**Pre-analysis plan**

**Elite behaviour and political participation:**

**A survey experiment in Tanzania**

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**1. Introduction**

This study builds and expands on on our earlier work on effects of elite behaviour on citizen political participation. In Kolstad and Wiig (2015) we use country-year panel data to test the effect of self-serving elite behaviour, proxied by portfolio investment in tax havens, on voter turnout. Using fixed effects estimation, we find that for well-functioning democracies there is a positive relation between the use of tax havens and voter turnout, suggesting that self-serving elite behaviour is associated with citizen political mobilization rather than voter apathy. While country fixed effects and further time-variant covariates are included to address endogeneity, it is difficult to fully allay such concerns using observational data. This pre-analysis plan therefore details an experimental study which will be performed to provide further evidence on the causal effect of self-serving elite behaviour on political participation. The experiment randomly assigns individuals to treatments (detailed below) where they are given information on elite behaviour and to a control group, in order to test the effect of giving such information on political behaviour, and to compare effects of giving information in different ways.

The study tests an implicit assumption in prominent political economy theories that elite misbehaviour will lead to citizen mobilization (Acemoglu and Robinson, 2006). On a theoretical level, this may not be obvious, as elite misbehaviour could lead to voter abstention or apathy as citizens lose confidence in the democratic system (Kostadinova, 2009), and some empirical studies using observational data suggest that the net effect of for instance corruption on voter turnout is negative (Stockemer et al., 2013). Our experimental approach aims to advance this literature by identifying causal effects of elite behaviour on citizen participation. In doing so, we also contribute to a growing literature using experimental methods to study voter turnout, but which has focused more on treatments related to the voting process, rather than on elite behaviour (Wantchekon, 2003; Aker et al, 2013; Collier and Vicente, 2014; Vicente, 2014). Our treatments include information about elite use of tax havens, and our study therefore also contributes to the literature on tax havens (Hines and Rice, 1994; Diamond and Diamond, 2002; Desai et al., 2006; Rose and Spiegel, 2007; Dharmapala and Hines, 2009; Slemrod and Wilson, 2009; Hines, 2010; Andersen et al., 2013; Blanco and Rogers, 2014; Johannesen and Zucman, 2014). Existing studies have a focus on definitions, economic consequences of tax havens and effects of international initiatives to reduce their negative consequences, where we instead treat the use of tax havens as a reflection or epitomization of an important underlying political economy feature of a country, that of self-serving elite behaviour.

The research questions we mean to address are the following:

1. How does elite behaviour as captured by tax haven use affect political participation?
2. How does the form of information about elite behaviour affect political participation?

We will conduct a survey experiment among eligible voters in Dar es Salaam, Tanzania, to answer these questions. Respondents will be randomly assigned to two treatment groups and a control group. Each of the treatment groups will be exposed to information about elite use of tax havens. The information to the first treatment group will be provided in a fairly neutral form, focusing on what tax haven use is and what it entails. Information for the second treatment group will be more morally charged, additionally emphasizing the self-serving nature and possible unfairness of tax haven use. Each of the treatments take the form of watching a video about 90 seconds long. The control group will be shown no video, a decision we arrived at after carefully considering the possibility of creating a placebo video whose contents would at the same time not affect participation in the control group, while also being as engaging to the respondents as the treatment videos. In principle, 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. Difficulties of creating a convincing placebo is shared by many other social science projects, and the absence of one means that we cannot completely rule out that the estimated treatment effects may come from the act of watching the video rather than from the content of the video. Our aim is nevertheless to estimate the causal effects of the tax haven treatments by comparing outcomes with the control group, and we see it as unlikely that the simple act of watching a short video would have much of an effect here. For the comparison of the effects of the two different treatments, one neutral and one more morally charged, the same complications are not in place, and our approach allows us to estimate a causal effect of moving from a neutral information intervention to a charged one.

After the treatments, respondents will be asked to answer a set of questions relating to political and social participation, and a set of background questions. The treatments create exogenous variation in beliefs about or attention to issues of elite behaviour, and permit us to analyze the effect of elite behaviour on participation.

