

# **Pre analysis plan for medium run results for the project “Jobs and political participation - Evidence from a Field Experiment in Ethiopia”**

Lovise Aalen, Andreas Kotsadam, Janneke Pieters, and Espen Villanger\*

## **Abstract**

The present project is the first to identify the individual level effects of jobs on female political participation using a field experiment. We identify the effects of jobs on political participation by collaborating with 25 large companies in Ethiopia to randomly assign jobs to equally qualified applicants. We already have some data from 6, 12, 18 months follow ups. The job offers increase formal employment, earnings, and earnings shares within couples in the short run. We also find that job offers reduce the participation in community meetings but does not affect political interest. A likely mechanism for our results is increased time pressure for the women who got the jobs. Consistent with such an interpretation we find reductions in leisure time and in time spent on social and religious activities. Another hypothesis could be that participation in meetings are not meaningful and that non-participation is actually empowering. We will now collect longer run data on the same and new outcomes. In particular, we will collect data on contentious political action (protests), political preferences, and voter turnout (both intentions and actual turnout in the 2020 election). The changes in Ethiopia over the period also makes it interesting to see if jobs have a different relationship with political participation before and after the liberalization. In this plan we describe the analytic decisions that will be made in the analysis of the data in the project. That is, we describe the hypotheses to be tested and how they will be tested. The description includes how the variables are coded, how we will deal with attrition and missing values, and how the estimation equations will look like. We also conduct a power analysis which suggests that we are able to identify relatively small effects. All deviations from the plan will be highlighted in the final paper.

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\* Lovise Aalen and Espen Villanger: Christian Michelsen Institute. Andreas Kotsadam: The Ragnar Frisch Centre for Economic Research, e-mail: [andreas.kotsadam@frisch.uio.no](mailto:andreas.kotsadam@frisch.uio.no). Janneke Pieters: Wageningen University.

## 1. Introduction

Women have substantially lower rates of political participation than men in most developing countries. A prominent explanation for this is the resource hypothesis which claims that men have more material resources in terms of education and income and therefore have the capacity to participate more.

There is a strong expectation in the literature that employment, in particular, will bring about changes in women's agency in multiple arenas of life, particularly by enabling and motivating them to participate politically. In developed democracies, the correlation between employment and participation is clear (Ross 2008, Schlozman, Burns, and Verba 1999). The higher percentage of women working, the more women tend to vote or become representatives in various political forums. Formal employment is argued to affect female political participation by changing women's gender attitudes and identities, by increasing their decision-making power within the household, giving them access to networks and making them informed about their interests (Isaksson et al., 2014; Iversen & Rosenbluth, 2008; Ross, 2008).

A growing body of literature on developing, and/or authoritarian contexts presents a slightly different picture. In general, the factors driving women's political participation in developing countries are much less clear than in the developed world, and the evidence is mixed, and scarce (Coffe and Bolzendahl 2010, Isaksson et al. 2014). In contrast to Western societies, research from Sub-Saharan Africa show that poor, less educated citizens may participate on an equal level as others (Bratton 1999). Both cross-national and case studies from authoritarian contexts show that participation could be less a consequence of individual factors and behavior than in democracies. Instead, it could be a result of regime-directed mobilization (Bahry and Silver 1990), where participation is deliberately used to strengthen authoritarian rule instead of giving citizens the chance to influence decision making (Aalen and Muriaas 2018, Benton 2016). Research from Ethiopia before the political transition in 2018 shows that women with formal employment are *participating* equally in politics, but are actually less *interested* in politics than those without (Aalen, Kotsadam and Villanger 2019).

There is, however, limited evidence for the causal impacts of employment. Previous studies, both from the developed and developing contexts and from across political regime types, often just compare workers to non-workers. In this way, the findings may be confounded by reverse causality and omitted variable biases. That is, without a credible design for causal inference it is difficult to rule out that political participation creates jobs or that there is some third variable

driving both employment and political participation. The present project is the first to identify the individual level effects of jobs on female political participation using random assignment of jobs.

