to assign the program to a foreign donor, but the difference is not large.

- Most of our sample did not perceive YOP's invention as a political favor, a form of patronage, or even a gift. Rather respondents viewed YOP as programmatic in nature.

- Asked about their group's nomination or selection, most people did not attribute it to a politician or political motive. When asked who nominated their project, the most common answers were the unelected district bureaucratic office for NUSAF, or "don't know". They attributed selection to national or local politicians only about 13% of the time, and treatment had no effect on this perception. Rather treatment led 7% of people to change their answer from "don't know" to the specific unelected bureaucratic office. Most also attributed the reason they were funded as also technocratic or random.

- Those who attribute YOP to someone other than the government were also more likely to support the opposition. This could simply be partisanship coloring opinions, but we can't reject the possibility that misattribution drives our treatment effect on

[32]Both answers were correct, since NUSAF was funded via a large credit from the World Bank, and the government received significant technical assistance from the World Bank to implement. Regrettably, multiple answers were not collected on this survey question, and so we cannot be sure that people did not attribute the program both to the government and the World Bank.

Table 5: Self-reported beliefs about the NUSAF program

	2012 Sample			
	(1)	(2)	(3)	(4)
	Control	ITT, with controls		
Dependent variable in 2012	Mean	Coeff.	Std. Err	N
Who was mainly responsible for giving N. Uganda the NUSAF program?				
The President/NRM/national government	0.559	-0.017	[.024]	1848
District or local politician/official	0.013	0.002	[.005]	1848
Foreign donor (e.g. World Bank, NGO)	0.318	0.025	[.022]	1848
Don't Know	0.122	-0.012	[.015]	1846
What do you think the main motivation was in giving YOP to the people of northern Uganda?				
To develop/assist the north	0.919	0.011	[.012]	1857
To increase political support	0.054	-0.007	[.010]	1858
To make donors happy	0.010	-0.009	[.004]**	1857
Don't know	0.017	0.005	[.006]	1858
Who selected groups to receive YOP funding?				
National government	0.066	0.004	[.013]	1855
District chairperson (elected official)	0.077	0.001	[.015]	1855
NUSAF district technical officer	0.337	0.073	[.022]***	1855
District executive committee	0.071	0.015	[.013]	1855
Community facilitator	0.098	-0.018	[.014]	1855
No answer	0.350	-0.074	[.023]***	1858
Why were groups chosen/not chosen for funding?				
The best quality projects were selected	0.135	0.319	[.023]***	1853
Hard work of group leaders/facilitators	0.146	0.074	[.020]***	1858
Bribe to facilitator	0.010	0.000	[.004]	1853
Relationship with district chairperson	0.073	-0.039	[.011]***	1853
Random	0.152	-0.050	[.017]***	1853
Don't know	0.484	-0.304	[.024]***	1853
Do you think the selection was fair?	0.422	0.407	[.024]***	1856
Thinks likely to receive future program next year	0.761	0.026	[.021]	1868

Notes: Columns (1) and (2) report the control and treatment group means, weighted by the inverse probability of selection into each endline sample. Columns (3)-(4) report the intent-to-treat (ITT) estimated coefficient and p-value from YOP program assignment, using equation 1. Standard errors are heteroskedastic-robust and clustered by group.

incumbent and opposition support. Table 6 reports an ITT regression where we include post-treatment government attribution as a covariate, and interact it with treatment. On average, those who attribute YOP to someone other than the government increase their support for the opposition by 0.166 standard deviations. Opposition support is 0.090 standard deviations lower among those who attribute YOP to the government, but the coefficient on the interaction is not statistically significant.

- We see little effect of beliefs about program selection on opposition support. Among those who thought program selection was fair, opposition support rose by 0.138 standard deviations, compared to 0.123 among those who perceived selection as unfair. Among those who thought program selection was random, opposition support rose 0.157 standard deviations, compared to 0.123 among those who perceived selection as non-random (see Appendix B.6).

Overall, a lack of attribution might explain why the ruling party did not get rewarded at the polls by YOP beneficiaries. But it seems unlikely to explain the decline in Presidential voting or increased electoral action on behalf of the opposition. Hence there must be another mechanism.

4.2 Effects of income and financial freedom

One possibility is an income effect of some kind. At least three theories connect income levels to political behavior.

1. A strand of democratization theory called "modernization theory" argues that economic prosperity hastens democratization. This literature usually emphasizes the relationship between economic and political elites. But there is also a "micro" strand of this literature that argues that reducing poverty creates engaged citizens or more democratic preferences. One example is Welzel et al. (2003), who marshall theory, case

Table 6: Heterogeneity in political impacts by post-program attribution

	Dependent variable (z-score)			
	NRM Presidential support		Opposition support	
	(1)	(2)	(3)	(4)
Assigned to treatment	-0.039	-0.037	0.118	0.166
	[0.052]	[0.079]	[0.053]**	[0.082]**
Attributes program to government	0.201	0.203	-0.172	-0.130
	[0.049]***	[0.070]***	[0.049]***	[0.068]*
Assigned x Government attribution		-0.004		-0.086
		[0.098]		[0.099]
Observations	1,848	1,848	1,848	1,848
R^2	0.107	0.107	0.093	0.093

Notes: This table displays heterogeneity in the ITT results by attribution. Columns (1) and (3) reproduce treatment effects on partisanship adding a dummy for government attribution from Table 2. In the remaining columns, we include a dummy for government attribution and an interaction term between the dummy and treatment assignment. Self-reported beliefs about attribution and selection are post-treatment, and could be affected by treatment status (see Table 5 for ITT effects on these variables).

evidence, and correlations to argue that anti-poverty programs create more self-aware, assertive, critical citizens, who will prefer to act on their political ideals.

2. There is also some evidence that financial independence makes citizens more willing to hold governments accountable. For instance, De Kadt and Lieberman (2015) find that access to public services is correlated with lower support for incumbents across Southern Africa. Using attitudinal survey data, they suggest that improvements in service delivery increase voter expectations of government in terms of service delivery and corruption, and incumbents are punished for disappointing these expectations.

3. Other evidence suggests that financial independence untangles poor people from clientelistic networks. Clientelism is effective in elections principally because some constituents are poor (Weitz-Shapiro, 2012). In her qualitative study of Mexican politics (and the vote buying machine of another semi-autocratic party, the PRI), Magaloni (2006) argues that financially independent voters are less dependent on favors from the ruling party, and thus are more likely to support the opposition. Larreguy et al. (2015) argue that in clientelistic regimes programmatic policies can reduce clients' dependence on political patrons and reduce the power of patrons, and that this is a powerful force

> that can cancel out the rewards that come at the polls for good programs. They find support for this proposition from an urban titling program in Mexico that reduced the value of clientelistic goods and services that patrons had to offer. Hite-Rubin (2015), studying an experimental microfinance initiative in the Philippines, also finds that impersonal microcredit decreased incumbent support. She argues that this is not because it increases incomes but because it untangles people from the credit relationships that underlie party politics and turnout efforts. And Bobonis et al. (2017) show that, in northeast Brazil, vulnerability to drought is associated with closer support for political parties, and citizens who receive cisterns are less likely to be political clients to a party.

While we cannot reject any of these explanations outright, we think four facts and patterns weigh in favor of financial independence untangling people from clientelism.

