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

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Abstract

A Ugandan government program allowed groups of young people to submit proposals to start skilled enterprises. Among 535 eligible proposals, the government randomly selected 265 to receive grants of nearly \$400 per person. Blattman et al. (2014) showed that, after four years, the program raised employment by 17% and earnings 38%. This paper shows that, rather than rewarding the government in elections, beneficiaries increased opposition party membership, campaigning, and voting. Higher incomes are associated with opposition support, and we hypothesize that financial independence frees the poor to express political preferences publicly, being less reliant on patronage and other political transfers.

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

1 Introduction

What are the political impacts of development programs? Governments that deliver public or private goods to their constituents hope to be rewarded at the polls, even when those policies are "programmatic" in that they are targeted based on need or merit rather than in a political or clientelistic manner. There are good reasons for this belief. In developed democracies there are longstanding arguments and evidence that voters punish or reward incumbents for effective policies, for general economic conditions, and even for events well 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.[2]

There is now a good deal of evidence that voters reward governments for programmatic policies in middle-income democracies, especially from various social safety net programs in Latin America. Golden and Min (2013), reviewing this evidence, note that most studies have found that as transfers to a district rise, voter turnout and incumbent vote share tend to rise as well.[3] Nonetheless it is probably too early to draw firm conclusions. Golden and Min not only note some exceptions to this pattern, but also raise concerns of publication bias against null findings.[4] Indeed, as this paper will show, transfer programs have sometimes unexpected political consequences.

Also striking is that we know very little about the effects of programmatic policies on politics in low-income countries. The evidence we have comes mainly from high- and middle-income countries, and it is especially scarce on programs that are not explicitly clientelistic

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[1]A large literature argues that voters reward incumbents for general economic conditions ("sociotropic voting") because they themselves are doing better or stand to gain ("pocketbook voting") (e.g. Kinder and Kiewiet, 1981; Gomez and Wilson, 2001). Achen and Bartels (2004) and Healy et al. (2010) also show that voters punish politicians for irrelevant events, such as shark attacks or sports game outcomes, suggesting voters may follow a form of blind retrospection.

[2]These largely programmatic appeals and competition are at the heart of a more traditional theory of responsible party government and programmatic politics rather than patronage-based government (Kitschelt and Wilkinson, 2007; Golden and Min, 2013).

[3]In Uruguay, Manacorda et al. (2011) find that households that benefited from a CCT are 11 to 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 conditional cash transfer (CCT) program have 7% higher turnout and 9% higher incumbent vote share (though Imai et al. (2016) have pointed out that this is primarily driven by increases in registration rather than higher turnout by registered voters, and Schober (2016) argues that the effect is limited to turnout and not incumbent vote share).

[4]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.

or political pork—that is, programs that are legislated by a particular political party but distributed in a relatively non-partisan way such that benefits cannot easily be withdrawn or tied to political support.[5] Patronage and pork are common and so deservedly get a lot of attention in the literature. But parties also compete programmatically, and it is important to understand the political rewards of programmatic policies as well.

Another reason to be interested in the poorest countries is that many of their social programs are funded by foreign aid. In Uganda, for example, foreign governments and the World Bank fund the government to implement dozens of health, education, and economic programs—totaling about 40% of the national budget. The specific program we study 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.[6] Uganda, for example, has a semi-autocratic regime that tries to use programs and patronage to insulate itself from political competition. It seems important to understand how large aid programs affect local politics. But in spite of the undoubtedly 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 or are affected by the balance of political power in the country.[7]

In 2006-07 Uganda's central government, with assistance from The World Bank, developed the Youth Opportunities Program (YOP) 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 train in and start independent trades. Thousands of groups applied. Local government nominated proposals for funding after being reviewed for technical qualifications by a government bureaucracy created for the program. In 2008, the government bureaucratic agency identified 535 eligible groups and worked with the authors to award the grants randomly to 265. Successful groups received one-time grants to pay for training and start-up costs. The grants averaged $382 per group member—roughly the annual income of the average applicant. A majority of people

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[5]Thachil (2011), for instance, has argued that the BJP political party in India benefited electorally when its grassroots organizations provided generalized social welfare services to a non-traditional demographic of poor and low-caste households.

[6]See Moss et al. (2006) for a review of this literature. Besley and Persson (2011) also find that taxation develops state capacity and accountability, and aid can undermine both.

[7]This idea of aid donors as "anti-politics machines" has been argued generally by Ferguson (1990) and in the specific case of Uganda by Tangri and Mwenda (2008).

attributed the program to the incumbent government, though foreign donors also received significant credit.

YOP had large impacts on employment and earnings. We experimentally evaluated the economic impacts in 2010 and 2012 in a companion paper (Blattman et al., 2014) and found that most group members invested the grants, partly in training but mainly in physical capital. Four years later, YOP participants were more than twice as likely to be practicing a skilled trade than the control group, and had 38% higher earnings and 17% more hours of work. The absolute income gains are small (just under a dollar a day in purchasing power parity or PPP terms). But this is a huge gain in relative terms for otherwise very poor people. Consequently, YOP is one of the few examples of an employment program that has documented substantial, cost-effective impacts on work levels and earnings.

In this paper we compare successful and unsuccessful applicants to understand the political impacts of YOP. 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 the program was completed, not only were YOP beneficiaries no more likely to vote for the ruling party than the control group, they were also more likely to work to get opposition parties elected. We document the program, the randomized evaluation, the direct economic impacts, the longer term effects on political behavior, and possible explanations for the results we obtain. We cannot draw firm conclusions from the evidence, but overall the patterns are consistent with the idea that programmatic policies and economic success free people to express their political preferences and decouple them from clientelistic systems.

For instance, YOP had little impact on election turnout or approval ratings for the ruling party and incumbent President. If anything there was a modest decrease in support for the President and ruling party. 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.

Unexpectedly, 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 (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 more than 21 percentage points less likely to vote for an incumbent ruling candidate than an opposition one.

Naturally, one explanation is systematic measurement error: that people who received

YOP are simply more likely to tell us they voted for the opposition (or control group members are more likely to report they voted for the President). While possible, we don't see any treatment effects on expressed preferences or support for either party. Rather we see only treatment effects on voting and other public political behavior (such as encouraging others to vote for a particular party). If survey measurement error is correlated with treatment, it is not clear why reported behavior would be affected but not party preferences.

Another possible explanation is that wealth and financial independence free voters from clientelistic networks and allow them to act on their true political preferences—an effect consistent with evidence from South Africa, Mexico and the Philippines (Magaloni, 2006; Larreguy et al., 2015; De Kadt and Lieberman, 2015; Hite-Rubin, 2015). Three patterns in Uganda are consistent with this view. First, voting and public actions for the opposition change more than stated party preferences. Second, we see a strong correlation between earnings and this public opposition support in our sample, suggestive evidence that this effect of YOP on political action is mediated by the income change. Finally, YOP beneficiaries were also slightly less likely to be mobilized to turn out to vote by election operatives.

