Appendix for "Citizen Preferences and Public Goods: Comparing Preferences for Foreign Aid and Government Programs in Uganda"

Helen V. Milner, Daniel L. Nielson, and Michael G. Findley

Contents

Sampling Details

To recruit the subject pool, we used a random sampling procedure in which any Ugandan adult had a roughly equal chance of being selected. We started with 2002 census data to select the subject pool, matching the number of parliamentary constituencies by region proportional to the census data. Fifty-five constituencies were selected, with 15 in the Central region, 15 in the North, 14 in the West, and 11 in the East. We then selected two sub-counties in each constituency, one parish in each sub-county, and one polling station in each parish so that, finally, each parliamentary constituency had two polling stations that served as the sampling start points (SSPs). Uganda's one-party dominance prompted us to oversample opposition strongholds.

At the assigned polling stations, enumerators began at the main intersection and each walked in a different direction, away from the other enumerators. They surveyed houses on the left side of the street, starting with the second house and every other house thereafter. Upon completion, they counted one house to skip and surveyed again. Residents were home in the vast majority of houses sampled. A twenty-page training manual spells out the sampling process and is available upon request.

Eighty-four local Ugandan enumerators administered the instrument to 3,582 respondents in the neighborhoods and villages of the four different regions of Uganda during the months of June and July 2012. The average interview time was 59.7 minutes. The instrument was translated into 11 local languages that the enumerators spoke, and enumerators were assigned to sampling start points based on their linguistic expertise; 420 (12 percent) of the interviews were conducted in English because the subject sampled did not speak an African language in common with the enumerator (due for the vast majority of cases to the fact that the subject was away from her or his home locale).[1]

The primary sampling objective was political representativeness, which was largely achieved within a few percentage points for each political party. The sampling procedure achieved a somewhat less precise match for other demographic factors, as shown in Table A1. Still, for most categories of religions and tribes, the study sample was within a few percentage points of the 2002 Ugandan census – the most recent available at the time the survey experiment was fielded (alas, a report with matching categories for the 2014 census has not yet been released for comparison). We oversampled Langi tribe members and undersampled Basoga and Bagisu members, mostly due to logistical challenges of reaching the most remote locations.

Measures of wealth are challenging in East Africa with its large proportion of people working in subsistence or near-subsistence agriculture. Roofing material is often used as a proxy, with metal roofs seen as a signal of greater wealth than thatched roofs. Our study appeared to sample significantly more households with iron roofs than reported in the

---

[1] To ensure that enumerator selection did not affect any results, we conducted robustness tests in which we included enumerator fixed effects. None of the substantive results change when doing so.

2002 census. But this likely also reflects the fact that the country has undergone rapid economic growth averaging 7 percent over the last 20 years; poverty has been significantly reduced over that period (World Bank 2015).

We further randomized the adult within the household to whom the instrument was administered. To accomplish this, enumerators obtained a list of all adults in the household (by gender, alternating homes) and then randomly chose one of those adults and asked whether they would complete the interview. Our procedure worked reasonably well; gender, education, gender, age, party, religion, and regional variables were not significantly related to whether subjects received given experimental conditions. See Table A2.

Table A1: Demographic Representativeness Compared to 2002 Census

Religion	Study Sample	2002 Census
Catholic	38.0	41.9
Protestant	47.6	43.2
Muslim	12.8	12.1
Traditional	0.5	1.0
Other	1.1	1.8
Ethnicity		
Baganda	19.7	17.3
Langi	14.2	6.2
Banyankore	13.8	9.8
Bakiga	6.8	7.0
Lugbara	5.6	4.3
Iteso	5.5	6.6
Basoga	5.3	8.6
Acholi	4.0	4.8
Bagisu	1.6	4.7
Other	23.6	30.7
Roof Material		
Iron	64.4	54.0
Thatched	33.2	43.9

Source: 2002 Census:
http://www.ubos.org/onlinefiles/uploads/ubos/pdf%20documents/2002%20Census%20Final%20Reportdoc.pdf, tables 3.1, 3.3, 4.4

Table A2: Randomization Checks

Assignment to Treatment	b/se
Education	0.017
	(0.015)
Male	-0.051
	(0.100)
Age	0.002
	(0.004)
NRM	-0.089
	(0.103)
Christian	0.828
	(0.537)
Muslim	1.021*
	(0.553)
Poverty (dichotomous)	0.122
	(0.106)
Knowledge (dichotomous)	0.032
	(0.121)
Foreign media exposure	-0.047
	(0.182)
Runyankole	0.034
	(0.165)
Nationalist	0.010
	(0.115)
Western region	0.183
	(0.146)
Eastern region	0.118
	(0.160)
Northern region	-0.063
	(0.141)
Constant	0.657
	(0.575)
N	3251

Education Condition

"The Post Primary Education and Training Adaptable Program Lending Project seeks to increase access to lower secondary education, improve the quality of lower secondary education, and enhance primary education and training. The project may require your community to providing funding for maintenance in the future. [This project

will be funded by the RANDOMLY ASSIGNED FUNDER]. How much would you support this project?"

