Does an economics education produce technocratic paternalists?
Experimental evidence from Tanzania

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Abstract

When confronted with information that ordinary citizens do not care that strongly about efficiency, do economists change their views of optimal public policy? In a randomized experiment on tax preferences conducted among business and economics students in Tanzania, we supplied the treatment group with information that ordinary citizens disagree with implications of efficiency-based optimal tax theory. Tax preferences were then measured using discrete choice experiments. The results show that the treated students modify their position in the direction of public opinion, an effect driven by students with longer exposure to economics. An economics education hence seems to produce professionals who are part democrats and part technocratic paternalists.

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1 Introduction

In their highly influential work on nudges, Thaler and Sunstein (2003; 2008) define the use of small changes in the choice architecture to affect people's choices as a practice of libertarian paternalism. They defend such practices with reference to the bounds and biases that affect people's choices, often making choices suboptimal by people's own standards. Judging by the huge number of papers in the economics literature that have pursued the idea of nudges in public policy,[1] this seems a position many economists agree with or with which they are at least comfortable, critical views seeing libertarian paternalism as inconsistent with liberal principles notwithstanding (Grüne-Yanoff, 2012). An important question, however, is whether this revealed preference for libertarian paternalism in the economics profession reflects a more radical acceptance of illiberal paternalism among economists, of maintaining beliefs that certain policies are in the interest of people in situations where it is not at all clear that the same people would, or in fact do, agree that this is the case.

This paper is not about nudges, but about whether an economics education produces professionals with illiberal paternalist tendencies.[2] This question is particularly relevant in a development context, in light of frequent arguments that economics and economists promote, facilitate or provide the ideological underpinnings of a neoliberal policy agenda (Chwieroth, 2007; Carant, 2017). Market and trade liberalization, deregulation, privatization and decentralization, reduction in public spending, and tax breaks to encourage foreign investment all appear to have been promoted for their efficiency-promoting qualities, with little apparent emphasis on their reception among developing country citizens. The credit (or blame) for this agenda if often placed on IMF and World Bank economists (Beeson & Islam, 2005; Bush & Martiniello, 2017). However, developing country economic professionals have

also been associated with the same agenda (Chorev & Shrank, 2017). One can debate whether neoliberal policies are always good or bad economics (Rodrik, 2017). However, the charge of illiberal paternalism vis-à-vis developing country citizens lingers.

In this article we present results from a field experiment where we analyse the extent to which students exposed to training in economics display illiberal paternalist tendencies. More specifically, the experiment tests whether students with training in economics are technocratic paternalists who stick to the policy prescriptions of efficiency-based optimal tax theory when given information that ordinary citizens disagree with implications of this theory, or democrats who adapt their position to that of popular opinion. Our subjects are 230 master and PhD students of business and economics at two universities in Tanzania, who vary in the number of years they have had of training in economics. In the experiment, subjects were randomly assigned to a control and a treatment group. Both groups were reminded of the efficiency-based Ramsey (1927) type argument that internationally mobile companies should be taxed less heavily than immobile companies (Sørensen, 2007). In addition, subjects in the treatment group were informed that ordinary citizens disagree with the implications of this theory and believe that mobile companies should not get favourable tax treatment. The latter statement is a factual one which we base on a study of tax preferences of ordinary citizens in Tanzania documented in Kolstad, Wiig, & Fjeldstad (2018). After treatment, tax preferences of the subjects were elicited using discrete choice experiments (DCEs).

In our first and main DCE, subjects compared pairs of companies differing in a number of attributes including international mobility, expressing preferences about which company should be taxed the hardest. The results show that the control group expressed preferences

consistent with the argument from optimal tax theory, taxing mobile companies less heavily than immobile ones. The additional information given to the treatment group moved them significantly in the direction of popular opinion, resulting in an insignificant difference in taxation between mobile and immobile companies for this group. Further analysis reveals that these results are driven by students with longer exposure to economics, *both* the preference for lower taxation of mobile companies in the control group, *and* the treatment effect. This strongly suggests that our results provide evidence on how *economists* behave, rather than how people in general take in and aggregate information. The absence of significant results for the mobility attribute among those with lower exposure to economics also suggests that our results are not entirely explained by experimenter demand effects, as we would then expect to also see these types of effects in the responses of this group of subjects. We present additional results to address this concern.

The results from our first DCE can be interpreted in two ways; that students with training in economics are somewhat democratic and trade off theory against popular views, or that they are extremely democratic and have lexicographic preferences for respecting popular opinion over theory. Results from our second DCE support the first interpretation. In this second DCE, subjects decided between pairs of tax policies, where their levels of popular support and support from economic experts were two of their attributes. The results show that both popular approval and approval by economic experts make a tax policy more likely to be chosen by our subjects, with the difference in the effects of these two attributes insignificant. In sum, these results suggest that professionals trained in economics act partly as technocratic paternalists and partly as democrats when deciding on public policy. We also provide additional descriptive information to support this interpretation.

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Our results for the other company attributes also provide insights into how economic professionals navigate questions of tax legitimacy in a developing country context where governance tends to be weak. Some of the views of the appropriate bases for taxation are not surprising; our subjects favour higher taxes on more profitable companies, and progressive over regressive taxation. Other views seem harder to square with those of disinterested economic professionals basing their decisions purely on economic theory. For instance, our subjects favour higher taxes on foreign owned companies than domestic ones, consistent with views of the population in general (Kolstad et al., 2018). While identity may be important to economic decisions (Akerlof & Kranton, 2000), it is tempting to view these findings in light of the fact that people typically navigate multiple identities (Sen, 2006); in our case our subjects are both economic professionals and Tanzanian citizens. A striking result from our second DCE is also that our subjects' choice of tax policy is unaffected by the revenue raised. This may suggest that in a country context where confidence in the government to appropriately use tax revenues for socially beneficial ends is low, effects of taxation on economic activity may be more important than considerations about how taxes affect the budget constraint of the government. This raises additional questions of tax legitimacy in the context of government imperfections which should be analysed in further work.

The paper is structured as follows. Section 2 elaborates the details of our experiment and empirical approach. In Section 3, we present the results of the experiment, and discuss interpretations and implications. Section 4 concludes.

2 Empirical Strategy

The field experiment was conducted in the period May to November 2017, our subjects were 230 Master's and PhD students in business and economics from Mzumbe University (Dar es Salaam Campus College) and Dar es Salaam University in Tanzania. The basic structure of our experiment is illustrated in Figure 1. The experiment was conducted in regular classes for the students, and through an online survey that the students logged onto. We generated 20 different login codes, which were randomized among students. Each login code assigned subjects to either the control group or the treatment group, and subsequently to one of 10 blocks in the two discrete choice experiments (DCEs) that followed. In between the two DCEs, the students also answered a number of additional survey questions on their socio-economic background and social, political, and economic views.
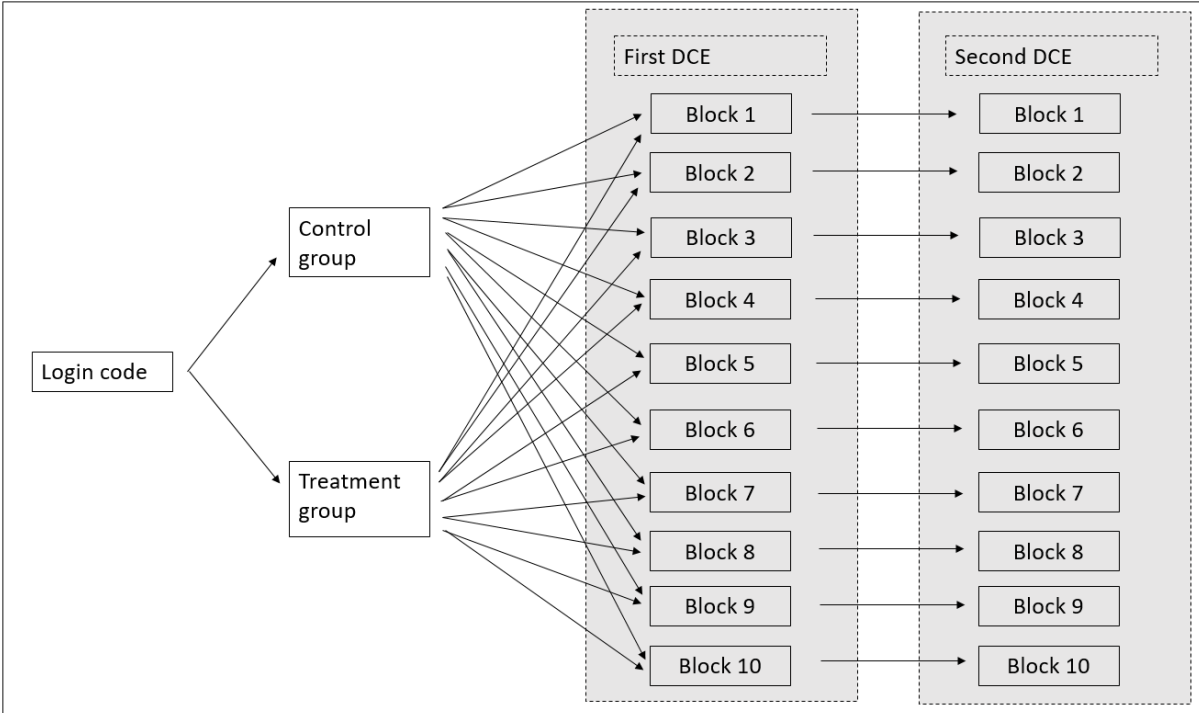

Figure 1. Basic structure of experiment

After inputting the login code, students in both the control and treatment group were taken to a first screen with text emphasizing their identity as economists, and using a simple argument based on Ramsey (1927) that for reasons of efficiency, mobile corporations should according to optimal tax theory be taxed less heavily than immobile corporations.[3] After this first screen of text, subjects in the control group were taken to the first discrete choice experiment, while subjects in the treatment group proceeded to a second screen of text. The second screen informed students in the treatment group that according to a survey of ordinary citizens in Dar es Salaam, ordinary Tanzanians disagree with the implications of optimal tax theory just presented, and want mobile firms to be taxed more heavily or as heavily as less mobile ones. The information used here was based on a separate discrete choice experiment conducted by the authors among citizens of Dar es Salaam (see Kolstad et al. (2018) for details). After reading this second screen of information, subjects in the treatment group were taken to the first discrete choice experiment. In other words, both control and treatment groups were exposed to a message emphasizing their identity as economists and an efficiency argument for taxing mobile companies less heavily, while the treatment group was additionally treated with the information that ordinary citizens disagree with the argument, believing that mobile companies should be taxed more heavily. It should be noted that while the background and current study programme of our subjects is academically weaker than at more advanced universities elsewhere, their economics education is fairly traditional and includes taxation theory.