There are interesting parallels here to "modernization theory"—the idea that economic development drives democracy. Welzel et al. (2003), for instance, argue that material security increases people's preferences for liberty and expressive political action. Our results are consistent with this view, though we do not have the attitudinal data to test it more directly.

There are other possible explanations for the null effect on support for the ruling party. Our sample could simply attribute the program to foreign funders and either fail to reward (or punish) the incumbent government.[8] 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.[9] While possible, these explanations do not seem to fit the patterns we observe. First, they do not explain the increase in voting and public support for the opposition. Second, people vary in whether they attribute the program to the government and recall that assignment was random, and the treatment effects are not statistically significantly different in these subgroups. Nonetheless, the difference is in the direction we might expect (YOP recipients who attribute the program to the government or recall assignment was random are less likely to support the opposition) and so we cannot dismiss these explanations outright.

We did not anticipate these results, and political behavior was not a primary or prespec-

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

[9]In Bangladesh, Guiteras and Mobarak (2014) 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.

ified outcome of the initial YOP evaluation. Thus, we must take these results with some caution. Nonetheless, this study is a good example of how large program evaluations in developing countries can be theoretically generative, by providing new and sometimes counterintuitive results. In particular, we advance the hypothesis that anti-poverty programs may free poor people from patronage networks and other pressures, enabling them to vote their true preferences.

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.[10] 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.[11] 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.

2   Context, intervention, and experiment

2.1   Setting

Uganda, a small landlocked country in east Africa, is extremely poor but with a stable and growing economy.[12] Since 2006, two major parties and a number of smaller ones have competed in national elections every five years, but the ruling National Resistance Movement (NRM) party and its leader, President Yoweri Museveni, have been in power continuously for 30 years.

While there is some competition at the local level, the ruling party suppresses political opposition at the national level, and cements its position through various forms of patronage. For this reason, most analysts consider Uganda a "hegemonic party system" or a "multiparty autocracy" (e.g. Tripp, 2010; Magaloni et al., 2013).

Museveni and the NRM are committed to economic growth and poverty reduction through economically liberal policies. Uganda is commonly called a Western "donor darling" for this

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[10]On microfinance see Banerjee (2013). On business skills training see McKenzie and Woodruff (2012). On vocational skills training programs see the discussion in Blattman et al. (2014).

[11]See Haushofer and Shapiro (2013); de Mel et al. (2008); Fafchamps et al. (2014); Blattman et al. (2013); Banerjee et al. (2015).

[12]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.

reason (Jones, 2009). Indeed, at the outset of this study, foreign aid constituted about 40% of the national budget (Hickey, 2013). The ruling party's development plans have mainly focused on modernization and structural transformation in addition to poverty alleviation (Hickey, 2005, 2013). At the same time, critics have argued that the ruling party has used aid in general, and rural development programs in particular, to solidify support in the context of increasing opposition competition (Mwenda and Tangri, 2005).

Northern underdevelopment

Northern Uganda is home to a third of the country's population, and its economy historically focused on subsistence agriculture, cattle herding, and some commercial agriculture. While Uganda's income per capita doubled in the past two decades, this growth was concentrated in south-central Uganda (Hausmann et al., 2014). 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, control, 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. Neighboring countries, especially South Sudan, began to grow rapidly. With this political uncertainty resolved, and growth in linked markets, by 2008 the northern economy began to catch up.

A programmatic approach to northern recovery and development

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.

From 2003 to 2010, the centerpiece of the government's northern development and security strategy was a decentralized development program, the Northern Uganda Social Action Fund, or NUSAF. NUSAF was Uganda's second-largest development program, after the national agricultural extension program. Starting in 2003, 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.

2.2 The Youth Opportunities Program

YOP invited groups of young adults, aged roughly 16 to 35, to apply for cash grants in order 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, and hence were unable to reach their potential.

In 2006 there was little hard evidence on this theory and strategy, especially in Africa, and one could reasonably worry that giving $7,500 to a group of inexperienced and low-skilled 25-year-olds was not a successful development strategy. But in the ensuing years a growing base of evidence has suggested that poor people in low-income countries generally have high returns to capital and other inputs into microenterprise development (Banerjee and Duflo, 2011; Blattman and Ralston, 2015; Banerjee et al., 2015).[13]

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.[14] In our sample, most groups are mixed

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[13]Like most of rural Africa, potential entrepreneurs have virtually no access to capital or loans. Formal insurance was unknown and almost no formal lenders were present in the north at the outset of this study in 2008. While village savings and loan groups are common, loan terms seldom extend beyond three months, with annual interest rates of 100 to 200%. This is common even with non-profit microfinance, and one reason microfinance seldom leads to investment and poverty alleviation (Banerjee et al., 2013). Because of high fees, real interest rates on savings are negative.

[14]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.

(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, and could request up to $10,000.[15] In preparing the proposal, groups selected their own trainers, typically a local artisan or small institute. These are commonplace in Uganda (as in much of Africa) 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 and 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.

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 terms). Unless otherwise noted, all UGX amounts reported in this paper are 2008

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[15]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 in decisions. Most groups proposed a single trade for all, but a third of groups proposed that different members would train two 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 trades such as carpentry or welding.

UGX, and all USD are converted at market exchange rates.[16]

2.3 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 (Grossman and Lewis, 2013; Green, 2010, 2011). 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 also closely involved in the design of the program, and monitored impropriety, limiting the program's ability to reward supporters. Ugandan activists and press made frequent (and subsequently justified) allegations of corruption and impropriety in NUSAF, especially at the district level, but accusations of mass patronage or vote-buying were uncommon (e.g. Ojwee, 2008; Kavuma, 2010).[17] Corruption in NUSAF, including ghost projects and 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 the 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, as we show in Appendix A.1, 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. Indeed, the nomination process sought to avoid this kind of patronage by design. Targeting was highly decentralized, with groups nominated by local leaders who may or may not be affiliated with the NRM.[18] Group nomination at the regional level was undoubtedly shaped by a range of local social and political considerations (how could such a valuable program not be), but to the best of our knowledge it was not captured or influenced by parties or party political operatives. 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 impression is that the national government viewed NUSAF as a way to build support for the ruling party through programmatic effectiveness. The return of multi-party

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

[17]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.

[18]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.

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).

2.4 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 and 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.[19]

It's important to note that the study population was only moderately affected by war and political instability. None 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. In many ways, 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.[20]

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.

