DO ANTI-POVERTY PROGRAMS SWAY VOTERS? EXPERIMENTAL EVIDENCE FROM UGANDA

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Abstract—High-impact policies may not lead to support for the political party that introduces them. In 2008, Uganda's government encouraged groups of youth to submit proposals to start enterprises. Of 535 eligible groups, a random 265 received grants of nearly $400 per person. Prior work showed that after four years, the Youth Opportunities Program raised employment by 17% and earnings by 38%. Here we show that recipients were no more likely to support the ruling party in elections. Rather, recipients slightly increased campaigning and voting for the opposition. Potential mechanisms include program misattribution, group socialization, and financial independence freeing voters from transactional voting.

I. Introduction

WHAT are the political impacts of development programs? Governments that deliver programs to their constituents hope to be rewarded at the polls. They hope for rewards even when those policies are targeted programmatically, based on need or merit, rather than in a clientelistic way. There are strong reasons to think voters reward governments for good policy. In developed democracies, there is evidence that voters punish or reward incumbents for effective policies, economic conditions, and even events beyond the government's control (Kinder & Kiewiet, 1981; Healy, Malhotra, & Mo, 2010). Forward-looking voters may also be swayed by effective programs, viewing programmatic policies as a signal that the regime is either competent or shares their preferences.

There is now a good deal of evidence that voters reward governments for programmatic policies in middle-income democracies, especially from social safety net programs in Latin America. Golden and Min (2013) note that most studies have found that as transfers to a district rise, voter turnout and incumbent vote share tend to rise as well.[1] Nonetheless, it is probably too early to draw firm conclusions. Golden and Min (2013) not only suggest exceptions: they also raise concerns of publication bias against null findings.

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from high- and middle-income countries and from more overtly clientelistic programs, where the benefits can easily be withdrawn or tied to political support.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

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

The Youth Opportunities Program (YOP) in Uganda offers a chance to investigate this. In 2006–2007, Uganda's central government implemented a program to help poor and unemployed young adults become self-employed artisans, such as carpenters or tailors. YOP targeted the underdeveloped northern districts and invited young people in these districts to form small groups and submit proposals on how they would use a cash grant to start independent trades. Thousands of groups applied, and local bureaucrats nominated proposals for funding. In 2008, they identified 535 eligible groups and awarded grants to 265 of them via lottery. Successful groups received grants of about $382 per person to pay for training and start-up costs. This was roughly the annual income of the average applicant.

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

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[1] Manacorda, Miguel, and Vigorito (2011) find that Uruguayan conditional cash transfer (CCT) recipients are 11 to 13 percentage points more likely to support the current government than the previous one. Baez et al. (2012) show that Colombian recipients of health and education transfers were more likely to register, vote, and support the government. Pop-Eleches and Pop-Eleches (2012) use a discontinuity in a Romanian cash transfer program to show that receipt buys turnout and incumbent support. De La O (2013) finds that Mexican CCT-receiving villages have 7% higher turnout and 9% higher incumbent vote share (though Imai, King, & Rivera, 2016, have pointed out that this is driven by increases in registration not turnout, and Schober, 2016, argues that the effect is limited to turnout only). These populations are wealthier than the target population in Uganda, whom we estimate earn no more than 40% as much as these Latin American study samples.

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people said YOP was aimed at developing the North rather than at increasing political support.

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

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

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

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

What explains the null effect on ruling party support and the increase in opposition political activities? We walk through the evidence on possible mechanisms. First, our sample could attribute YOP to foreign funders and fail to reward (or punish) the incumbent government. Or they could see that they were randomly assigned to YOP and so have no reason to reward an incumbent. As it happens, a majority of groups gave the incumbent government credit for YOP. Few remembered or knew that they had been selected randomly. But YOP recipients who did not attribute the program to the government were more likely to support the opposition.

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

Income is just one possible mechanism. There are others that we are not able to test. Groups may have exposed youth to new political ideas or collective action. Or YOP may have increased beneficiaries' exposure to local politicians. The association between income and public opposition support is important and unexpected, however. We believe this calls for more research on the downstream political effects of government and aid programs.

II. Context, Intervention, and Experiment

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

While there is a higher degree of party competition at the local level, the ruling party suppresses political opposition for the presidency and cements its position through various forms of patronage. For this reason, most analysts consider Uganda a "hegemonic party system" or "multiparty autocracy" (Tripp, 2010). Although the ruling party has a built-in advantage, elections are still fairly competitive. Participation rates are high, and election day itself is perceived as free and fair by Ugandans and the international community. The ruling party's advantage comes not from its interference in the actual vote or extensive fraud, but rather from the use of public funds during the campaign, extensive patronage, and the intimidation of opposition candidates (Mwenda & Tangri, 2005). For instance, vote buying is extensive in the weeks leading up to the election, peaking the evening before. This vote buying is much more common among the ruling party, possibly because of the diversion of public funds and other corruption (Larreguy et al., 2017).