## **2. The field experiment, data and main empirical strategy**

We have been collaborating with large companies that were hiring new workers. The companies first assessed all job applicants and determined whether each applicant was eligible for the job or not. Then, from the pool of eligible candidates at each company, a list of all women with a partner<sup>†</sup> was compiled (this is our study sample), from which the company randomly selected half to receive a job offer. The randomization was done using computers and the applicants were informed about the procedure before the randomization was conducted.

Pre- and post- longitudinal data have already been collected using a detailed structured survey instrument. The women were interviewed before they started working and the first follow up data collection was around 6 months after the first interview. The dates for the baseline data collection varies and depend in particular on when the firms hired. Some of the firms hired new batches of workers several times during the period. In addition to questions on politics, the survey instrument includes modules gathering demographic and background information, including poverty measures and other socio economic variables, and a module on domestic violence and female empowerment.

### *Variables measuring political participation, interest, and preferences*

The surveys included a set of questions about political participation. These will be coded as follows:

Participation last year: In the previous surveys we have two questions about the respondent's participation during the last year. These questions were asked as follows: "Here is a list of actions that people sometimes take as citizens. For each of these, please tell me whether you, personally, have done any of these things during the past year.

*Attended a community meeting*

*Got together with others to raise an issue"*

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<sup>†</sup> The data collection initially focused on impacts on intimate partner violence, which is why the study population consists of partnered women.

The answers can be “1=Yes, often”, “2=Yes, several times”, “3= Yes, sometimes”, “4=yes, rarely”, and “5=Not at all”. We create the dummy variables *Meeting last year* and *Raise issue last year* as 1 if the answer is 1-4 and 0 if it is 5. We will also reverse code the variables and investigate the effects on the continuous versions.

Interested: Based on the question “How interested would you say you are in politics and government?” The possible answers are “1= very interested”, “2=somewhat interested”, “3= not very interested”, and “4= not at all interested”. We create a dummy variable, *Interested*, that equals 1 if the answer is 1 or 2 and 0 if the answer is 3 or 4. We will also reverse code the variable and investigate the effects on the continuous versions.

Networks: The survey asked about the existence, membership, and roles in networks. The following 12 networks are included, in addition to an “other” category: Mahiber(tswa...etc), Other religious associations, Women’s association, Microfinance cooperative, One-to-five networks, Development teams, Idir, Equib, Trade union, Informal Workers group, Users association, customary institution. The first question is “Does this exist in your neighborhood or workplace? (Yes=1, No=0)”. Then the women are asked: “Are you currently member of the specified network? (Yes=1, No=0)”. Another question of interest is “Do you have an important role in the network? (Yes=1, No=0)”. Based on these questions we create dummy variables for each network type X for membership. *Member X* equals 1 if member, 0 if not or if the network does not exist in the area (since this may be endogenous). We also create dummy variables for each network type X for leadership/important role. *Leader X* equals 1 if the role is important, 0 if not or if the network does not exist in the area. We then create two count variables, theoretically ranging from 0 to 13, for the number of networks (*Number of networks – member*, and *Number of networks – leader*). Based on these count measures we also create two dummy variables, *Member of any network* and *Leader of any network*, which equal one if the corresponding count variables are positive. We also create the variables *Network X exists*, which equals 1 if the network exists in the area 0 if not.

Kebele meetings: Based on the question, “Do you attend meetings in the Kebele?” The possible answers are "Yes", and "No". We create a dummy variable, *Kebele meetings*, that equals 1 if yes and 0 if no.

*In the new survey we will add the following questions:*

Intend to vote: Based on the question, “Would you vote if there was an election next week?”. The answers will be Yes or No and we will dummy code them so that *Intend to vote* equals 1 if yes and 0 if no. **This will be our main dependent variable.**

Party choice: If there was an election next week, which party would you vote for? We will create dummy variables for EPRDF, Ethiopia Citizens for Social Justice, MEDREK, Oromo Liberation Front, NAMA, and Other. We will code non-voters as zero.