In the first discrete choice experiment, subjects were randomized into one of 10 blocks (based on the assigned login codes). In each block, they were presented with comparisons between two companies A and B as illustrated in Figure 2. For each comparison, they had to decide

which of the two companies should be taxed the highest. The companies were characterized as having six attributes including mobility, as shown in the figure, and with the full list of levels for each attribute given in Table 1. The attributes and levels included are identical to the choice experiment conducted among ordinary citizens in Kolstad et al. (2018), whose results we use in the treatment. The attributes were selected based on their centrality in theories of taxation and development (in particular profits, sector and exports), but also to control for characteristics respondents may associate with mobile corporations (which in addition to the aforementioned attributes include country of origin and local employment effects).[4] In each block, subjects were given ten of these comparisons, and the order of the attributes was randomized across blocks to avoid order effects. An orthogonal design approach was used to design the experiment in order to make the attribute levels independent.[5]

		Company A	Company B
Sector	Type of business activity that the company is conducting	Agriculture	Mining, oil, gas
Exports	Part of sales outside Tanzania	None	A quarter of sales
Mobility	How easily the company could move all operations out of Tanzania	With difficulty	Easily
Country of origin	The country the company is from	India	China
Local employees	Part of workforce that are Tanzanians	None	All
Profits	How much money the company makes after costs	12 billion TSh	10 billion TSh

Figure 2. Sample comparison first discrete choice experiment

Table 1. Attributes and levels of first discrete choice experiment

Attribute	Explanation	Levels	Variable name	Variable type
Mobility	How easily the company could move all operations out of Tanzania	Easily With difficulty	Mobile (omitted category)	Dummy
Profits	How much money the company makes after costs	10 billion TSh 11 billion TSh 12 billion TSh 13 billion TSh	Profits	Continuous
Sector	Type of business activity that the company is conducting	Manufacturing Mining, oil, gas Services Agriculture	Manufacturing (sector) Mining, oil gas (sectorI Services (sector) (omitted category)	Dummy Dummy Dummy
Local employees	Part of workforce that are Tanzanians	None Half All	Local employees (share)	Continuous
Country of origin	The country the company is from	China Great Britain India Tanzania	China (country of origin) Great Britain (country of origin) India (country of origin) (omitted category)	Dummy Dummy Dummy
Exports	Part of sales outside Tanzania	None A quarter of sales	Exports (share of sales)	Continuous

The design generates 20 observations for each respondent (ten choice sets, two companies in each), a total of 4600 observations for our sample. Our dependent variable is a dummy variable for whether or not a company was chosen to be given higher taxes in each comparison. Our main independent variable is a dummy variable for high mobility, with low mobility the omitted category. For the five other attributes, variables used in estimations are shown in Table 1. We use conditional logit estimation for our main results, interacting our mobility variable with a treatment dummy to test for differences between treatments in tax preferences for mobile firms (the individual treatment dummy term is subsumed by the fixed effects):

$$\Pr\left(y_{ijt} = 1 \middle| x_{ijt}\right) = F(\alpha_{ij} + \beta_1 mobile_{ijt} + \beta_2 mobile_{ijt} * treatment_i + x'_{ijt}\gamma) \qquad (1)$$

where x_{ijt} is the vector of attribute levels for individual i's choice set j and alternative t, and x'_{ijt} is the same vector excluding the mobility attribute. This is essentially a logit estimation with fixed effects at the choice set level, where F is the cumulative logistic distribution $F(z) = \frac{\exp(z)}{1+\exp(z)}$. We show in robustness tests that results are qualitatively similar using mixed logit. We have also run more general models interacting the treatment dummy with all the variables, our main result for mobility holds and there is little additional information to gain from this in terms of differences between treatment and control.

While the first discrete choice experiment focused on preferences across tax objects (corporations) with different attributes, we attempt to corroborate and add to any findings from this in the second choice experiment, which entails a comparison of tax policies with different attributes. Among these attributes we explicitly include economic expert support for a policy (high or low) and popular support for the policy (high or low), in addition to three other attributes. A sample comparison of two tax policies A and B are given in Figure 3. The full list of attributes and levels are presented in Table 2. Importantly, the attributes include government revenue, which allows us to address (and dismiss) the possibility that responses to the first DCE were driven by beliefs that taxing certain companies would lead to higher government revenue.

As in the first discrete choice experiment, we use an orthogonal design with the order of attributes randomized across the ten blocks. In this second experiment, however, each respondent faced only three comparisons, generating a total of 1380 observations. The resulting data are analysed using conditional logit (and results are robust to other approaches). While the main objective of our second DCE is to estimate and compare the

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effects of expert and popular support for the preferred choice of tax policies across respondents, to see if either matters and one is more important than another, we do also present an estimation where the tax policy efficiency loss is interacted with the treatment variable. A more general model where all variables are interacted with the treatment variable did not detect any further differences between groups.

	Tax Policy A	Tax Policy B
Government revenue	10 billion TSh	12 billion TSh
Support by economic experts	High	Low
Efficiency loss	High	Low
Popular support	High	Low
Distributional profile	Progressive	Regressive

Figure 3. Sample comparison second discrete choice experiment

Table 2. Attributes and levels of second discrete choice experiment

Attribute	Levels	Variable name	Variable type
Efficiency loss	High	Efficiency loss high	Dummy
	Low	(omitted category)	
Popular support	High	Popular support high	Dummy
	Low	(omitted category)	
Support by economic experts	High	Expert support high	Dummy
	Low	(omitted category)	
Distributional profile	Progressive	Progressive	Dummy
	Regressive	(omitted category)	
Government revenue	10 billion TSh	Government revenue	Continuous
	12 billion TSh		

The treatment and control groups are balanced across a number of socio-economic background variables, as shown in Table B1 in Appendix B. Our students are about 30 years on average (our sample includes after work students), half are male, two in five were born in Dar es Salaam, business administration is their most common major, four in five work in addition to being a student. Overall, as revealed by the asset questions, our subjects are from more privileged backgrounds than the average Tanzanian, and in an additional question of self-assessed class background, most students classify themselves as middle class (not reported in Table B1).

3 Results

3.1 Results From First Discrete Choice Experiment

Our main results are presented in Table 3. The results are presented in terms of odds ratios rather than coefficients from the conditional logit estimation, to ease interpretation. In other words, estimates above one for a variable mean that a company with the corresponding characteristic is more likely to be chosen as the one to tax more heavily, estimates below one make the company less likely to be chosen. The first column shows results from the full sample and includes the interaction effect between mobility and treatment (with the individual treatment dummy subsumed in the fixed effects), columns two and three split the sample according to control and treatment groups, respectively. The results show that the control group expresses preferences for significantly lower taxes for mobile companies, consistent with the efficiency argument they were reminded of at the start of the experiment (we discuss the issue of selection briefly below). Moreover, the interaction effect is significant and greater than one, meaning that the treatment group moved in the direction of popular opinion of not

accepting lower taxes for mobile companies, resulting in an insignificant odds ratio for

mobility in the treatment group as seen in column three.

Table 3. Main results. Odds ratios from conditional logit estimation

	(1)	(2)	(3)	(4)
Sample	Full	Control group	Treatment group	Full
Dependent variable	Company choice	Company choice	Company choice	Company choice
Mobile	0.838***	0.836***	1.092	1.039
	(0.05)	(0.05)	(0.07)	(0.11)
Interaction Mobile*Treatment	1.311***			0.899
	(0.11)			(0.12)
Profits	1.066***	1.058*	1.074**	1.066***
	(0.03)	(0.04)	(0.04)	(0.03)
Manufacturing (sector)	1.501***	1.621***	1.385***	1.523***
	(0.11)	(0.17)	(0.15)	(0.11)
Mining, oil, gas (sector)	1.940***	1.974***	1.894***	1.976***
	(0.15)	(0.21)	(0.21)	(0.15)
Services (sector)	1.089	1.119	1.054	1.097
	(0.08)	(0.12)	(0.11)	(0.08)
Local employees (share)	0.755***	0.739***	0.771***	0.761***
	(0.05)	(0.06)	(0.07)	(0.05)
China (country of origin)	1.330***	1.420***	1.246**	1.337***
	(0.10)	(0.15)	(0.14)	(0.10)
Great Britain (country of origin)	1.410***	1.656***	1.199	1.412***
	(0.11)	(0.18)	(0.13)	(0.11)
India (country of origin)	1.336***	1.338***	1.340***	1.350***
	(0.10)	(0.14)	(0.14)	(0.10)
Exports (share of sales)	1.754***	1.718**	1.788**	1.793***
	(0.31)	(0.42)	(0.45)	(0.31)
Interaction Mobile*Years studied economics				0.920**
				(0.03)
Interaction Mobile*Treatment*Years studied economics				1.158***
				(0.05)
Pseudo R2	0.054	0.064	0.048	0.058
N	4600	2380	2220	4580

Note: Odds ratios from conditional logit estimation, robust standard errors in parentheses, *** indicates significance at the 1% level, ** at 5%, * at 10%. Variables reflect the attributes included in the first discrete choice experiment, as explained in Table 1. Interaction Mobile*Treatment is the two-way interaction of the mobile attribute with the treatment dummy. Interaction Mobile*Years studied economics is the two-way interaction of the mobile attribute and the number of years a respondent has studied economics. Interaction Mobile*Treatment*Years studied economics is the three-way interaction of the mobile attribute, the treatment dummy, and the number of years a respondent has studied economics. The individual treatment dummy term is subsumed in the fixed effects in estimations (1) and (4), as are the individual Years studied economics variable and the two-way interaction of the treatment dummy and the Years studied economics variable in estimation (4).