Both the ruling and opposition parties run extensive rallies, party mobilization effects, and vote-buying campaigns around the country. The biggest difference is that the ruling party has more resources. Nonetheless, competition is fairly intense for parliamentary seats and powerful district positions. Also, even if the president is unlikely to lose the election, the ruling party responds aggressively to opposition support with campaigning, policy change, pork projects, and vote buying as opposition vote share sends a powerful message.

One of the government's recent priorities has been to develop the north of the country. The North is more distant from trade routes and, as an area of early opposition support, received less public investment from the 1980s onward. The North was also held back by insecurity. From 1987 to 2006, a low-level insurgency destabilized North-Central Uganda, wars in Sudan and the Democratic Republic of Congo fostered mild insecurity in the Northwest, and armed banditry were commonplace in the Northeast. From 2003–2006, peace came to Uganda's neighbors, and Uganda's government increased efforts to pacify and develop the North. South Sudan also began to grow rapidly. With this political uncertainty resolved, the northern economy began to catch up by 2008.

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 competitive and urban NRM support wanes, the ruling party is seeking political support in areas such as the North. While pork and patronage around elections are commonplace, the national government has also pursued a set of broad-based and relatively nonpoliticized programs that serve its broader development objectives.

A. *The Youth Opportunities Program*

From 2003 to 2010, the government's northern development and security strategy centered around the Northern Uganda Social Action Fund (NUSAF), Uganda's second-largest development program. Communities and groups could apply under various NUSAF cash grants components for either community infrastructure construction or livestock for the ultrapoor.

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

YOP had five key elements. First, 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. Half the groups existed already, often for several years, such as farm cooperatives or sports, drama, or microfinance clubs. New groups formed specifically for YOP were often initiated by a respected community member. In our sample, most groups are mixed (about one-third female on average); 5% groups are all female and 12% all male.

Second, groups had to submit a written proposal that described how they would use the grant for nonagricultural skills training and enterprise start-up costs. They could request up to $10,000. Groups selected their own trainers, typically a local artisan or small institute. These are commonplace in Uganda, which has is a tradition of artisans taking on paid students as apprentices.

Third, groups had to receive formal advising. Many applicants were illiterate, so YOP required facilitators (usually a local government employee or community leader) to meet with the group and help prepare the written proposals. Groups chose their own facilitators, and the NUSAF office paid facilitators 2% of funded proposals (up to $200).

Fourth, 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 and were supposed to visit projects they planned to fund. Districts said they prioritized early applications and disqualified incomplete ones. While this is in line with our observations, unobserved quality and political calculations could have played a role. However, local elected politicians generally had little active role in the project.

Fifth, 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 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 UGX, and all USD are converted at market exchange rates (UGX 1720 per USD) or PPP rates (UGX 688 per USD).

B. *Was NUSAF a Patronage Program?*

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

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

We also see no evidence that YOP targeted supportive villages, party members, or swing voters. For example, there is no significant correlation between the percent of the vote going to the incumbent party in the 2004 election and the per capita NUSAF funds received between 2004 and 2007 at the subcounty level (see appendix A.1). Indeed, the decentralized nomination process was designed to avoid this central government patronage. We observed the selection, deliberation, and auditing process firsthand, and group members seemed to be a mix of first-come, first-serve, meritocratic, and ad hoc priorities and procedures.

Our discussions with government and World Bank officials suggest that the national government viewed NUSAF as a way to build support for the ruling party through programmatic effectiveness. The return of multiparty 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.

In terms of taking credit, the government did not make explicit efforts to market this as coming from the NRM or central government. Nonetheless, it is common knowledge that public finance is highly centralized in Uganda, and all revenues and major expenditures come from the national government. Simply put, the Office of the President is responsible for virtually every major development program in the country.

C. *Experimental Design*

YOP was oversubscribed, and we worked with the national NUSAF office to randomize funding among eligible proposals. Thousands of groups submitted proposals in 2006. NUSAF funded hundreds in 2006–2007, prior to our study. By 2008, fourteen eligible districts had funds remaining.

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 former combatants in the study groups. Little distinguishes our sample from other poor Ugandan youth.

District governments nominated two and a half times the number of groups they could fund. The districts submitted roughly 625 proposals to the national NUSAF office, where staff 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 groups too large or small.

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 people) to treatment and 270 groups (5,828 people) to control, stratified by district. Control groups were not wait-listed 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 YOP in the future. Spillovers between study villages are unlikely, as the 535 groups were spread across 454 communities in a population of more than 5 million. Control groups are typically very distant from treatment villages.