First, while the absolute income effects are small, vote buying in Uganda is also a cheap affair. Around election time it is common for incumbents, especially the ruling party, to give very small cash gifts to encourage turnout. A gift may be a few dollars or less. This happens openly at rallies throughout the lead-up to the election, and secretly in the days right before election day. Opposition parties have significantly fewer funds for vote buying, and so this is an predominantly ruling party tactic (Blattman et al., 2016). In principle, with greater income, people who received YOP may have chosen to trade off their chances of a cash gift at election time (or other political patronage) in order to act on an intrinsic preference for publicly supporting their preferred party.

Second, people change their political behaviors in support of a party, rather than their party preferences. Across the political outcomes displayed in Table 2, the largest and most statistically significant impacts are on actions (voting, joining a party, or acting on behalf of a party) and not party preference. Given the large number of components, we must take these impacts with some caution. The differences across components are not statistically significant. Nonetheless, the pattern is consistent with people changing behaviors more than partisan preferences.[33] We view this pattern as more consistent with relative financial freedom changing people's public actions and identity, rather than underlying policy preferences.

Third, active and public opposition support is correlated with wealth, and increases in wealth are associated with increases in opposition support.[34] This too is consistent with the hypothesis that relative financial independence reduces clientelism. Table 7 reports five OLS regressions examining the relationship between the endline index of opposition support (from Table 2) and the endline income index.

Higher incomes are associated with more active and public opposition support. In Column 1 of Table 7, we report the results from a regression of opposition support on endline income for the control group only, controlling for all baseline covariates (including baseline income). This is not a causal estimate of income on opposition support, but it does indicate how the variation in income that is not explained by demographics or initial income correlates with opposition support. It is moderate in size (0.13 standard deviations) and significant at the 1% level. It is roughly similar to the correlation in the full sample, in Column 2.[35]

We can also examine the evidence on income as a mediating factor, and try to estimate how much of the effect on opposition support is due to a rise in income. Column 3 replicates the simple ITT on opposition support from Table 2, as a baseline reference. Column 5 adds the endline income index, while column 5 also includes 8 other potential mechanisms (for simplicity and consistency, we include every outcome family reported in either this paper or

[33]The change in political behavior is not large, compared to the relatively large income impact of YOP. Possibly the elasticity of political behavior change to income change is small. But then, the income change is small in absolute terms. The relevant benchmark might not control group incomes, but rather the economic and political benefits that come from supporting the opposition.

[34]Note, however, that this is a period of general growth and incomes rose over time for both the treatment and control groups. Incomes simply increased more for the treatment group. If there were a mechanical connection between wealth and opposition support we might expect to see a generalized increase in opposition support. This implies that the relative wealth gain is important. We can only speculate why this might be the case. It could be that the price of a vote is proportional to income, or something else about the general political equilibrium that only relative income differences matter.

[35]If we omit the baseline covariates, we estimate nearly identical OLS coefficients on income (regressions not shown). If we make the very strong assumption that all effects on opposition support are mediated through income changes (i.e. the exclusion restriction) then we can use assignment to treatment as an instrument for the effect of income on opposition support. This IV coefficient (not shown) is roughly four times as large as the OLS coefficients. It is biased upwards by any other mediators correlated with treatment, income and opposition support. Thus we must take it with caution.

Table 7: Opposition support and income

	Dependent variable: Index of opposition support in 2012 (z-score)				
	Control group	Full sample	Full sample		
	(1)	(2)	(3)	(4)	(5)
Assigned to treatment			0.115	0.086	0.106
			[0.053]**	[0.052]*	[0.047]**
2012 income, z-score	0.131	0.125		0.119	0.097
	[0.043]***	[0.031]***		[0.031]***	[0.028]***
Kin relations (z-score)					0.082
					[0.028]***
Community participation (z-score)					-0.007
					[0.023]
Public good contributions (z-score)					-0.003
					[0.023]
Anti-social behavior (z-score)					0.002
					[0.029]
Protest attitudes and participation (z-score)					-0.069
					[0.027]**
Has migrated since baseline					0.037
					[0.032]
Index of 2011 election influence (z-score)					0.334
					[0.033]***
Existence of a patron (z-score)					0.141
					[0.070]**
Group cooperation (z-score)					-0.003
					[0.011]
Observations	934	1,858	1,858	1,858	1,850
Baseline controls and district fixed effects?	Y	Y	Y	Y	Y

Notes: The 2012 income index is a standardized mean effects index of reported earnings, non-durable consumption, and durable assets. The other outcome indexes represent mean effects indexes of all outcomes analyzed in this paper or the original economic impact analysis in Table VIII of Blattman et al. (2014). Treatment effects on these other outcomes are reported in Appendix B.10. Columns (1) and (2) report the OLS regression of opposition support on income in the control group and the full sample. Column (3) replicates the simple ITT on opposition support, from Table 2 above, for comparison purposes. Columns (4) and (5) examine possible mediators of the treatment effect, adding first the endline income measure then all outcome indexes.

Blattman et al. (2014)).[36] The results suggest income is a mediating factor. After controlling for income in 2012 (columns 3 versus 4), the treatment effect on opposition support falls by 25% ($p < 0.01$), while endline income is just as correlated with opposition support as in Columns 1 and 2. This suggests that a large fraction of the treatment effect we see in Column 4 is coming through an increase in income. When we add in the other eight mechanisms and compare columns 4 and 5, the treatment effect remains similar to that in column 4 (a difference of 2.0 percentage points, $p = 0.27$). Although the coefficient on income slightly drops when adding in these eight mechanisms ($p = 0.05$), the correlation between endline income and opposition support is still high and positive ($p < 0.01$). This suggests that a large portion of the effect we observe on opposition support comes through increases in income.[37]

Fourth, recall that support for the ruling party falls quite steeply at the local but not the national level. One possibility is that, at the national level, the impact of higher incomes on opposition support is counterbalanced by gratitude for the national government's role. Thus votes for the President could moves very little, because the two effects balance out. At the local level, however, this attribution and reciprocity effect are much smaller. Thus the income effects of the program weigh more heavily in political behavior. We view this as a speculative but interesting hypothesis.

Finally, a reasonable implication of the financial freedom story is that treatment should increase the respondent's independence from party operators and patrons. We see mixed evidence on this front. Table 8 reports treatment effects on instances of election influence

[36] Six of these (family cohesion, community participation, public good contributions, anti-social behaviors, protest index, and migration) are families secondary outcomes from Blattman et al. (2014) while the other two (election intimation and existence of a patron) are families of secondary political variables collected for the purpose of the paper. These encompass all secondary outcomes collected during the four year follow-up.

[37] While other endline indexes are significantly correlated with opposition support, this is not sufficient to mediate the effect of treatment on income. To do so, they must also be correlated with treatment, and none are correlated with both treatment and the outcome to a significant degree other than income. This is why we see no fall in the treatment effect when variables are added to Column 6. We expand on this in Appendix B.7, where we perform a more formal mediation analysis. Consistent with the findings reported here, we estimate that almost 25 percent of the treatment effect comes from the measured increase in income—large compared to other mediation analyses of this nature.