Since our subjects are students at different levels and pursuing different areas of

specialization, there is variation in the previous exposure our subjects have to economics. This

allows us to test whether their responses are likely due to their economics background, and

not simply reflective of how humans aggregate information in general. To this end, the

estimation in column four of Table 3 adds two further interaction terms results for which are

reported at the bottom of the column.[6] The first of these terms interacts the mobility attribute with the number of years a subject has studied economics, which lets us look at variation in responses to this attribute in the control group according to their exposure to economics. The second of the these terms is a three-way interaction between the mobility attribute, the treatment dummy, and the number of years a subject has studied economics, which captures variation in the treatment effect related to this attribute for those with more or less exposure to economics.

For ease of exposition, we display the average marginal effects of mobility on company choice in Figure 4 (in log odds rather than odds ratios), by treatment group and number of years subjects have studied economics. The blue line captures average marginal effects for the control group, and suggests that mobility has essentially no effect on tax choices of control group subjects with little previous exposure to economics, and that the effect grows more negative as exposure increases, becoming significantly negative around having 2-3 years of economics training. Average marginal effects for the treatment group are represented by the red line, which suggests no effect of the treatment on choices related to the mobility attribute for those with little exposure to economics. As exposure increases, however, the marginal effect of the mobility attribute becomes increasingly more positive in the treatment group compared to the control group, with the treatment effect becoming significant between 2 to 3 years of economics training. In sum, these results suggest that both the negative effect of mobility on tax choices in the control group, and the positive effect of the treatment on this attribute, are driven by students with greater exposure to economics. In other words, our results seem to reflect the background of our subjects in economics, rather than some more general human trait also found in other groups.
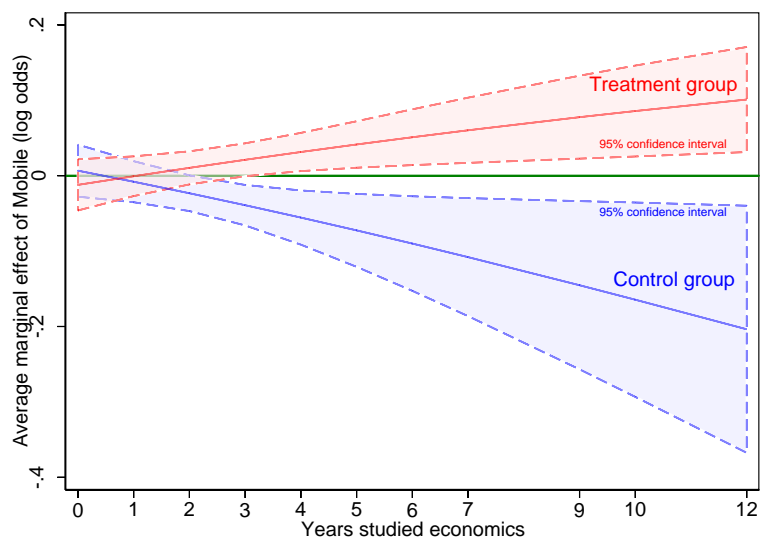
Figure 4. Average marginal effects of mobility on company choice, by treatment group and years studied economics

For the other variables in Table 3, respondents choose to tax more profitable companies more heavily, which is not surprising. Their views on taxation favour agricultural enterprises (the omitted category) over manufacturing and mining, oil and gas, while services are not taxed significantly differently from agriculture. The preference for taxing agriculture less heavily than other sectors mirrors findings from results for ordinary citizens in Kolstad et al. (2018), and could reflect a close connection to agriculture through culture, history and the occupations of relatives. There is a clear domestic favouritism tendency in wanting to tax less heavily companies with a large share of Tanzanian employees, and in imposing higher taxes on foreign companies, whether Chinese, British or Indian, than domestic ones (the omitted category). Perhaps surprisingly, given that the subjects are economics students likely to have studied export led growth strategies, our subjects prefer to tax companies more heavily that export a greater share of their sales. Given our focus on mobility in the messages for both the control and treatment groups, there seems to be little difference in responses between treatments in other attributes. A more general model which includes treatment interactions with all variables reveals that only in the case of British companies do we find a significant

difference; respondents in the control group want these to be taxed more heavily than respondents in the treatment group. Results presented so far are all robust to using mixed logit estimation, corresponding results are shown in Table C1 in Appendix C. Mixed logit makes it possible to look at heterogeneities in responses, and as the standard deviations at the bottom of the table show, there is considerable heterogeneity in most variables, but less so in views that foreign companies and companies with greater export shares should be taxed more heavily.

3.2 Interpreting the Results in the Control Group

The negative result for mobility in the control group could be an effect of the reminder of optimal tax theory they get at the start of the experiment, which emphasized lower taxes for mobile companies for efficiency reasons. However, it could also be due to selection of students into economics. At least two types of selection are possible, students of economics could be politically conservative and for this reason favour efficiency and lower taxes for mobile companies, or they could be from an economic background where they would benefit from lower taxes on mobile assets. In the absence of a control group where no message was given, we cannot rule out selection completely. However, Appendix D presents some additional descriptive results consistent with an effect of the message rather than selection. Students expressed a strong identity as economists, consistent with our information succeeding in making the professional identity of our subjects salient. Answers to survey questions on political and redistributive attitudes suggest that our subjects are not particularly conservative, nor are they predominantly from families that would gain economically if taxes on mobile assets such as financial capital were reduced.

3.3 Interpreting the Treatment Effect

The treatment, which informs our subjects that ordinary Tanzanians do not agree with the implications of the efficiency-based argument for lower taxes on mobile companies, makes responses on this attribute move in the direction of popular opinion. As mentioned, one interpretation of this is that our economics students are partly technocratic and partly democratic, giving some weight both to economic theory and to the views of ordinary citizens. Other interpretations are also possible. It could be that economics students are smart (rather than part) technocrats, who come to understand the importance of political feasibility in implementing economic policy.[7] We do not have the data to distinguish these two interpretations.

In Appendix E, we provide additional descriptive results which are consistent with the part technocrat - part democrat interpretation, while making some other possible interpretations less credible. More direct evidence for our main interpretation is also obtained from our second DCE, whose results we discuss in the subsequent subsection. Responses to survey questions reveal that our subjects are very democratically inclined. They express strong faith in economic experts and theory, but less so in popular knowledge on economic knowledge and taxation, suggesting that they are also technocratic. We see little evidence that our subjects are elitist rather than technocratic. Nor does it seem to be the case that our treatment has triggered concern for redistributive issues rather than respect for popular opinion.

The fact that we find an insignificant result for the mobility attribute in our treatment group could potentially also reflect greater confusion created in a group presented with two

countervailing arguments. However, if this was the case, we should observe more noise in the results for mobility in this group. The standard deviations presented in the mixed logit estimation in Appendix C do not suggest that this is the case. Related to this, our results could also reflect greater apparent clarity of experimenter demand effects in the control group (where it is clear in which direction the given argument points) than in the treatment group (where two different arguments point in opposite directions). Responses to survey questions (both structured and open-ended) designed to address possible experimenter demand effects suggest this is not the case, as reported in Appendix E.

3.4 Results From the Second Discrete Choice Experiment

A more direct result corroborating the interpretation that both technocratic and democratic arguments matter for economists' views of tax policy comes from our second discrete choice experiment. Table 4 presents results for the different levels of the attributes assigned to the alternative tax policies, again presented as odds ratios. As the results in column one show, respondent's probability of choosing a certain tax policy increases in both popular support and economic expert support for the policies. However, the effects of these two variables are not significantly different, which reaffirms the above interpretation of economists' public policy preferences being part technocratic and part democratic.

Table 4. Results from second discrete choice experiment. Odds ratios from conditional logit estimation.