D. *Data and Participants*

We randomly selected five people from each group to be tracked and interviewed three times over four years—a panel of 2,677 people (7 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. No political data were collected at baseline because the government did not want to be seen collecting such data in advance of YOP. Enumerators and local officials mobilized group members to complete a survey of demographic data on all members, as well as group characteristics.

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

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

TABLE 1.—SELECTED BASELINE DESCRIPTIVE STATISTICS AND TESTS OF BALANCE

	Baseline ($n = 2{,}598$)			Found in 2010 ($n = 2{,}005$)			Found in 2012 ($n = 1{,}868$)		
		Treatment – Control			Treatment – Control			Treatment – Control	
Select Covariates in 2008	Control Mean (1)	Difference (2)	*p*-Value (3)	Control Mean (4)	Difference (5)	*p*-Value (6)	Control Mean (7)	Difference (8)	*p*-Value (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 nonagricultural 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	0.08	0.02	0.11	0.08	0.01	0.29	0.07	0.02	0.18
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 of 2008 UGX)	62.19	6.89	0.30	62.11	10.24	0.17	63.96	6.62	0.41
Savings in past 6 months (000s of 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	0.90	0.00	0.88	0.92	0.00	0.92	0.92	−0.01	0.56
Voted in 2006 presidential election	0.72	0.03	0.20	0.73	0.04	0.07	0.75	0.00	0.96
Member of a political party	0.11	0.02	0.06	0.12	0.01	0.39	0.12	0.02	0.13
Currently on a community committee	0.17	0.01	0.62	0.18	0.00	0.82	0.18	0.02	0.40
Parish vote share for Museveni, 2006	0.33	0.00	0.93	0.33	0.00	0.92	0.32	0.00	1.00
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	

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; 2010 and 2012 refer to the respondents located in each year, respectively.

Participants. Table 1 reports baseline descriptive statistics for selected baseline variables. We report all 57 variables in appendix B.1. The experimental sample was 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. Most lived in villages of 100 to 2,000 households. On average they had reached eighth grade. Given that the most war-affected districts did not participate in the YOP evaluation, only 3% were involved with an armed group in any fashion. In 2008, they reported 11 hours of work a week. Half these hours were low-skill labor or petty business, while the other half was in agriculture—rudimentary subsistence and cash cropping on small rain-fed plots with little equipment or inputs. Almost half of our sample reported no employment in the past month, and only 8% were engaged in a skilled trade. Cash earnings in the past month averaged $1 a day. Savings in the past six months were $15 on average, and only 11% reported any savings.

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

The treatment group was 5 percentage points more likely to be found at baseline in 2008, because of the thirteen unfound groups (who did not know their treatment status). In 2012, controls were 7 percentage points less likely to be found. Most of these unfound controls were from the thirteen never-found groups. If unfound control individuals were 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 control for baseline characteristics associated with attrition, and we test the sensitivity to various attrition scenarios.

E. *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. As the experiment predated the social science registry, the trial was not formally preregistered. 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), treatment effects on secondary outcomes should be treated with some caution.

We estimate intent-to-treat (ITT) effect on outcomes, Y, via the regression

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

where T is an indicator for assignment to treatment for person i in group j, X is the vector of baseline covariates displayed in appendix table B.1, the γ are district fixed effects (required because the probability of assignment to treatment varies by strata), and ε is an error term clustered by group. We weight observations by their inverse probability of selection into the two-phase endline tracking (see appendix A.2). We include covariates in part to account for imbalance in baseline characteristics across arms. Table 1 reports balance tests (for all variables, see appendix B.1). Of 57 covariates, 6 (10.5%) of the treatment-control differences have $p < 0.05$, and 8 (14.0%) have $p < 0.10$. A test of joint significance from an OLS regression on a treatment indicator reveals that baseline characteristics are jointly significant with $p = 0.05$.[2]

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

III. Results

A. *Economic Impacts of the Program*

YOP led to large and persistent increases in investment, work, and income. Blattman et al. (2014) report detailed ITT estimates on economic outcomes two and four years after the interventions. Briefly, 89% of groups assigned to a YOP grant received it. A majority of groups and members invested the funds in skills training and business materials, as planned.

By 2012, assignment to YOP was associated with 224,986 UGX ($130) greater capital stocks, a 57% increase over the control group. With these investments, YOP led these young people to shift their occupations toward skilled work and cottage industry, increasing their labor supply overall. After four years, those assigned to YOP were more than twice as likely to practice a skilled trade, and they worked 5.5 more hours per week than the control group—a 17% increase.

YOP's aim was to increase income. An index of consumption, asset, and labor earnings measures increased by 0.17 standard deviation with YOP after two years and by 0.24 standard deviation after four years. Since these people are fairly poor and underemployed, this earnings increase is modest in absolute terms—just under a dollar a day in PPP terms. But relative to the control group's earnings, this is a 38% increase in cash earnings, a hugely important change for someone earning so little per day. Both men and women saw income gains. 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 those of control women compared to a 29% gain for men.