Figure 1: Eligible districts and number of study communities (treatment and control) per parish
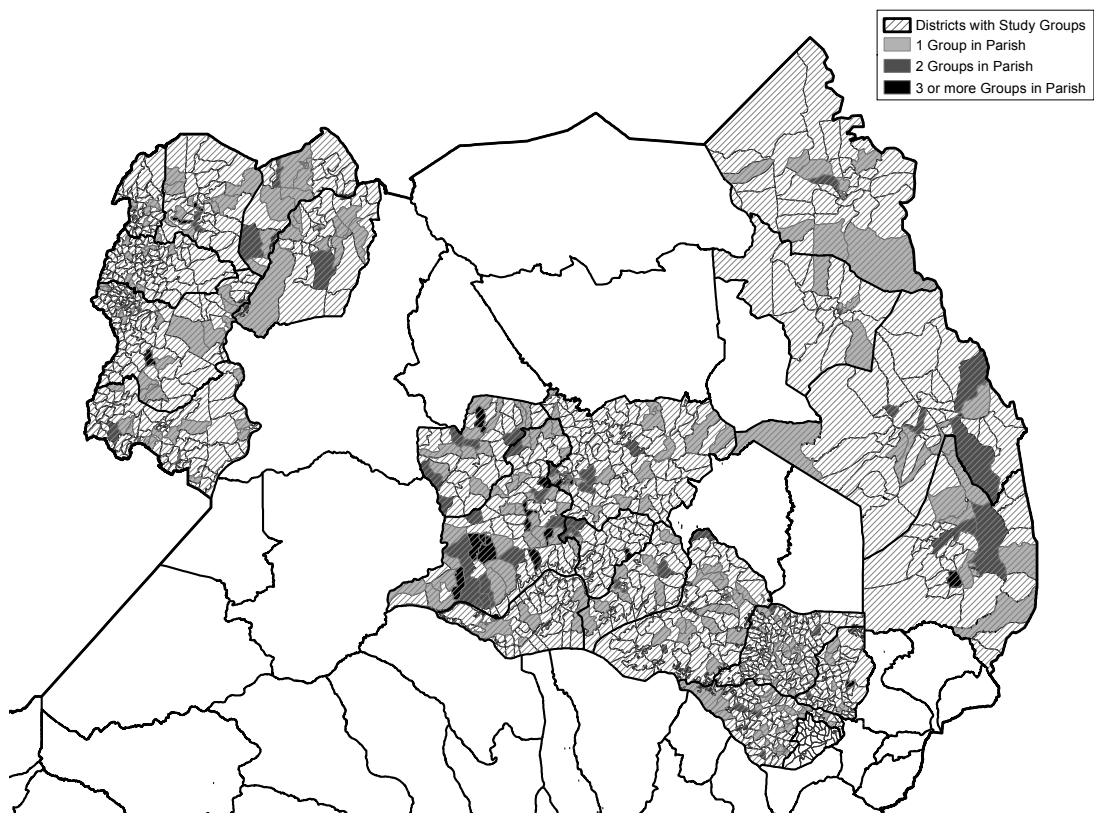
Notes: The figure shows the distribution of communities participating per parish using 2007 district boundaries. The majority of parishes had either one or two groups apply.

Table 1: Selected baseline descriptive statistics and tests of balance

Select covariates in 2008	Baseline (n=2598)			Found in 2010 (n=2005)			Found in 2012 (n=1868)		
	Control	Treatment – Control		Control	Treatment – Control		Control	Treatment – Control	
	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.

2.5 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. 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.[21] The NUSAF office disbursed funds between July and September 2008 via the central bank.

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.

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

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[19]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.

[20]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.

[21]Members were mixed up then lined up, and enumerators selected every N/5 person to survey (where N is the total number present). 4% of the groups had missing members, and these were not included in the baseline survey. 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.

14

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.[22]

Although poor by any measure, these applicants were slightly wealthier and more educated than their peers. If we compare our sample to their age group and gender a 2008 population-based household survey, our sample has 1.7 years more education, 0.15 standard deviations more wealth, is 7.5 percentage points more urban and 5.4 percentage points more likely to be married, and has 1.6 fewer household members (see Appendix A.2).

Tracking and panel attrition

YOP applicants were a young, mobile population. Nearly 40% had moved or were away temporarily at each endline survey. To minimize attrition we used a two-phase tracking approach, as outlined in Appendix A.3. In the first phase we tracked all 2,677 members of the sample, and in a second phase we did intensive tracking of a randomized sample of unfound people. Our response rate was 97% at baseline, and effective response rates at endline (weighted for selection into endline tracking) were 85% after two years and 82% after four.

Of slightly greater concern is correlation between attrition and treatment, reported in Appendix Table A.3. The treatment group was 5 percentage points more likely to be found at baseline in 2008. There is no treatment-control imbalance in 2010, although controls are more likely to have been lost in 2008 and the treatment group in 2010. In 2012, controls were 7 percentage points less likely to be found. 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.

2.6   Randomization balance

The computer-based randomization generated some imbalances across treatment arms. We report balance tests for selected variables in Table 1 (for the full list see Appendix B.1). 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

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[22]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).

linear, not in log form), and 5 percentage points more access to small loans. 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 treatment assignment treatment indicator reveals that baseline characteristics are jointly significant with $p = 0.05$.

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.

2.7 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.[23] 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.

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

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

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 endline tracking.

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[23]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.

Table 2: Economic impacts of the program after four years

Dependent variable in 2012	2010 (N=2,005)			2012 (N=1,868)		
	Control	ITT, with controls		Control	ITT, with controls	
	Mean	Mean	SE	Mean	Mean	SE
	(1)	(2)	(3)	(4)	(5)	(6)
Transfers and investment						
Group received YOP cash transfer	0.000	0.886	[0.019]***			
Business assets (000s 2008 UGX)	290.2	377.0	[78.217]***	392.8	225.0	[62.601]***
Employment						
Average employment hours per week	24.9	4.1	[1.070]***	32.2	5.5	[1.284]***
No employment hours in past month	0.100	-0.011	[0.015]	0.05	-0.022	[0.009]***
Engaged in any skilled trade	0.170	0.272	[0.025]***	0.22	0.261	[0.026]***
Income						
Index of income measures, z-score	-0.05	0.17	[0.049]***	-0.06	0.24	[.049]***
Monthly cash earnings (000s 2008 UGX)	35.2	14.61	[4.073]***	47.8	18.19	[4.898]***
Durable assets (z-score)	-0.06	0.101	[0.047]**	0.150	0.181	[0.055]***
Non-durable consumption (z-score)				-0.011	0.180	[0.051]***

Notes: Columns (1) and (4) report the control group mean, weighted by the inverse probability of selection into each endline sample. Columns (2)-(3) and (5)-(6) report the intent-to-treat (ITT) estimate and standard error (SE) of program assignment at each endline. 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. Continuous economic outcomes such as hours worked and earnings have a long upper tail, and some of these large values are potentially due to enumeration errors. Extreme values will be highly influential in any treatment effect, so we top-code all currency-denominated, hours worked, and employee variables at the 99th percentile.

