How do voters respond to information on self-serving elite behaviour?

Evidence from a randomized survey experiment in Tanzania

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ABSTRACT

Does self-serving elite behaviour make citizens more politically active? This paper presents the results of a randomized field experiment where voters in Tanzania were given information about elite use of tax havens. Information provided in a neutral form had no effect on voting intentions. Information phrased in more morally charged terms led to a reduction in voting intentions. Additional evidence suggests that rather than increase the perceived importance of voting, charged information tends to undermine confidence in political institutions and the social contract. The effects are particularly pronounced among the less well off, indicating that increased transparency in the absence of perceived agency may not improve democratic accountability.

*Without participation,
there's no democracy*

INTRODUCTION

Without participation, there's no democracy. While there is disagreement on the sufficient conditions for a political system to be called democratic, citizen participation is a necessary condition even in the most minimal definitions of democracy (Schumpeter, 1950), or in views of democracy which emphasize its deliberative side (Habermas, 1996). If we focus on elections, a key argument is that they are vital for a country to be run according to the will of the people rather than in the interest of narrow economic and political elites. For elections to have this function, however, this requires that citizens respond to attempts by elites to capture more of the social surplus. It seems important, then, that citizens are informed of elite activities of this kind. A range of individuals and organizations also work on the implicit assumption that providing information to citizens of self-serving elite behaviour is important to make them participate in the political process. These include investigative journalists, activists, non-government organizations, or even anti-elite or anti-establishment parties or candidates. An added assumption often appears to be that to propel citizens into political action, messages of self-serving elite behaviour should be given a form that is evocative, or sufficiently charged to stoke their moral indignation. The messages of the Occupy Wall Street and similar movements come to mind. But are messages of self-serving elite behaviour always an effective way to promote participation among citizens? In political systems where there is a substantial degree of elite capture, there is a danger that such information highlights the dysfunctions of the system and the relative powerlessness of citizens. It is hence not obvious that providing information on elite behaviour is effective in increasing citizen participation.

This paper reports results from a randomized survey experiment which tests the effect on political participation of providing information about self-serving elite behaviour. The experiment was conducted among 600 eligible voters in Dar es Salaam, the major city of Tanzania, a country with an imperfect democratic system, where multiparty elections have been held regularly since 1995, but where the incumbent party has never lost an election. The respondents were randomly assigned to two treatment groups and a control group. The first treatment group was shown a 90-second video with information about the Tanzanian elite's use of tax havens, given in a neutral language and form. The second treatment group was shown a video containing the same information, but using more charged language, where the unfairness of the elite's use of tax havens was emphasized.[1] The control group watched no video. After the video, respondents were asked questions about political participation, views on democratic processes and institutions and other political issues, and a range of socio-economic background variables.

Randomization means that we can attribute any differences in subsequent responses between groups to the video treatments. Our results show that the neutral information treatment had no effect on political participation. And strikingly, the charged information treatment had a significantly negative effect on participation, reducing voting intentions by about 9 percentage points compared to the control group. In the context of an imperfect democracy in which our experiment was conducted, information about elite misbehaviour seems at best ineffective in promoting citizen political participation, and at worst counter-productive if provided in a morally accentuated form. Further analysis of mechanisms suggests that the charged information treatment tends to make respondents take less favourable views of the prevailing social contract and of how much confidence one can have in political institutions. While explorative, our analyses of heterogeneous effects provide further detail and nuances to our findings. The negative effects of the charged treatment on voting predominantly reflect effects among those with little wealth, suggesting that participation is particularly adversely affected for groups with low perceived agency. For voters with less previous voting experience, the results indicate that neutral information on elite behaviour may have a positive effect on voting intentions, which is encouraging but also suggests that the effect of information may wear off after contact with a democratic system whose flaws become apparent with experience.

Our analysis addresses a substantial empirical literature on the effects of information on political preferences and behaviour. The paper most closely related to ours is Chong et al. (2015), who conduct an experiment providing information about corruption at the mayoral

level to Mexican voters, finding it to reduce voter turnout. Our results confirm that a negative effect of self-serving elite behaviour is also present at a higher political level, and additionally underscores that the form the information is given in matters, that confidence in political institutions and the social contract are important mechanisms through which information works, and that the effect may depend on the agency and democratic experience of eligible voters. Our analysis is also related to and motivated by the experimental literature on effects of negative campaigning, primarily conducted in the United States, which presents mixed results on voter turnout (Ansolabehere et al., 1994; Lau et al., 2007; Fridkin and Kenney, 2011; Barton et al., 2016). In the context of a less democratic system, our results suggest that demobilizing effects of charged information predominate, and we find similar negative effects as these studies on confidence in political institutions. More generally, we add to the experimental and quasi-experimental literature which seeks to identify causal effects of information on voting behaviour, including information on the voting process and voting encouragement (Aker et al, 2013), information about candidate performance (Ferraz and Finan, 2008; Banerjee et al., 2011, Humphreys and Weinstein, 2012), and information about economic topics such as inequality (Kuziemko et al. 2015).

More broadly, our results have implications for the literature that sees transparency as important in keeping public officials accountable, promoting favourable economic and social outcomes. The accountability effect of information through a free press is emphasized in the theoretical work of Besley and Prat (2006), and supported empirically by studies across as well as within countries (Brunetti and Weder, 2003; Besley and Burgess, 2002; Reinikka and Svensson, 2011). Our results suggest that citizens view information on elite capture as both a signal of its extent but also of the ineffectiveness of the political system in addressing capture, and that certain forms of media treatment of elite capture may well make the effect of the latter signal predominate. In less well functioning democracies, greater accountability through information may hence be difficult to achieve without more fundamental reform of the political system. Through these observations, we add to a literature on limitations to and unintended effects of transparency (Holmstrøm, 1999; Bac, 2001; Stasavage, 2004; Prat, 2005). By indicating that the less well-off may be more difficult to mobilize politically, our results are also consistent with arguments in the modernization literature that democratic participation increases with income (Lipset, 1959).

Through the specific design of our interventions, we contribute to an emerging literature on tax havens. International policies and initiatives to reduce detrimental effects of low-tax, nontransparent financial centres have to a large extent focused on the exchange of economic information between countries. The reach and effectiveness of existing approaches in this area have been questioned (Johannessen and Zucman, 2014), with some observers expressing optimism that recent leaks of confidential records from Panama, Switzerland and Luxembourg generate the public pressure necessary for a more comprehensive and effective global regime to be created (Seabrooke and Wigan, 2016). Fundamentally, however, the incentives of governments to support, implement, and make use of information exchange facilities depends on the pressures they face to reduce elite tax avoidance and evasion. Our results suggest that such incentives may be weak and difficult to create, at least in the form of electoral accountability and within the context of an imperfect democracy. This indicates that developing countries may at the same time be the countries that suffer the most from the existence of tax havens (Johannesen et al., 2016), and the countries least likely to address the challenges created by them.

The paper is structured as follows. Section 2 presents a brief conceptual framework underlying our analytical approach. The experimental design and empirical approach are detailed in Section 3. Section 4 presents descriptive statistics. Our main results are presented in Section 5, with evidence on mechanisms and heterogeneity across groups presented in Section 6. Section 7 concludes.

CONCEPTUAL FRAMEWORK

In the rational choice model of voting due to Downs (1957), individual voters compare their expected benefit of voting with the costs of voting. In the formal treatment of Riker and Ordeshook (1968), voters get a net utility B if their preferred candidate wins, have a probability p of being pivotal in deciding the election outcome, face voting costs C, and get a consumption value D of voting stemming from the fulfilment of a civic duty to vote. An individual thus votes if $pB+D>C$. The effect of giving voters new information about self-serving elite behaviour in this setup is not obvious. It can increase the importance of getting the right candidate elected and/or strengthen the perceived civic duty to vote, raising B or D, respectively. On the other hand, the information may highlight the deficiencies of a political system, suggesting that the elite comes out on top whoever wins the election, or undermining the civic duty to vote or the perceived probability of being pivotal in a flawed electoral system, hence reducing B, D or p. The latter negative effects are likely more important in the context of an imperfect democracy such as that of Tanzania than in more well-functioning democracies, but *a priori* the balance of positive and negative effects on turnout is nevertheless not obvious.