B. *Impacts of the Program on Political Behavior*

YOP is unlike the sort of clientelistic program most commonly used in transactional politics and vote buying, such as public sector jobs. It was a large-scale state employment program that was foreign financed and relatively technocratic and nonpoliticized in its targeting and implementation, and, unlike a public sector job, the grant was by its nature impossible to revoke once given. Indeed, it transferred resources directly to voters, much like land titling, conditional cash transfers, skills training, or other public programs. These are commonly labeled programmatic policies rather than pork programs or traditional patronage. Moreover, as a one-time dispbursement, participants did not have further interactions with the government agency after receiving the grant.

*Theory and predictions***.** Some evidence suggests that voters reward incumbents for programmatic policy. For instance, comparing areas with varying exposure to conditional cash transfer programs in Latin America, several studies 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 (Manacorda et al., 2011; Zucco, 2013; Diaz-Cayeros, Estevez, & Magaloni, 2016). Similarly, Casaburi and Troiano (2016) see an increase in incumbent vote share after a successful antitax evasion program, and Larreguy et al. (2015) see incumbent vote share rise after a land titling program.

The literature provides several reasons why people assigned to treatment should reward a ruling party for

[2] For instance, at baseline, the treatment group report 2 percentage points more vocational training, 0.07 standard deviation greater wealth, 56% greater savings (though only in the linear, not in log, form), and 5 percentage points more access to small loans. Group-level balance tests (the level of randomization) yield the same conclusions (not shown). The missing thirteen control groups could cause the imbalance. We estimate that if the missing controls had baseline values 0.1 to 0.2 standard deviation above the control mean, it would account for the full imbalance (see appendix B.3).

programmatic policies, and together they led us to hypothesize that assignment to treatment would increase partisanship and electoral support for the ruling party. One is that economically successful voters tend to reward the incumbent. Overall, YOP recipients experienced a large increase in wealth and may have rewarded the incumbent as a consequence, independent of whom they attribute the responsibility of the program to. This idea that voters are naive and make simple calculations is supported by the literature on how natural or idiosyncratic events can sometimes boost incumbents' popularity (Healy et al., 2010).

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

The YOP program was one of the largest development programs ever run in Uganda. As such, actually receiving 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. This led us to predict that YOP beneficiaries might reciprocate with votes for the ruling party.

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

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

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

Table 2 reports our main results on the impacts of receiving the program on political behavior and attitudes toward 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. 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 preelection 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 0.04 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. 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. We can certainly rule out an increase in support for the ruling party.

Parish-level data also support 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 to 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). The table is not shown, but the regression is analogous to the treatment effects estimated above. There are 420 eligible parishes in the sample.

Second, support for and actions on behalf of an opposition party increased by 0.12 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. In the components of this family index, all treatment effects are positive. The proportionally largest and statistically significant changes are to feeling close to the opposition party, working for the opposition, being a member of the

TABLE 2.—IMPACTS ON PARTISAN ATTITUDES AND ACTIONS, BY INCUMBENT AND OPPOSITION PARTY, 2012 SAMPLE

	(1)	(2)	(3)	(4)
	Control	ITT, with controls		
Dependent Variable in 2012	Mean	Coefficient	SE	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	[.020]	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	0.12	[.053]**	1,858
Would vote opposition if election tomorrow	0.17	0.01	[.020]	1,858
Like or strongly like any opposition party	0.36	0.03	[.023]	1,844
Feels close to any opposition party	0.10	0.03	[.016]**	1,833
Worked to get the opposition elected	0.04	0.03	[.011]***	1,844
Member of an opposition party	0.05	0.03	[.013]**	1,849
Voted or supported an opposition party in 2011	0.12	0.04	[.018]**	1,755

Column 1 reports the control group mean, weighted by the inverse probability of selection into phase 2 tracking. Columns 2 and 3 report the estimated intent-to-treat (ITT) coefficient and standard error at the endline survey, using equation (1). Standard errors are heteroskedastic robust and clustered by group. Significant at $^{***}p < 0.01$.

opposition, and actual voting for the opposition. In this context, "working to get a candidate elected" can include being a party activist (e.g., organizing events and rallies), but this role is rare, especially among young people. Rather, in most cases, this reflects more informal activities, such as persuading friends and family to support one's 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 one-third increase. While we have to take the patterns within any family with caution, note that stated preferences for the opposition change proportionally less and are not statistically significant. If we adjust p-values for the two main family comparisons (NRM/presidential and opposition support), the p-value on the opposition support family index is 0.07.

One feature of our population is that they are mainly under 35 years old, with about a quarter eligible to vote for the first time. The results, shown in appendix B.5, are not driven by young and inexperienced voters. There is no statistically significant difference between first-time and older voters. If anything, the treatment effect is slightly higher when we exclude first-time voters.