Table A3: Experimental Results — individual dependent variables

	Strong Support	Tell Support	Willing to Sign	Signed Petition	Willing to SMS	Sent SMS
All respondents						
Control/Gov't	0.73	0.91	0.82	0.77	0.59	0.02
N	528	520	528	538	538	202
Aid	0.77	0.94	0.83	0.80	0.64	0.05
N	3007	2967	3008	3017	3017	1143
Difference	0.03*	0.03**	0.02	0.04*	0.04*	0.02*
T-test	1.68	2.31	0.83	1.88	1.92	1.91
P-value	0.094	0.021	0.405	0.060	0.056	0.057
Passed manipulation check						
Control/Gov't	0.68	0.88	0.78	0.72	0.54	0.03
N	349	341	349	357	357	149
Aid	0.82	0.96	0.85	0.83	0.68	0.06
N	1887	1874	1888	1893	1893	852
Difference	0.13***	0.07***	0.08***	0.10***	0.15***	0.03*
T-test	4.98	4.01	3.30	4.10	5.12	1.91
P-value	0.000	0.000	0.001	0.000	0.000	0.058
2SLS (Instrument: Assignment to treatment; Instrumented: Perceptions in MC)						
Aid Treatment	0.07*	0.06**	0.03	0.07*	0.08*	0.04*
Std. Error	(0.04)	(0.03)	(0.04)	(0.04)	(0.05)	(0.02)
N	3523	3477	3524	3543	3543	1341

Positive differences mean that the second proportion is larger than the first proportion, implying aid is preferred to government funding. Note that if a subject stated s/he did not want to sign the petition (third column) we still presented them the possibility of signing the petition (fourth column). The higher Ns for willingness to SMS in the fifth column (e.g., 538 and 3017) are a result of subject refusals to answer the petition questions (where corresponding Ns are lower: 528 and 3008) but willingness to answer those that followed. Also, the Ns decrease in the "Sent SMS" condition (relative to "Willing to SMS") because we only calculate Sent SMS for subjects who owned a phone. Statistical significance indicated as follows: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$. All tests of statistical significance are two-tailed.

Principal Components Analysis

We also conducted principal component analysis on the six dependent variables and we uncover two factors with eigenvalues greater than 1 (a conventional threshold for keeping factors). See Table A4. Five of the six DVs load more heavily on the first factor than on the second one.2 On the first factor, willingness to sign a petition and actually signing the petition loaded most heavily, followed by telling of one's support and willingness to send an SMS. After running PCA, we can predict scores to get the factor indices. All of the DVs load positively on the first factor so positive values suggest stronger support for the projects and negative values weaker support.

If we estimate t-tests on the two factors, we find that for both factors, the mean for foreign donors is positive while the mean for the control is negative. See Table A5. The difference between the treatment and control is significant at the 0.10 level for factor 1 and at the 0.05 level for factor 2. Thus, specifying the dependent variables in yet other ways provides additional support for the key differences identified in the paper, demonstrating the robustness of the overall relationships we find.

Table A4: Principal components analysis: Mass respondents

	Factor 1	Factor 2
Strong support	0.317	-0.021
Tell support	0.408	-0.121
Willing to sign	0.546	-0.024
Signed	0.544	-0.005
Willing to SMS	0.374	0.206
Sent SMS	-0.006	0.971
Eigenvalue	2.748	1.021

Note: Rotated results.

---

[2] Sending an SMS loads very heavily onto the second factor. This is likely because the variable is coded as 0 if a respondent did not send an SMS even if they do not have a cell phone. Since very few respondents actually sent an SMS (about 3% of mass respondents) and since most respondents did respond affirmatively to the other DVs, it is not surprising that SMS did load on its own factor.