The above decision-theoretic framework has been criticized for getting around predictions of low turnout in an ad hoc manner by including a civic duty to vote (Feddersen, 2004). Game-theoretic, group-based voting theories have been developed to give more structure and content to the consumption value of voting concept represented by the D term. Ethical voting theories assume that voters identify with and pursue the goals of distinct groups, for instance acting like group rule utilitarians as in Fedderson and Sandroni (2006). Leader mobilization theories similarly assume that voters identify with distinct groups, but emphasize the role of group leaders in applying social pressure or providing material incentives to get their group members to vote (Uhlaner, 1989; Morton, 1991; Shachar and Nalebuff, 1999). In our context, this can be related to the common observation that African politics is highly clientilistic, with promises of material reward and voting often following ethnic lines (Chabal and Daloz, 1999; Van de Walle and Butler, 1999). In practice, however, the resources transferred through clientilism are mainly seen to benefit narrow elites, with very small material gains accruing to citizens (Van de Walle, 2003). In a clientilist system, it is possible that our information treatments highlight its uneven benefits, undermining the credibility of leader promises of material gains to group members. More generally, group membership includes an element of a public good contribution which may be undermined if benefits of membership are seen as highly unequally distributed. On the other hand, and similarly to the individual voting model, the information may also serve to highlight the importance of the one's own group winning the election, and hence increase turnout. In other words, the effect of information on self-serving elite behaviour is ambiguous also in these models.

The possibility that different forms of information can have different impacts has been extensively analyzed in the literatures assessing the effects of negative and positive campaigns or advertising on political behaviour (Ansolabehere et al., 1994; Lau et al., 2007; Fridkin and Kenney, 2011; Barton et al., 2016), on consumer decisions (Levin, 1987), on health related behaviour (Meyerowitz and Chaiken, 1987), and on environmental choices (Spence and Pidgeon, 2010). While our experiment compares neutral and charged information treatments, rather than positive and negative ones, similar theoretical arguments can be applied. Like negative advertising, our charged information may be more informative than the neutral information, or stand out more against a backdrop of positive information and experience, making our charged treatment more memorable and likelier to be mentally processed by the voters. In line with prospect theory (Kahneman and Tversky, 1979), our charged information treatment may make the loss from self-serving elite behaviour more salient and hence provoke a greater behavioural response. Moreover, our charged treatment may be met with stronger emotional responses by our subjects which may feed through to behaviour. While the direction of the effect of our treatments on turnout is ambiguous since they may contain information both on the importance of getting the right candidate elected but also on the extent to which the political system is dysfunctional, these theoretical

arguments suggest that the effect of the charged treatment is likely to be more extreme in either direction.

Meta-studies of the literature on negative campaigning suggest that it has a negative effect on beliefs in political efficacy, trust in government, and general perceptions of the political system (Lau et al., 2007; Fridkin and Kenney, 2011). These negative intermediate effects are not, however, found to result in a reduction in voter turnout. We test the effect of our information treatments on beliefs in democracy, faith in the social contract, and confidence in political institutions, and expect these to be negative. In the context of the less well-functioning democracy in which our experiment was conducted, these negative effects may also be relatively more influential on turnout than what is found in the negative campaigning literature that largely focuses on well-developed democracies like the United States. Our analysis also explores the possibility that charged information may not only reduce perceived political efficacy, i.e. the extent to which your vote matters, but that individuals whose perceived agency and self-efficacy is low may be more susceptible to negative effects of information.

We build and expand on a substantial empirical literature on the determinants of voter turnout, see Blais (2006) and Cancela and Geys (2016) for reviews. Effects of self-serving elite behaviour has been given little attention in this literature. The experimental study of Chong et al. (2015) finds that corruption information reduces turnout in local elections, we extend this to higher level elite behaviour. The effect of corruption on turnout has also been examined in studies using observational data, with mixed results (Kostadinova, 2009; Stockemer et al., 2013). We show that our treatments are balanced across important determinants of turnout identified by the existing literature.

RESEARCH DESIGN

Context and timing

The survey experiment was preceded by a pilot, conducted in Dar es Salaam in February 2015, which included a total of 150 eligible voters from 10 randomly chosen polling districts. The pilot was used to test the sampling strategy, described in further detail below. Importantly, it was also used to assess the level of knowledge among voters of issues related to elite behaviour, and in particular their knowledge of the tax haven concept and elite use of such havens. While about a quarter of the respondents in the pilot claimed to know the term tax havens, this is likely an overstatement of the true proportion. When we instead used the term "Swiss billions" to denote the phenomenon, a third of respondents stated that they knew the term. The term "Swiss billions" has been used extensively in the media and in public debates in Tanzania and refers to the implication of 99 Tanzanian nationals in the so-called Swiss leaks case. The Swiss leaks case was based on leaks from a former employee of the Swiss bank HSBC in 2008, the information was passed on to the French newspaper Le Monde in early 2014, and subsequently analyzed and published by The International Consortium of Investigative Journalists (ICIJ).[2] The leaked data identified bank accounts in HSBC of more than 100,000 citizens of 203 countries, totalling more than $100 billion, and a number of the account holders so far identified are from the elite of their respective countries, including royalty, politicians and top officials, wealthy industrialists and so on. The 99 Tanzanian nationals involved have not been named. However, their total holdings in the Swiss bank has been put to $114 million, which translates to about 200 billion Tanzanian Shillings, hence the term "Swiss billions". The case has been repeatedly discussed in Tanzanian newspapers since the disclosure was made by the ICIJ, up to the year of the survey, with later reports focusing on legal and political processes ongoing in Tanzania. While newspaper reports have been critical, they have often also kept a certain balance, emphasizing that keeping money in a Swiss bank account in not necessarily illegal. Since the term meets with some level of knowledge among Tanzanian voters, and is also concrete and easily relatable to citizens who are unaware of the case, we chose to use it as a central part of our information treatments in the survey experiment (detailed below), as opposed to using more abstract terminology.

Our survey experiment was conducted from 30 October to 13 November 2015. In order words, data collection started five days after the general election in Tanzania on 25 October, and one day after the official election results were announced on 29 October. While it would have been interesting to implement our information treatments before the election, and look at effects on actual voting in the general election, rather than hypothetical voting questions after the election, the questions of elite behaviour our treatments raise were deemed too sensitive for local research permits to be granted for an experiment that involved the actual election. In this respect, analyses of the effect of raising contentious issues on voting face some inherent limitations that analyses of less contentious issues such as voter education do not encounter to the same extent. Our analysis hence focuses on effects of the treatments on intentions to vote rather than actual voting.

The general election that preceded our survey experiment took place in the context of what can be described as an imperfect democracy.[3] While multi-party general elections were introduced in 1995, this and every subsequent election was won by the party Chama Cha Mapinduzi (CCM), which has ruled the country since independence. The country is hence not a consolidated democracy, in the sense that there has been a transition of power from an incumbent to an opposition party following any election. Being in control of the state, the ruling party has considerable measures and resources under its control which it can use to influence the outcome of elections, but CCM has also enjoyed a degree of popularity in some regions, particularly in rural areas. The 2015 general election was, however, more competitive than previous ones. The incumbent president, Jakaya Kikwete, stepped down after two terms (the term limit) and John Magufuli, the Works minister, became the presidential candidate of the ruling CCM. Four opposition parties, including the main opposition party Chadema, formed a coalition named Ukawa, and nominated Edward

Lowassa as their presidential candidate, a former CCM prime minister who switched sides to the opposition shortly before the election. The CCM candidate won with 58.5 per cent of the votes against the Ukawa candidate's 40 per cent. Turnout in the 2015 presidential election was 62.4 per cent of the voting age population, considerably higher than the 40.7 per cent of the preceding election, probably reflecting a higher level of competitiveness.[4]