Protest: We will add “Attended a demonstration or protest march” and “Expressed a political opinion through social media” to the questions about political participation last year and we will code the variables, *Protest last year* and *Express last year*, the same way as we coded the other two variables.

Voted in election X: Based on the questions: “Did you vote in the election year X”, where X will be 2005, 2010 and 2015. The answers will be Yes or No and we will dummy code them so that *Voted in election X* equals 1 if yes and 0 if no. If we conduct a survey after the planned 2020 election we also add a question about that election in the follow up wave.

Pressured now: Based on the question, "Do you feel pressured nowadays to attend community meetings?" The possible answers are "Yes, often", "Yes, several times", "Yes, sometimes", "Yes, rarely", and "Not at all". We create a dummy variable, *Pressured now*, that equals 1 for any of the yes alternatives and 0 for "Not at all".

Pressured before: Based on the question, " What about before the regime of Abiy, did you feel pressured to attend community meetings? " The possible answers are "Yes, often", "Yes, several times", "Yes, sometimes", "Yes, rarely", and "Not at all". We create a dummy variable, *Pressured now*, that equals 1 for any of the yes alternatives and 0 for "Not at all".

Lose influence: Based on the question, "Do you think that people that cannot attend community meetings due to work get less influence in the community?" The possible answers are "Yes", and "No". We create a dummy variable, *Lose influence*, that equals 1 if yes and 0 if no.

Lost influence before: Based on the question, "What about before Abiy came to power , do you think that people that could not attend community meetings due to work got less influence in the community? " The possible answers are "Yes", and "No". We create a dummy variable, *Lost influence before*, that equals 1 if yes and 0 if no.

Political priorities: We create a variable “*Roads and Infrastructure*” based on the following question: “If you could only choose between water or roads and infrastructure, which problem do you think is the most important one facing Ethiopia that the government should address?”. The answer categories will be “1=Water”, “2= Roads and Infrastructure”, “3= I do not know”. We will code the variable *Roads and Infrastructure* as 1 if the answer is 2 and as 0 if it is 1 or 3.

Discuss politics with friends: We ask respondents, “If you think about your friends and the things you talk about, what are the topics you discuss?” They can choose up to three things. If they answer something coded as *I1=political issues* we dummy code a variable *Discuss narrow politics* to be equal to one. If they answer something coded as either *I1=political issues*, *I6=women’s rights*, or *I13= grievances and dispute resolution* we dummy code a variable *Discuss broad politics* to be equal to one.

Redistribution: We will also create three different variables on redistributive preferences and taxes. These questions are of the following form:

“Please say whether you strongly agree, agree, disagree, or strongly disagree with these statements

(1 = *Strongly agree*      2 = *Agree*      3 = *Disagree*      4= *Strongly disagree*)

*In Ethiopia, the national government should aim to reduce the economic differences between the rich and the poor*

*In Ethiopia, the national government should have taxes on people owning houses to reduce the economic differences between the rich and the poor*

*In Ethiopia, the national government should have taxes on people working to reduce the economic differences between the rich and the poor.”*

These variables will be coded as binary variables: *Reduce differences*, *Tax houses*, and *Tax work*. The binary variables will be set in order to create 2 groups, as similar in size as possible, while respecting the order of the answers (from 1 to 4). For example, if 25% answer 1, 25% answer 2, 25% answer 3 and 25% answer 4, then the binary variable will be equal to 1 if the respondent answers 1 or 2 and zero otherwise. On the other hand, if for example 20% answer 1, 10% answer 2, 5% answer 3 and 65% answer 4, then the binary variable will be equal to one of the respondent answers 1, 2 or 3. We will only consider the answers of the respondents in the control group when creating the coding rules for the binary variables.