Table 8: Program impacts on other political outcomes

	Full sample			
	(1)	(2)	(3)	(4)
	Control	ITT, with controls		
Dependent variable in 2012	Mean	Coeff.	Std. Err.	N
Index of 2011 election influence (z-score)	0.03	0.04	[.049]	1858
Was offered money in exchange for vote (0-3)	0.52	0.06	[.048]	1857
Was threatened during campaign (0-3)	0.23	0.04	[.034]	1857
Was intimidated during campaign (0-3)	0.90	-0.01	[.057]	1857
Was taken to the poll on election day	0.04	-0.02	[.008]**	1858
Any of patrons tried to influence you	0.22	0.02	[.019]	1839
Existence of a patron (z-score)	-0.09	0.14	[.050]***	1850
There is a family member he can go to if in need	0.39	0.04	[.024]*	1844
There is a big man he can go to if in need	0.29	0.04	[.024]*	1840
There is politician he can go to if in need	0.23	0.07	[.021]***	1837
Any of patrons tried to influence you during 2011 election	0.22	0.02	[.019]	1839

Notes: Column (1) reports the control group mean, weighted by the inverse probability of selection into each endline sample. Columns (2)-(3) report the estimated intent-to-treat (ITT) coefficient and standard error at endline, using equation 1. Standard errors are heteroskedastic-robust and clustered by group.

and patron-client ties. We do not see any significant change in most threats and incentives to vote. Treated people were, however, about half as likely to be taken to the poll on election day — a fall of 2 percentage points relative to a mean of 4 percentage points in the control group. The mean is low because such voter mobilization on election day is outlawed in Uganda. A mean effects index of election influence shows no statistically significant impact. YOP increases the likelihood of having a patron, but this is not the kind of patron that mobilizes people for elections.[38]

[38]Only 22% of respondents reported that a patron tried to influence their electoral actions, and this increased by 2 percentage points (not significant) with treatment. We only asked about attempts to influence, not success, and so this does not rule out the possibility that the treated disentangled themselves from election pressure and patronage. But nor is the pattern consistent with the financial freedom story. One interpretation is that business activities and wealth strengthen general financial and social networks, including political networks. Another is that active and public support for a political party (in this case, opposition parties) creates political connections.

4.3 Measurement error

Our outcomes are self-reported and, in principle, are vulnerable to systematic measurement error. We would estimate a false treatment effect if people who received YOP were more likely to report voting for the opposition, or otherwise express their opposition preferences publicly. This could arise, for example, because the control group aspires to future government programs and thinks that saying they voted for the President will increase their chances, even when talking to a supposedly independent study firm.

Social desirability or other bias is a risk, but we think it is unlikely for four reasons. First, the survey asked people whether they expected to receive future transfers, and 76% of both the treatment and control groups said they felt it was likely they or their group would receive a program from a charity or the government in the future. There is no difference. Second, systematic measurement error is difficult to reconcile with the pattern of treatment effects we observe, in particular the absence of any impact on attitudes towards the ruling party and its challengers. It is possible that treatment affects the likelihood of reporting opposition voting/membership/activities but not party support, but this narrows the set of plausible systematic measurement error stories that could explain our results. Third, as with attrition (discussed above) the degree of systematic measurement error correlated with treatment would have to be huge to account for our treatment effects. Finally, we also check for potential bias introduced from enumerator effects and find little evidence of their presence.[39]

[39] Di Maio and Fiala (2017) discuss how enumerator effects can significantly impact responses for sensitive political questions such as were asked in this survey. While enumerator bias is difficult or impossible to eliminate, they suggest a way to check that the bias is at least balanced by treatment status by examining the adjusted R-squared in a regression where treatment status is the outcome variable and enumerator dummies are the explanatory variables. When we do this we obtain an adjusted R2 of 0.049 for assigned to treatment and 0.046 for actually treated. We consider this to be a relatively low number, considering no effort was made to balanced enumerators, and conclude that any bias introduced form enumerator effects is likely similar in treatment and control samples.

4.4 Other explanations

A handful of other mechanisms are possible, but we do not have the data to judge. One possibility, for example, is that increased opposition political engagement could arise from group socialization. People needed to apply in groups. About half of the groups existed prior to the application. Ongoing group interaction could have exposed youth to different political ideas, lowering the cost of political action. We do not have data to test this possibility, but it seems reasonable that new groups would have a bigger effect. But if we compare treatment effects between groups that existed prior to the program to the ones that were created for the purpose of applying for the grant, we find that treatment has the largest effects among pre-existing groups, not new ones.

Finally, the program may have shaped political behaviors by increasing beneficiaries' exposure to the state.[40] For many young people, this may have been one of their first interactions with a state program, local and national. To the extent they perceived the program as poorly designed, corrupt, or otherwise problematic, it could have colored their view of the national and local politicians, or even increased their own sense of efficacy. Perhaps this explains lower support for the ruling party. Unfortunately, we have no data to test this.

5 Conclusions

We analyze the political consequences of a large-scale, successful employment program in Uganda. We find that, rather than rewarding the incumbent ruling party for this programmatic policy, treated young people are slightly less likely to vote for the President and are more likely to engage in campaigning for the opposition. There are multiple possible mechanisms. We see suggestive evidence for at least one: that opposition support is associated

[40] Looking at U.S. welfare recipients, for instance, Soss (1999) finds that beneficiaries develop program-specific beliefs about the usefulness of political participation, their efficacy, and state effectiveness. Moreover local politicians may have used this opportunity to attempt to claim credit for the program (Guiteras and Mobarak, 2016).

with wealth increases, and this is consistent with a story where more successful youth are able to vote their conscience rather than succumb to incentives or pressures to support the ruling party. Of course, we cannot rule out other channels, and do not wish to do so. The income channel is likely only a part of the story. We simply do not have strong evidence for or against most of the alternatives.

Prior evidence has often pointed in the opposite direction—that incumbents are rewarded for patronage and programmatic policies—and so it is possible that this result is unique to Uganda or even this context. We would expect context to play a huge role in any treatment effect of a policy on political behavior, and any number of factors could influence the recipient's reaction to YOP: the nature of the program, the issues at play in this election, or the fact that these are largely first and second-time voters. For example, many of the other programs that have been studied examine repeated cash transfers over time, rather than one-time grants, allowing political parties to claim credit repeatedly. These program features could change the political interpretation and effects.

The Government of Uganda did not continue to run YOP-like programs under the second incarnations of NUSAF. Based on our interviews with World Bank and government officials, we believe this was due to general political difficulties with implementing cash grant programs. While the government has a sincere interest in developing the north, policy makers have been concerned about giving out cash. Other similar programs, like livestock distribution, are common in government programming. In order to continue the spirit of YOP, but in a significantly cheaper manner, the government is pursuing a number of loan guarantee programs, where youth that do not have access to credit pay back loans that have been guaranteed by the government.

Nonetheless, the direction of our treatment effect runs against the received wisdom. Prominent reviews of the literature on distributive politics have called attention to incomplete evidence and possible publication bias. For example, Golden and Min (2013) note that, "it is hard not to suspect that the cases that are studied are often selected precisely because they display prima facie evidence of political distortions in allocative decisions" (p.86). They go on to note that "either that the study of allocations is incomplete, a problem identified by Cox (2010), or that the cumulative results of this research agenda are biased—or both."