To our knowledge, the question of tax havens and the case of the Swiss billions did not feature substantially in the candidate's political campaigning for the general elections. With both main candidate's having links to the political and economic elite, there would have been little political incentive to bring these issues up. Our experiment was hence conducted in the context of a recently completed election where these issues were not the subject of much discussion, and the information we provided to our treated respondents is new in the sense that it did not follow on similar kinds of information presented by the main candidates in the electoral campaigns. Our results hence do not seem contingent on some pre-existing introduction of these issues in the election campaign immediately preceding the experiment. On the contrary, the absence of these types of issues in the campaign, and the fact that both main candidates were from the ruling elite suggest that we are in a setting where voters may perceive the political system to be particularly susceptible to capture by the elite. Even in this context, however, it is not obvious how information on elite behaviour will affect voting intentions, stressing or increasing the perceived pervasiveness of capture could lead to mobilization or to dissociation of voters from the electoral process. It should be noted that since taking office on 5 November, President Magufuli has embarked on an apparent drive to reduce corruption in the public sector. While some symbolic actions were taken immediately following his inauguration, the major and much publicized activities were introduced after our data collection had completed, including the sacking of dozens of port officials and the arrest of the head of the Tanzania Revenue Authority. Our survey was hence conducted in a setting where public perceptions of elite capture and limitations of democratic elections to address this were on the negative side.

Sampling and survey design

A pre-analysis plan for the survey experiment was submitted to the AEA RCT registry on 9 November 2015.[5]

Sampling was done as specified, following the approach thoroughly tested in the pilot. From a list of all polling stations in Dar es Salaam in the 2010 election (polling station information from the 2015 election was not available to us when preparing the survey), we randomly selected 24 polling stations. In each of these locations, a team of eight enumerators walked pre-defined routes evenly spaced in eight different directions from the polling stations, selecting every third household along the way. In each household, a random person at or above the age of 18 and of the enumerator's gender was selected for an interview (there were four enumerators of each gender). A total of 25 interviews were conducted in this manner in the catchment area of each polling station, for a total of 600 interviews.

Interviews were conducted in Swahili, and data collected electronically on tablets using ODK (Open Data Kit) software. Maps for the enumerators to follow when sampling households were also stored on the tablets. Each interview took about 30 minutes to complete, and respondents were asked to respond to a questionnaire with six different sections, containing the following sets of questions:

1. Background questions (only age and gender)
2. Treatment video or control group
3. Political participation, voting
4. Other political participation
5. Views of democracy and politics
6. Background questions

To avoid having responses primed by early questions, we collected only age and gender in the first section of the interview, as these were part of the selection process of respondents (respondents had to be above voting age and of the enumerator's gender). We then moved

immediately to randomization of respondents into one of two video treatments (detailed below), or to the control group. This was done through eight lists prepared in advance of random choices between the three possibilities, one list for each enumerator. The enumerators moved down their list, crossing off the current video shown (or the control option) and moving on to the next one on the list in the next interview. This resulted in approximately 200 respondents in each of the three groups. Randomization was hence at the individual level, and not blocked by polling station. Balance tests (see Section 4) show that randomization was successful in terms of balance on the pre-specified co-variates, and also on distance from the respondents' dwelling to their respective polling stations. After the treatment/control stage, the enumerators proceeded directly to a section on voting and political participation, from which our dependent variable is taken. This was followed by questions on other political participation than voting, beliefs about how well democracy works, views on the social contract, and confidence in various political institutions then followed. Finally, a set of socio-economic background questions was collected in the last section.

Treatments

In the treatment section, respondents were randomly assigned to watch one of two videos, or to the control group, where no video was shown.[6] Both videos contain definitions and explanation of tax haven use, starting from the highly publicized case of the Swiss billions. Both videos also contain information on what the use of tax havens entails in terms of reduced tax revenue for Tanzania, and therefore less money available to spend on public services or infrastructure, specifically schools, hospitals and roads. The treatment videos differ, however, in the tone and language used. The first treatment video is comparatively neutral in tone and language, as neutral as it can be when discussing not only definitions of tax haven, but also some of its implications for Tanzania. The second video is more morally charged, using words like "hiding" money abroad about tax haven use, of wealthy individuals "avoiding to pay the taxes we are all supposed to pay", and focusing on effects on the respondent and his or her family rather than general effects for Tanzania. The visual side of the videos is mostly the same, and different only in the addition of a shady looking wealthy tax evader in the second treatment video. Each video is about 90 seconds long, and was shown to the respondent on the tablet used for data collection, with headphones for the respondents.

Empirical strategy

Given successful randomization to treatment and control groups, differences in responses across the groups will reflect a causal effect of exposure to the videos, and not other underlying differences between the groups. In the absence of a placebo treatment for the control group, we cannot completely rule out the possibility that the estimated effects reflect the act of watching a video rather than the content of the videos. However, it seems unlikely that our results reflect the act of watching a video, and the fact that we find different results for our two video treatments suggests that content matters. We note that previous experimental studies using video treatments differ in terms of their placebo choice; Ravallion et al. (2015) use no placebo in their experiment testing effects of showing a movie on rights wo villagers in India, while Bernard et al. (2014) use a TV entertainment programme as a placebo for a treatment featuring a documentary on entrepreneurial role models. In our case, it seems difficult to conceive of a placebo video sufficiently neutral as to have no possible effect on participation, while at the same time not being boring to watch, so we decided to not use a placebo.

   The variables used in our analysis are presented in Table 1. The dependent variable is intention to vote.[7] This is a dummy variable based on responses to the question "If there was a new general election tomorrow, would you vote?". We do, however, make one important adjustment to this variable.[8] Due to social desirability bias, more people will likely say they would vote than actually vote if there was a new election tomorrow. We therefore recode from "Yes" to "No" the responses of subjects who claim to have voted in the 2015 general election, but fail to answer correctly two control questions on the appearance of the ballot boxes used in the election, and of the ballot sheets. In other words, we assume that if

people misrepresent their actual voting behaviour in the election that just was, they will also misrepresent their intention to vote in a new election tomorrow. The importance of verifying claims by eligible voters of their voting behaviour was highlighted by our pilot. In the pilot data, 72 per cent of respondents reported having voted in the 2010 general election, a considerable over-statement since actual turnout rates in that election were about 40 per cent of the voting age population. This form of misrepresentation is a well-known problem in this kind of survey data; 80 per cent of respondents to the 2012 Afrobarometer survey in Tanzania similarly claim to have voted in the 2010 election. As the data section below suggests, a correction using the two control questions works well in bringing claims of voting in the 2015 election close to actual turnout rates. The adjustment is also important to correctly estimate the association between intention to vote and covariates such as gender (men have a significantly greater tendency to misrepresent their voting behaviour in our sample) and voting in the preceding election (without the adjustment, the estimated correlation between voting in the 2015 election and intention to vote in a new election is negative).