	(1)	(2)	(3)
Sample	*Full*	*Full*	*Full*
Dependent variable	*Tax choice*	*Tax choice*	*Tax choice*
Efficiency loss high	0.818**	0.713***	1.006
	(0.07)	(0.08)	(0.19)
Interaction Efficiency loss high*Treatment		1.330*	0.805
		(0.21)	(0.21)
Popular support high	1.250***	1.254***	1.252***
	(0.10)	(0.10)	(0.10)
Expert support high	1.446***	1.447***	1.450***
	(0.12)	(0.12)	(0.12)
Progressive	1.379***	1.389***	1.395***
	(0.12)	(0.12)	(0.12)
Government revenue	1.020	1.018	1.014
	(0.04)	(0.04)	(0.04)
Interaction Efficiency loss high*Years studied economics			0.869**
			(0.05)
Interaction Efficiency loss high*Treatment*Years studied economics			1.225**
			(0.10)
Pseudo R2	0.067	0.070	0.075
N	1380	1380	1374

Note: Odds ratios from conditional logit estimation, robust standard errors in parentheses, *** indicates significance at the 1% level, ** at 5%, * at 10%. Variables reflect the attributes included in the second discrete choice experiment, as explained in Table 2. Interaction Efficiency loss high*Treatment is the two-way interaction of the Efficiency loss high dummy with the treatment dummy. Interaction Efficiency loss high*Years studied economics is the two-way interaction of the Efficiency loss high dummy and the number of years a respondent has studied economics. Interaction Efficiency loss high*Treatment*Years studied economics is the three-way interaction of the Efficiency loss high dummy, the treatment dummy, and the number of years a respondent has studied economics. The individual treatment dummy term is subsumed in the fixed effects in estimations (2) and (3), as are the individual Years studied economics variable and the two-way interaction of the treatment dummy and the Years studied economics variable in estimation (3).

Results for the other attributes reveal that our subjects prefer progressive to regressive tax policies, while a high efficiency loss decreases support for a tax policy. Column two of Table 4 interacts the efficiency variable with the treatment indicator, and the results show that the treatment group deemphasizes efficiency compared to the control group.[8] In other words, adding popular opinion to the message given to subjects takes away some of their focus on efficiency aspects of taxation. As shown in column three, where the efficiency attribute and its interaction with the treatment dummy are interacted with the years subjects have studied economics, both the emphasis on efficiency in the control group and decreased emphasis on efficiency caused by the treatment, are driven by students with longer exposure to economics. We trace out the average marginal effects of the efficiency loss attribute on tax choice in

Figure 5, which shows that the marginal effect is significantly more positive in the treatment group from about 3-4 years of exposure to economics.
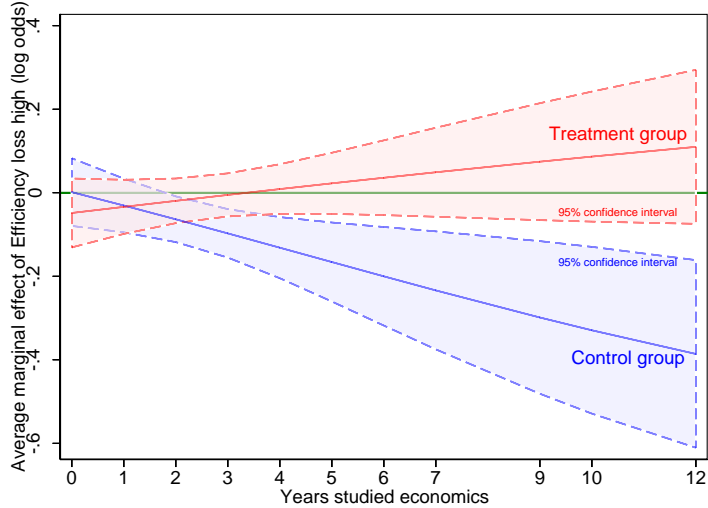

Figure 5. Average marginal effects of efficiency loss on tax choice, by treatment group and years of economics

In additional results presented in Table E3 in Appendix E, we replace the Years studied economics interaction terms with corresponding interaction terms for an economics Identity Index, constructed from three questions capturing the degree of identification of subjects with economics (see notes to Table D1 in Appendix D for details on its construction). For both of our discrete choice experiments, the Identity Index interactions were insignificant, suggesting that the effect of exposure to economics does not run through a strengthened identity in the role as an economist. While this may seem paradoxical, we should keep in mind that exposure to economics can change both how strongly one identifies with the subject, and what one takes identifying with economics to mean. In other words, both the strength and meaning of identifying with a role as an economist may be affected. Students' perceptions of what economics is and what it means to be an economist may change substantially over the course

of their studies, and this may be what drives our main results. In a sense, one can argue that exposure to economics results in informed rather than blind socialization.

Going back to Table 4, the only tax policy attribute revealed to not matter for our respondents is government revenue. This helps allay concerns that results from our first DCE were due to beliefs that taxing certain types of companies would raise more government revenue; if respondents do not care about revenue, this should not influence choices of which company to tax the highest. The revenues result is also surprising in itself and has several possible explanations. One is that the two levels of this attribute used in the experiment, 10 and 12 billion Tanzanian Shilling, are not sufficiently differentiated to produce any significant effect. Another explanation could be a lack of confidence among our subjects that the Tanzanian government would spend tax revenues in a socially beneficial way. In other words, while our subjects care about inequality and efficiency effects at the collection side of taxation, the spending side is not seen as relevant due to imperfections of the Tanzanian government. Our survey does not contain questions that directly address this, but does include one question on democratic accountability which to an extent supports this interpretation. We asked respondents for their level of agreement with the statement "Overall, democracy works well in Tanzania", with agreement stated on the 1-5 scale used for the previously mentioned questions. Figure E6 in Appendix E presents the distribution of responses, where agreement has been rescaled from 0 to 1. The main response is slightly below a "Neither agree nor disagree" response, and as many respondents agree as disagree with the statement. This indicates that confidence in the political system in Tanzania is not very strong among our respondents.

4 Conclusion

According to Broome (1986), it is in principle possible to argue that what is good for someone can be distinct from what they prefer, that in some sense "betterness" can be distinct from preferences. But if this is the case, who decides what is better for people? And to what extent do economists take it upon themselves to decide? Our experiment suggests that an economics education produces professionals who act from a mix of technocratic paternalist motives based on economic theory and democratic motives based on what people actually prefer, when formulating views on optimal public policy. This provides new insights into how economists act when in the role of the benevolent dictator that often features in their models, and how they navigate different identities of professional, democrat, citizen, and public servant. It also raises new questions, including on the effect of receiving an education in other disciplines such as medicine or law or political science, and the extent to which behaviour follows similar patterns in trading off insights from their respective fields and popular opinion.

In our experiment, randomization into control and treatment means that our results are driven by differences in the information provided rather than other differences between treatment groups. The results for each attribute in our discrete choice experiments are also conditional on the other attributes, which means that our results for the mobility attribute are not driven by perceptions that mobile companies may be different from immobile ones in profitability, sector, local employment, country of origin, or export orientation. Moreover, while subjects in our treatment group do not know that the popular opinion they are exposed to was also elicited using an identical DCE, and hence could think that ordinary people tend to associate mobility with other company characteristics like profitability or sector, this should serve to attenuate our treatment effect for the mobility attribute, which would mean that our

estimate is an under-estimate of the impact of popular views on economist assessments, and the true effect is even larger.

Our main result is striking given the high vulnerability of developing economies to the location decisions of mobile corporations (Goldberg & Pavcnik, 2007). In a context of scarcity, the finding that our subjects de-emphasize efficiency when it contrasts with public opinion suggests that some serious revision is required to claims that economists tend to act as advocates of efficiency in their advisory work (Schultze, 1982; Nelson, 1987). Moreover, previous work using observational data to analyse these questions faces the challenge that economic professionals may find a reason or excuse for value-driven interventions in public choice arguments, i.e. in failures of a political system to act in the interests of the general public. Our experiment cuts through this difficulty by essentially designating the subjects as benevolent dictators being asked to arbitrate directly between efficiency concerns and popular demands.

It should nevertheless be noted that DCEs face the general challenge that each of the attributes may correlate in respondents' minds with unspecified attributes of the options presented, and results for each of them may therefore be driven by underlying and unobserved attributes. For instance, the result that our respondents prefer to tax more profitable companies higher could reflect a preference for equality and taxing richer corporate owners more, or a preference for taxing companies with greater market power more, which in our context could also be linked to the extent to which a company is politically connected, enjoying rents from these connections. Our approach does not identify the precise link. It is

also possible that an underlying factor like this explains the puzzling result of higher taxes on firms with greater exports.

Considerable criticism has been levied at economics education for under-communicating important advances in the field related to market imperfections that are relevant to real world problems, resulting in recent innovations such as the CORE project (CORE Team, 2017). Our findings suggest that in the process of improving economics curricula, providing students with a better understanding of political economy analyses is essential. In order to take citizen preferences into account, students and professionals need to be aware of and able to assess them. In the absence of such information, our subjects appear to stick to theoretical arguments. Our results are broadly consistent with a division of labour where society sets the policy priorities, and economists advise on implementation. They suggest that ethical considerations remain important to economics students, which is relevant for understanding their behaviour in public service, but also in business where they need to consider effects of their decisions on stakeholder interests. Finally, our findings pose a challenge to unequivocal claims that economists pursue explicitly ideological agendas.

The role of professionals and the professions in the Global South has been neglected compared to their developed country counterparts (Chorev & Shrank, 2017). Arguably, studying developing country economics students is important as they are likely to become centrally placed in influencing policy decisions in contexts where making good decisions is of critical importance for local and regional development. Since our subjects are locally trained economists, the practical significance of our results depends on their relative influence in setting economic policy. Further work is needed to establish whether they are more or less

attuned to local citizen preferences for policy than advisors from the international financial institutions. Among the many good reasons for including local experts in designing policy, it seems important to establish whether their degree of receptiveness to the local political context is one.

Acknowledgments

Endnotes

[1] For a recent review, see Hummel & Maedche (2019).

[2] We think of illiberal paternalism as supporting or advising policy or decisions that run contrary to popular preferences. In particular, we focus on technocratic paternalism, which can be seen as a form of illiberal paternalism based on theory, or norms, or received wisdom within a certain discipline or profession.

[3] The texts were presented in Swahili, the English translation is provided in Appendix A.