The answer is especially important for aid agencies who support supposedly programmatic policies. Scholars and politicians have warned that aid might help rulers stick to power by indirectly undermining development of civil society. Regimes also commonly target aid towards political supporters, translating aid into votes (Jablonski, 2014; Hodler and Raschky, 2014). Uganda, for example, has a semi-autocratic regime that tries to use programs and patronage to insulate itself from competition. It thus seems important to understand how large aid programs affect local politics. But in spite of the high political stakes of national development programs, Western donors prefer to view their development interventions in solely technical terms, overlooking how their reforms and resources affect the balance of political power in the country (Ferguson, 1990).

This particular program evaluation is also important because there are relatively few examples of government interventions that increase incomes. Most microfinance and skills training interventions are implemented by NGOs and seldom have any impact on employment or earnings. Unconditional cash transfers, livestock, or asset transfer programs have had more success at increasing employment and earnings, but these studies have generally not measured changes in political behavior (Blattman and Ralston, 2015). This suggests there is an important opportunity to conduct more "downstream experiments", collecting political opinion data from the beneficiaries of existing evaluations of government programs.

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Appendix for online publication

A Additional design details

A.1 Was NUSAF politically targeted?

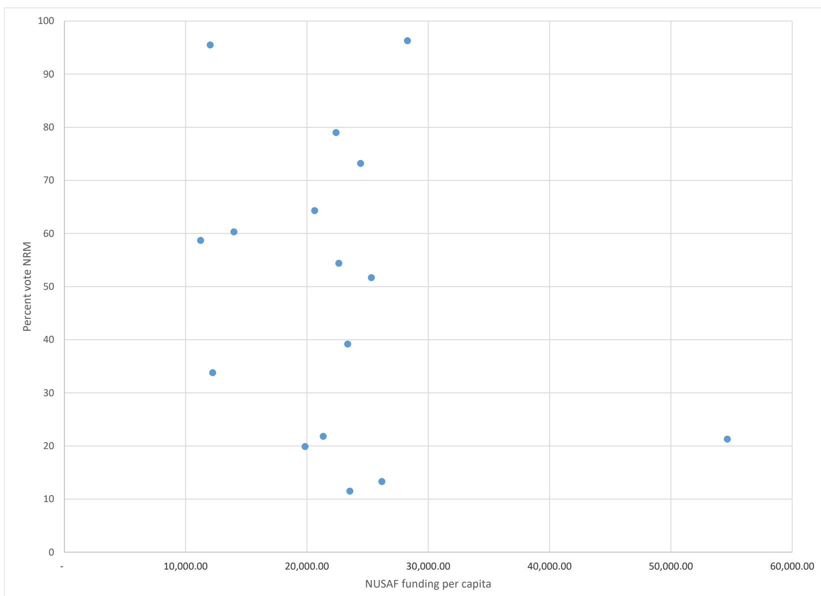
As discussed in Section 2.2, we see little correlation between NUSAF funding and the percentage of votes cast for the ruling NRM party in the previous election at the subcounty level.

Figure 1 presents the NUSAF funding per capita (in Ugandan Shillings) for each of the districts in northern Uganda and the percent of the vote going to the NRM.[41] For any level of support, the majority of districts are in the same range, approximately 10,000 USH to 30,000 USH of funding per person. The one exception is Kitgum district, where funding per capita was very high. As this was the most conflict affected area, and NUSAF was on paper a post-conflict development project, it is likely that funding was purposefully targeted to this area for this mission. However, it is also the district with the lowest support of the NRM, and so could have been subject to manipulation by the central government. In either case, due to funding issues described in the main paper, Kitgum is not part of our sample here.

Manipulation of funding destination by the central government could also have been achieved at the subcounty level, though this would have been a harder level to target due to the complexity of the budgeting process in Uganda and the large number of subcounties present. Table A.1 presents the results of a test for the correlation between the percent of votes for the NRM and the natural log of the funds per capita in each of the subcounties. The first column shows there is a negative and statistically significant relationship between percent of votes and funding. However, this result is once again heavily skewed by data from

[41]The data on NUSAF funding comes from administrative records that include all NUSAF projects funded from 2004 to July 2007, one year before the disbursement to the YOP sample and about a year after the most recent national election. Data on election returns come from

Figure A.1: NUSAF Funding and NRM voter share



Notes: This figure presents a scatterplot of NRM voter share in 2006 and the log of total NUSAF funding per capita by the subcounty level.

Table A.1: Correlation between voter share and NUSAF funding

	Outcome: 2006 NRM voter share			
	No district fixed effects		District fixed effects	
	Correlation	p-value	Correlation	p-value
	(1)	(2)	(3)	(4)
Log of NUSAF funding per capita	-0.660	<0.01	-0.120	0.78
Observations	313		313	
R^2	0.04		0.27	

Notes: This table displays the results of a regression of 2006 NRM voter share on the log of NUSAF funding per capita on the subcounty level. We exclude district fixed effects in columns (1) and (2) and include them in columns (3) and (4). Standard errors are heteroskedastic-robust.

Kitgum district. In the second column we include district dummies. The results are now much smaller and not significant.

A.2 Two-stage surveys and response rates

Both endline surveys (2010 and 2012) were rolled out in two phases In Phase 1, we attempted to interview all 2,677 people in their last known location. In 2010, 37% were not found in their last know location, rising to 39% in 2012, and so they became eligible for tracking in Phase 2. In Phase 2, we selected a random sample of the unfound—53% in 2010 and 38.5% in 2012—stratifying by district and by the proportion unfound in the group for in-depth tracking. For this subset of unfound groups, we made three attempts to find them in their new locations and found 75% of them in 2010 and 59% in 2012. In the analysis, groups are weighted to account for this two-stage process. Those found in Phase 1 receive unit weight, those selected for Phase 2 tracking are weighted by the inverse of their selection probability, while those not selected for Phase 2 tracking are dropped. We have no reports of survey refusal, and no reward was offered for survey completion. See table A.2 for a more detailed presentation of effective response rates.

Table A.2: Survey response rates

Survey	Selection and tracking, by survey phase					Effective response rates				
	Total sought	Found, phase1	Select, phase 2	Found, phase 2	Final # of observations	All	Control	Treatment	Difference (percent-age points)	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
2008 baseline	2,677	97.0%	-	-	2,598	97.0%	94.4%	99.8%	5.3	<0.001
2010 endline	2,677	63.4%	53.0%	74.7%	2,005	85.4%	85.6%	85.3%	-0.8	0.717
2012 endline	2,677	61.0%	38.5%	58.6%	1,868	82.1%	79.1%	85.5%	7.1	0.004

Notes: Column (1) reports the full study sample sought in each round–in general, five people per group over 535 groups, save for one groups where baseline data on seven individuals was accidentally collected. Column (2) reports the percentage of these found in a first survey phase, where each respondent was sought at least once in the town they lived at baseline. Each endline had a second survey phase that tracked a random sample of migrants and other unfound individuals, and Column (3) reports average percentage randomly selected. This percentage varied exogenously by stratum according to the proportion missing and expense of tracking in that district. Column (4) reports the percentage of those sought in phase two successfully surveyed. Column (5) reports the final number of observations by survey round. Columns (6)-(9) report the corresponding response rates overall, by treatment status, and the treatment-control difference (calculated via regression, controlling for baseline district). Columns (6)-(9) are weighted by the inverse probability of selection in phase two of the survey (which varies by strata, with weights ranging from 1 to 4), and are referred to as "effective" response rates. Unfound respondents randomly dropped in phase two receive zero weight. Column (10) reports p-value on the difference term, using robust standard errors clustered at the group level.