We test for balance between our treatment and control groups on a number of co-variates specified in Table 1. These include age and gender of respondents, whether they were born in Dar es Salaam, education, household headship status, wealth, religion, occupation, and whether they voted in the 2015 general election. Our covariates also include polling station fixed effects. Our wealth indicator is an asset index constructed through factor analysis of questions of whether the respondent's household owns a TV, a radio, a motor vehicle, and the number of rooms the household occupies. We also asked directly about income in the survey, but see the replies as less reliable than those on assets, and in addition the non-response rate on the income question was high (19 per cent declined to answer). For education, we use three dummies for completion of primary, secondary and tertiary education, with no completed education the excluded category. Religion is captured by two dummies for Christianity and Islam, with other religions the excluded category. Occupation is represented by three broad indicator variables, capturing whether

TABLE 1
Main variables

Variable	Explanation
Dependent variable	
Voting intention	Dummy variable based on response to question "If there was a new general election tomorrow, would you vote?" (1 - Yes, 0 - No), adjusting from 1 to 0 those who state having voted in 2015 election but fail to correctly answer two control questions on appearance of ballot box and ballot sheet.
Treatment variables	
Treated	Dummy variable of whether respondent watched either the neutral or the charged video (1 – Yes, 0 – No)
Treated neutral	Dummy variable of whether respondent watched the neutral video (1 – Yes, 0 – No)
Treated charged	Dummy variable of whether respondent watched the charged video (1 – Yes, 0 – No)
Control variables	
Age	Age of respondent (number of years)
Male	Gender of respondent (dummy variable, 1 – male, 0 – female)
Born in Dar es Salaam	District of birth of respondent (dummy variable, 1 – Dar es Salaam, 0 – other)
Education, primary	Respondent has completed primary school (dummy variable, 1 – Yes, 0 – No)
Education, secondary	Respondent has completed secondary school (dummy variable, 1 – Yes, 0 – No)
Education, tertiary	Respondent has completed tertiary school (dummy variable, 1 – Yes, 0 – No)
Head of household	Respondent is head of household (dummy variable, 1 – Yes, 0 – No)
Asset index	Household asset index based on factor analysis of the following asset variables: ownership of TV, radio, motor vehicle, number of rooms the household occupies
Religion, Christian	Respondent is a Christian (dummy variable, 1 – Yes, 0 – No)
Religion, Muslim	Respondent is a Muslim (dummy variable, 1 – Yes, 0 – No)
Occupation, self-employed	Respondent is self-employed (dummy variable, 1 – Yes, 0 – No)
Occupation, private sector employee	Respondent is employed in the private sector (dummy variable, 1 – Yes, 0 – No)
Occupation, government employee	Respondent is government employee (dummy variable, 1 – Yes, 0 – No)
Voted in 2015 election	Dummy variable based on response to question "Did you vote at the general election this year?" (1 - Yes, 0 - No), adjusting from 1 to 0 those who state having voted but fail to correctly answer two control questions on appearance of ballot box and ballot sheet.
Distance to polling station	Geodesic distance (in kilometers) from the dwelling of the respondent to their polling station.

the respondent is self-employed, employed in the private sector, or employed in the public sector, with other or no employment the excluded category. The indicator variable of whether the respondent voted in 2015 is adjusted in the same manner as our dependent variable, changing responses from "Yes" to "No" where a respondent could not correctly answer the two control questions on appearance of the ballot boxes and ballot sheets used in the election.[9] While not specified as a covariate in the pre-analysis plan, we have also used the GPS coordinates of the respondents dwellings and their respective polling stations to calculate their physical distance from the polling station, in order to show that the treatment and control groups are balanced on this variable.

We start by comparing the outcome in the control group with the two treatment groups collapsed into one. This is done through ordinary least squares estimation of the following equation:

$$y_{i,s} = \alpha + \beta_T T_{i,s} + \beta_X X_{i,s} + \gamma_s + \varepsilon_{i,s} \tag{1}$$

Here $y_{i,s}$ is the outcome variable voting intention for individual i in the catchment area of polling station s, $T_{i,s}$ is an indicator variable taking the value one if individual i is in one of the two treatment groups, and zero otherwise, $X_{i,s}$ is a vector of control variables and γ_s polling station fixed effects. We report results both with and without covariates (including polling station fixed effects). We estimate all equations using robust standard errors, and do not cluster errors since randomization into treatments and control is done at the individual level.

The main part of our analysis centres on the comparison of each of the two treatments with the control. To this end, we use the following specification:

$$y_{i,s} = \alpha + \beta_{T1} T1_{i,s} + \beta_{T2} T2_{i,s} + \beta_X X_{i,s} + \gamma_s + \varepsilon_{i,s} \tag{2}$$

Here we regress the outcome variable on two separate treatment variables, $T1_{i,s}$ is an indicator of whether the respondent was exposed to the neutral treatment, and $T2_{i,s}$ an indicator of exposure to the charged treatment. We also add a test of whether the effects of the two treatments are different using two-sided t-tests.[10]

Following our main analysis, we analyze mechanisms behind the results and heterogeneous effects across our covariates. We did not specify these further analyses in the pre-analyses plan, as there were so many possibilities depending on what we found as the main effects, and a too tight pre-specification of this analysis may result in the loss of important insights. This trade-off between the credibility that pre-specification generates and the potential costs in terms of developing highly complex pre-specification with limits on potential learning from the data has also been noted in recent assessments of the upsides and downsides of pre-analysis plans (Olken, 2015). Our analysis of mechanisms and heterogeneity in section 6 can therefore be considered explorative.

DATA

Summary statistics for our sample are presented in Table 2. Our adjusted voting variables show that 62.5 per cent of voters intended to vote if there was a new election tomorrow, and that 64.3 per cent reported voting in the 2015 general election. These adjusted figures are very close to actual turnout rates of 62.4 per cent of the voting age population in this election, and much more realistic than the unadjusted intentions to vote of 71.3 per cent, and unadjusted claims to have voted in the 2015 elections of 77.5 per cent. It appears our approach of using control questions on ballot box and ballot sheet appearance have worked quite well, at least in terms of aggregate numbers.

In terms of socio-demographic variables, the mean voter in our sample is 35 years old, half are male, just under a third were born in Dar es Salaam, the median voter has completed primary education, half are household heads, there are a few more Muslims than Christians but few of any other belief, and most are self-employed and in practice working in the informal sector. The asset index is not directly informative of the general level of wealth, but on the underlying variables 83 per cent own a radio, 72 per cent a TV, 21 per cent a motor vehicle, and the household of the median respondent occupies 3 rooms. The distance to the polling station variable shows that our respondents range from living right next to the polling station to living 4.7 kilometres away, with the mean distance being a quarter of a kilometre.

TABLE 2
Summary statistics, main
variables, full sample

	Obs	Mean	Std. Dev.	Min	Max
Voting intention	600	0.625	0.485	0	1
Age	599	35.100	12.985	18	78
Male	600	0.500	0.500	0	1
Born in Dar es Salaam	599	0.306	0.461	0	1
Education, primary	600	0.547	0.498	0	1
Education, secondary	600	0.303	0.460	0	1
Education, tertiary	600	0.062	0.241	0	1
Head of household	600	0.498	0.500	0	1
Asset index	597	0.000	1.000	-2.065	2.287
Religion, Christian	600	0.423	0.494	0	1
Religion, Muslim	600	0.532	0.499	0	1
Occupation, self-employed	598	0.627	0.484	0	1
Occupation, private sector employee	598	0.124	0.330	0	1
Occupation, government employee	598	0.025	0.157	0	1
Voted in 2015 election	600	0.643	0.479	0	1
Distance to polling station	599	0.252	0.260	0.007	4.737

Note: Voting intention is a dummy variable of whether respondent would vote if there was an election tomorrow, adjusting from 1 to 0 respondents who claim to have voted in 2015 election but could not correctly answer control questions on ballot box and ballot sheet appearance. Male is a dummy variable of whether the respondent is male. Born in Dar es Salaam is a dummy variable of whether respondent was born in Dar es Salaam. The three education variables are dummies capturing whether respondent has completed primary, secondary and tertiary education, respectively. Head of household is a dummy variable of whether the respondent is head of the household. Asset index is a household asset index based on factor analysis of the following asset variables: ownership of TV, radio, motor vehicle, number of rooms the household occupies. The religion variables are dummy variables of whether the respondent is a Christian or a Muslim, respectively. The occupation variables are dummy variables for whether the respondent is self-employed, works as a private sector employee or as a government employee, respectively. Voted in 2015 election is a dummy variable of whether respondent states to have voted in the 25 October 2015 general election, adjusting from 1 to 0 respondents who claim to have voted in 2015 election but could not correctly answer control questions on ballot box and ballot sheet appearance. Distance to polling station is the geodesic distance (in kilometers) from the dwelling of the respondent to their polling station.