This pre-analysis plan presents the sampling procedure, the design of the experiment, then details empirical strategies and the hypotheses to be tested. The plan was registered in the AEA RCT registry 9 November 2015. While the survey experiment started 30 October 2015, we did not have access to the data from the survey experiment before completing and registering this pre-analysis plan.

**2. Research strategy**

To inform the research, a pilot was conducted in February 2015 based on 150 randomly selected respondents in 10 polling districts in Dar-es-Salaam, Tanzania. The treatment videos were tested using focus groups in September 2015.

The survey experiment will be conducted in Dar es Salaam. 600 eligible voters will take part, and will be selected as follows. From the list of all polling stations in Dar es Salaam in the 2010 election (polling station information from the 2015 election was not available to use when we started preparing the survey), we have randomly selected 24 polling stations. In each of these locations, a team of eight enumerators walk pre-defined routes evenly spaced in eight different directions from the polling stations, selecting every third household. In each household, a random person above the age of 18 and of the enumerator’s gender was selected for an interview (there are four enumerators of each gender). A total of 25 interviews will be conducted in the catchment area of each polling station, for a total of 600 interviews.

**3. Research design**

Each respondent was asked to respond to a questionnaire with six different sections, where the interview took in total about 30 minutes to complete. 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 on to the treatment, and then directly to the outcome variables on political behaviour, while questions of background variables were asked in the final sections. The questonnaire thus had the following structure:

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

In stage 2, respondents are randomized into one of the two treatments, or to the control group. This is done through lists of random choices between the three possibilities made in advance and given to each enumerator. The enumerators move down the list, crossing off the current video shown and moving on to the next one on the list in the next interview. This will give approximately 200 respondents in each of the three groups. Randomization is hence at the individual level, and not blocked by location.

Interviews are conducted electronically using tablets, and the videos were also shown to the respondents on the tablets. ODK (Open Data Kit) is used to collect data electronically on the tablets. ODK has the capacity to collect data offline, and can collect different types of data. Data can also be uploaded to the Internet at any time connection is established. Videos and maps for the enumerators are stored on the tablets in advance.

*3.1 Treatments*

In the treatment section, respondents are randomly assigned to watch one of two videos, or to the control group, where no video is shown. Both videos contain definitions and explanation of tax haven use, starting from a highly publicized case knows as the “Swiss billions” case where Tanzanian nationals have been reported to have stored billions of Tanzanian shillings in Swiss banks. As tax havens may be a difficult concept to use among ordinary Tanzanian voters, in the pilot we tested the use of the term Swiss billions as an alternative, and found that it provided a term people knew or could relate to. Both videos also contain information on what the use of tax havens entail in terms of reduced tax receipts for Tanzania, and therefore less money available to spend on public services or infrastructure, specifically schools, hospitals and roads. The treatment videos differ in the tone and language used. The first treatment video is neutral in tone and language. The second is 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. After treatment, the treated individuals are asked to answer a few questions about the videos, and invited by the enumerators to reflect a little on their content, in order to ensure that the treatments make the issues salient for the respondents.

*3.2 Outcome variables*

Our main outcome variables come from the section of the interview that immediately follows the treatments. The following three variables are our main outcome variables:

1. Intention to vote. This is question B.3 in the questionnaire: “If there was a new general election tomorrow, would you vote?” (1 - Yes, 0 - No)
2. Retrospective voting. This outcome variable is a dummy reflecting whether the respondent would have voted if he/she could have gone back to the election day and decided anew (1 – Yes, 0 – No). It is constructed by combining answers to question B.5 “Do you regret voting/not voting in the general election this year?” (1 - Yes, 0 - No) and question B.1 in the questionnaire, “Did you vote at the general election this year?” (1 – Yes, 0 – No). These are coded into the retrospective voting variable, which takes on the values of question B.1 if the response to question B.5 is no (the voter does not regret voting/not voting), and coded to the opposite of the values of question B.1 if the response to question B.5 is yes (the voter regrets voting/not voting).
3. Other political participation. This outcome variable will be 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). The seven dummy variables include the five dummy variables from questions C8, C9, C10, C12, C14 and two more dummy variables created by combining questions C2 to C4, and C5 to C7, respectively.[[1]](#footnote-1)

We have specified these variables to cover different time perspectives: Intention to vote the immediate future (tomorrow), while retrospective voting is backward looking and the other participation variable is forward looking (in six months). While the first two outcome variables are likely to be highly correlated, distinguishing between forward and backward looking assessments of this kind has to our knowledge not been done in these forms of studies, and a firm prior is difficult to specify. The last variable is different from the first two in considering non-voting forms of political participation.