3 Economic impacts of the program

YOP led to large and persistent increases in investment, work, and income. Table 2 reports ITT estimates on economic outcomes two and four years after the interventions, as documented in our companion paper economic impacts of YOP (Blattman et al., 2014). We summarize them here before moving on to the political impacts.

Compliance Of the 265 groups assigned to a cash grant, 89% received it. 21 groups could not access funds because of problems with identifying the group leaders and banking details, bank complications, collection delays, or corruption.[24]

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[24]Only 8 groups reported that they never received funds due to some form of theft or diversion. The groups who did and did not receive funds (for any reason) are generally similar along baseline characteristics, but groups were slightly more likely to be treated if they were educated and wealthier and did not have too many members (regressions not shown). These traits probably lowered the probability of a disqualifying error in the proposals.

Investments   A majority of groups and members invested the funds in skills training and business materials, as planned. Between 2008 and 2010, 68% of the treatment group enrolled in vocational training, compared to 15% of the control group and, on average, treatment translated into 340 more hours of vocational training than controls. Among those who enrolled in any training, 38% trained in tailoring, 23% in carpentry, 13% in metalwork, 8% in hairstyling, and the remainder in miscellaneous other trades.

Even so, the majority of the grants were invested in capital, as the median group estimated they spent just 11% on skills training, compared to 65% on tools and materials (the remaining 24% was shared in cash or spent on other things).[25] The control group reported UGX 290,200 ($167) of business assets in 2010 and UGX 392,800 ($228) in 2012. By 2010 treatment had increased capital stocks by UGX 377,023 ($219), a 131% increase over the control group. By 2012 stocks had increased by UGX 224,986 ($130), a 57% increase over the control group.

Afterwards, group members typically went their own way to start individual businesses rather than form firms or cooperatives.[26]

Employment impacts   With these investments, YOP led these young people to shift their occupations toward skilled work and cottage industry, thus increasing their labor supply overall. After four years, people in groups assigned to receive a grant were more than twice as likely to practice a skilled trade—typically as self-employed artisan in carpentry, metalworking, tailoring, or hairstyling. After four years the treatment group worked 5.5 more hours weekly than the control group—a 17% increase.

Income impacts   YOP's ultimate aim was to reduce poverty, and these capital investments and increases in labor supplied were means to an end: increases in earned income. Income is notoriously difficult to measure, especially in poor and rural areas (like northern Uganda) 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

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[25]Our survey data and qualitative interviews suggest that groups made bulk purchases of tools and other materials, but these were distributed and individually owned. Groups commonly elected management committee members to handle procurement, making major training and tool purchases in bulk, largely for the cost savings involved. These tools were typically distributed equally to individual members, but about half the respondents said they shared some small or large tools with other group members. In the 2010 survey, 90% of group members said they felt the grant was equally shared, and 92% said the leaders received no more than their fair share. Most of the remainder reported only minor imbalances.

[26]Nearly all treatment groups reported meeting together after the grant, typically several times a year. Half said their facilitator still engaged with the group, in part because they are from the area, had previous ties to the group, or were interested in their progress. Control groups reported meeting just as frequently, in large part because many of these groups preexisted and serve other purposes, and part because they hoped to receive transfers in the future.

consumption and asset measures are thought to be better measures of stable or "permanent" income.[27]

All three measures increase significantly in the treatment group, as does a mean effects index of all three measures standardized to have mean zero and unit standard deviation. This overall index is useful for summarizing the three measures and reducing the number of hypotheses we test. It suggests that YOP increased incomes by 0.17 standard deviations after two years and by 0.24 standard deviations after four years. But while this index reduces multiple comparison concerns, the components give a more concrete sense of the impacts.

The control group reported monthly cash earnings of UGX 30,825 ($18) in 2008, UGX 35,200 ($20) in 2010, and UGX 47,800 ($28) in 2012.[28] Such growth may have come in part from a growing economy, but it also arose from young people gradually increasing their hours worked, capital stocks, and output over time by investing earnings. Assignment to receive a YOP grant increased monthly earnings by UGX 14,605 ($8.50) in 2010 and UGX 18,186 ($10.57) in 2012. 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 roughly 40% increase in earnings—a hugely important change for someone earning so little per day. We see similar patterns in two alternative measures of income: durable and nondurable consumption. Both rose over time and had large program effects.

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 treatment 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. 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.

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[27]See Blattman et al. (2014) for a full discussion.

[28]The 2008 survey has data on gross cash revenues only, whereas gross and net earnings are available in 2010. For the 2008 value of net earnings, we use the 2008 gross amount multiplied by the 2010 ratio of gross to net. This number is merely for descriptive purposes and has no bearing on treatment effect estimates.

4 Impacts of the program on political behavior

4.1 Theoretical motivation

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.[29] 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.

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 received more assistance rewarded incumbents, sometimes even after the program benefits had finished.[30] 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 plausible reasons why people assigned to treatment should reward the ruling party at the polls for programmatic policies, and together they led us to hypothesize that assignment to treatment would increase partisanship and electoral support for the NRM and Museveni.

The first reason, commonly called "pocketbook voting", argues that economically successful voters tend to reward the incumbent (Kramer, 1971; Fiorina, 1976). 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 events, shark attacks or football games, can sometimes boost incumbents' popularity (Healy et al., 2010; Achen and Bartels, 2004). One explanation is that poorly

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[29]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.

[30]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.

informed voters interpret good fortune as plausible new information about an incumbent's quality or characteristics (Ashworth et al., 2016).

A second reason is the theory of retrospective voting, where voters reward incumbents because 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.[31] This led us to predict that YOP beneficiaries might reciprocate with votes for the ruling party.

In general, at the outset of the study there was little theoretical apparatus or literature leading us us to predict the opposite effect: that YOP could augment support for the opposition. We return to this question in the discussion and conclusions section below.

4.2   National election outcomes

YOP was first and foremost an employment program, and so the economic outcomes reported above were our primary and pre-specified outcomes. Nonetheless, the literature on the effects of central government programs on national political support led us to add questions on partisanship and electoral behavior (before and after the election) to the endline survey. We focus on these outcomes here, starting with partisan attitudes and actions.

Partisan attitudes and actions

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.

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[31]Of course, for there to be a differential effect on treated individuals, the actual receipt of YOP would 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 3: Program 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]	1,858
Would vote NRM if election tomorrow	0.75	-0.02	[.022]	1,858
Like or strongly like the NRM	0.81	-0.02	[.02]	1,845
Feels close to the NRM	0.55	0.01	[.024]	1,833
Worked to get the NRM elected	0.29	0.01	[.023]	1,844
Member of the NRM	0.40	-0.02	[.026]	1,849
Voted or supported the President in 2011	0.88	-0.04	[.018]**	1,755
Approve or strongly approve of President	0.85	-0.02	[.018]	1,847
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. Standard errors are heteroskedastic-robust and clustered by group. The number of observations in Column 4 may differ from the total number of people survey (1,868) because a small number of people, typically less than 1–2%, declined to answer the political questions.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 3 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.[32] 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 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.[33] We can certainly rule out an increase in support for the ruling party.[34]

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.[35] 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

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[32]We standardize the components, average them, and re-standardize. Thus each component receives equal weight.