[4] For a detailed conceptual framework, see Kolstad et al. (2018).

[5] Lacking prior estimates for our population we did not use an efficient design approach.

[6] Note that the individual treatment dummy and years of economics studies terms are subsumed by the fixed effects, as is their two-way interaction.

[7] We thank an anonymous reviewer for this point.

[8] The results are similar in a fuller specification where we interact all the variables with the treatment indicator, but no other interaction effects are significant, so we do not report this specification here.

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Appendix A. Treatments

Text box A 1. Information provided across treatments

First screen (Control and treatment group):

"As a student of **economics**, you may know that optimal tax theory is an important **economic theory.**
This theory implies that a company that can **easily move** to another country should be **taxed less** heavily than a company that is immobile.
The reason is that taxing **mobile companies less** will create a **more efficient** outcome."
(Select OK to continue)

Second screen (Treatment group only):

"A recent survey done in Dar es Salaam finds that **Tanzanian citizens** do not agree with these implications of optimal tax theory.
These ordinary citizens believe that companies that can **easily move** out of Tanzania should be taxed **more heavily** than immobile companies, or at the same level.
These citizens think it would be wrong to let more mobile companies pay less in taxes.
In their opinion, **mobile companies should pay more** or as much as immobile companies."
(Select OK to continue)

Appendix B. Descriptives

Table B 1. Balance across treatment arms

	Control	Treatment	p-value (control vs treatment)
Age	29.933	30.135	0.772
	(0.525)	(0.458)	
Male	0.496	0.505	0.896
	(0.046)	(0.048)	
Born in Dar es Salaam	0.429	0.396	0.622
	(0.046)	(0.047)	
Major: Procurement and logistics	0.269	0.108	0.002
	(0.041)	(0.03)	
Major: Accounting and finance	0.092	0.162	0.115
	(0.027)	(0.035)	
Major: Business administration	0.580	0.658	0.226
	(0.045)	(0.045)	
Major: Applied economics	0.059	0.072	0.687
	(0.022)	(0.025)	
Years studied economics	2.398	2.532	0.621
	0.166	0.212	
Spending weekly (TSh)	123569.622	131102.027	0.719
	(13723.899)	(15812.98)	
Main income source: Job	0.689	0.730	0.499
	(0.043)	(0.042)	
Main income source: Family	0.151	0.108	0.331
	(0.033)	(0.03)	
Main income source: Loans	0.017	0.009	0.601
	(0.012)	(0.009)	
Main income source: Scholarship	0.025	0.072	0.102
	(0.014)	(0.025)	
Main income source: Other	0.118	0.081	0.355
	(0.03)	(0.026)	
Household assets: Radio	0.899	0.919	0.604
	(0.028)	(0.026)	
Household assets: TV	0.908	0.937	0.407
	(0.027)	(0.023)	
Household assets: Motor vehicle	0.882	0.910	0.495
	(0.03)	(0.027)	
Household size	5.958	5.586	0.294
	(0.252)	(0.249)	
Household no. of rooms	4.076	4.099	0.917
	(0.148)	(0.169)	
Working in addition to being student	0.798	0.802	0.948
	(0.037)	(0.038)	
Income cash last month (TSh)	1201496.076	55166840.225	0.319
	(112573.87)	(54044094.199)	
Planned work after studies: Self-employed	0.235	0.180	0.304
	(0.039)	(0.037)	
Planned work after studies: Private sector	0.345	0.306	0.538
	(0.044)	(0.044)	
Planned work after studies: Public sector	0.336	0.396	0.346
	(0.043)	(0.047)	
Planned work after studies: Other	0.084	0.117	0.408
	(0.026)	(0.031)	
N	119	111	

Note: Means of control and treatment group respondent characteristics, with standard deviations in parentheses.

Appendix C. Alternative estimations

Table C 1. Mixed logit estimation

	(1)	(2)	(3)	(4)
Sample	*Full*	*Control group*	*Treatment group*	*Full*
Dependent variable	Company choice	Company choice	Company choice	Company choice
Mean				
Mobile	0.785**	0.762**	1.092	1.065
	(0.09)	(0.09)	(0.11)	(0.17)
Interaction Mobile*Treatment	1.348**			0.884
	(0.20)			(0.21)
Profits	1.092**	1.110**	1.120*	1.112***
	(0.04)	(0.06)	(0.07)	(0.05)
Manufacturing (sector)	1.687***	1.787***	1.552***	1.721***
	(0.17)	(0.26)	(0.25)	(0.18)
Mining, oil, gas (sector)	2.290***	2.429***	2.557***	2.511***
	(0.28)	(0.41)	(0.49)	(0.32)
Services (sector)	1.091	1.067	1.130	1.117
	(0.10)	(0.14)	(0.16)	(0.11)
Local employees (share)	0.688***	0.662***	0.677**	0.701***
	(0.07)	(0.09)	(0.11)	(0.07)
China (country of origin)	1.484***	1.658***	1.356*	1.479***
	(0.16)	(0.25)	(0.21)	(0.15)
Great Britain (country of origin)	1.613***	2.046***	1.322*	1.611***
	(0.17)	(0.32)	(0.21)	(0.17)
India (country of origin)	1.449***	1.398**	1.558***	1.441***
	(0.15)	(0.22)	(0.24)	(0.15)
Exports (share of sales)	2.030***	2.036**	2.537**	2.260***
	(0.53)	(0.74)	(1.05)	(0.65)
Interaction Mobile*Years studied economics				0.880**
				(0.06)
Interaction Mobile*Treatment*Years studied economics				1.218**
				(0.10)
Standard deviation				
Mobile	2.062***	2.821***	1.691***	1.916***
	(0.25)	(0.51)	(0.26)	(0.29)
Interaction Mobile*Treatment	0.864			0.994
	(0.17)			(0.19)
Profits	1.376***	1.412***	1.366***	1.395***
	(0.09)	(0.13)	(0.13)	(0.11)
Manufacturing (sector)	1.747***	1.355	2.381***	1.710***
	(0.26)	(0.70)	(0.50)	(0.31)
Mining, oil, gas (sector)	2.706***	2.326***	3.030***	2.677***
	(0.43)	(0.62)	(0.60)	(0.43)
Services (sector)	0.821	1.661	0.656	0.704*
	(0.21)	(0.55)	(0.18)	(0.14)
Local employees (share)	1.710*	0.596**	2.344***	1.896***
	(0.49)	(0.13)	(0.75)	(0.33)
China (country of origin)	1.306	1.440	1.164	0.699**
	(0.26)	(0.32)	(0.27)	(0.11)
Great Britain (country of origin)	0.782	0.865	1.164	0.810
	(0.15)	(0.34)	(0.34)	(0.21)
India (country of origin)	1.351	2.061***	1.258	1.153
	(0.37)	(0.56)	(0.22)	(0.44)
Exports (share of sales)	2.595	0.357	9.603***	4.819***
	(3.23)	(0.47)	(5.57)	(2.32)
Interaction Mobile*Years studied economics				1.032
				(0.17)
Interaction Mobile*Treatment*Years studied economics				1.088
				(0.08)
N	4600	2380	2220	4580

Note: Odds ratios from mixed logit estimation, robust standard errors in parentheses, *** indicates significance at the 1% level, ** at 5%, * at 10%. Variables reflect the attributes included in the first discrete choice experiment, as explained in Table 1. Interaction Mobile*Treatment is the two-way interaction of the mobile attribute with the treatment dummy. Interaction Mobile*Years studied economics is the two-way interaction of the mobile attribute and the number of years a respondent has studied economics. Interaction Mobile*Treatment*Years studied economics is the three-way interaction of the mobile attribute, the treatment dummy, and the number of years a respondent has studied economics. The individual treatment dummy term is subsumed in the fixed effects in estimations (1) and (4), as are the individual Years studied economics variable and the two-way interaction of the treatment dummy and the Years studied economics variable in estimation (4).

Appendix D. Mechanism results for the control group

In Figure D1, we tabulate responses to an identity index created from responses to the following three questions: "Being an economist is an important part of my identity", "Economic models are a useful representation of how people make decisions", and "From a social point of view, more students should take economics rather than other subjects". Subjects responded their disagreement or agreement with these statements on a five point scale (1 – Disagree very strongly, 2 – Disagree, 3 – Neither agree nor disagree, 4 – Agree, 5 – Agree very strongly). To create the identity index, we took the average across questions, and rescaled from 0 to 1, with higher values reflecting greater agreement with the statements. As Figure D1 shows, students expressed a strong identity as economists, the mean answer on the three questions being "Agree", which is consistent with our information succeeding in making the professional identity of our subjects salient. However, our results could also reflect a possibility that our subjects have a strong sense of professional identity to begin with. In Table D2, first column, we run a conditional logit regression on our control group data, interacting the mobility dummy with the identity index. The results show that the effect of mobility does not significantly differ for subjects with high and low identity scores.
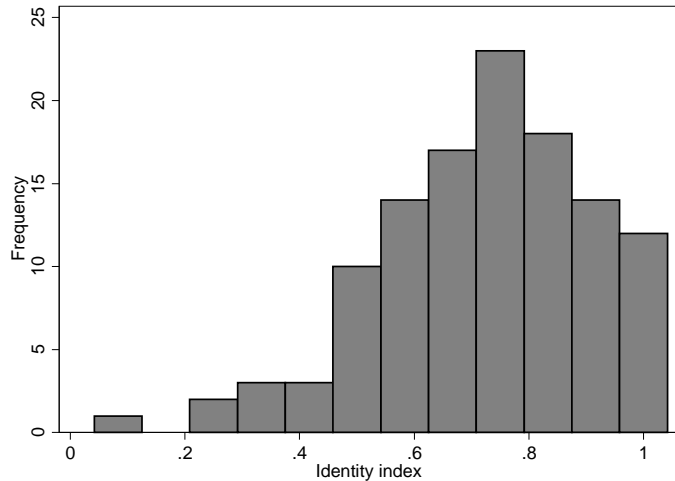
Figure D 1. Histogram of identity index

Note: The Identity index is created from responses to the following three questions: i) "Being an economist is an important part of my identity", ii) "Economic models are a useful representation of how people make decisions", and iii) "From a social point of view, more students should take economics rather than other subjects". Disagreement or agreement with these statements were elicited on a five point scale (1 – Disagree very strongly, 2 – Disagree, 3 – Neither agree nor disagree, 4 – Agree, 5 – Agree very strongly). The index averages responses across questions and rescales them from 0 to 1, with higher values reflecting greater agreement with the statements.