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

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

We can also examine exactly how much these observed covariates matter. As shown in table B.5, if we estimate a simple average treatment effect with strata fixed effects but no covariate controls, we get a coefficient of 0.121 (standard error of 0.053) on opposition support. Table B.7 shows robustness to the inclusion of inverse probability weighting for the propensity to go unfound based on all covariates to further account for differential attrition. With this addition, the estimate remains the same, at 0.117 (standard error of 0.053).

To believe that attrition drives our results, we would have to believe that a set of unobserved characteristics unrelated to the covariates we observe have an order of magnitude larger association with treatment and voting. For instance, the control group could only appear to like the ruling party more, because opposition supporters in the control group are less likely to answer the survey as they resent being excluded from the program. We regard these as possible but unlikely scenarios.[3]

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

TABLE 3.—PROGRAM IMPACTS ON GENERAL POLITICAL PARTICIPATION AND PARTISAN ACTION, IRRESPECTIVE OF PARTY, 2012 SAMPLE

	(1)	(2)	(3)	(4)
	Control	ITT, with controls		
Dependent Variable in 2012	Mean	Coefficient	SE	N
Index of general electoral political action (z-score)	−0.11	0.06	[.053]	1,858
Attended voter education meeting	0.48	0.03	[.026]	1,858
Got together with other to discuss vote	0.56	−0.03	[.025]	1,857
Reported a campaign malpractice	0.10	0.02	[.017]	1,857
Voted in the presidential election	0.91	0.00	[.014]	1,857
Attended an election rally (0-3)	1.24	0.04	[.050]	1,858
Participated in an political primary (0-3)	0.71	0.05	[.049]	1,857
Worked to get a candidate/party elected (0-3)	0.64	0.10	[.051]*	1,852
Member of a political party (0-3)	0.85	0.02	[.051]	1,851

Column 1 reports the control group mean, weighted by the inverse probability of selection into each tracking. Columns 2 and 3 report the estimated intent-to-treat (ITT) coefficient and standard error at endline, using equation (1). Standard errors are heteroskedastic-robust and clustered by group. Significant at $*p < 0.10$.

*General political behavior***.** Increased political action seems to be concentrated among opposition supporters, since it is not associated with a similar increase in political participation in the full sample. Table 3 reports impacts on political participation in general, regardless of party. These include measures from table 2 where we ignore the distinction between ruling party and opposition, but also includes nonpartisan 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, reported 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. Ninety-one percent of the sample reported voting, perhaps leaving little room for improvement on this metric, but we 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: 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 driven by the increase in activity on behalf of the opposition.

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

We first consider incumbent LC5s who served during the YOP disbursement period and reran for election. This is about half of all races. The table also displays treatment effects for whether the individual voted in the LC5 election (a measure of local political participation) and also the approval for current local councilors. Treatment led to a 5.7 percentage point decrease in voting for or supporting the incumbent LC5, regardless of party (not significant). But support for NRM incumbents fell dramatically, by 12.6 percentage points (significant at the 1% level), while support for opposition incumbents rose slightly (not statistically significant). We do not have party affiliation data for LC3s, and LC1s are not officially affiliated with a party. But treatment did not lead to increased support for the current LC1, LC3, or LC5, nor did it significantly increase the likelihood of voting in the local elections.

IV. Discussion

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

A. *Misattribution*

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

TABLE 4.—PROGRAM IMPACTS ON LOCAL POLITICAL PARTICIPATION AND PARTISANSHIP, 2012 SAMPLE

	(1)	(2)	(3)	(4)
	Control	ITT, with controls		
Dependent Variable in 2012	Mean	Coefficient	SE	N
Races with an incumbent LC5				
Voted or support the previous incumbent LC5 (0-1)	0.560	−0.057	[.037]	890
Races where incumbent was from ruling party	0.650	−0.126	[.042]***	603
Races where incumbent was from opposition	0.422	0.026	[.069]	289
All races:				
Voted in the LC5 election (0-1)	0.867	0.014	[.016]	1,852
Approve or strongly approve the current LC1 (0-1)	0.795	0.001	[.021]	1,853
Approve or strongly approve the current LC3 (0-1)	0.784	0.002	[.020]	1,856
Approve or strongly approve the current LC5 (0-1)	0.773	−0.034	[.022]	1,852

Column 1 reports the control group mean, weighted by the inverse probability of selection into tracking. Columns 2 and 3 report the estimated intent-to-treat (ITT) coefficient and standard error at endline, using equation (1). Standard errors are heteroskedastic-robust and clustered by group. Significant at ***$p < 0.01$.