Table 3 provides evidence that the randomization was successful in the sense that there is balance on co-variates. The three first columns provides means for the each balancing variable for the neutral treatment, charged treatment, and the control group, respectively. The subsequent three columns report the p-value from a t-test of the difference of means on each balancing variable between the two treatment groups and the control group. There are only two significant differences in that the neutral treatment group contains a lower proportion of people born in Dar es Salaam than the control group, and the charged treatment group has a lower proportion of government employees than the control group. In total, there are no more differences than you would expect by chance. The final column of Table 3 contains the p-value of an F-test of the null hypothesis that the treatment arms do not predict the means on each balancing variable. The results are consistent in there being no significant differences across treatment and control groups. As the last row of the table shows, this includes the variable capturing the distance from the respondent's dwelling to the polling station.

TABLE 3
Balance treatments
and control

	Treated neutral	Treated charged	Control	p-value (neutral vs charged)	p-value (neutral vs Control)	p-value (charged vs Control)	Orthogonality test
Age	34.673	35.829	34.838	0.389	0.896	0.449	0.650
	(0.913)	(0.980)	(0.865)				
Male	0.457	0.515	0.530	0.240	0.139	0.769	0.291
	(0.035)	(0.036)	(0.036)				
Born in Dar es Salaam	0.266	0.289	0.364	0.609	0.034	0.114	0.092
	(0.031)	(0.033)	(0.034)				
Education, primary	0.563	0.567	0.510	0.928	0.291	0.260	0.452
	(0.034)	(0.036)	(0.036)				
Education, secondary	0.279	0.289	0.343	0.828	0.161	0.245	0.330
	(0.031)	(0.033)	(0.034)				
Education, tertiary	0.077	0.052	0.056	0.299	0.387	0.860	0.552
	(0.019)	(0.016)	(0.016)				
Head of household	0.510	0.505	0.480	0.929	0.549	0.617	0.814
	(0.035)	(0.036)	(0.036)				
Asset index	-0.013	-0.025	0.038	0.901	0.611	0.525	0.799
	(0.071)	(0.070)	(0.072)				
Religion, Christian	0.428	0.407	0.434	0.675	0.896	0.588	0.851
	(0.034)	(0.035)	(0.035)				
Religion, Muslim	0.529	0.557	0.510	0.576	0.706	0.356	0.650
	(0.035)	(0.036)	(0.036)				
Occupation, self-employed	0.623	0.639	0.619	0.741	0.936	0.685	0.911
	(0.034)	(0.035)	(0.035)				
Occupation, private sector employee	0.130	0.124	0.117	0.840	0.677	0.833	0.917
	(0.023)	(0.024)	(0.023)				
Occupation, government employee	0.019	0.010	0.046	0.454	0.138	0.034	0.102
	(0.010)	(0.007)	(0.015)				
Voted in 2015 election	0.615	0.680	0.636	0.173	0.663	0.359	0.379
	(0.034)	(0.034)	(0.034)				
Distance to polling station	0.242	0.274	0.242	0.277	0.970	0.296	0.530
	0.011	0.026	0.015				
N	208	194	198				

Note: The first three columns shows means and robust standard errors in parentheses, for the two treatment groups and the control group, respectively. Columns four shows p-values from t-tests of differences of means of each balance variable between the neutral and charged treatment group, and columns five and six the same for comparisons between each of the treatment groups and the control group. The orthogonality test in the final column, gives the p-value of an F-test of whether the treatment arms predict the balance variable. Age is the age of respondent in years. Male is a dummy variable of whether the respondent is male. Born in Dar es Salaam is a dummy variable of whether respondent was born in Dar es Salaam. The three education variables are dummies capturing whether respondent has completed primary, secondary and tertiary education, respectively. Head of household is a dummy variable of whether the respondent is head of the household. Asset index is a household asset index based on factor analysis of the following asset variables: ownership of TV, radio, motor vehicle, number of rooms the household occupies. The religion variables are dummy variables of whether the respondent is a Christian or a Muslim, respectively. The occupation variables are dummy variables for whether the respondent is self-employed, works as a private sector employee or as a government employee, respectively. Voted in 2015 election is a dummy variable of whether respondent states to have voted in the 25 October 2015 general election, adjusting from 1 to 0 respondents who claim to have voted in 2015 election but could not correctly answer control questions on ballot box and ballot sheet appearance. Distance to polling station is the geodesic distance (in kilometers) from the dwelling of the respondent to their polling station.

MAIN RESULTS

Our main results are presented in Table 4. The first column shows results for our combined treatment variable, i.e. the effect on voting intention of having seen either of the two videos. While the point estimate is negative, it is not significant. Columns 3 presents results for the combined treatment variable with the co-variates added, including polling station fixed effects, and results are essentially the same. The more interesting results emerge when we distinguish between the two treatments, as is done in columns 2 (without covariates) and 4 (with covariates). The neutral treatment has no effect on voting intentions. However, the charged information treatment has a significantly negative effect on intentions to vote. According to our estimates, being exposed to charged information on self-serving elite behaviour reduces intentions to vote by between 8.5 and 9.3 percentage points. The p-value of a t-test that the two treatments have the same effect is included in the bottom row of column two, and confirms that the effects of the two treatments are different. In sum, our main results suggest that providing voters with neutral information on elite behaviour has no effect on participation, but providing them with charged information significantly and substantially reduces political participation.

Results for the co-variates suggest that there is a strong positive correlation between intention to vote and having voted in the 2015 election, as one would expect. Men also have significantly lower intentions to vote than women. While data on actual participation by gender is not available for the 2015 election in Tanzania, official data from the Tanzanian National Election Commission state that 53% of registered voters were women.[11] While Isaksson et al. (2014) suggest a small voting gap of 2–3 per cent in favour of men in Tanzania, their study uses reported voting by respondents to Afrobarometer surveys, which is known to result in over-reporting. And in our data, while men are more likely to report having voted, our control questions also reveal that they are less likely to actually have voted than women. In fact, of all the covariates in our specification, being male is the only factor that is significantly associated with misrepresenting your voting decision (results available on request). When in addition our sample is an urban one in which gender equality is greater than in rural areas, it is entirely plausible that women have higher intentions to vote than men. We also find a small (in economic terms) negative effect of age on voting. The other results indicate that conditional on having voted, gender and age, there is no association between voting intention and district of origin, education, headship status, income, religion, or occupation.

TABLE 4
Impact of exposure to information
on elite behaviour

Dependent variable	(1) Voting intention	(2) Voting intention	(3) Voting intention	(4) Voting intention
Treated	-0.024		-0.040	
	(0.042)		(0.040)	
Treated neutral		0.032		0.015
		(0.047)		(0.047)
Treated charged		-0.085*		-0.093**
		(0.049)		(0.047)
Age			-0.004**	-0.004**
			(0.002)	(0.002)
Male			-0.205***	-0.199***
			(0.041)	(0.041)
Born in Dar es Salaam			-0.047	-0.045
			(0.046)	(0.045)
Education, primary			-0.075	-0.074
			(0.071)	(0.071)
Education, secondary			-0.026	-0.024
			(0.079)	(0.078)
Education, tertiary			-0.108	-0.112
			(0.116)	(0.114)
Head of household			-0.020	-0.024
			(0.050)	(0.050)
Asset index			0.025	0.024
			(0.021)	(0.021)
Religion, Christian			-0.113	-0.110
			(0.087)	(0.085)
Religion, Muslim			-0.108	-0.103
			(0.086)	(0.084)
Occupation, self-employed			-0.033	-0.034
			(0.049)	(0.049)
Occupation, private sector employee			-0.069	-0.069
			(0.070)	(0.069)
Occupation, government employee			0.006	0.001
			(0.136)	(0.136)
Voted in 2015 election			0.192***	0.197***
			(0.041)	(0.041)
Constant	0.641***	0.641***	1.120***	1.100***
	(0.034)	(0.034)	(0.163)	(0.160)
Polling station fixed effects	No	No	Yes	Yes
r2	0.001	0.010	0.157	0.165
N	600	600	593	593
p-value (treated neutral=treated charged)		0.016		0.026