The result on mobility in the control group do not seem to reflect conservative opinions among students. In Figure D2, we present an index of how conservative students are, based on three underlying questions: "The role of the state in the economy should be minimized", "Provision of services like health and education should be done by the private sector", and "Differences in income are largely due to how hard people work". Similar to the identity index, subjects expressed their level of agreement with the statements on a 1-5 scale; we averaged responses across the three questions, and rescaled from 0 to 1. As seen in Figure D2, students are not really that conservative, the mean answer is slightly below Neither agree nor disagree on these questions.
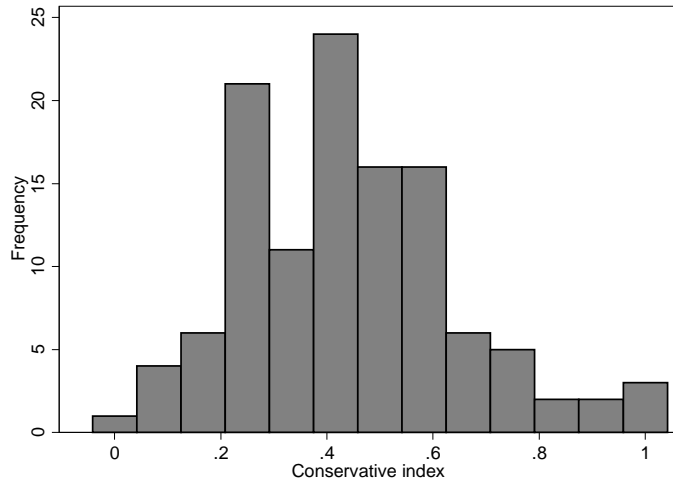
Figure D 2. Histogram of conservative index

Note: The Conservative index is created from responses to the following three questions: i) "The role of the state in the economy should be minimized", ii) "Provision of services like health and education should be done by the private sector", and iii) "Differences in income are largely due to how hard people work". Disagreement or agreement with these statements were elicited on a five point scale (1 – Disagree very strongly, 2 – Disagree, 3 – Neither agree nor disagree, 4 – Agree, 5 – Agree very strongly). The index averages responses across questions and rescales them from 0 to 1, with higher values reflecting greater agreement with the statements.

In Text box D1, we present two additional questions on redistributive preferences which were asked in the survey. Both questions start with a situation of unfair inequality; two anonymous individuals do the same job, one is paid 20,000 Tanzanian Shilling, the other nothing. Our subjects were asked whether they would want to redistribute money from the paid to the unpaid person, with the possibility of partly or fully equalizing payment. However, there is an efficiency loss to redistribution making the sum of money to allocate smaller with redistribution, with an efficiency loss of 75 per cent at full equalization in the first question, and 90 per cent in the second. Table D1 shows the distribution of responses. 75 per cent of our respondents want to redistribute partly or fully, even in the case where the efficiency loss is at the highest. Again, this confirms the impression from the conservative index that students are not tremendously conservative. Contrary to our expectations, more conservative students were also significantly more in favour of higher taxes on mobile corporations, as seen in the

results in Table D2, second column, where the mobile dummy has been interacted with the conservative index.

Text box D 1. Redistribution questions

Redistribution question with 75 per cent efficiency loss	Redistribution question with 90 per cent efficiency loss
Imagine two people that you don't know who work equally hard at the same job. One person receives 20.000 TSh for the job, the other person gets nothing. You can take some money from the first person and give to the second. But taking from one and giving to the other is costly, due to administration costs. So the two people get less money in total the more equally you divide the money. Which of these three options would you choose?	Again imagine two people that you don't know who work equally hard at the same job. One person receives 20.000 TSh for the job, the other person gets nothing. You can take some money from the first person and give to the second. But taking from one and giving to the other is even more costly, due to administration costs. So the two people get less money in total the more equally you divide the money. Which of these three options would you choose?
A. Let the first person keep 20.000 TSh, and the second person get nothing. In total they get 20.000 TSh. B. Let the first person keep 11.000 TSh, and give the second person 1.500 TSh. In total they get 12.500 TSh. C. Let the first person keep 2.500 TSh, and give the second person 2.500 TSh. In total they get 5.000 TSh.	A. Let the first person keep 20.000 TSh, and the second person get nothing. In total they get 20.000 TSh. B. Let the first person keep 10.500 TSh, and give the second person 500 TSh. In total they get 11.000 TSh. C. Let the first person keep 1.000 TSh, and give the second person 1.000 TSh. In total they get 2.000 TSh.

Table D 1. Responses to redistribution questions, proportions in control group

	(1)	(2)
Choice	*75% efficiency loss*	*90% efficiency loss*
No redistribution	25.21	24.37
Some redistribution	24.37	19.33
Full equalization	50.42	56.30

Note: Distribution of responses given to redistribution questions specified in Text Box D1.

Our student subjects are not representative of the general population; 90 per cent self-classify as middle class (half and half lower and upper middle class); half have fathers who are employees and a third high level employees, which speaks to a more privileged background than the average Tanzanian. There is, however, little to suggest that our control group results reflect a selection into economics studies of subjects with a personal or family interest in taxing more mobile assets less heavily. Our survey included the question "My family would gain economically if taxes on mobile assets such as financial capital were reduced, and taxes on immobile assets such as land and properties were increased", with agreement expressed on a 1-5 scale as for the preceding questions. Figure D3 shows a histogram of the responses

where agreement has been rescaled from zero to one. The mean response is to neither agree nor disagree to the question, but our subjects seem to split into two groups on this issue. However, in Table D2 column three we interact the mobile dummy with the rescaled responses to this question. While those whose families would gain from lower taxes on mobile assets tend to have lower probabilities of choosing mobile companies to be more heavily taxed in our first discrete choice experiment, the interaction effect is not statistically significant.
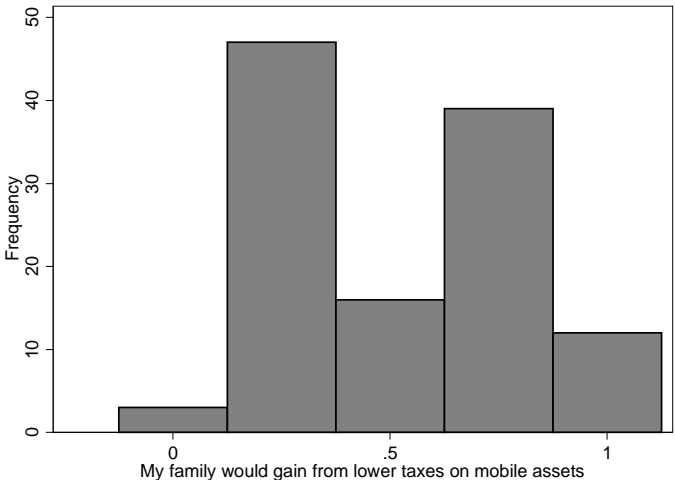
Figure D 3. Histogram of question of family gain from lower taxes on mobile assets

Note: Histogram capturing distribution of agreement with statement specified (in abbreviated form) on x-axis. Disagreement or agreement were elicited on a five point scale (1 – Disagree very strongly, 2 – Disagree, 3 – Neither agree nor disagree, 4 – Agree, 5 – Agree very strongly), and rescaled from 0 to 1 in the figure, with higher values reflecting greater agreement with the statement.

Table D 2. Conditional logit results (abbreviated) with interactions, control group

	(1)	(2)	(3)
Sample	Control group	Control group	Control group
Dependent variable	Company choice	Company choice	Company choice
Mobile	0.694	0.569***	0.972
	(0.18)	(0.08)	(0.13)
Interaction Mobile*Identity Index	1.296		
	(0.44)		
Interaction Mobile*Conservative Index		2.438***	
		(0.71)	
Interaction Mobile*Family gain			0.764
			(0.17)
Pseudo R2	0.064	0.070	0.065
N	2340	2340	2340

Note: Odds ratios from conditional logit estimation, robust standard errors in parentheses, *** indicates significance at the 1% level, ** at 5%, * at 10%. All attribute variables included, some suppressed in output. In the interaction terms for Mobile, the Identity Index, Conservative Index, and Family gain variables are as specified in notes to Figure D1, D2, and D3, respectively, with their main effects subsumed in the fixed effects.