Table A5: T-tests of factor means: Mass respondents

	Factor 1	Factor 2
All respondents		
Govt	-0.128	-0.070
N	520	520
Aid	0.022	0.012
N	2966	2966
Difference	0.150*	0.082**
Passed Manipulation Check		
Govt	-0.354	-0.098
N	341	341
Aid	0.165	0.064
N	1873	1873
Difference	0.519***	0.162***

Positive differences mean that the second proportion is larger than the first proportion, implying aid is preferred to government funding. Two-tailed statistical significance: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Subgroup Analysis

Table A6: Subgroup effects, multilevel models

		Strong Support	Tell Support	Willing to Sign	Signed Petition	Willing to SMS	Sent SMS	PCA
Corruption	Gov X Low corruption	0.115***	0.005	0.038	0.061	0.178***	-0.001	0.343**
		(0.043)	(0.025)	(0.037)	(0.039)	(0.047)	(0.036)	(0.163)
	Treat X Low corruption	0.058**	0.023	0.015	0.042*	0.108***	0.008	0.235**
		(0.027)	(0.015)	(0.023)	(0.024)	(0.029)	(0.022)	(0.102)
	Treat X High corruption	0.060***	0.026**	0.011	0.028	0.056**	0.021	0.170*
		(0.023)	(0.013)	(0.020)	(0.021)	(0.025)	(0.019)	(0.088)
	Gov X Low corrup = Treat X Low corrup	0.156	0.432	0.514	0.614	0.113	0.793	0.476
	N	3293	3251	3294	3308	3308	1235	3250
NRM	Gov X NRM	0.128***	0.097***	0.103***	0.118***	0.099**	-0.027	0.630***
		(0.038)	(0.022)	(0.033)	(0.035)	(0.042)	(0.031)	(0.146)
	Treat X non-NRM	0.086***	0.068***	0.039	0.068**	0.070**	0.018	0.383***
		(0.032)	(0.018)	(0.028)	(0.029)	(0.035)	(0.025)	(0.122)
	Treat X NRM	0.123***	0.094***	0.081***	0.101***	0.094***	-0.009	0.549***
		(0.032)	(0.018)	(0.027)	(0.029)	(0.035)	(0.025)	(0.122)
	Gov X NRM = Treat X NRM	0.848	0.792	0.327	0.465	0.855	0.397	0.408
	N	3293	3251	3294	3308	3308	1235	3250
Male	Gov X Female	0.019	0.004	0.016	0.003	-0.081**	-0.048	-0.027
		(0.037)	(0.021)	(0.032)	(0.034)	(0.041)	(0.030)	(0.141)
	Treat X Female	0.035	0.021	0.007	0.015	-0.035	-0.012	0.055
		(0.028)	(0.016)	(0.025)	(0.026)	(0.031)	(0.022)	(0.109)
	Treat X Male	0.046	0.031*	0.014	0.022	0.002	0.005	0.118
		(0.028)	(0.016)	(0.024)	(0.026)	(0.031)	(0.021)	(0.108)
	Gov X Female and Treat X Female	0.565	0.290	0.709	0.662	0.136	0.150	0.449
	N	3293	3251	3294	3308	3308	1235	3250
Education	Gov X High education	0.065	0.034	0.072**	0.065*	0.130***	-0.042	0.449***
		(0.040)	(0.023)	(0.034)	(0.036)	(0.044)	(0.031)	(0.153)
	Treat X Low education	0.038	0.036**	0.000	0.013	0.028	0.002	0.137
		(0.025)	(0.014)	(0.022)	(0.023)	(0.028)	(0.023)	(0.096)
	Treat X High education	0.082***	0.037**	0.078***	0.090***	0.149***	-0.007	0.481***
		(0.028)	(0.016)	(0.024)	(0.025)	(0.030)	(0.024)	(0.105)
	Gov X High educ. and Treat X High educ.	0.606	0.870	0.825	0.425	0.612	0.128	0.805