TABLE 5.—SELF-REPORTED BELIEFS ABOUT THE NUSAF PROGRAM, 2012 SAMPLE

	(1)	(2)	(3)	(4)
	Control	ITT, with controls		
Dependent Variable in 2012	Mean	Coefficient	SE	N
Who was mainly responsible for giving North Uganda the NUSAF program?				
The president/NRM/national government	0.559	−0.017	[.024]	1,848
District or local politician/official	0.013	0.002	[.005]	1,848
Foreign donor (e.g., World Bank, NGO)	0.318	0.025	[.022]	1,848
Don't know	0.122	−0.012	[.015]	1,846
What do you think the main motivation was in giving YOP to the people of northern Uganda?				
To develop/assist the North	0.919	0.011	[.012]	1,857
To increase political support	0.054	−0.006	[.010]	1,858
To make donors happy	0.010	−0.009	[.004]**	1,857
Don't know	0.017	0.005	[.006]	1,858
Who selected groups to receive YOP funding?				
National government	0.066	0.004	[.013]	1,855
District chairperson (elected official)	0.077	0.001	[.015]	1,855
NUSAF district technical officer	0.337	0.072	[.022]***	1,855
District executive committee	0.071	0.016	[.013]	1,855
Community facilitator	0.098	−0.018	[.014]	1,855
No answer	0.350	−0.074	[.023]***	1,858
Why were groups chosen/not chosen for funding?				
The best-quality projects were selected	0.135	0.319	[.023]***	1,853
Hard work of group leaders/facilitators	0.146	0.075	[.020]***	1,858
Bribe to facilitator	0.010	0.000	[.004]	1,853
Relationship with district chairperson	0.073	−0.039	[.011]***	1,853
Random	0.152	−0.050	[.017]***	1,853
Don't know	0.484	−0.304	[.024]***	1,853
Do you think the selection was fair?	0.422	0.407	[.024]***	1,856
Thinks likely to receive future program next year	0.761	0.026	[.021]	1,868

Columns 1 and 2 report the control and treatment group means, weighted by the inverse probability of selection into tracking. Columns 3 and 4 report the intent-to-treat (ITT) estimated coefficient and p-value from YOP program assignment, using equation (1). Standard errors are heteroskedastic-robust and clustered by group. Significant at **$p < 0.05$, *$p < 0.01$.

a foreign donor (56% and 32% of the control group), typically the World Bank. (Both answers were correct, since NUSAF was funded by a large credit from the World Bank, and the government received significant technical assistance from the World Bank to implement. Regrettably, multiple answers were not collected on this survey question, and so we cannot be sure that people did not attribute the program to both the government and the World Bank.) People assigned to treatment were slightly more likely to assign the program to a foreign donor, but the difference is not large.

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

Third, those who attributed YOP to someone other than the government were also more likely to support the opposition. This could simply be partisanship coloring opinions, but we cannot reject the possibility that misattribution drives our treatment effect on incumbent and opposition support. Table 6 reports an ITT regression where we include posttreatment government attribution as a covariate and

TABLE 6.—HETEROGENEITY IN POLITICAL IMPACTS BY POSTPROGRAM ATTRIBUTION

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

This table displays heterogeneity in the ITT results by attribution. Columns 1 and 3 reproduce treatment effects on partisanship adding a dummy for government attribution from table 2. In the remaining columns, we include a dummy for government attribution and an interaction term between the dummy and treatment assignment. Self-reported beliefs about attribution and selection are posttreatment, and could be affected by treatment status (see table 5 for ITT effects on these variables). Significant at $^*p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

interact it with treatment. On average, those who attributed YOP to someone other than the government increased their support for the opposition by 0.166 standard deviation. Opposition support is 0.086 standard deviation lower among those who attribute YOP to the government, but the coefficient on the interaction is not statistically significant.

Fourth, we see little effect of beliefs about program selection on opposition support. Among those who thought program selection was fair, opposition support rose by 0.140 standard deviation compared to 0.124 among those who perceived selection as unfair. Among those who thought program selection was random, opposition support rose 0.158 standard deviation, compared to 0.126 among those who perceived selection as nonrandom (see appendix B.6).