A.3 Survey experiment

To manipulate participant ideas about the implementation of the program, we conducted a survey experiment during the four-year endline. The goal of the survey experiment was to manipulate respondents' ideas about who was behind the implementation of the program (World Bank versus the government) and how participants were selected (randomly selected or nominated by the LC V). Individuals were randomized into one of five groups and in each group the introductory script of the survey varied along these two dimensions.

1. World Bank, Random. These surveys emphasized that the program was principally made possible by the action of the World Bank and that the groups were selected randomly to receive funding.

2. World Bank, LC V. These surveys emphasized that the program was principally made possible by the action of the World Bank, but groups were selected by the NUSAF district technical officer (NDTO) under the supervision of the LC V Chairperson.

3. Government, Random. These surveys emphasized that the program was principally made possible by the action of the government and that the groups were selected randomly to receive funding.

4. Government, LC V. These surveys emphasized that the program was principally made possible by the action of the government, but groups were selected by the NDTO under the supervision of the LC V Chairperson.

5. Neutral. None of the above information was presented.

Table A.3 displays the results of our survey experiment. We regress program attribution on an indicator for completing a survey where the introduction said the Government/World Bank was behind the program, including covariates and block fixed effects. We do the same for indicators for the randomization prompt. The results show that the experiment was not successful: individuals who were told the government was behind the program were 5

Table A.3: Survey experiment results

	First stage attribution			First stage selection	
	Government	World Bank		Random	Not random
	(1)	(2)		(3)	(4)
Attribute program to:			Believes selection was		
Government	-0.03	-0.04	Random	0.02	0.02
	[.031]	[.030]		[.017]	[.018]
World Bank	0.05	0.04	Not random	-0.02	-0.02
	[.029]*	[.028]		[.017]	[.018]

Notes: This table displays ITT results from our survey experiment. In column (1), we regress program attribution on an indicator for completing a survey where the introduction said the government was behind the program plus covariates and block fixed effects. In column (2), we include an indicator for completing a survey that said the World Bank was behind the program. In column (3), we regress believe in selection process on an indicator for completing a survey where the introduction said selection was random plus covariates and block fixed effects. In column (4), we include an indicator for completing a survey that said selection was not random.

percentage points more likely to believe the World Bank funded the program. Similarly, individuals who were told selection was not random were 2 percentage points less likely to believe selection was not random.

B Additional analysis

B.1 Baseline balance

Table B.1 displays the results of a regression of treatment on each baseline covariate, controlling for district fixed effects and clustering standard errors by group.

B.2 Correlates of attrition

Table B.2 examines baseline correlates of attrition. We regress an indicator for attrition on all baseline covariates including district fixed effects. Those who are younger, more risk averse or work as casual laborers are more likely to attrit. Since attrition is higher among the young and initially poorer, the average impact of treatment is predicted to be higher. At the same time, the more literate are more likely to be unfound and so this could depress

Table B.1: Baseline balance

	Control		Control - Treat	
	Mean	SD	Diff	p-value
	(1)	(2)	(3)	(4)
Grant amount applied for, USD	7,497.44	2,219.95	143.82	0.29
Applicant group size	22.53	6.83	0.03	0.96
Grant amount per member, USD	363.05	159.40	14.09	0.25
Group existed before application	0.45	0.50	0.03	0.42
Group age, in years	3.80	2.00	-0.05	0.80
Within-group heterogeneity (z-score)	-0.03	0.92	-0.03	0.75
Quality of group dynamic (z-score)	-0.02	1.02	0.05	0.53
Distance to educational facilities (km)	6.84	6.50	0.48	0.35
Individual unfound at baseline	0.06	0.23	-0.05	0.00
Age at baseline	24.75	5.22	0.17	0.55
Female	0.35	0.48	-0.02	0.38
Large town or urban area	0.23	0.42	-0.02	0.61
Risk aversion index (z-score)	-0.02	1.00	-0.01	0.75
Any leadership position in group	0.28	0.45	-0.00	0.88
Group chair or vice-chair	0.11	0.31	0.01	0.33
Weekly employment, hours	10.70	15.82	0.57	0.48
All non-agricultural work	5.99	12.47	-0.45	0.44
Casual labor, low skill	1.03	5.19	-0.11	0.63
Petty business, low skill	2.24	6.95	0.21	0.52
Skilled trades	1.78	8.41	-0.33	0.40
High-skill wage labor	0.04	0.58	0.08	0.02
Other non-agricultural work	0.91	4.76	-0.29	0.10
All agricultural work	4.66	10.08	1.04	0.04
Weekly household chores, hours	8.96	17.59	0.30	0.73
Zero employment hours in past month	0.48	0.50	-0.04	0.18
Main occupation is non-agricultural	0.26	0.44	0.00	0.92
Engaged in a skilled trade	0.08	0.27	0.00	0.81
Currently in school	0.04	0.21	-0.01	0.45
Highest grade reached at school	7.95	2.92	-0.07	0.62
Able to read and write minimally	0.75	0.43	-0.03	0.17
Received prior vocational training	0.07	0.26	0.02	0.07
Digit recall test score	4.16	2.00	-0.04	0.64
Index of physical disability	8.68	2.52	-0.14	0.29
Wealth Index	-0.16	0.96	0.07	0.12
Savings in past 6 mo. (000s 2008 UGX)	19.25	98.19	10.89	0.02
Monthly gross cash earnings (000s 2008 UGX)	62.19	129.04	6.89	0.30
Can obtain 100,000 UGX ($58) loan	0.33	0.47	0.05	0.01
Can obtain 1,000,000 UGX ($580) loan	0.10	0.30	0.01	0.46

Continued on following page

Table 10: Baseline balance (continued)

	Control		Control - Treat	
	Mean	SD	Diff	p-value
	(1)	(2)	(3)	(4)
Registered to vote in 2006	0.92	0.27	-0.01	0.57
Voted in 2006 presidential election	0.73	0.45	0.03	0.21
Voted in 2005 referendum	0.60	0.49	0.01	0.67
Voted in 2005 district election	0.68	0.47	0.01	0.59
Member of a political party	0.11	0.31	0.02	0.06
Participated in election of community leaders in past year	0.45	0.50	0.01	0.72
Attended community meetings in past month	0.47	0.50	-0.00	0.83
Is a community mobilizer	0.45	0.50	-0.01	0.50
Currently a community leader	0.26	0.44	0.01	0.61
Currently on a community committee	0.17	0.38	0.01	0.60
Would accept nomination to be community leader	0.68	0.47	-0.01	0.75
Ethnicity: Acholi	0.00	0.03	0.01	0.01
Ethnicity: Alur	0.02	0.14	0.00	0.37
Ethnicity: Bagwere	0.04	0.19	0.01	0.24
Ethnicity: Iteso	0.14	0.35	-0.02	0.20
Ethnicity: Karamojong	0.06	0.23	0.00	0.84
Ethnicity: Langi	0.44	0.50	0.00	0.74
Ethnicity: Lugbara	0.10	0.30	0.00	0.66
Ethnicity: Madi	0.08	0.26	0.01	0.58
Observations		1574		
p value on F-statistics on all covariates		0.045		

Notes: Columns (1) and (2) report the control mean and standard deviation, respectively. A small number of missing values are imputed at the median. Column (3) and (4) report the difference between control and treatment and corresponding p-value from ordinary least squares regressions of each baseline covariate on a treatment indicator, controlling for block fixed effects and clustering by group.

their predicted returns from a grant.