	N	3293	3251	3294	3308	3308	1235	3250
Poverty	Gov X High poverty	0.075**	-0.015	0.019	0.057*	0.092**	-0.030	0.152
		(0.038)	(0.022)	(0.032)	(0.034)	(0.041)	(0.031)	(0.143)
	Treat X Low poverty	0.062**	0.010	-0.009	0.025	0.041	0.017	0.103
		(0.028)	(0.016)	(0.024)	(0.025)	(0.031)	(0.021)	(0.106)
	Treat X High poverty	0.072**	0.025	0.034	0.065**	0.099***	-0.010	0.251**
		(0.028)	(0.016)	(0.025)	(0.026)	(0.031)	(0.023)	(0.109)
	Gov X High poverty and Treat X High poverty	0.918	0.016	0.540	0.779	0.840	0.448	0.369
	N	3293	3251	3294	3308	3308	1235	3250
Knowledge	Gov X High media	-0.039	-0.006	0.003	-0.001	0.084*	0.003	-0.012
		(0.040)	(0.023)	(0.035)	(0.037)	(0.044)	(0.031)	(0.153)
	Treat X Low media	0.018	0.023	0.010	0.016	0.036	0.024	0.102
		(0.025)	(0.014)	(0.021)	(0.023)	(0.027)	(0.023)	(0.095)
	Treat X High media	0.015	0.021	-0.009	0.015	0.085***	0.015	0.081
		(0.029)	(0.016)	(0.025)	(0.026)	(0.031)	(0.025)	(0.109)
	Gov X High media and Treat X High media	0.104	0.170	0.686	0.597	0.965	0.590	0.467
	N	3293	3251	3294	3308	3308	1235	3250
Pol. Connect.	Gov X High connections	0.060	0.043*	0.069*	0.087**	0.002	0.030	0.295*
		(0.041)	(0.024)	(0.036)	(0.038)	(0.045)	(0.032)	(0.157)
	Treat X Low connections	0.034	0.033**	0.005	0.018	-0.006	0.021	0.091
		(0.024)	(0.014)	(0.021)	(0.022)	(0.026)	(0.020)	(0.091)
	Treat X High connections	0.081***	0.046***	0.064***	0.097***	0.108***	0.041*	0.417***
		(0.027)	(0.015)	(0.023)	(0.024)	(0.029)	(0.021)	(0.102)
	Gov X High connect and Treat X High connect	0.576	0.887	0.871	0.760	0.011	0.698	0.393
	N	3249	3207	3250	3264	3264	1229	3206
Aid exposure	Gov X High aid exposure	-0.015	-0.031	-0.017	-0.011	-0.022	0.025	-0.130
		(0.051)	(0.024)	(0.044)	(0.046)	(0.051)	(0.036)	(0.189)
	Treat X Low aid exposure	0.017	0.013	0.003	0.021	0.028	0.021	0.075
		(0.025)	(0.014)	(0.021)	(0.023)	(0.027)	(0.019)	(0.095)
	Treat X High aid exposure	0.041	0.014	-0.017	-0.003	-0.005	0.035	0.015
		(0.043)	(0.018)	(0.036)	(0.038)	(0.040)	(0.024)	(0.154)
	Gov X High exposure and Treat X High exposure	0.095	0.021	0.982	0.812	0.637	0.761	0.261
	N	3293	3251	3294	3308	3308	1235	3250

Note: All models include the following control variables: Corruption, NRM, Male, Education, Media exposure, Poverty, region. All models are multilevel models with random effects at the constituency level. Statistical significance indicated as follows: *** p <0.01; ** p < 0.05; * p < 0.10. All tests of

statistical significance are two-tailed. Significance level of *Gov X low corrupt and Treat X low corrupt* (as well as the alternate subgroups) is a test of whether the difference in the coefficients of *Gov X low corrupt* and *Treat X low corrupt* is different from zero.

Table A7: Conditionality subgroup effects for those receiving the aid condition

DV	*Aid Conditions Help*		*Aid Conditions Harm*		*Difference*
	N	Proportion or Average	N	Proportion or Average	Proportion or Average
Strong Support	1353	0.815	1261	0.733	-0.082***
Tell Support	1346	0.958	1243	0.928	-0.031***
Willing to Sign Petition	1353	0.868	1261	0.814	-0.054***
Signed Petition	1358	0.84	1263	0.789	-0.051***
Willing to SMS	1358	0.651	1263	0.643	-0.008
W/ Phone & SMS	472	0.055	512	0.047	-0.008
PCA	1346	0.19	1243	-0.071	-0.261***

Statistical significance indicated as follows: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$. All tests of statistical significance are two-tailed.

Additional Manipulation Check Tests

      To probe robustness further, we used information about who passed the manipulation check in two additional ways. We first employed a selection model, where we estimated the probability of passing the manipulation check as a first stage and simultaneously estimated the effect of the treatment on outcome measures for respondents who passed the manipulation. For the selection model, exposure to media, which was significantly related to passing the manipulation check but never significantly related to the outcome measures of support for aid, plausibly satisfies the exclusion restriction and serves as the instrument for the simultaneous estimation. As mentioned above, the manipulation check was asked of both respondents in the treatment and the control group. The coefficient for the aid treatment for the "Willing to SMS" outcome is appropriately signed but not significant.

      Interestingly, as shown in Table A8, the strongest effects of subjects' perceptions of clientelism appear to manifest in influencing whether or not subjects passed the manipulation checks. Perceptions of clientelism were positively, strongly, and significantly associated with the probability that subjects could recall the donor of the assigned project. So, there is strong collinearity between these covariates. The same was true for poverty, with wealthier people more likely to pass the check; it was also the case across most outcomes (save the two SMS measures) for education, with better-educated people more likely to correctly recall the donor. The covariates appeared to be less consistently associated with subjects' support for aid, although better-educated people appeared more likely both to express willingness to and actually sign the petition and send the SMS message.