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

B. *Effects of Income and Financial Freedom*

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

1. A strand of democratization theory called modernization theory argues that economic prosperity hastens democratization. This literature usually emphasizes the relationship between economic and political elites. But there is also a micro strand of this literature that argues that reducing poverty creates engaged citizens or more democratic preferences. One example is Welzel, Inglehart, and Kligermann (2003), who marshal theory, case evidence, and correlations to argue that antipoverty programs create more self-aware, assertive, critical citizens who will prefer to act on their political ideals.
2. There is some evidence that financial independence makes citizens more willing to hold governments accountable. For instance, De Kadt and Lieberman (2017) find that access to public services is correlated with lower support for incumbents across southern Africa. Using attitudinal survey data, they suggest that improvements in service delivery increase voter expectations of government in terms of service delivery and corruption, and incumbents are punished for disappointing these expectations.
3. Other evidence suggests that financial independence untangles poor people from clientelistic networks. Clientelism is effective in elections principally because some constituents are poor (Weitz-Shapiro, 2012). In her qualitative study of Mexican politics, Mexican politics, Magaloni (2006) argues that financially independent voters are less dependent on favors from the ruling party and thus are more likely to support the opposition. Larreguy et al. (2015) argue that in clientelistic regimes, programmatic policies can reduce clients' dependence on political patrons, and reduce the power of patrons and that this is a powerful force that can cancel out the rewards that come at the polls for good programs. They find support for this proposition from an urban titling program in Mexico that reduced the value of clientelistic goods and services that patrons had to offer. Hite-Rubin (2015), studying an experimental microfinance initiative in the Philippines, also finds that impersonal microcredit decreased incumbent support. She argues that this is not because it increases incomes but because it untangles people from the credit relationships that underlie party politics and turnout efforts. And Bobonis et al. (2017) show that in northeast Brazil, vulnerability to drought is associated with closer support for political parties, and citizens who receive cisterns are less likely to be political clients to a party.

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

First, while the absolute income effects are small, vote buying in Uganda is also a cheap affair. Around election time, it is common for incumbents, especially the ruling party, to give very small cash gifts to encourage turnout. A gift may be a few dollars or less. This happens openly at rallies throughout the lead-up to the election and secretly in the days right before election day. Opposition parties have significantly fewer funds for vote buying, and so this is a predominantly ruling party tactic (Larreguy et al., 2017). In principle, with greater income, people who received YOP may have chosen to trade off their chances of a cash gift at election time (or other political patronage) in order to act on an intrinsic preference for publicly supporting their preferred party.

Second, people change their political behaviors in support of a party rather than their party preferences. Across

TABLE 7.—OPPOSITION SUPPORT AND INCOME

	Dependent Variable: Index of Opposition Support in 2012 (z-score)				
	Control Group	Full Sample		Full Sample	
	(1)	(2)	(3)	(4)	(5)
Assigned to treatment			0.117 [0.053]**	0.088 [0.052]*	0.107 [0.047]**
2012 income, z-score	0.131 [0.043]***	0.125 [0.031]***		0.119 [0.031]***	0.097 [0.028]***
Kin relations (z-score)					−0.003 [0.023]
Community participation (z-score)					0.001 [0.029]
Public good contributions (z-score)					−0.068 [0.028]**
Antisocial behavior (z-score)					0.037 [0.032]
Protest attitudes and participation (z-score)					0.335 [0.033]***
Has migrated since baseline					0.143 [0.071]**
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	1,841
Baseline controls and district fixed effects?	Yes	Yes	Yes	Yes	Yes

The 2012 income index is a standardized mean effects index of reported earnings, nondurable consumption, and durable assets. The other outcome indexes represent mean effects indexes of all outcomes analyzed in this paper or the original economic impact analysis in table VIII of Blattman et al. (2014). Treatment effects on these other outcomes are reported in appendix B.10. Columns 1 and 2 report the OLS regression of opposition support on income in the control group and the full sample. Column 3 replicates the simple ITT on opposition support, from table 2, for comparison purposes. Columns 4 and 5 examine possible mediators of the treatment effect, adding first the endline income measure then all outcome indexes. Significant at $*p < 0.10$, $**p < 0.05$, and $***p < 0.01$.

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

Third, active and public opposition support is correlated with wealth, and increases in wealth are associated with increases in opposition support. This too is consistent with the hypothesis that relative financial independence reduces clientelism. Table 7 reports five OLS regressions examining the relationship between the endline index of opposition support (from table 2) and the endline income index. Higher incomes are associated with more active and public opposition support. In column 1 of table 7, we report the results from a regression of opposition support on endline income for the control group only, controlling for all baseline covariates (including baseline income). This is not a causal estimate of income on opposition support, but it does indicate how the variation in income that is not explained by demographics or initial income correlates with opposition support. It is moderate in size (0.13 standard deviation) and significant at the 1% level. It is roughly similar to the correlation in the full sample, in column 2.

We can also examine the evidence on income as a mediating factor and try to estimate how much of the effect on opposition support is due to a rise in income. Column 3 replicates the simple ITT on opposition support from table 2 as a baseline reference. Column 5 adds the endline income index, while column 5 also includes eight other potential mechanisms (for simplicity and consistency, we include every outcome family reported in either this paper or Blattman et al., 2014). The results suggest income is a mediating factor. After controlling for income in 2012 (columns 3 versus 4), the treatment effect on opposition support falls by 25% ($p < 0.01$), while endline income is just as correlated with opposition support as in columns 1 and 2. This suggests that a large fraction of the treatment effect we see in column 4 is coming through an increase in income. When we add in the other eight mechanisms and compare columns 4 and 5, the treatment effect remains similar to that in column 4 (a difference of 2.0 percentage points, $p = 0.27$). Although the coefficient on income slightly drops when adding in these eight mechanisms ($p = 0.05$), the correlation between endline income and opposition support is still high and positive ($p < 0.01$). This suggests that a large portion of the effect we observe on opposition support comes through increases in income.