[33]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.

[34]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.

[35]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.

Table 4: 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. Standard errors are heteroskedastic-robust and clustered by group. The number of observations in Column 4 may differ from the total number of people survey (1,868) because a small number of people, typically less than 1–2%, declined to answer the political questions.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.

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 the control group to 16% in the treatment, a relative gain of 33%. 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. Our results are robust to alternate specifications but are sensitive to alternative attrition scenarios (see Appendix B.4). 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.

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 4 reports impacts on political participation in general, irrespective of party. These include measures from Table 3 where we ignore the ruling party/opposition distinction, but also includes non-partisan political participation (or potentially 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.

4.3 Subnational election outcomes

As we will see below, more than 87% of respondents attributed the YOP program to the national government and ruling party. Nonetheless, given the close involvement of subcounty and district officials in the nomination process, we anticipated that beneficiaries might reward local candidates as well. These could include local councilors at the district level (called LC5s), at the subcounty level (called LC3s) and the village level (called LC1s).

Table 5 displays the program's impact on support for incumbent LC5s who served during the YOP disbursement and re-ran for election (about half of all races). It 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. We are principally interested in support for incumbent LC5s, and we break down support based on whether the LC5 was NRM or opposition.

Treatment led to a 0.057 percentage point decrease in voting for or supporting the incumbent LC5, regardless of party (not statistically significant). However, looking at the subgroups reveals that support for NRM incumbents fell dramatically, by 12.5 percentage

Table 5: 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	0.649	-0.125	[.042]***	601
Races where incumbent was from opposition	0.419	0.028	[0.069]	287
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. Standard errors are heteroskedastic-robust and clustered by group. The number of observations in Column 4 may differ from the total number of people survey (1,868) because a small number of people, typically less than 1–2%, declined to answer the political questions. 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.

points (significant at the 1% level), while support for opposition incumbents rose slightly (not statistically significant). This difference between opposition and ruling party LC5 support is statistically significant at the 1% level. 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.

5   Discussion

What accounts for these unexpected results, where the beneficiaries of a large state program not only turn away from the incumbent President, but actually increase their public support for the opposition? This section explores possible explanations, at least to the limits of what our data can do. We first show that there is no lack of attribution of the YOP program to the government, and so this is an unlikely explanation. Second, we show suggestive

evidence that increase in wealth is a major driver of this change in behavior, consistent with a mechanism already suggested in the literature: wealthier people feel freer to vote their conscience, possibly because they are less tied to patronage networks or less reliant on local politicians and strongmen. Indeed, YOP beneficiaries were less likely to be mobilized on election day. That said, they are not less likely to be enmeshed in general patronage relations, perhaps because their greater wealth brings some influence.

Before developing these more substantive explanations, however, we should note that all of our data are self-reported and vulnerable to systematic measurement error. Nonetheless, we think it's unlikely that measurement error accounts for our results. If those who received YOP were more likely to report voting for the opposition, or otherwise expressing their opposition preferences publicly, then this could account for the treatment effects we observe. This could arise 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. We cannot eliminate this possibility. Such measurement error, however, 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.

5.1 Did the ruling party get credit for NUSAF? Program attribution and beliefs

One possibility is that respondents did not attribute the YOP program, or their own selection into the program, to the ruling party. We see little evidence for this view. While a majority of our sample of YOP applicants attributed the program to the national government, they did not perceive YOP as a political favor, a form of patronage, or even a gift. Rather respondents viewed YOP as programmatic in nature. While this programmatic perception might explain the absence of any increase in ruling party support, it is hard to see how it explains the decline in Presidential voting or increased electoral action on behalf of the opposition.

Table 6 presents summary statistics and treatment effects on respondents' beliefs about the program. NUSAF was widely perceived as programmatic, in that 92% of the control group said the purpose of NUSAF was northern development, versus 6% who said it was to increase political support.

Most respondents attributed the broader NUSAF program (including YOP) to either the

Table 6: Self-reported beliefs about the NUSAF program

Dependent variable in 2012	Control (n=932)	Treatment (n=924)	Regression Difference Coeff.	p-value
	(1)	(2)	(3)	(4)
Who was mainly responsible for giving N. Uganda the NUSAF program?				
The President/NRM/national government	0.555	0.523	-0.034	0.131
District or local politician/official	0.013	0.012	0.001	0.755
Foreign donor (e.g. World Bank, NGO)	0.319	0.364	0.041	0.056
Don't Know	0.125	0.110	-0.010	0.529
What do you think the main motivation was in giving YOP to the people of northern Uganda?				
To develop/assist the north	0.916	0.910	0.006	0.601
To increase political support	0.055	0.064	-0.001	0.926
To make donors happy	0.009	0.002	-0.008	0.026
Don't know	0.020	0.024	0.003	0.697
Who selected groups to receive YOP funding?				
National government	0.071	0.080	0.003	0.826
District chairperson (elected official)	0.063	0.092	0.009	0.461
NUSAF district technical officer	0.340	0.403	0.074	0.001
District executive committee	0.077	0.096	0.016	0.212
Community facilitator	0.100	0.077	-0.019	0.143
No answer	0.348	0.251	-0.081	0.000
Why were groups chosen/not chosen for funding?				
The best quality projects were selected	0.131	0.449	0.317	0.000
Hard work of group leaders/facilitators	0.152	0.233	0.090	0.000
Bribe to facilitator	0.009	0.010	0.002	0.692
Relationship with district chairperson	0.071	0.019	-0.043	0.000
Random	0.157	0.095	-0.063	0.000
Don't know	0.479	0.194	-0.303	0.000
Do you think the selection was fair?	0.424	0.828	0.402	0.000
Thinks likely to receive future program next year	0.753	0.759	0.037	0.070

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. 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.

central government (56% of the control group) or a foreign donor (32%), typically the World Bank. 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.[36] People assigned to treatment were slightly more likely to assign the program to a foreign donor, but the difference is not large.

However, when asked who was the main group or individual responsible for selecting the groups for funding, only 15.7% recalled that the selection was random. Instead, the majority of respondents either did not know or identified their district NUSAF technical official (an unelected bureaucratic position) as responsible for funding. Those assigned to treatment were more likely to assign responsibility to the local NUSAF technical officer, perhaps due to the fact that this officer was in frequent contact with treatment groups throughout the program.