Table A8: Selection-Model Results Predicting Manipulation Check & Outcomes

Variables	Support Project	Tell Support	Willing to Sign	Signed Petition	Willing to SMS	Sent SMS	DV Sum
Main Outcomes							
Aid Treatment	0.252***	0.477***	0.183*	0.251**	0.281***	0.398*	0.605***
	(0.055)	(0.095)	(0.109)	(0.120)	(0.065)	(0.227)	(0.097)
Perceive Corruption	-0.112*	-0.014	0.062	-0.064	-0.171***	0.168	-0.183*
	(0.064)	(0.096)	(0.097)	(0.127)	(0.057)	(0.191)	(0.110)
Education	0.002	0.002	0.053***	0.048***	0.043***	-0.054***	0.030*
	(0.009)	(0.010)	(0.008)	(0.012)	(0.006)	(0.017)	(0.016)
Poverty	-0.002	0.210***	0.119	0.151	0.132***	-0.038	0.243***
	(0.041)	(0.047)	(0.094)	(0.103)	(0.029)	(0.110)	(0.071)
Constant	3.670***	0.896***	-0.341	-0.292	0.042	-1.454**	3.465***
	(0.234)	(0.187)	(0.209)	(0.317)	(0.119)	(0.566)	(0.401)
Manipulation Check Selection							
Aid Treatment	-0.103*	-0.095	-0.100	-0.107*	-0.126**	0.004	-0.112*
	(0.062)	(0.063)	(0.063)	(0.064)	(0.064)	(0.104)	(0.062)
Perceive Corruption	0.218***	0.214***	0.218***	0.213***	0.210***	0.164*	0.213***
	(0.050)	(0.051)	(0.050)	(0.050)	(0.050)	(0.090)	(0.050)
Education	0.026***	0.025***	0.026***	0.025***	0.022***	0.011	0.026***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.012)	(0.007)
Poverty	-0.130***	-0.124***	-0.131***	-0.129***	-0.115***	-0.152***	-0.130***
	(0.027)	(0.028)	(0.028)	(0.028)	(0.027)	(0.048)	(0.027)
Media Exposure	0.027***	0.029***	0.027***	0.028***	0.036***	0.020**	0.027***
	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.010)	(0.007)
Constant	0.152	0.115	0.156	0.162	0.069	0.561**	0.165
	(0.131)	(0.133)	(0.132)	(0.131)	(0.130)	(0.234)	(0.130)
Observations	3487	3467	3488	3499	3499	1314	3499
Rho	-0.356	-0.973	0.619	0.431	-0.961	-0.213	-0.667

Statistical significance is indicated as follows: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$. All tests of statistical significance are two-tailed.

As noted in the text, in a non-selection model framework we also interacted the aid treatment with passing the manipulation check. In doing so, we find that the results are strengthened further. Appendix Table A9 below shows the results along with marginal effects plotted in Appendix Figure A1.

Table A9: Interaction Effects of Aid Treatment with Manipulation Check

Independent Variable	Willing to Tell Support	Willing to Sign Petition	Actually Signed Petition	Willing to Send SMS	Sent SMS	DV Sum
Aid Treatment	-0.827*	-0.686***	-0.630***	-0.525***	0.344	-0.472***
	(0.434)	(0.264)	(0.243)	(0.186)	(1.075)	(0.119)
Passed Manipulation Check	-1.078**	-0.669**	-0.708***	-0.636***	0.402	-0.560***
	(0.458)	(0.289)	(0.266)	(0.210)	(1.136)	(0.136)
Aid Treat. * Manip. Check	1.776***	1.068***	1.092***	0.989***	0.315	0.937***
	(0.487)	(0.308)	(0.283)	(0.226)	(1.200)	(0.147)
Education	0.040*	0.095***	0.087***	0.106***	-0.045	0.058***
	(0.023)	(0.014)	(0.014)	(0.012)	(0.042)	(0.007)
Relative Poverty	0.224**	0.229***	0.232***	0.167***	-0.0653	0.137***
	(0.095)	(0.060)	(0.056)	(0.047)	(0.174)	(0.031)
Knowledge	0.006	-0.024*	-0.021	0.058***	-0.040	0.009
	(0.022)	(0.014)	(0.013)	(0.012)	(0.040)	(0.007)
NRM Party Member	0.793***	0.546***	0.483***	0.259***	-0.328	0.308***
	(0.153)	(0.098)	(0.092)	(0.078)	(0.280)	(0.052)
Constant	1.906***	0.788**	0.603*	-1.012***	-2.709**	3.194***
	(0.577)	(0.357)	(0.332)	(0.273)	(1.284)	(0.176)
Observations	**3,248**	**3,291**	**3,305**	**3,305**	**1,234**	**3,248**

Statistical significance is indicated as follows: *** p <0.01; ** p < 0.05; * p < 0.10. All tests of statistical significance are two-tailed.