Appendix E. Mechanisms difference treatment and control groups

In this Appendix, we provide additional descriptive results which are consistent with the part technocrat - part democrat interpretation of our experimental results, while making some other possible interpretations less credible. Figure E1 presents a histogram of a democracy index, created from the respondents' level of agreement with the following two questions: "Democracy is preferable to any other kind of government" and "We should choose our leaders in this country through open, regular and honest elections". As for previous questions, agreement was signalled on a scale 1-5, we aggregate answers across the two questions, and normalize into an index between 0 and 1. As the Figure shows, students are overall very democratically inclined, the mean response is to answer "agree" to the two questions.
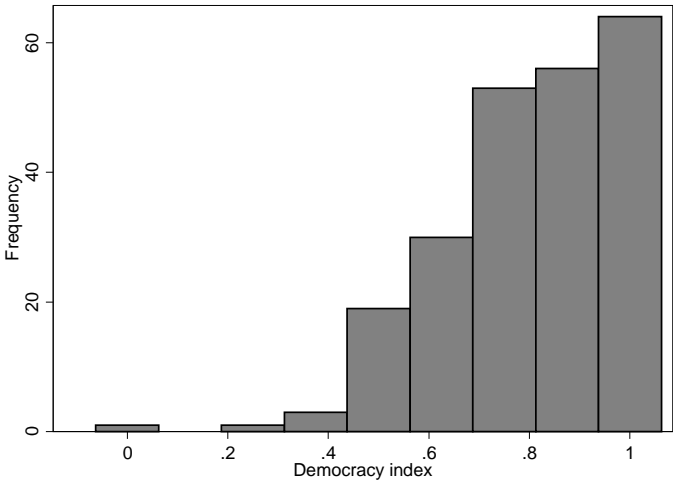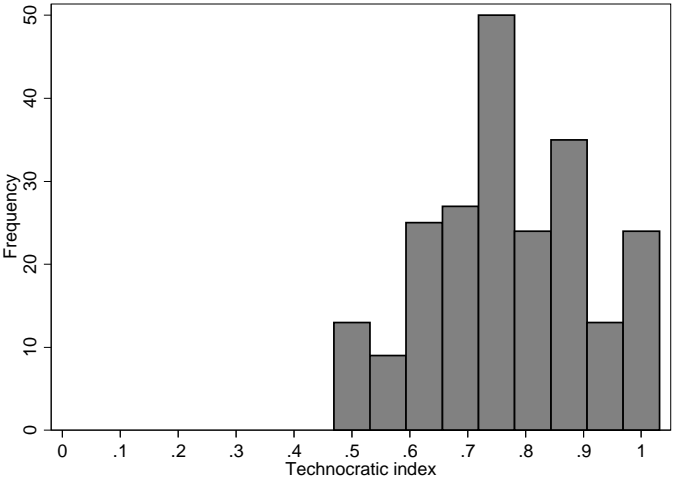

Figure E 1. Histogram of scores on democracy index

Note: The Democracy index is created from responses to the following two questions: i) "Democracy is preferable to any other kind of government", and ii) "We should choose our leaders in this country through open, regular and honest elections". Disagreement or agreement with these statements were elicited on a five point scale (1 – Disagree very strongly, 2 – Disagree, 3 – Neither agree nor disagree, 4 – Agree, 5 – Agree very strongly). The index averages responses across questions and rescales them from 0 to 1, with higher values reflecting greater agreement with the statements.

In Figure E2, we present evidence that our subjects are technocratically inclined. A technocratic index has been constructed from four underlying questions: "Economic experts should have a greater say than popular opinion in shaping economic policy", "Economic theory

is a better guide for economic policy than popular opinion", "Tanzanian voters are knowledgeable about economic issues in general", and "Tanzanian voters are knowledgeable about tax issues". Agreement was once again voiced on 1-5 scale. To create the technocratic index we invert the last two questions, add the responses to all four questions, and normalize between 0 and 1. Figure E2 shows that our economist subjects are fairly technocratic, the mean response to the questions is "Agree" (with the last two questions inverted), which is also reflected in answers to individual questions as presented in Figure E3.

Figure E 2. Histogram of scores on technocracy index

Note: The Technocratic index is created from responses to the following four questions: i) "Economic experts should have a greater say than popular opinion in shaping economic policy", ii) "Economic theory is a better guide for economic policy than popular opinion", iii) "Tanzanian voters are knowledgeable about economic issues in general", and iv) "Tanzanian voters are knowledgeable about tax issues". Disagreement or agreement with these statements were elicited on a five point scale (1 – Disagree very strongly, 2 – Disagree, 3 – Neither agree nor disagree, 4 – Agree, 5 – Agree very strongly). The index averages responses across questions (with responses to the latter two questions inverted) and rescales them from 0 to 1, with higher values reflecting greater technocratic sentiments.
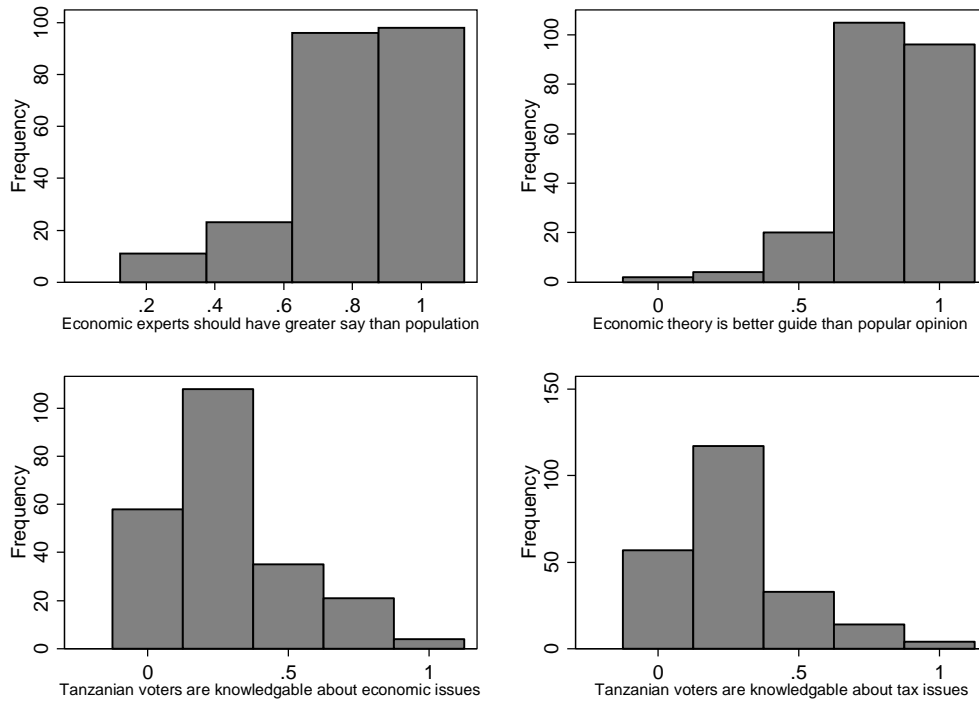
Figure E 3. Histograms of underlying answers to technocracy questions

Note: Histograms capturing distribution of agreement with statements specified on the x-axes. Disagreement or agreement were elicited on a five point scale (1 – Disagree very strongly, 2 – Disagree, 3 – Neither agree nor disagree, 4 – Agree, 5 – Agree very strongly), and rescaled from 0 to 1 in the figure, with higher values reflecting greater agreement with the statements.

An alternative interpretation of our treatment effect is that democratic views are tempered not by technocratic views, but by elitist attitudes, i.e. of sentiments of superiority over or scorn towards lower classes of ordinary citizens. Figure E4 presents an index of elitism, constructed from answers to three underlying questions; "Income differences in society are acceptable since they just reflect survival of the fittest", "Providing aid to the poor does not work since they will just consume the assistance and stay poor", and "It is acceptable for someone in my family to marry someone from a lower social class". Agreement as before is voiced from 1-5, we invert responses to the last question, add them together, and normalize into an index between zero and one. As seen in Figure E4, our respondents do not profess particularly elitist attitudes, the mean response is somewhere below the middle of the index, and this is confirmed also in responses to the individual questions presented in Figure E5.
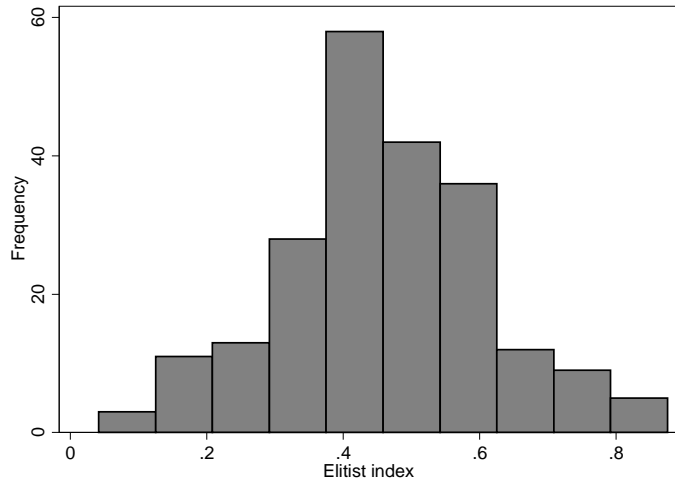
Figure E 4. Histogram of scores on elitism index