The fact that groups did not identify the national government as responsible for funding selection could explain why treated individuals were less likely to reward the government. But we see only modest evidence that treatment effects on partisanship varied with program attribution. Table 7 reports an ITT regression where we include post-treatment government attribution as a covariate, and interact it with treatment. Unfortunately, we do not have pre-treatment data on attribution, which means we have to interpret results with caution. Nonetheless, the coefficients on treatment and the treatment interaction in Table 7 suggest that the increase in opposition support is not limited to the people who attribute the program to a foreign donor. On average, those who attribute YOP to someone other than the government increase their support for the opposition by 0.168 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. (It is also worth noting that this difference is overstated to the extent that people who came to support the opposition post-program are less likely to be charitable towards the government and give them responsibility for YOP.)

We also see little effect of beliefs about program selection on the opposition support treatment effects. 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 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 (see Appendix B.6).

Overall, a lack of attribution could help explain why the ruling party did not get rewarded

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[36]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 7: 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	-0.038	0.118	0.168
	[0.052]	[0.079]	[0.053]**	[0.082]**
Attributes program to government	0.201	0.203	-0.172	-0.128
	[0.049]***	[0.069]***	[0.049]***	[0.068]*
Assigned x Government attribution		-0.002		-0.090
		[0.097]		[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 3. 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 6 for ITT effects on these variables). Hence we must interpret these heterogeneity effects with caution.

at the polls by YOP beneficiaries, but it is more difficult to understand the rise in opposition support among the treated.

5.2 Financial freedom and voting one's conscience

There is a strand of democratization theory called "modernization theory" that argues that economic prosperity contributes to democratization. This is a varied literature that often emphasizes the relationship between economic and political elites, but there is also a "micro" strand of this literature that argues that reducing poverty will create more engaged citizens. One example is Welzel et al. (2003), who marshall theory, case evidence, and correlations from the World Values Survey to argue that increased material security is associated with preferences for liberty and political self-expression. They argue, in effect, that anti-poverty programs create more self-aware, assertive, critical citizens, who will prefer to act on their political ideals.

There is also some evidence from other countries that financial independence makes them 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.

Other evidence suggests that financial independence untangles poor people from clientelistic networks. In her qualitative study of Mexican politics (and the vote buying machine of another hegemonic 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 programmatic policies could reduce clients' dependence on political patrons and reduce the power of patrons. 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.

Patronage is an important aspect of Ugandan politics. Despite a growing number of opposition candidates winning office, especially in LC3 and LC5 races, the ruling party and the national government control the vast majority of patronage in the country. This can include opportunities for contracts, casual and permanent job opportunities, and so forth. Vote buying is also common in Uganda, in particular cash gifts to encourage turnout on behalf of the incumbent. Opposition parties have significantly fewer funds for vote buying, and so this is an predominantly ruling party tactic (Blattman et al., 2016). 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.

We do not have any way to directly test these propositions. Instead, we examine patterns in the data, and find some suggestive evidence that economic success is associated with more public action and political participation for the opposition.

Changes in actions rather than preferences   At least one pattern in our data is consistent with the more successful beneficiaries acting on their opposition preferences: people change their political behaviors in support of a party more than their party preferences. Looking at the ITT estimates on opposition support in Table 3, note that 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 per se. 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 their behaviors more than their partisan preferences.

Table 8: Opposition support and income

	Control group	Full sample			Full sample	
				Dependent variable: Index of opposition support in 2012 (z-score)		
	OLS (1)	OLS (2)	IV (3)	OLS (4)	OLS (5)	OLS (6)
Assigned to treatment				0.115 [0.053]**	0.086 [0.052]*	0.106 [0.047]**
2012 income, z-score	0.131 [0.043]***	0.125 [0.031]***	0.479 [0.224]**		0.119 [0.031]***	0.097 [0.028]***
Kin relations (z-score)						-0.003 [0.023]
Community participation (z-score)						0.002 [0.029]
Public good contributions (z-score)						-0.069 [0.027]**
Anti-social behavior (z-score)						0.037 [0.032]
Protest attitudes and participation (z-score)						0.334 [0.033]***
Has migrated since baseline						0.141 [0.070]**
Index of 2011 election influence (z-score)						0.082 [0.028]***
Existence of a patron (z-score)						-0.007 [0.023]
Group cooperation (z-score)						-0.003 [0.011]
Observations	934	1,858	1,858	1,858	1,858	1,850
Baseline controls and district fixed effects?	Y	Y	Y	Y	Y	Y

Notes: The 2012 income index is a standardized mean effects index of the three endline income measures, as reported in Table 2. 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.8. Columns (1) and (2) report the OLS regression of opposition support on income in the control group and the full sample. Column (3) instruments for endline income with the assignment to treatment dummy. Column (4) replicates the simple ITT on opposition support, from Table 3 above, for comparison purposes. Columns (5) and (6) examine possible mediators of the treatment effect, adding first the endline income measure then all outcome indexes. All regressions include 13 district (randomization stratum) fixed effects, and a vector of control variables that includes all of the baseline covariates reported in Appendix B.1.

Association with wealth We also see that opposition support is correlated with wealth, and have suggestive evidence that increases in wealth are associated with increases in opposition support. Table 8 reports five OLS regressions and one instrumental variables (IV) regression, examining the relationship between the endline index of opposition support (from Table 3) and the endline income index (from Table 2).

First, higher incomes are associated with more opposition support. In Column 1 of Table 8, 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 and employment levels). 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.[37]

Second, 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, in Column 3, 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.

Third, alternatively we can 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.[38] Column 4 replicates the simple ITT on opposition support from Table 3, as a baseline reference. Column 5 adds the endline income index, while column 6 also includes 8 other potential mechanisms (for simplicity and consistency, we include every outcome family reported in either this paper or Blattman et al. (2014)).[39]

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[37]If we omit the baseline covariates, we estimate nearly identical OLS coefficients on income (regressions not shown).

[38]See Imai et al. (2011) for a discussion of mediation analysis. They call the main assumption for causal identification "sequential ignorability" because two ignorability assumptions are made sequentially. First, given the observed pretreatment confounders, the treatment assignment is assumed to be unconfounded—a straightforward assumption in an experiment. The second part implies that the observed mediator is ignorable given treatment status and baseline covariates. In Column 5 this mediator is endline income, and in Column 6 it is all endline outcome indexes, although our main interest continues to be income). The second part of the sequential ignorability assumption is unlikely to hold, but the analysis nonetheless provides suggestive information about the importance of income in mediating the effect of the YOP program on political behavior.

[39]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.

The results suggest income is a major mediating factor.[40] After controlling for income in 2012 (columns 4 versus 5), 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 5 and 6, the treatment effect remains similar to that in column 5 (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 very high and very positive ($p < 0.01$). This suggests that a large portion of the effect we observe on opposition support comes through increases in income.[41]

Exposure to electoral influence and patrons Finally, a reasonable (though not necessary) implication of the financial freedom story is that treatment should increase the respondent's independence from party operators and patrons. Because we did not anticipate changes in patron ties, we did not collect data on self-reported patronage or future expectations of party support. We do, however, have self-reported data on attempts to influence the respondent during the campaign, and his perceptions of his access to patrons in times of need. We see some evidence that the treated were not targets of unlawful "get-out-the-vote" efforts around the 2011 election, which may suggest fewer attempts to influence their vote, but across a range of measures of election influence and intimidation we see little treatment effects. Also, rather than seeing treated less entangled in patron-client networks, they report being more involved in patron-client networks (though not necessarily ones related to the election). Wealth could simply improve social networks and political access. Nonetheless, it suggests there is no simple relationship between wealth and "detangling" from patron-client networks.