Figure A1: Marginal Effects of Aid Treatment by Manipulation Check (Subjects passing check in red; subjects failing check in blue)
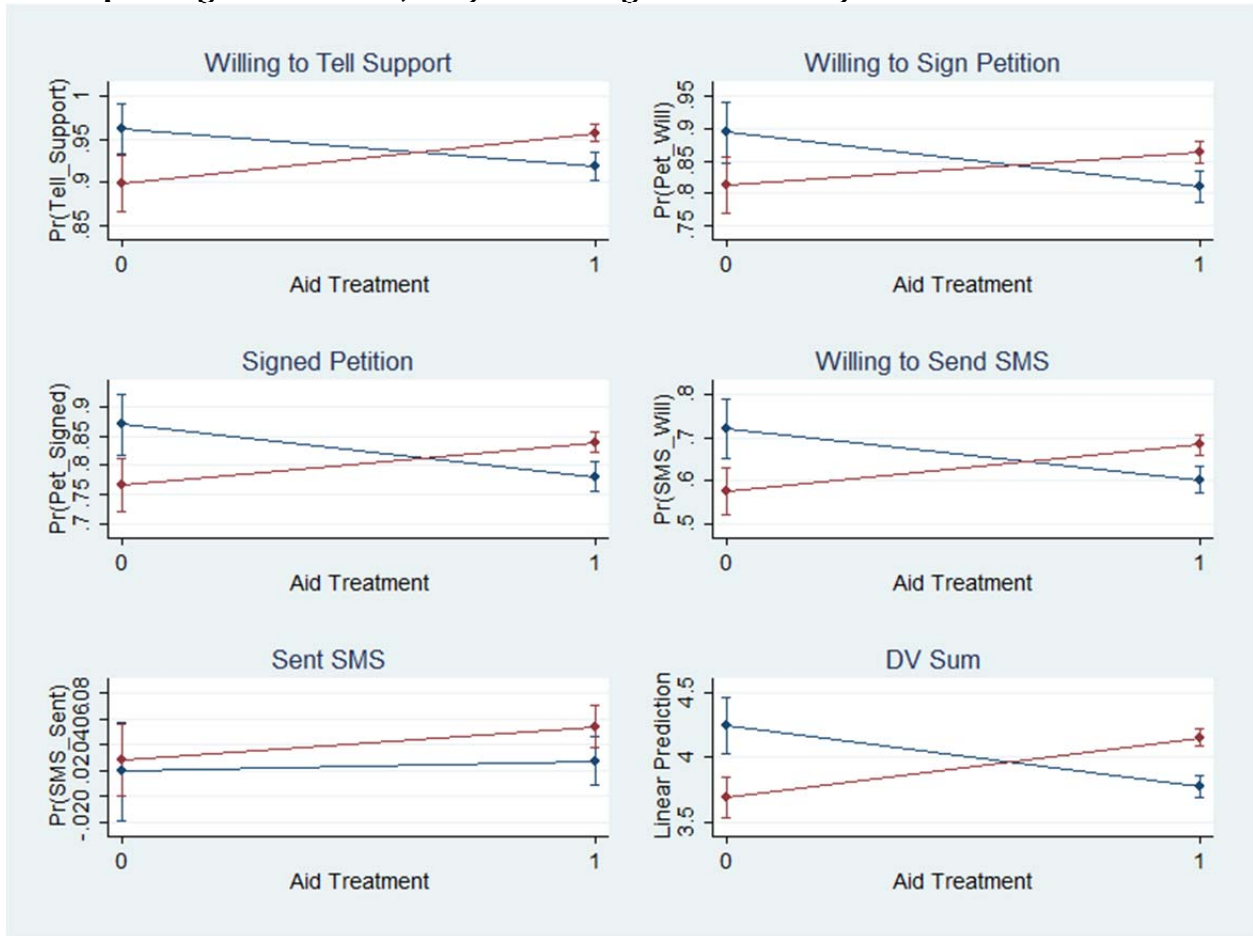
Aid As Free Resources

In September 2012 (about one month after completing the main experiment), we performed a subsequent study of 460 randomly selected subjects to explore citizen sensitivity to costs. We randomly assigned half of the subjects to receive project information without the cost condition; the other half received the cost condition.

Assignment to the cost condition had no significant effect on support for the projects. Less than three percent of subjects somewhat or strongly opposed the projects, but more than 97 percent either somewhat or strongly supported the projects. Subjects thus seem relatively indifferent to costs for development projects they perceive as desperately needed. While the cost condition may be weak, available evidence suggests that general indifference to the costs of vital development projects is a more plausible explanation.