Note: The Elitist index is created from responses to the following three questions: i) "Income differences in society are acceptable since they just reflect survival of the fittest", ii) "Providing aid to the poor does not work since they will just consume the assistance and stay poor", and iii) "It is acceptable for someone in my family to marry someone from a lower social class". Disagreement or agreement with these statements were elicited on a five point scale (1 – Disagree very strongly, 2 – Disagree, 3 – Neither agree nor disagree, 4 – Agree, 5 – Agree very strongly). The index averages responses across questions (with responses to the final question inverted) and rescales them from 0 to 1, with higher values reflecting greater elitism.
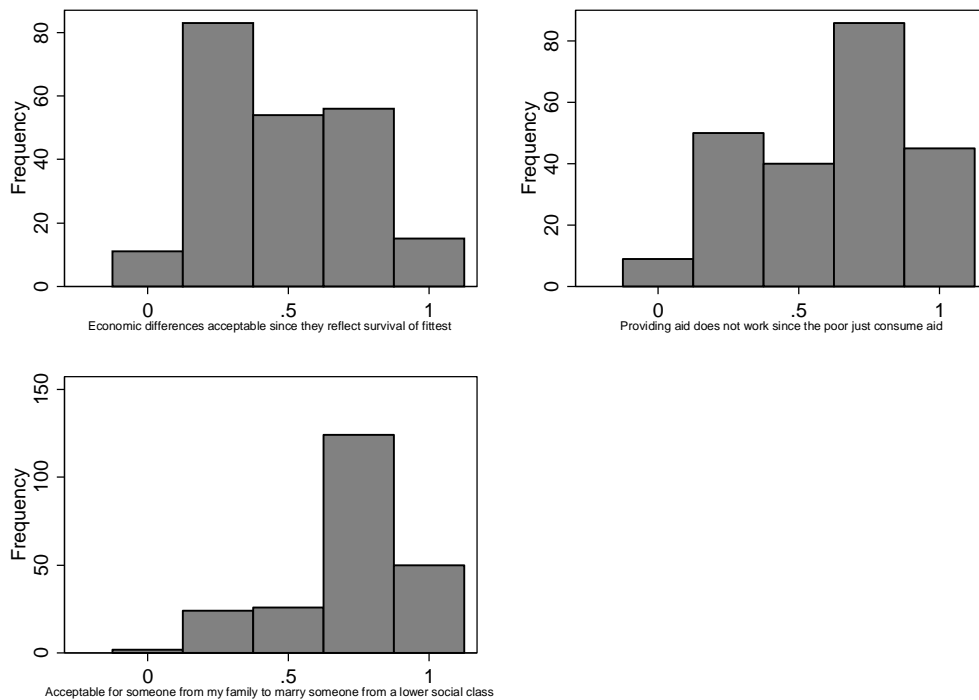

Figure E 5. Histograms of underlying variables of elitism index

Note: Histograms capturing distribution of agreement with statements specified on the x-axes. Disagreement or agreement were elicited on a five point scale (1 – Disagree very strongly, 2 – Disagree, 3 – Neither agree nor disagree, 4 – Agree, 5 – Agree very strongly), and rescaled from 0 to 1 in the figure, with higher values reflecting greater agreement with the statements.

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It is also possible that our treatment moves responses towards citizen preferences not out of respect for their opinions, but through a greater emphasis on redistributive issues triggered by our treatment. In other words, we could be observing an effect of inequality becoming more salient as a result of the treatment, rather than deference to public opinion. To test for this, Table E1 presents results from regressions based on the redistribution variables presented in Text box D1. The dependent variable is the extent to which a respondent chooses to redistribute between the two individuals, taking the value 0 in the case of no distribution (alternative A), 0.5 in the case of some redistribution (alternative B), and 1 in the case of full equalization of payments (alternative C). We regress this variable on the treatment indicator, for the 75 per cent efficiency loss case in column one of Table E1, and for the 90 per cent efficiency loss case in column two. The treatment has no significant related to redistributive preferences in either case. In the third column, we regress levels of agreement with an additional question of whether economics is primarily concerned with distributional issues, and find no effect of our treatment on responses to this question. In sum, these results suggest that increased salience of distributional issues is not driving the treatment effect.

Table E 1. Regressions of redistribution variables on treatment

	(1)	(2)	(3)
Sample	Full	Full	Full
Dependent variable	Redistribution (75% efficiency loss)	Redistribution (90% efficiency loss)	"In economics, we are primarily concerned with distributional issues"
Treatment dummy	-0.049	-0.088	0.010
	(0.06)	(0.06)	(0.03)
Constant	0.626***	0.660***	0.686***
	(0.04)	(0.04)	(0.02)
R2	0.003	0.011	0.001
N	230	230	224

Note: Ordinary least squares estimations, robust standard errors in parentheses, *** indicates significance at the 1% level, ** at 5%, * at 10%. Dependent variables in columns 1 and 2 based on redistribution questions in Text box D1, taking the value 0 in the case of no distribution (alternative A), 0.5 in the case of some redistribution (alternative B), and 1 in the case of full equalization of payments (alternative C). Dependent variable in column 3 level of agreement with statement of whether economics is primarily concerned with distributional issues, five point scale (1 – Disagree very strongly, 2 – Disagree, 3 – Neither agree nor disagree, 4 – Agree, 5 – Agree very strongly) rescaled from 0 to 1.

Our results could reflect greater apparent clarity of experimenter demand effects in the control group (where it is clear in which direction the given argument points) than in the treatment group (where two different arguments point in opposite directions). To test this, we added questions at the very end of the survey designed to address possible experimenter demand effects. We generated an experimenter demand index from three questions; "I have a clear understanding of what this survey is about", "I think I know what the researchers behind this survey expect to find", and "I get the feeling that the researchers behind this survey expect me to answer in a certain way". Agreement with these statements was given on a scale 1-5, we added up the responses across the three questions, and normalized into an experimenter demand index running from zero to one. Column one in Table E2 presents results from a regression of this index on the treatment indicator, and shows that the treatment group did not have a significantly different view of the purpose of the study than those of the control group. In column two, we also regress an indicator value for whether respondents answered "Don't know" to any of the three questions (coded as missing in the regression in column one), and we find that the treatment group actually had a significantly lower proportion of respondents providing "Don't know" answers to these questions than the control group. These results hence do not support the idea that our treatment effect reflect differential transparency of experimenter demand effects across treatments. Moreover, our survey ended with the open question "In your opinion, what is this study about?". In the responses our students typed in, no one was even close to guessing that our study was about testing the relative influence of technocratic and democratic arguments on economists' views of optimal public policy.

Table E 2. Regressions of experimenter demand variables on treatment

	(1)	(2)
Sample	*Full*	*Full*
Dependent variable	*Experimenter demand index*	*Experimenter demand missing values*
Treatment dummy	-0.005	-0.088**
	(0.02)	(0.04)
Constant	0.666***	0.151***
	(0.02)	(0.03)
R2	0.000	0.020
N	205	230

Note: Ordinary least squares estimations, robust standard errors in parentheses, *** indicates significance at the 1% level, ** at 5%, * at 10%. The experimenter demand index is created from responses to the following three questions: i) "I have a clear understanding of what this survey is about", ii) "I think I know what the researchers behind this survey expect to find", and iii) "I get the feeling that the researchers behind this survey expect me to answer in a certain way". Disagreement or agreement with these statements were elicited on a five point scale (1 – Disagree very strongly, 2 – Disagree, 3 – Neither agree nor disagree, 4 – Agree, 5 – Agree very strongly). The index averages responses across questions and rescales them from 0 to 1, with higher values reflecting greater agreement with the statements. The experimenter demand missing values variable is an indicator value for whether respondents answered "Don't know" to any of the three questions (coded as missing in the regression in column one).

Table E 3. Heterogeneous effects, Identity Index.

Panel A	(1)	Panel B	(2)
Discrete choice experiment	*First*	*Discrete choice experiment*	*Second*
Dependent variable	*Company choice*	*Dependent variable*	*Tax choice*
Mobile	0.695	Efficiency loss high	0.561
	(0.17)		(0.24)
Interaction Mobile*Treatment	1.841*	Interaction Efficiency loss high*Treatment	1.488
	(0.65)		(0.92)
Interaction Mobile*Identity Index	1.297	Interaction Efficiency loss high*Identity Index	1.397
	(0.43)		(0.80)
Interaction Mobile*Treatment*Identity Index	0.622	Interaction Efficiency loss high*Treatment*Identity Index	0.855
	(0.29)		(0.71)
Covariates	All attributes	Covariates	All attributes
Pseudo R2	0.054	Pseudo R2	0.071
N	4520	N	1380

Note: Odds ratios from conditional logit estimation, robust standard errors in parentheses, *** indicates significance at the 1% level, ** at 5%, * at 10%. All attribute variables included, some suppressed in output. In the interaction terms for Mobile, the Identity Index is as specified in the note to Figure D1, with its main effect subsumed in the fixed effects.
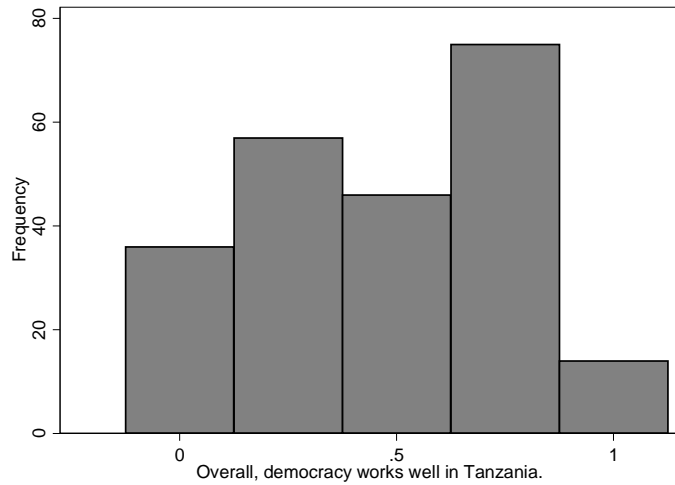
Figure E 6. Histogram of views of how well democracy works in Tanzania

Note: Histogram capturing distribution of agreement with statement specified on x-axis. Disagreement or agreement were elicited on a five point scale (1 – Disagree very strongly, 2 – Disagree, 3 – Neither agree nor disagree, 4 – Agree, 5 – Agree very strongly), and rescaled from 0 to 1 in the figure, with higher values reflecting greater agreement with the statement.