Note: Ordinary least squares regressions with robust standard errors in parentheses, *** indicates significance at the 1% level, ** at 5%, * at 10%. The p-value in the bottom row is from a t-test of whether the effect of the charged treatment is different from that of the neutral treatment. Voting intention is a dummy variable of whether respondent would vote if there was an election tomorrow, adjusting from 1 to 0 respondents who claim to have voted in 2015 election but could not correctly answer control questions on ballot box and ballot sheet appearance. Treated is a dummy variable of whether the respondent watched either the neutral or charged video. Treated neutral is a dummy variable of whether the respondent watched the neutral video. Treated charged is a dummy variable of whether the respondent watched the charged video. Age is the age of respondent in years. Male is a dummy variable of whether the respondent is male. Born in Dar es Salaam is a dummy variable of whether respondent was born in Dar es Salaam. The three education variables are dummies capturing whether respondent has completed primary, secondary and tertiary education, respectively, with no completed education the omitted category. Head of household is a dummy variable of whether the respondent is head of the household. Asset index is a household asset index based on factor analysis of the following asset variables: ownership of TV, radio, motor vehicle, number of rooms the household occupies. The religion variables are dummy variables of whether the respondent is a Christian or a Muslim, respectively, with other religions the omitted category. The occupation variables are dummy variables for whether the respondent is self-employed, works as a private sector employee or as a government employee, respectively, with other occupations the omitted category. Voted in 2015 election is a dummy variable of whether respondent states to have voted in the 25 October 2015 general election, adjusting from 1 to 0 respondents who claim to have voted in 2015 election but could not correctly answer control questions on ballot box and ballot sheet appearance.

MECHANISMS AND HETEROGENEOUS EFFECTS

In light of the above results, the question of why charged information on elite behaviour tends to reduce intentions to participate politically becomes important. Our survey included a set of eleven questions related to the respondent's view of how well democracy and in particular elections work in general and in Tanzania, the extent to which they believe elite actions undermine the social contract, and their confidence in specific political institutions including parliament, political parties and central and local governments. In terms of sequencing, these questions were all asked after the treatment, and are therefore used to assess how the treatments affected or activated different forms of views, rather than for heterogeneity analysis. In our analysis of this data, we want to illuminate what kinds of views were affected by the treatments, but also want to avoid the challenge that with eleven dependent variables unaffected by the treatments and a significance level of 10 per cent, significant results for our charged treatment variable will on average be found for one of them. We therefore aggregate the eleven variables into three composite indices reflecting the respondent's belief in democracy (how well democracy and elections work), their faith in the social contract (and specifically the extent to which it is being undermined by elite actions), and their confidence in political institutions (parliament, political parties, central and local government). In constructing the indices, the underlying variables are weighted by their inverse standard deviations in the control group, but the results are robust to other weights, both equal weights and weights calculated using factor analysis (results available on request).[12]

TABLE 5
Mechanisms

Dependent variable	(1) Belief in democracy	(2) Faith in social contract	(3) Confidence in political institutions
Treated neutral	0.008	-0.299**	-0.168*
	(0.070)	(0.135)	(0.096)
Treated charged	-0.113	-0.536***	-0.303***
	(0.073)	(0.130)	(0.093)
Constant	3.740***	3.106***	3.372***
	(0.279)	(0.493)	(0.321)
Covariates	Yes	Yes	Yes
r2	0.122	0.153	0.133
N	472	458	570
p-value (treated neutral=treated charged)	0.088	0.043	0.149

Note: Ordinary least squares regressions with robust standard errors in parentheses, *** indicates significance at the 1% level, ** at 5%, * at 10%. The p-value in the bottom row is from a t-test of whether the effect of the charged treatment is different from that of the neutral treatment. The three dependent variables are composite indices constructed as the average of a set of underlying variables, weighted by inverse standard deviation in the control group. The Belief in democracy variable is the weighted average of the respondent's agreement with the following five statements: "Overall, democracy works well in Tanzania", "Democracy is preferable to any other kind of government", "We should choose our leaders in this country through open, regular and honest elections.", "Elections can change the way the country is run", and "Your vote matters to the way the country is run". The Faith in social contract variable is the weighted average of the respondent's agreement with the following two statements: "Wealthy people undermine democracy in Tanzania", and "Wealthy people undermine people's willingness to pay taxes in Tanzania", rescaled so as to have higher values indicate more faith in the social contract. The Confidence in political institutions variable is the weighted average of the respondent's agreement with the following four statements: "I have confidence in the central government", "I have confidence in the local government", "I have confidence in the political parties", and "I have confidence in the parliament". For all the underlying variables, agreement is measured according to the scale 1 – Disagree very strongly, 2 – Disagree, 3 – Neither agree nor disagree, 4 – Agree, 5 – Agree very strongly. Treated neutral is a dummy variable of whether the respondent watched the neutral video. Treated charged is a dummy variable of whether the respondent watched the charged video. The covariates include all covariates included in Table 4.

The results when we use these three composite indices as dependent variables in additional regressions are reported in Table 5. The coefficient for the charged treatment is consistently negative across all three indices, but larger and significant for the Faith in social contract and Confidence in political institutions indices only. A possible interpretation is that providing information on the use of tax havens has more of an effect on views on elite-citizen interactions and on confidence in concrete political institutions than on more abstract views on democracy. The effects are sizeable, on the underlying five point scale the charged treatment reduces assessments of the social contract and political institutions by about a half and a third of a point, respectively. The neutral treatment also has negative effects on the Faith in social contract and Confidence in political institutions indices, but for all three indices these effects are less negative compared to the charged treatment (and significantly so for the first two indices as captured by the p-value at the bottom of the table). While both treatments hence seem to generate or activate negative views of the social contract and political institutions, our results suggest that the effect is only strong enough to influence voting intentions in the case of the charged information treatment.

Voters may respond differently to information depending on their political and socio-economic background. Table 6 summarizes the main findings of our analysis of possible heterogeneous effects across covariates. Three covariates appear particularly important.[13] In the first column of the table, we interact our two treatment variables with the asset index. The asset index runs from approximately -2 to 2, with a mean of 0. Each of the treatment coefficients hence captures the effects of the treatments on voting intentions for those with mean assets. The effect of the charged treatment for this group is negative, and as the subsequent interaction with the asset index shows, it is even more negative for those with less than mean assets, i.e. an asset index that is negative. In Figure 1, we elaborate the marginal effects of the charged and neutral treatments for different levels of the asset index. As the figure shows, there is a negative effect of the charged information treatment at levels of assets below the mean, whereas above the mean the effect is indistinguishable from zero. This suggest that the charged information affects voting intentions primarily of less wealthy voters. One interpretation of this is that poorer voters have less agency to influence the direction of their own lives and perceive themselves as less likely to have a political impact through voting, making them respond more negatively to suggestions that the political system is not working. In our control group, we do also see a positive correlation between the asset index and the extent to which voters believe their vote matters to the way the country is run (p<0.047). Moreover, the results presented in column two of Table 6 suggest a similar conclusion. Here, the treatment variables are interacted with a dummy for whether the respondent is the head of his or her household. The treatment effect of the charged information is negative for respondents who are not household heads, while there is no significant effect for household heads (as captured by the p-value at the bottom of the table), again suggesting that those with less agency tend to respond negatively to the charged information treatment.