Additionally, there is little evidence that the average Ugandan pays more for government programs than foreign ones. First, the vast majority of Ugandans are exempt from paying any income tax. The threshold for income tax payment is 1.56 million Ugandan shillings (USh) per year, which equates to roughly 604 U.S. dollars (in October of 2012). Our average subject reported earning 20,543 USh per week, which sums to 1.07 million USh per year – well below the threshold. Indeed, only 488 of 3,582 subjects, or 13.6 percent, reported earnings exceeding the income-tax threshold, meaning that 86 percent of subjects were exempt from income tax. And of this group, it is unclear what proportion actually pays taxes. Subjects should not be sensitive to tax costs if they do not pay taxes.[3] It is possible that a larger share of Ugandans occasionally pays sales or value-added tax when they engage in the formal economy. But the proportion of formal transactions is tiny given that most Ugandans do business in informal markets and pay cash. Moreover, Ugandan tax collection is notoriously ineffective. As Martin (2013, p. 2) notes about Uganda, "direct taxation of poor citizens has actually gone down over the past 15 years."

Using self-reported income data, we considered whether those making enough to pay taxes responded differently than those who did not. If taxes are important, then we should observe differences between the two groups. As reported in Appendix Table A10, however, we do not observe any significant differences in most cases. All of this evidence leads us to conclude that there are not significant cost differences for the average Ugandan between aid- and government-financed projects.

Further, our study employs a between-subjects design, so subjects are not comparing foreign aid to government projects directly. Subjects only see one condition with no additional project mentioned. This enables us to look for meaningful differences in levels of support between identical projects that are randomly assigned as originating from a foreign donor or the government. The characteristics of individuals and their prior experiences or beliefs should not affect results since these factors are, in expectation, balanced by randomization.

---

[3] Those below the tax threshold in our survey have a slight but significant preference for aid over government projects, suggesting again that costs are not driving our result. Those who might pay taxes also support aid more than government but the difference is not statistically significant.

Table A10: T-tests of means by mass respondents who pay/do not pay taxes

	Aid			Govt			
	No tax	Tax	Diff.	No tax	Tax	Diff	Diff-in-diff
Strong support	0.77	0.78	-0.02	0.73	0.78	-0.06	0.04
Tell of support	0.94	0.94	0.00	0.91	0.88	0.03	-0.03
Willing to sign petition	0.83	0.84	-0.01	0.82	0.78	0.04	-0.04
Signed petition	0.80	0.82	-0.02	0.77	0.74	0.03	-0.05
Willing to send SMS	0.61	0.77	-0.15***	0.58	0.70	-0.13**	-0.03
Sent SMS	0.05	0.04	0.02*	0.03	0.02	0.00	0.02

Statistical significance indicated as follows: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$. All tests of statistical significance are two-tailed.

The Government Control Condition

The control condition may represent a combination of people who believe it implied either the government or a foreign donor; that is, support for the control is equal to some average of support for foreign aid projects combined with support for government projects. Because we know two of these three values—the outcome in the control condition overall and the outcome in the foreign aid condition, we can calculate the third: the level of support that subjects would provide had they been given the government control condition explicitly.

First, we know the average value that respondents gave in support of the projects if they were assigned a foreign donor. In the survey we asked about support for the projects using 6 different aid donors, assigning each subject a donor at random. Our data show that across all these foreign donors, the respondents did not differentiate significantly between them.[4] On average subjects in the treatment condition supported the foreign-funded

---

[4] Except in one donor comparison (U.S. preferred over China) for two of 6 outcomes (telling support to local leaders and sending an SMS).

projects at a higher level than did the control group. This implies that we can calculate an average value of support among subjects for projects led by any foreign donor.

Second, on average the control group's level of support for the projects was lower than the average for all the foreign-donor treatment groups. Third, our post-survey data show that 51 or 52%, depending on the random assignment of electricity or education project, respectively, believe that the control was a government project. So the actual value of support for the control group for those who thought it was the government can be deduced from this. Logically, this value must be lower than that for the group that was given the foreign aid conditions.