Note, however, that this is a period of general growth, and incomes rose over time for both the treatment and control groups. Incomes simply increased more for the treatment group. If there were a mechanical connection between wealth and opposition support, we might expect to see a generalized

TABLE 8.—PROGRAM IMPACTS ON OTHER POLITICAL OUTCOMES, FULL SAMPLE

	(1)	(2)	(3)	(4)
	Control	ITT, with controls		
Dependent Variable in 2012	Mean	Coefficient	SE	N
Index of 2011 election influence (z-score)	0.03	0.04	[.050]	1,858
Was offered money in exchange for vote (0-3)	0.52	0.07	[.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	[.050]***	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

Column 1 reports the control group mean, weighted by the inverse probability of selection into tracking. Columns 2 and 3 report the estimated intent-to-treat (ITT) coefficient and standard error at endline, using equation (1). Column 4 reports the number for each regression. Standard errors are heteroskedastic-robust and clustered by group. Significant at $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

increase in opposition support. This implies that the relative wealth gain is important. We can only speculate why this might be the case. It could be that the price of a vote is proportional to income or something else about the general political equilibrium that only relative income differences matter.

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

Finally, a reasonable implication of the financial freedom story is that treatment should increase the respondent's independence from party operators and patrons. We see mixed evidence on this front. Table 8 reports treatment effects on instances of election influence and patron-client ties. We do not see any significant change in most threats and incentives to vote. Treated people were, however, about half as likely to be taken to the poll on election day—a fall of 2 percentage points relative to a mean of 4 percentage points in the control group. The mean is low because such voter mobilization on election day is outlawed in Uganda. A mean effects index of election influence shows no statistically significant impact. YOP increases the likelihood of having a patron, but this is not the kind of patron that mobilizes people for elections. Only 22% of respondents reported that a patron tried to influence their electoral actions, and this increased by 2 percentage points (not significant) with treatment. We only asked about attempts to influence, not success, and so this does not rule out the possibility that the treated disentangled themselves from election pressure and patronage. But the pattern is not consistent with the financial freedom story either. 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.

C. *Measurement Error*

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

Social desirability or other bias is a risk, but we think it is unlikely for four reasons. First, the survey asked people whether they expected to receive future transfers, and 76% of both the treatment and control groups said they felt it was likely they or their group would receive a program from a charity or the government in the future. There is no difference. Second, systematic measurement error is difficult to reconcile with the pattern of treatment effects we observe, in particular the absence of any impact on attitudes toward the ruling party and its challengers. It is possible that treatment affects the likelihood of reporting opposition voting/membership/activities but not party support, but this narrows the set of plausible systematic measurement error stories that could explain our results. Third, as with attrition, the degree of systematic measurement error correlated with treatment would have to be huge to account for our treatment effects. Finally, following Di Maio and Fiala (2017), we also check for potential bias introduced from enumerator effects and find little evidence of their presence.

D. *Other Explanations*

A handful of other mechanisms are possible, but we do not have the data to judge. One possibility, for example, is that increased opposition political engagement could arise from group socialization. People needed to apply in groups. About half of the groups existed prior to the application.

Ongoing group interaction could have exposed youth to different political ideas, lowering the cost of political action. We do not have data to test this possibility, but it seems reasonable that new groups would have a bigger effect. But if we compare treatment effects between groups that existed prior to the program to the ones that were created for the purpose of applying for the grant, we find that treatment has the largest effects among preexisting groups, not new ones.

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

V. Conclusion

We analyze the political consequences of a large-scale, successful employment program in Uganda. We find that rather than rewarding the incumbent ruling party for this programmatic policy, treated young people are slightly less likely to vote for the president and more likely to engage in campaigning for the opposition. There are multiple possible mechanisms. We see suggestive evidence for at least one: that opposition support is associated with wealth increases, and this is consistent with a story where more successful youth are able to vote their conscience rather than succumb to incentives or pressures to support the ruling party. Of course, we cannot rule out other channels and do not wish to do so. The income channel is likely only a part of the story. We simply do not have strong evidence for or against most of the alternatives.

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

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

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

The answer is especially important for aid agencies that support supposedly programmatic policies. Scholars and politicians have warned that aid might help rulers stick to power by indirectly undermining development of civil society. Regimes also commonly target aid toward political supporters, translating aid into votes (Jablonski, 2014). Uganda, for example, has a semiautocratic regime that tries to use programs and patronage to insulate itself from competition. It thus seems important to understand how large aid programs affect local politics.

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

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