B.3 Sensitivity of baseline balance to baseline non-response

Table B.3 looks at the sensitivity of randomization balance to alternate values for the missing control groups. The table examines four baseline covariates displaying randomization imbalance at baseline: durable assets, prior vocational training, ability to obtain a 100,000 UGX loan, and savings in the past 6 months. All covariates are standardized and missing data in the treatment group are imputed to the mean, or zero. However, missing control group data are imputed to the mean (zero) plus 0.05, 0.10, 0.15, 0.20, or 0.25 SD of the covariate, thus gradually increasing the values of the covariates in the control group towards balance. In general, imputed values of 0.10 to 0.20 SD are sufficient to bring the regression differences to zero.

B.4 Robustness

We perform two sets of additional treatment analyses. First, as shown in Table B.5, our results are robust to alternative regression specifications. Column 1 displays coefficients from our preferred specification in the paper. In columns 2 to 5, we test four alternate specifications. First, we drop all controls and only include randomization block fixed effects. In the second specification, we add only demographic covariates. Next, we add all human and physical capital controls. The last specification shows group-level effects (the unit of randomization).

Second, we check that our results are robust to alternative attrition scenarios. In Table B.6, we calculate lower and upper bounds for the coefficients of our three main dependent variables: Index of NRM/Presidential support, index of opposition support, and index of general election political action. Following Karlan et al. 2015, we calculate lower bounds (Panel A) by imputing relatively high values to missing observations in the control group (control group mean plus 0.025, 0.05, 0.10 or 0.50 SD of the found control distribution), and

Table B.2: Correlates of attrition

Baseline covariate	Dependent variable: Indicator for attrition					
	2010 endline			2012 endline		
	Coeff.	Std. Err.	Effect of 1 SD change in covariate	Coeff.	Std. Err.	Effect of 1 SD change in covariate
	(1)	(2)	(3)	(4)	(5)	(6)
Assigned to treatment	0.020	[0.020]	.	-0.050	[0.023]	.
Grant amount applied for, USD	0.000	[0.000]	-0.030	0.000	[0.000]	0.000
Group size	0.000	[0.005]	0.003	-0.005	[0.004]	-0.034
Grant amount per member, USD	0.000	[0.000]	0.035	0.000	[0.000]	-0.010
Group existed before application	-0.016	[0.024]	.	-0.030	[0.025]	.
Group age, in years	0.001	[0.005]	0.002	-0.002	[0.006]	-0.003
Within-group heterogeneity (z-score)	0.013	[0.011]	0.013	0.026	[0.013]**	0.026
Quality of group dynamic (z-score)	0.008	[0.013]	0.008	-0.009	[0.016]	-0.009
Distance to educational facilities (km)	0.002	[0.002]	0.015	0.000	[0.003]	-0.002
Age at baseline	-0.003	[0.002]*	-0.018	-0.006	[0.002]***	-0.030
Large town/urban area	0.081	[0.030]***	.	0.143	[0.036]***	.
Risk aversion index (z-score)	0.039	[0.011]***	0.039	0.046	[0.012]**	0.046
Management committee member	-0.044	[0.018]**	.	-0.042	[0.024]	.
Chairperson or vice-chairperson	0.013	[0.027]	.	0.023	[0.036]	.
Weekly work hours: Casual labor	0.003	[0.002]*	0.017	0.003	[0.003]	0.013
Weekly work hours: Own business	0.001	[0.001]	0.005	-0.001	[0.002]	-0.010
Weekly work hours: Skilled trades	0.002	[0.001]*	0.018	0.000	[0.002]	0.004
Weekly work hours: High-skill wage labor	0.001	[0.009]	0.001	-0.017	[0.010]	-0.014
Weekly work hours: Other non-ag work	0.003	[0.003]	0.013	-0.002	[0.002]	-0.007
Weekly work hours: All agricultural work	-0.005	[0.001]***	-0.056	-0.005	[0.001]	-0.052
Weekly household chores, hours	-0.001	[0.000]	-0.012	-0.001	[0.001]	-0.017
Zero employment hours in past month	-0.134	[0.032]***	.	-0.149	[0.034]	.
Main occupation is non-agricultural	-0.171	[0.037]***	.	-0.094	[0.047]	.
Engaged in a skilled trade	-0.061	[0.036]*	.	-0.043	[0.053]	.
Currently in school	-0.083	[0.034]**	.	-0.067	[0.052]	.
Highest grade reached at school	-0.002	[0.003]	-0.007	0.000	[0.004]	0.000
Able to read and write minimally	0.065	[0.021]***	.	0.048	[0.026]	.
Received prior vocational training	-0.034	[0.030]	.	-0.051	[0.037]	.
Digit recall test score	-0.008	[0.004]**	-0.016	0.016	[0.006]***	0.033
Index of physical disability	-0.006	[0.002]***	-0.014	-0.002	[0.003]	-0.004
Durable assets (z-score)	0.016	[0.011]	0.017	-0.008	[0.012]	-0.009
Savings in past 6 mo. (000s 2008 UGX)	0.000	[0.000]	0.011	0.000	[0.000]***	0.035
Monthly cash earnings (000s 2008 UGX)	0.000	[0.000]*	-0.014	0.000	[0.000]	-0.017
Can obtain 100,000 UGX ($58) loan	-0.024	[0.020]	.	-0.011	[0.022]	.
Can obtain 1,000,000 UGX ($580) loan	-0.014	[0.028]	.	0.005	[0.037]	.
Observations		2,232			2,111	
Mean of dependent variable		-0.146			-0.179	
p-value on F-test of joint significance, all covariates		<0.001			<0.001	

Notes: Columns (1)-(2) and (4)-(5) report the coefficients and standard errors from a weighted least squares regression of an indicator for attrition on the baseline covariates used in all treatment effects regressions and listed in Table II (excluding the indicator for unfound at baseline). Weights are the inverse of the probability of selection into endline tracking. To provide a sense of magnitude, columns (3) and (6) report the product of the standard deviation of the baseline variable (in Table II) and the coefficients in Columns (1) and (4), with the exception of indicator variables. Robust standard errors are clustered at the group level. *** p<0.01, ** p<0.05, * p<0.1

Table B.3: Sensitivity of baseline randomization balance to imputation of missing control group data