Table 9 reports treatment effects on instances of election influence and general patron-client ties. We do not see any significant change in most threats and incentives to vote one way or the other. 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

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[40]Though it may not satisfy the exclusion restriction assumed in Column 3.

[41]While other endline indexes are significantly correlated with opposition support, this is not sufficient to mediate the treatment effect 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 coefficient when these other variables are added to Column 6. We expand on these points, and illustrate treatment effects, in Appendix B.7, where we perform a more formal mediation analysis based off of Keele et al. (2015). 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.

day is outlawed in Uganda. A mean effects index of election influence shows no statistically significant impact.

We find that the program has a positive and statistically significant effect on the existence of a patron—a family member, "big man", or politician— the respondent feels he or she can go to when in need of something. It is unclear whether these are political patrons who mobilize people for political support, as we do not see a statistically significant rise in one of these patrons trying to influence their political behavior during the election. Only 22% of respondents reported that a patron tried to influence their 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.

6   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 less likely to vote for the President and are more likely to engage in campaigning for the opposition. We show that the lack of reward for the incumbent is likely not coming from a lack of attributing the program to the incumbent, since a majority of beneficiaries do in fact attribute the program to the government. We see suggestive evidence that opposition support is associated 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.

Existing evidence points 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.

Table 9: 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	[.05]	1,858
Was offered money in exchange for vote (0-3)	0.52	0.06	[.048]	1,857
Was threatened during campaign (0-3)	0.23	0.04	[.034]	1,857
Was intimidated during campaign (0-3)	0.90	-0.01	[.057]	1,857
Was taken to the poll on election day	0.04	-0.02	[.008]**	1,858
Any of patrons tried to influence you	0.22	0.02	[.019]	1,839
Existence of a patron (z-score)	-0.09	0.14	[.05]***	1,850
There is a family member he can go to if in need	0.39	0.04	[.024]*	1,844
There is a big man he can go to if in need	0.29	0.04	[.024]*	1,840
There is politician he can go to if in need	0.23	0.07	[.021]***	1,837
Any of patrons tried to influence you during 2011 election	0.22	0.02	[.019]	1,839

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. Standard errors are heteroskedastic-robust and clustered by group. The number of observations in Column 4 may differ from the total number of people survey (1,868) because a small number of people, typically less than 1–2%, declined to answer the political questions.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.

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."

Our hypothesis—that the program, by creating wealth, led to the financial freedom to vote one's true political preferences—is just that, a hypothesis. We do not have the data or design to test the mechanism at work. Nonetheless, it accords with the conclusions of political scientists in contexts as different as Mexico and the Philippines, and strikes us as an important hypothesis for the literature, and future experiments, to take seriously. Relatively few employment and anti-poverty program evaluations collect data on program attribution and resulting political attitudes and behavior. Most regions, however, offer off-the-shelf political survey questionnaires with nationally representative data for comparison (e.g. Afrobarometer, Latinobarometer, etc.). Past experimental anti-poverty programs are also fodder for downstream studies of political impacts. We hope this paper motivates such data collection and addresses the gap in evidence, to investigate the idea that the escape from poverty might be associated with political freedoms.

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Appendix for online publication

A   Additional design details

A.1   Was NUSAF politically targeted

As discussed in Section 2.3, 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.[42] 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 Kitgum district. In the second column we include district dummies. The results are now much smaller and not significant.

A.2   Comparison to general population

Table A.2 compares baseline statistics for the NUSAF sample with those from a 2008 clustered population-based household survey, the Northern Uganda Survey (NUS). The Uganda National Statistics Bureau collected the NUS on behalf of Uganda's Office of the Prime Min-

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[42]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
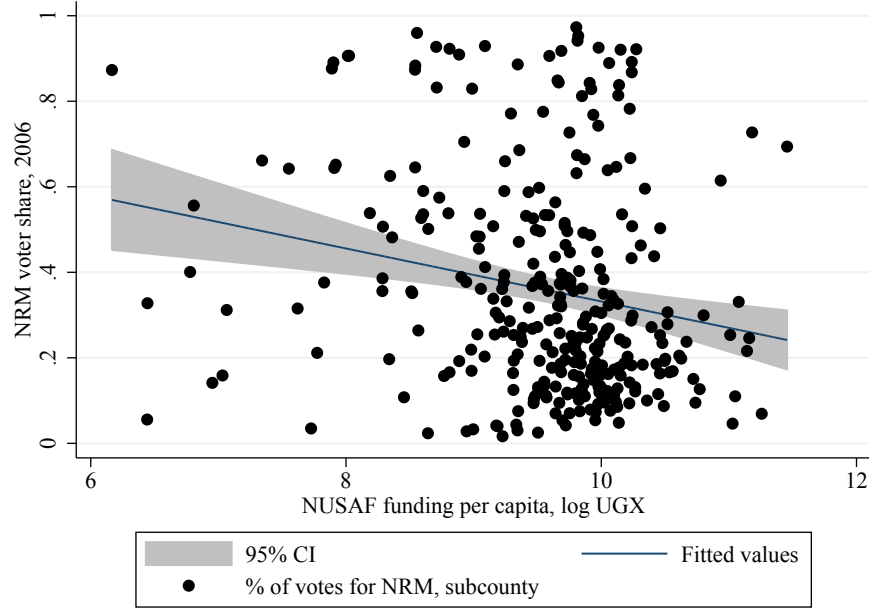
Notes: This figure presents a scatterplot of NRM voter share in 2006 and the log of total NUSAF funding per capita by the subcounty level.

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.

ister, in part to help the government assess the impacts of NUSAF on the north. The NUS was conducted in all NUSAF districts and focused on consumption, labor market activity, and health and education in the household. NUS sampling probabilities are estimates of the probability of being sampled in the full northern population.

A.3 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.3 for a more detailed presentation of effective response rates.