*3.2 Control variables*

In the main analysis, we will control for the following variables, and these variables (except the polling station fixed effects) will also be used to test for balance between the control and treatment groups.

* Age (count variable, number of years)
* Gender (dummy variable, 1 – male, 0 – female)
* District of birth (dummy variable, 1 – Dar es Salaam, 0 – other)
* Education (three dummy variables capturing completion of primary, secondary, and tertiary education, with no completed education the omitted category)
* Household head (dummy variable, 1 – respondent is household head, 0 - not head)
* Asset index (based on factor analysis of the following asset variables: ownership of TV, radio, vehicle, number of rooms the household occupies)
* Religion (two dummy variables for Christians and Muslims, with other beliefs the omitted category)
* Occupation (three dummies for self-employed, private sector employment, government employment, with other employment status the omitted category)
* Voted in the general election 2015 (dummy variable, 1 – Yes, 0 – No)
* Polling stations fixed effects

**4. Empirical strategy**

Consistent with our research questions, we have two hypotheses detailed below.

*Hypothesis 1: Information on elite behaviour has no effect on political participation*

We test this by comparing the two treatment groups (collapsed together) with the control group, for our three outcome variables. Our alternative hypotheses are that information on elite behaviour has a positive effect on political participation, or that information on elite behaviour has a negative effect on political participation. On theoretical grounds, there are plausible reasons why the effect could go in either direction.

Hypothesis 1 will be tested through OLS estimation (with robust standard errors) of the following equation for each of the three outcome variables:

(1)

where is the outcome for individual *i* in polling station s on outcome variable *j*=1,2,3 and is an indicator variable taking the value one if individual *i* is in one of the two treatment groups, and zero otherwise. We will also estimate an equation which controls for the background variables specified in Section 2, captured by the vector and polling station fixed effects :

(2)

Formally, hypothesis 1 can be expressed as:

(3)

And the alternative hypotheses as:

(4)

The second hypothesis is as follows:

*Hypothesis 2: The form of information given about elite behaviour does not affect political participation*

We test this by testing whether the treatment effects are different between the two treatment groups. This will be done through OLS estimation (with robust standard errors) of the following equation for each of the three outcome variables:

(5)

where is an indicator variable for individual *i* being in the treatment group receiving the neutral information treatment, and is an indicator variable for individual *i* being in the treatment group receiving the morally charged information treatment. We will also perform the same analysis adding the control variables specified earlier:

(6)

In this case, the most plausible alternative hypothesis is that the morally charged information treatment will enhance the effect of the neutral treatment. In other words, if the neutral treatment has a positive effect on political participation, the charged version will have an even more positive effect, and if the neutral treatment has a negative effect on participation, the charged version will have an even more negative effect. This leads us to formulate the following alternative hypothesis, for each of the outcome variables *j*=1,2,3:

(7)

Given these alternative hypotheses, we will use one-sided tests in this case.

Given our main findings, we intend to analyze mechanisms behind the results and heterogeneous effects across a number of dimensions. However, here there are so many possibilities depending on what we find 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). We are therefore reluctant to specify this any further in this pre-analysis plan, and accept that our analysis of mechanisms and heterogeneity will be explorative.

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1. The coding of the latter two dummies is done in a similar way as the above retrospecive voting variable: Question C2 (C5) gives us the whether the respondent is currently active in a political party (civil society organization), question C3 (C6) asks those who are not currently active whether they will become active in the next six months, and C4 (C7) asks those currently active whether they will stop being active in the next six months. Based on this we generate a dummy variable equal to C2 (C5) for those respondents who answer no to questions C3 (C6) and C4 (C7), and the opposite of C2 (C5) for those who answer yes to questions C3 (C6) and C4 (C7). In other words, this constructed dummy reflects whether the respondent will be active in the coming six months. [↑](#footnote-ref-1)