Baseline covariate exhibiting treatment imbalance (z-score)	Missing control group data imputed to the mean plus:	Balance statistics with imputed control group data							
		Control group			Treatment group			Regression difference	
		Mean	SD	Obs	Mean	SD	Obs	Coeff.	p-value
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Durable assets	+0.05 SD	-0.01	0.95	1,352	0.05	1.06	1,325	0.03	0.49
	+0.10 SD	0.02	0.97	1,352	0.05	1.06	1,325	0.01	0.90
	+0.15 SD	0.05	1.00	1,352	0.05	1.06	1,325	-0.02	0.69
	+0.20 SD	0.07	1.05	1,352	0.05	1.06	1,325	-0.05	0.39
	+0.25 SD	0.10	1.10	1,352	0.05	1.06	1,325	-0.08	0.20
Prior vocational training	+0.05 SD	0.02	0.97	1,352	0.02	1.04	1,325	0.04	0.39
	+0.10 SD	0.05	0.99	1,352	0.02	1.04	1,325	0.01	0.83
	+0.15 SD	0.08	1.02	1,352	0.02	1.04	1,325	-0.02	0.70
	+0.20 SD	0.11	1.07	1,352	0.02	1.04	1,325	-0.05	0.36
	+0.25 SD	0.13	1.12	1,352	0.02	1.04	1,325	-0.07	0.17
Can obtain 100,000 UGX loan	+0.05 SD	-0.02	0.96	1,352	0.09	1.02	1,325	0.09	0.03
	+0.10 SD	0.01	0.99	1,352	0.09	1.02	1,325	0.06	0.15
	+0.15 SD	0.04	1.02	1,352	0.09	1.02	1,325	0.03	0.45
	+0.20 SD	0.07	1.07	1,352	0.09	1.02	1,325	0.01	0.89
	+0.25 SD	0.09	1.12	1,352	0.09	1.02	1,325	-0.02	0.68
Savings in past 6 mo.	+0.05 SD	-0.02	0.82	1,352	0.06	1.16	1,325	0.06	0.11
	+0.10 SD	0.01	0.84	1,352	0.06	1.16	1,325	0.03	0.39
	+0.15 SD	0.03	0.88	1,352	0.06	1.16	1,325	0.01	0.87
	+0.20 SD	0.06	0.94	1,352	0.06	1.16	1,325	-0.02	0.65
	+0.25 SD	0.09	1.00	1,352	0.06	1.16	1,325	-0.05	0.33

Notes: This table recalculates balance for four baseline covariates displaying randomization imbalance at baseline, in Table B.1. Approximately 6% of control group observations are missing and a very small number of treatment group observations are missing (people who completed the survey but did not respond to a specific question). All covariates are standardized and missing treatment data are imputed to the mean, or zero. Missing control group data are imputed to the mean plus 0.05, 0.10, 0.15, 0.20, or 0.25 SD of the variable, thus gradually increasing the values of the covariates in the control group. Columns (1) to (6) report summary statistics (mean, SD, and number of observations) for the imputed treatment and control group values. Columns (7) and (8) recalculate treatment-control mean differences using an ordinary least squares regression of the covariate on assignment to treatment and district (randomization strata) fixed effects. The standard error in Column (8) is robust and clustered by group.

Table B.5: Robustness to alternate specifications

Outcome variable	Main specification (1)	No controls, district FE (2)	Plus demographics (3)	Plus human/ physical capital (4)	Randomization inference (5)
	Alternate specification				
Index of NRM/Presidential support	-0.041	-0.019	-0.019	-0.021	-0.041
	[.052]	[.054]	[.054]	[.053]	[.054]
Index of opposition support	0.115	0.121	0.111	0.110	0.115
	[.053]**	[.053]**	[.053]**	[.053]**	[.052]**
Index of general election political action	0.059	0.093	0.075	0.075	0.059
	[.053]	[.056]*	[.054]	[.053]	[.053]
District (randomization block) FE	Y	Y	Y	Y	Y
Demographics controls	Y	N	Y	Y	Y
Human/physical capital controls	Y	N	N	Y	Y
Group and political controls	Y	N	N	N	Y
Group level of analysis	N	N	N	N	Y
Observations	1858	1858	1858	1858	1858

Notes: The table displays four alternate specifications to test the robustness of our results. Column (1) displays our main specification. Column (2) displays the results of a regression of the outcome measure on treatment and randomization block (district) fixed effects without any controls. Column (3) adds in demographic controls while column (4) adds in both demographic controls and human and physical capital controls. Column (5) is the same as our main specification but calculates effects at the group-level The overall summary indexes are the standardized mean of its composite outcomes, standardized. Heterosketastic robust standard errors are reported in brackets.
$***p < 0.01$, $**p < 0.05$,$*p < 0.10$

Table B.6: Robustness to alternate attrition scenarios

		Impute missing dependent variable with mean = +(-) X SD for missing control (treatment) respondents				"Worst case"
	Main					Manski
Outcome variable (z-score)	specification	0.025 SD	0.05 SD	0.10 SD	0.25 SD	bound
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Upper bound						
NRM/Presidential support	-0.041	-0.020	-0.013	0.002	0.047	0.508
	[.052]	[.046]	[.046]	[.046]	[.046]	[.064]***
Opposition support	0.115	0.094	0.101	0.116	0.161	0.671
	[.053]**	[.045]**	[.045]**	[.045]**	[.046]***	[.071]***
General political action	0.059	0.080	0.087	0.102	0.147	0.804
	[.053]	[.047]*	[.047]*	[.047]**	[.047]***	[.078]***
Panel B: Lower bound						
NRM/Presidential support	-0.041	-0.035	-0.043	-0.058	-0.103	-0.529
	[.052]	[.046]	[.046]	[.046]	[.046]**	[.061]***
Opposition support	0.115	0.079	0.071	0.057	0.012	-0.601
	[.053]**	[.045]*	[.046]	[.046]	[.046]	[.081]***
General political action	0.059	0.065	0.057	0.042	-0.003	-0.682
	[.053]	[.047]	[.047]	[.047]	[.047]	[.075]***
Observations	1858	2025	2025	2025	2025	2025

Notes: The table reports robustness to alternative attrition scenarios. We impute missing observations for the dependent variables. In columns 2 – 5, we impute missing dependent variables for the treatment group as the found treatment mean minus a multiple of the standard deviation of the treatment distribution. Similarly, we impute missing dependent variables for the control group as the found control mean plus a multiple of the standard deviation of the control distribution. In column 6 we apply Manski bounds, imputing the minimum value for unfound treated members and the maximum for unfound controls. Each regression controls for baseline covariates and district fixed effects. The overall summary indexes are the standardized mean of its composite outcomes, standardized. Heterosketastic robust standard errors are reported in brackets.
$***p < 0.01$, $**p < 0.05$,$*p < 0.10$

relatively low values to missing observations in the treatment group (treatment group mean minus 0.025, 0.05, 0.10 or 0.50 SD of the found treatment distribution). The procedure is reversed to calculate upper bounds (Panel B): we impute relatively low (high) values to missing observations in the control (treatment) group. We also compute the lowest (and highest) bound (column 6) following Manski 1990, by imputing the maximum (minimum) value for unfound control members and the minimum (maximum) for unfound treated.

What this table shows is that the impacts of treatment on opposition support would be spurious if the unfound members of the control group are more likely to be government supporters than found members of the control group.