We can use these three pieces of information to calculate the mean and standard errors of the respondents who attributed the control condition to the government. Calculating the mean is straightforward. The mean of the control group is made up of the respondents who thought that the control was a foreign donor and those who thought the control was the government:

$$\widehat{Control} = \alpha * \widehat{Gov} + (1 - \alpha) * \widehat{Foreign}$$

where $\widehat{Control}$ and $\widehat{Foreign}$ are the average levels of support for the development projects under the control and treatment conditions, respectively. These values are known from the data and α is the percentage identifying the control as the government. Rearranging to solve for $\widehat{Gov}$ we derive:

$$\widehat{Gov} = \frac{\widehat{Control} - (1 - \alpha) * \widehat{Foreign}}{\alpha}$$

Calculating the standard error to create the confidence intervals is a little more difficult as we describe in the footnote.[5] Using these calculated means and standard deviations, we compare those receiving the foreign treatment to the control condition as reported in the paper to the portion of control respondents who thought the condition was the government. Figure A2 demonstrates the relative differences for the strong support condition. For all other outcome conditions, this relative ordering holds. As the figure

---

[5]

We know that the standard error is the standard deviation divided by the square root of N. The standard error for the control will be comprised of the standard error of the respondents who believed the government was the donor and those who believed that it was a foreign donor:

$$SE_{control} = \sqrt{\left(\frac{S^2{}_{Gov}}{N_{Gov}}\right) + \left(\frac{S^2{}_{For}}{N_{For}}\right)}$$

Again we can rearrange the formula to calculate the standard error of the government respondents:

$$\frac{S_{Gov}}{\sqrt{N_{Gov}}} = \sqrt{\left(SE^2{}_{control} - \frac{S^2{}_{For}}{N_{For}}\right)}$$

Since the standard error is just the standard deviation divided by the square root of N, we can calculate $\frac{S_{Gov}}{\sqrt{N_{Gov}}}$ to get the standard errors of the government respondents.

shows, the difference between those receiving the explicit foreign condition and those thinking the control represented the government is much larger than between the explicitly foreign condition and the undifferentiated control. Thus, the results we report work against our stated hypotheses and provide the most conservative test; had we named the government explicitly, we would have observed larger treatment effects and the results would be stronger.

Figure A2: Control Condition: Calculation of Government as Control
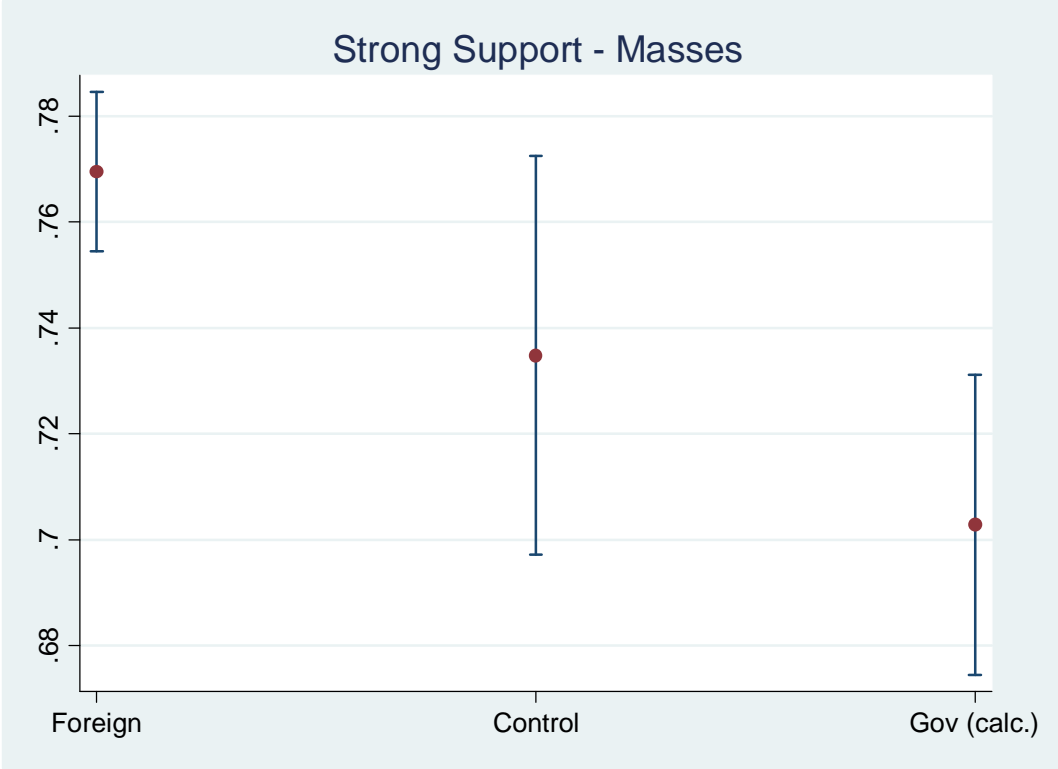
References

Martin, Lucy. 2013. "Taxation and Accountability: Experimental Evidence for Taxation's Effect on Citizen Behavior." Presented at the Workshop on African Political Economy, Washington, D.C.