A.4 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

Table A.2: Comparison to other population-based surveys

	Survey					
	YOP sample		Afrobarometer			
Covariate	2008	2012	2008	2010	2011	2012
	(1)	(2)	(3)	(4)	(5)	(6)
Age	24.96	25.03	33.49	33.98	35.38	36.13
Female	0.33	0.33	0.50	0.50	0.50	0.50
Education						
None	0.03	0.04	0.10	0.16	0.19	0.18
Primary	0.45	0.47	0.44	0.47	0.36	0.42
Secondary	0.46	0.37	0.45	0.35	0.43	0.37
University	0.05	0.11	0.01	0.01	0.02	0.02
Post-Graduate	0.00	0.00	0.00	0.00	0.00	0.00
Religion						
None			0.00	0.02	0.01	0.00
Catholic	0.44	0.45	0.85	0.83	0.80	0.85
Muslim	0.11	0.11	0.03	0.05	0.09	0.10
Pentecostal	0.15	0.15	0.09	0.08	0.08	0.05
Protestant	0.30	0.28	0.00	0.01	0.00	0.00
Other	0.01	0.01	0.03	0.02	0.02	0.00
Ethnic group						
Acholi	0.01	0.00	0.00	0.00		0.01
Alur	0.03	0.03	0.08	0.07		0.04
Ateso			0.19	0.13		0.18
Karamojong	0.07	0.07	0.00	0.00		0.18
Langi	0.35	0.35	0.47	0.29		0.24
Lugbara	0.12	0.12	0.06	0.12		0.18
Madi	0.08	0.08	0.14	0.07		0.07
Other	0.15	0.16	0.06	0.32		0.11
Member of a political party		0.50		0.65	0.68	
Worked for a political party		0.38		0.46	0.46	0.26
Strongly like or Like DP		0.10		0.09	0.06	
Strongly like or Like FDC		0.22		0.22	0.25	
Strongly like or Like UPC		0.22		0.24	0.24	
Strongly like or Like NRM		0.80		0.65	0.67	
Observations	2,598	1,868	447	504	448	576

Notes: This table compares the YOP-sample to the Afrobarometer survey. Afrobarometer responses limited to the districts in the YOP sample.

Table A.3: Survey response rates

	Selection and tracking, by survey phase					Effective response rates				
Survey	Total sought	Found, phase1	Select, phase 2	Found, phase 2	Final # of observations	All	Control	Treatment	Difference	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.

Table A.4: 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	0.03	0.02
	[.031]	[.030]		[.020]	[.020]
World Bank	0.05	0.04	Not random	-0.03	-0.06
	[.029]*	[.028]		[.032]	[.029]**

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.

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.4 displays the results of our survey experiment. The results show that the experiment was not successful: individuals who were told the government was behind the program were 5 percentage points more likely to believe the World Bank funded the program. Similarly, individuals who were told selection was not random were 6 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.

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.

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 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. Our first robustness check is to alternative specifications, which is displayed in Table B.4. We test four alternate specifications. In the first, we drop all controls and only include randomization block fixed effects. In our next one, we add only demographic covariates. Next we add all human and physical capital controls. Our final specification includes all covariates but uses randomization inference to calculate the standard errors. As shown in the table, our results are robust to these alternate specifications.

Our second robustness check is to alternative attrition scenarios. We impute outcome values for unfound individuals at different points of the observed outcome distribution. The most extreme bound, from Manski 1990, imputes the minimum value for unfound treated members and the maximum for unfound controls. Following Karlan et al. 2015, we also calculate less extreme bounds by imputing relatively high values of the dependent variables for missing control group members, and relatively low values for missing treatment group

Table B.2: Correlates of attrition

Baseline covariate	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.

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

Baseline covariate exhibiting treatment imbalance (transformed into 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.4: Robustness to alternate specifications

Outcome variable	Alternate specification				
	Main specification	No controls, district FE	Plus demographics	Plus human/ physical capital	Randomization inference
	(1)	(2)	(3)	(4)	(5)
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
Randomization inference	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 (4) is the same as our main specification but calculates standard errors using randomization inference. 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.5: Robustness to alternate attrition scenarios

| Outcome variable | Main specification | Impute missing dependent variable with mean = +/- X SD for missing control (treatment) respondents | | | | "Worst case" Manski bound |
| | | 0.025 SD | 0.05 SD | 0.10 SD | 0.25 SD | |
	(1)	(2)	(3)	(4)	(5)	(6)
Index of NRM/Presidential support	-0.041	-0.020	-0.013	0.002	0.047	0.508
	[.052]	[.046]	[.046]	[.046]	[.046]	[.064]***
Index of opposition support	0.115	0.079	0.071	0.057	0.012	-0.601
	[.053]**	[.045]*	[.046]	[.046]	[.046]	[.081]***
Index of general election 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 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.
$* * * p < 0.01, * * p < 0.05, * p < 0.10$

members.[43] Specifically, we impute missing dependent variables for the treatment (control) group as the found treatment (control) mean minus (plus) 0.025, 0.05, 0.10 or 0.50 SD of the found treatment (control) distribution.

Table B.5 reports ITT estimates under these attrition scenarios. Our results are generally not robust to alternate attrition scenarios as the point estimate on opposition support is generally positive but not significant.

B.5   Treatment effects by age

In table B.6, 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.

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

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[43]This assumes the dependent variable points in the positive direction. If treatment leads to a decrease in the outcome variable, as is the case for antisocial behaviors and antiviolent and anticriminal values, we impute in the opposite direction (i.e smaller values for control, larger values for treatment).

Table B.6: 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	0.061	0.142	0.136
	[0.090]	[0.061]**	[0.060]**
Age 20 or under			-0.053
			[0.071]
Assigned x age 20 or under			-0.098
			[0.108]
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 inthe 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.
$* * *p < 0.01, * * p < 0.05, *p < 0.10$

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.7, 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 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

Table B.7: 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	0.130	0.121	0.169	0.123
	[0.065]**	[0.110]	[0.143]	[0.057]**
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.

B.7   Mediation analysis

In Table B.8 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 5.2. 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.

B.8   Other outcomes

Table B.9 displays ITT effects on minor outcomes we collected that did not make it into the main paper.

Table B.8: Mediation analysis

Mediator M	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
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Income, z-score	0.24	[.049]***	0.12	[.031]***	0.11	[.053]**	0.25
Index of 2011 election intimidation, z-score	0.04	[.049]	0.12	[.031]***	0.11	[.053]**	0.04
Existence of a patron, z-score	0.14	[.050]***	-0.01	[.025]	0.11	[.053]**	0.02
Kin relations, z-score	0.05	[.047]	-0.05	[.025]*	0.11	[.053]**	0.02
Community participation, z-score	0.00	[.050]	0.01	[.028]	0.11	[.053]**	0
Public goods contributions, z-score	0.01	[.049]	-0.01	[.029]	0.11	[.053]**	0
Antisocial behaviors, z-score	0.00	[.047]	0.11	[.033]***	0.11	[.053]**	0
Protest attitudes and participation, z-score	-0.01	[.044]	0.35	[.033]***	0.11	[.053]**	0.03
Migrated	-0.08	[.026]***	0.17	[.080]**	0.11	[.053]**	0.11
Group cooperation, z-score	-0.22	[.128]*	-0.02	[.012]**	0.11	[.053]**	0.04

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.9: 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	1.57	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.