Table B.7: Robustness to attrition weights

Outcome variable	Alternate specification	
	Main specification	Using attrition weights
	(1)	(2)
Index of NRM/Presidential support	-0.041	-0.031
	[0.052]	[0.053]
Index of opposition support	0.115	0.115
	[0.053]**	[0.054]**
Index of general election political action	0.059	0.053
	[0.053]	[0.054]
Observations	1858	1858

Notes: The table displays our main specification and a version where we reweight for attrition. The overall summary indexes are the standardized mean of its composite outcomes, standardized. Heterosketastic robust standard errors are reported in brackets.
$***p < 0.01$, $**p < 0.05$,$*p < 0.10$

We have no theoretical reason to believe that attrition is correlated with government support, but obviously this is unobserved. As seen in Appendix B.2, at baseline the unfound tended to be younger, poorer, less literate farmers from larger communities. Our intent-to-treat estimates control for these observable correlates of attrition, reducing the risk of bias. Table B.7 also compares our main specification with one where we use inverse propensity score weights for the propensity to go unfound based on all covariates, using the leave-one-out method to estimate weights for each individual in the sample. The results are very similar to our main specification, suggesting that attrition is not driving our results.

B.5 Treatment effects by age

In table B.8, we analyze treatment effects by age to see if the effect is driven by first-time voters. At baseline we could not collect data on whether individuals previously voted and who they voted for, because of restrictions from the government partner and research funder (the World Bank). We do, however, have their age at baseline, which allows us to separate the sample by those who were old enough to vote in the previous election versus those who were not.

Table B.8: Impacts by age

	DV: Opposition support		
	Effect for those 20 or under in 2008	Effect for those over 20 in 2008	Entire sample
	(1)	(2)	(3)
Assigned to treatment	0.066	0.146	0.139
	[0.088]	[0.064]**	[0.063]**
Age 20 or under			-0.075
			[0.072]
Assigned x age 20 or under			-0.106
			[0.107]
Observations	371	1,487	1858

Notes: The table reports treatment effects on opposition by age as a proxy for first time voting. In column 1, we limit the sample to individuals aged 20 or under (or those who were not eligible to vote in the previous election). In column 2, we limit the sample to individuals above the age of 20 (or those eligible to vote in the previous election). In column 3, we use the entire sample and include a dummy for being below 20 and an interaction between treatment and the dummy.
$***p < 0.01$, $**p < 0.05$,$*p < 0.10$

The figure shows that potential first time voters (individuals who were under 18 in 2005/20 or under in 2008) see no rise in opposition support. The effects are concentrated among those who were eligible to vote in the previous election.

The lack of impact on young people offers some evidence that the effect we observe is more about preferences. The impacts are coming from individuals who have more experience voting These are not novices with an underdeveloped set of values. They are also more likely to know the consequences of voting. However, this is speculative so we take this result with caution.

B.6 Heterogeneity by fair and random selection

In Table B.9, we display treatment effects by individual's perceptions of the selection process. Among those who thought program selection was fair, opposition support rose by 0.13 standard deviations, compared to 0.121 standard deviations among those who perceived

Table B.9: Heterogeneity by fair and random

	DV: Opposition support			
	Thought selection was		Thought selection was	
	fair	not fair	random	not random
	(1)	(2)	(3)	(4)
Assigned to treatment	0.138	0.123	0.157	0.123
	[0.067]**	[0.113]	[0.143]	[0.058]**
Observations	1,160	696	234	1,624
R-squared	0.092	0.136	0.292	0.085

Notes: This table displays ITT results by individual's perceptions of selections. In columns 1 and 2 we show the treatment effect on individuals who thought selection was fair or not. In columns 3 and 4, we limit the sample to individuals who thought selection was random/not random.

selection as unfair. Among those who thought program selection was random, opposition support rose 0.169 standard deviations, compared to 0.123 standard deviations among those who perceived selection as non-random

B.7 Mediation analysis

In Table B.10 we conduct the mediation analysis described in Keele et al. (2015). In columns 1 and 2, we display treatment effects on all mediators displayed in section 4.2. In columns 3 and 4, we regress opposition support on treatment and each mediator, and display the coefficient and standard error from each mediator. In columns 5 and 6, we regress opposition support on treatment. In column 7, we display the percent of the effect on opposition support mediated by each of variable listed. This is calculated by multiplying the coefficients in column 1 by the coefficients in column 3, divided by the coefficients of column 5. We see that our income index mediates a quarter of the total effect on opposition support, which is large compared to other mediation analyses. The second largest factor is migration, which mediates only 10 percent of the effect we see. All other mediators explain only 5% of the effect we see on opposition support.

Table B.10: Mediation analysis

	Y: Opposition support; T: Treatment; M: Mediator						
	Reg. of M on T		Reg. of Y on T and M		Reg. of Y on T		
	Coeff. on T		Coeff. on M		Coeff. on T		Percent
	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.	mediated
Mediator M	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Income, z-score	0.24	[.053]***	0.11	[.031]***	0.11	[.054]**	0.24
Index of 2011 election intimidation, z-score	0.03	[.051]	0.12	[.032]***	0.11	[.054]**	0.03
Existence of a patron, z-score	0.14	[.051]***	-0.02	[.025]	0.11	[.054]**	0.02
Kin relations, z-score	0.05	[.048]	-0.05	[.026]*	0.11	[.054]**	0.02
Community participation, z-score	0.01	[.053]	-0.01	[.026]	0.11	[.054]**	0.00
Public goods contributions, z-score	0.02	[.050]	-0.02	[.029]	0.11	[.054]**	0.00
Antisocial behaviors, z-score	0.01	[.050]	0.10	[.033]***	0.11	[.054]**	0.01
Protest attitudes and participation, z-score	-0.02	[.045]	0.34	[.034]***	0.11	[.054]**	0.06
Migrated	-0.04	[.036]	0.15	[.081]*	0.11	[.054]**	0.05
Group cooperation, z-score	-0.21	[.133]	-0.02	[.012]**	0.11	[.054]**	0.05

Notes: Columns (1) and (2) represent regressions of each mediator on treatment. Columns (3) and (4) display regressions of opposition support on treatment and the mediator, Columns (5) and (6) display regressions of opposition support on treatment. Column (7) displays the percent of the effect of opposition support mediated by the variables listed. This is calculated as the coefficient in (1) times the coefficient in (2) divided by the coefficient of (3). See Keele et al. (2015) for more details. Standard errors are heteroskedastic-robust and clustered by group.We calculate the ITT via a weighted least squares regression of the dependent variable on a program assignment indicator, 13 district (randomization stratum) fixed effects, and a vector of control variables that includes all of the baseline covariates reported in Appendix B.1.

Table B.11: Program impacts on other outcomes

	Full sample			
	(1)	(2)	(3)	(4)
	Control	ITT, with controls		
Dependent variable in 2012	Mean	Mean	SD	N
Elections were free and fair (0-3)	2.125	-0.051	[.045]	1817
Thinks it is likely that powerful people can find out how they voted	1.570	0.024	[.054]	1776
Thinks tax officials are corrupt (0-3)	1.547	-0.019	[.043]	1572
The tax department always has the right to make people pay taxes	2.439	-0.037	[.048]	1782
Enumerator sent by the government	0.408	0.005	[.024]	1755
Enumerator sent by the International org	0.324	0.017	[.023]	1755
Enumerator sent by others	0.268	-0.022	[.022]	1755
Knows the name of LC3 and LC5 (0-1)	0.734	0.016	[.022]	2022

‘*Notes*: This table displays ITT impacts on outcomes not displayed in the main tables. We regress each outcome on treatment assignment, baseline covariates and block (district) fixed effects. We weight observations by the inverse of the probability of selection into the endline survey.

B.8 Other outcomes

Table B.11 displays ITT effects on minor outcomes we collected that did not make it into the main paper.