TABLE 6
Heterogeneous effects
over covariates

	(1)	(2)	(3)
Dependent variable	Voting intention	Voting intention	Voting intention
Interaction variable	Asset index	Head of household	Voted in 2015 election
Treated neutral (β_{T1})	0.011	-0.046	0.153**
	(0.047)	(0.063)	(0.075)
Treated neutral*Interaction variable (β_{T1INT})	-0.022	0.124	-0.223**
	(0.046)	(0.092)	(0.095)
Treated charged (β_{T2})	-0.093**	-0.167***	-0.031
	(0.047)	(0.064)	(0.083)
Treated charged*Interaction variable (β_{T2INT})	0.087*	0.154	-0.099
	(0.048)	(0.095)	(0.101)
Constant	1.099***	1.212***	1.006***
	(0.163)	(0.160)	(0.166)
Covariates	Yes	Yes	Yes
r2	0.173	0.169	0.173
N	593	593	593
p-value ($\beta_{T1}+\beta_{T1INT}=0$)	.	0.249	0.244
p-value ($\beta_{T2}+\beta_{T2INT}=0$)	.	0.849	0.022

Note: Ordinary least squares regressions with robust standard errors in parentheses, *** indicates significance at the 1% level, ** at 5%, * at 10%. Voting intention is a dummy variable of whether respondent would vote if there was an election tomorrow, adjusting from 1 to 0 respondents who claim to have voted in 2015 election but could not correctly answer control questions on ballot box and ballot sheet appearance. Treated neutral is a dummy variable of whether the respondent watched the neutral video. Treated charged is a dummy variable of whether the respondent watched the charged video. In each of the three columns, the treatment variables are interacted with the covariate given in the second row from the top of the column. Asset index is a household asset index based on factor analysis of the following asset variables: ownership of TV, radio, motor vehicle, number of rooms the household occupies. Head of household is a dummy variable of whether the respondent is head of the household. Voted in 2015 election is a dummy variable of whether respondent states to have voted in the 25 October 2015 general election, adjusting from 1 to 0 respondents who claim to have voted in 2015 election but could not correctly answer control questions on ballot box and ballot sheet appearance. The p-value in the bottom two rows are from tests of whether the sum of the treatment effects and their interaction term is significant. The covariates include all covariates included in Table 4.

FIGURE 1
Conditional effects of treatments with
95 per cent confidence intervals
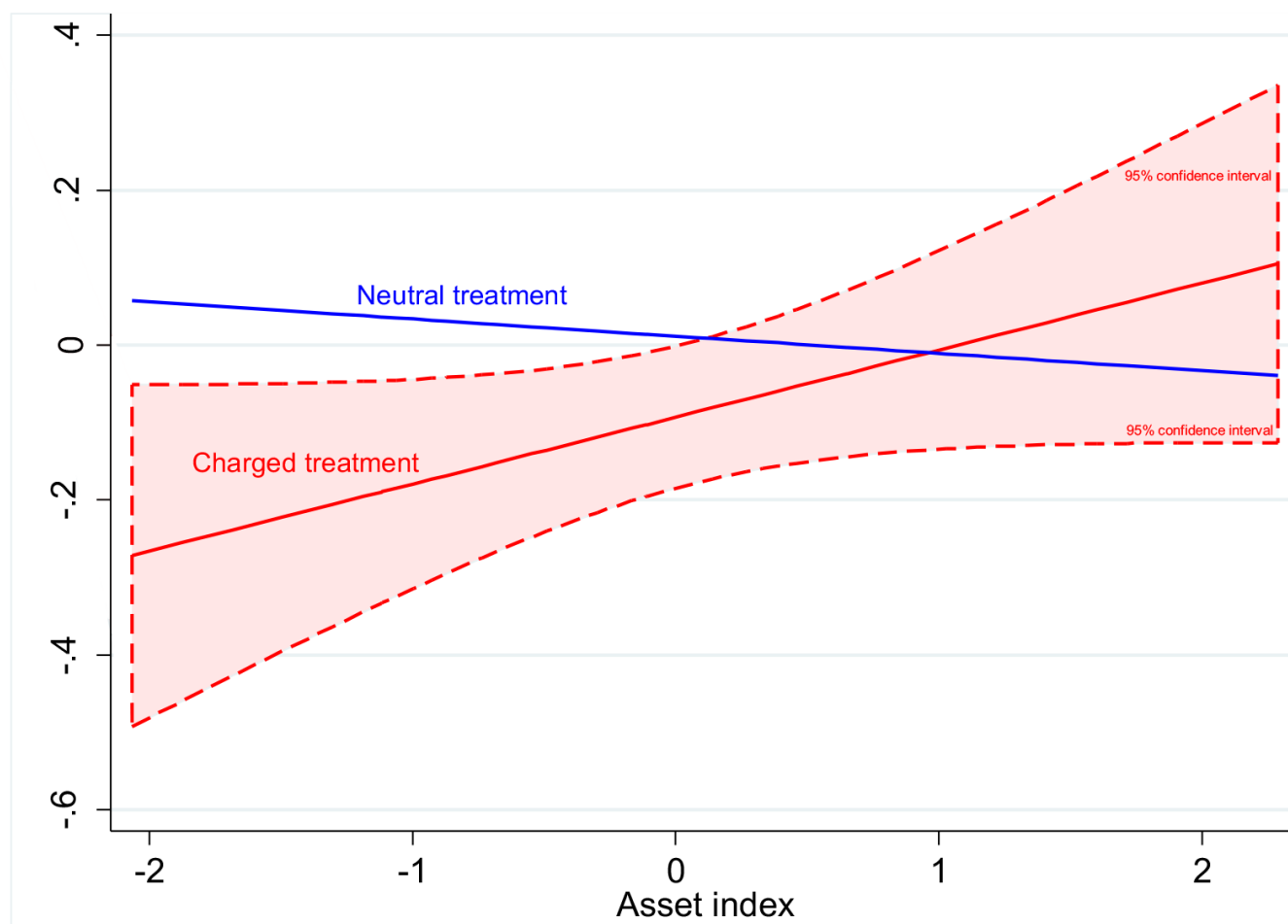
Note: The plot shows marginal effects of the charged and neutral treatments for different levels of the asset index. The asset index is a household asset index based on factor analysis of the following asset variables: ownership of TV, radio, motor vehicle, number of rooms the household occupies.

While our evidence mostly suggests that information on self-serving elite behaviour either has no effect or a negative effect on voter turnout, column three in Table 6 points to a possible nuance. Here the treatment variable is interacted with a dummy variable of whether the respondent voted in the election preceding the experiment. The coefficient for the neutral treatment at the top suggests that those who did not vote in the 2015 election had their voting intentions positively influenced by neutral information (by 15.3 percentage points). One possible explanation for this is that those who did not vote in the 2015 election on average have less previous experience with voting and the democratic system, and may hence be less susceptible to any system cynicism-generating effects of information about self-serving elite behaviour. Another explanation could be that those who did not vote are less ideologically or otherwise aligned with alternative political views, and hence more likely to be influenced by neutral information. The results suggest that neutral information may increase participation if it can be targeted specifically at those who do not normally vote. However, they also indicate that information on self-serving elite behaviour may become ineffective in influencing voting behaviour as experience with a flawed political system increases, which is also consistent with our finding that voting tends to decrease with the age of respondents.

CONCLUSIONS

Information has the potential to mobilize or to alienate. The results from our survey experiment in Tanzania indicate that, in the context of an imperfect democracy, providing eligible voters with information on self-serving elite behaviour either has no effect or a negative effect on voting intentions. Information of this kind tends to reduce faith in the social contract and confidence in political institutions, highlighting the dysfunctions of the political system more than the importance of the election outcome. Our results are consistent with experimental evidence that information on corruption decreases turnout (Chong et a., 2015). We show that the form of information matters; attempting to mobilize voters by stoking their moral indignation with elite behaviour may backfire. Moreover, the citizens whose political participation is most negatively affected by charged information on elite behaviour tend to be the less well off, possible reflecting a lower level of perceived agency. Given the importance of elections in holding politicians and the elite accountable, our results raise important questions. In particular, how do you introduce matters of elite behaviour and elite-citizen relations into political debates in a way that increases rather than reduces citizen political participation? Our results suggest that neutral information may mobilize those with limited electoral experience, but this leaves open the question of how to avoid detrimental effects on and keep the debate going among those who have gained experience with the democratic system.

The importance of context is underscored by the difference between our results and analyses from highly democratic countries. Empirical evidence from democratic states does not find an unequivocally negative effect of negative information on turnout (Lau et al., 2007; Fridkin and Kenney, 2011), there is some evidence that playing on moral indignation can increase participation (Valentino et al, 2011) and that voter turnout tends to increase with self-serving elite behaviour (Kolstad and Wiig, 2015). The results from our experiment suggest that effects of information in the context of a less well-functioning democracy may have less favourable effects on participation. Some possible implications of this should be noted. In many democratic states, parties and movements emerge which try to generate a voter and membership base by invoking voter indignation with deep elite-citizen divisions, with some apparent success. In less democratic states, the voter potential for such parties seems smaller, as messages of elite misbehaviour lead voters to withdraw from the electoral process. This is consistent with the widely noted absence of programmatic political parties with clear policy platforms in Africa (Chabal and Daloz, 1999; Van de Walle and Butler, 1999). A further implication of these findings it that there may be multiple equilibria in terms of democracy. On the one hand, well-functioning democracies may create the necessary debate and political conflict needed for politicians to be held accountable by an electorate whose participation is envigorated by these forms of political interaction. On the other hand, in captured democracies charged or adversarial political interaction may simply serve to disillusion voters about their influence on the way society is run, and a vital link between the performance of politicians and public officials and responses by voters is absent. If there are multiple equilibria of this kind, transforming a captured democracy into a well-functioning one may require more fundamental changes to the political system, whereas smaller interventions intended to inform voter decision making may be ineffective.

Some limitations of our study should be noted. While we find information on self-serving elite behaviour to have no effect or a negative effect on voting intentions, it is possible that it could increase other forms of political participation. While we have some data on other forms of participation and our results suggest this is not the case, our analysis of this is not exhaustive. In terms of accountability, it would be important to not only look at effects on voter turnout, but also on party choice, to see how information on self-serving elite behaviour affects political competition and an incumbent's probability of being re-elected. While we have data on party choice, too large a proportion of our respondents declined to answer this question for an analysis of this to be meaningful. This also means that we cannot assess whether information about self-serving elite behaviour affects supporters of the ruling party differently from supporters of the opposition. These are matters for further studies to pursue.

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ENDNOTES

1   Both videos can be viewed online at http://www.cmi.no/ news/1666-research-results, and the manuscripts for the videos are presented in Appendix A.

2   See http://www.icij.org/project/swiss-leaks for details (accessed 27 January 2016).

3   On the Polity IV democracy index, the country scored 4 (on a scale of 0–10 where higher values reflect greater democracy) in 2015, and Freedom House assesses the level of political rights to a 3 and civil liberties to a 4 (on a scale 1–7 where higher values reflect less democracy).

4   http://www.idea.int/vt/countryview.cfm?id=227 (accessed 27 January 2016).

5   https://www.socialscienceregistry.org/trials/939.

6   The videos can be viewed online at http://www.cmi.no/ news/1666-research-results, and the manuscripts for the videos are presented in Appendix A

7   In the pre-analysis plan we also specified two additional outcome variables. Retrospective voting, an outcome variable reflecting whether the respondent would change today his decision to vote/not vote in the 2015 general election. And other political participation, an outcome variable constructed through factor analysis of seven dummy variables reflecting non-voting forms of political participation over the coming six months (including being active in a political party, a civil society organization, in political meetings, in demonstrations, being more politically active in general, following politics more frequently in the media or discussing it more frequently with friends). There are no significant results for these two other outcome variables (results available on request).

8   In making this adjustment, we depart from the intention to vote outcome variable specified in the pre-analysis plan. Our results also hold (and are in fact stronger) if we instead drop the respondents misrepresenting their behaviour from the sample.

9   In the same way as for the adjustment of the dependent variable, this is a departure from the voting covariate specified in the pre-analysis plan.

10   This is more conservative than using one-sided tests as specified in the pre-analysis plan, which also does not specify the test to be used when the signs of the two treatment coefficients differ.

11   http://www.temco.udsm.ac.tz/images/stories/NEC/NUMBER-OF-REGISTERED-VOTERS-BY-GENDER.pdf. According to the International Institute for Democracy and Electoral Assistance, breakdowns of actual voting by gender is an uncommon statistic to report, http://www.idea.int/vt/survey/by_gender.cfm.

12   For details on the three indices, please see Appendix B.

13   Patterns across other covariates where less robust and are not reported here (results available on request).

APPENDIX A:
MANUSCRIPTS – TREATMENT VIDEOS

Neutral video

"Have you heard about the Swiss billions?

99 Tanzanians sent 205 billion Shillings to bank accounts in Switzerland in 2006/2007.

Wealthy people and big companies in Tanzania send money to many countries like Switzerland – countries we call tax havens.

By sending money to these countries, rich people pay less taxes to Tanzania.

When wealthy people send money abroad in this way and pay less taxes, tax revenues in Tanzania are reduced.

Taxes are spend on building and improving public services and infrastructure like schools, hospitals and roads.

When wealthy people pay less taxes, the government has less money to spend and cannot provide good schooling – there are no desks for classrooms and teachers become scarce.

Health services suffer, too – with less available medicines in the clinics and fewer doctors to consult with patients.

Tanzania could be able to afford better public services for its people, if wealthy people did not send billions to Switzerland and other tax havens."

Charged video

"Have you heard about the Swiss billions?

99 Tanzanians sent 205 billion Shillings to bank accounts in Switzerland in 2006/2007.

Wealthy, greedy people and big companies in Tanzania are hiding money in overseas bank accounts in countries like Switzerland – countries we call tax havens.

By sending money to these countries, wealthy people get richer by avoiding to pay the taxes that we are all supposed to pay.

When wealthy people send money abroad in this way they do not pay the required amount of taxes, therefore tax revenues in Tanzania are reduced.

Taxes are spend on building and improving public services and infrastructure like schools, hospitals and roads.

When wealthy people pay less taxes, the government has less money to spend and cannot provide good schooling for your children, there will be no desks for classrooms and teachers become scarce.

Health services for your family will suffer, too – with less available medicines in the clinics and fewer doctors to consult with patients.

Tanzania could be able to afford better public services for you and all, if wealthy people did not send billions to Switzerland and other tax havens.

These greedy, wealthy people don't do any justice to you or other Tanzanians!"

APPENDIX B:
DETAILED SPECIFICATION OF DEPENDENT VARIABLES IN MECHANISM ESTIMATIONS

TABLE B 1
Dependent variables in mechanism estimations

Variable	Explanation
Belief in democracy	Average level of agreement (weighted by inverse standard deviations in the control group) with the following statements: *"Overall, democracy works well in Tanzania"* *"Democracy is preferable to any other kind of government"* *"We should choose our leaders in this country through open, regular and honest elections"* *"Elections can change the way the country is run"* *"Your vote matters to the way the country is run"* Agreement with each statement measured according to the scale 1 – Disagree very strongly, 2 – Disagree, 3 – Neither agree nor disagree, 4 – Agree, 5 – Agree very strongly.
Faith in social contract	Average level of agreement (weighted by inverse standard deviations in the control group and rescaled so that larger values reflect more faith in social contract) with the following statements: *"Wealthy people undermine democracy in Tanzania"* *"Wealthy people undermine people's willingness to pay taxes in Tanzania"* Agreement with each statement measured according to the scale 1 – Disagree very strongly, 2 – Disagree, 3 – Neither agree nor disagree, 4 – Agree, 5 – Agree very strongly.
Confidence in political institutions	Average level of agreement (weighted by inverse standard deviations in the control group) with the following statements: *"I have confidence in the central government"* *"I have confidence in the local government"* *"I have confidence in the political parties"* *"I have confidence in the parliament".* Agreement with each statement measured according to the scale 1 – Disagree very strongly, 2 – Disagree, 3 – Neither agree nor disagree, 4 – Agree, 5 – Agree very strongly.

Does self-serving elite behaviour make citizens more politically active?

This paper presents the results of a randomized field experiment where voters in Tanzania were given information about elite use of tax havens. Information provided in a neutral form had no effect on voting intentions. Information phrased in more morally charged terms led to a reduction in voting intentions. Additional evidence suggests that rather than increase the perceived importance of voting, charged information tends to undermine confidence in political institutions and the social contract. The effects are particularly pronounced among the less well off, indicating that increased transparency in the absence of perceived agency may not improve democratic accountability.