Political efficacy: We create a measure of *Internal efficacy* based on the question. “Sometimes politics and government seem so complicated that a person like me cannot really understand what is going on”. Our measure of *External efficacy* is based on the question: ”People like me don’t have any say in what the government does”. Both of these questions have the answer categories *1 = Strongly agree*      *2 = Agree*      *3 = Disagree* *4= Strongly disagree* and will be coded using the same rules as for coding the Redistribution variables.

### Main independent variable

#### *Treatment*

The main independent variable is treatment status which equals 1 if the respondent was randomly assigned to a job offer and zero otherwise. We will also investigate the effects of jobs and then we code the following variables:

### Background variables

We will retain the continuous coding of age in years.

From the baseline data we will use the answer to the survey question: “Have you ever had a formal salaried job with salary before?” From this we create the variable *Any formal wage job* which equals one if the answer is yes.

We dummy code the religious affiliation of our respondents. The majority are Orthodox Christians and we let that be the base category (together with the few people answering Catholic or Other and create dummies for the other two main denominations (Muslim and Protestant).

We recode the years of schooling variable into low (<10 years), medium (10 years), and high (>10 years) and use low education as the base category.

### Main estimation equations:

There is a lot of uncertainty in how much the treatment will bite in terms of predicting formal employment at the fourth follow up. In particular, some people that are not assigned to treatment will find other jobs and people assigned to job quit. It is also unclear whether having had any factory work ever, months of factory work, or factory work during the last six months are most important in affecting political participation. Our main estimation will be intention to treat analyses (ITT), where we regress our outcome variables on the Treatment indicator, controlling for the outcome as recorded at baseline if it exists (and other baseline characteristics). We will

also explore results where we use instrumental variable analyses (IV), instrumenting various aspects of employment with the randomized treatment.

The main estimating equation to estimate the medium run outcomes is the following intention to treat model:

$$y_{i,t} = \alpha \text{Treated\_Job}_i + y_{i,t0} + x_{it0}\beta + c_i + e_{i,t}$$

Where  $y_{i,t}$  corresponds to *Political participation, interest, or preferences* (e.g. Intend to vote, Protest etc.) for woman  $i$  in time period  $t$  (e.g.  $t_4$  is the fourth follow up). This is regressed on a dummy variable that equals one if the woman was randomized to get the job and zero if not. This captures the so called intention to treat effect and it gives us an estimate of the effect of being randomized into treatment. As long as treatment status is randomly assigned we do not expect any other differences between treated and control women. We will test if the groups are similar and also include control variables in order to increase power and precision. In particular we include *Political participation* at baseline ( $t_0$ ) for the variables that were included at baseline, list fixed effects (blocking variables) as women are randomized within this unit, and a vector of individual level controls  $X$ . For the vote intention variable we can include answers to the questions about voting in previous elections. As employment may affect reporting, however, we will first have to check that there is balance on the variables between treatment and control. This balance test will be important as a check of reporting bias more generally.

We will experiment with the control variables to see if we can get more power by adding the other participation at baseline variables as controls in addition to the specific  $Y$  used and we will also see if we can improve precision in the estimates by picking optimal controls from the total list of controls using LASSO (Belloni et al. 2014; Ahrens et al. 2018). The main specification will, however, be the one with only the list fixed effects as controls. We use robust standard errors.

The vector of individual level controls are all taken from the baseline survey and are: *Age, Muslim, Protestant, Any formal wage job, Medium education, and High education*. We have already tested for baseline balance on these variables in the samples we have up until now and it seems balanced.

### **3. Data and empirical strategy for the exploratory analysis of potential mechanisms**

We have chosen to call our tests of mechanisms exploratory as there are many different tests that can be done and as it is difficult to exhaust the list ex-ante. We want to stress that even if

the list we give here turns out to be complete, the analysis will have to be seen as exploratory as the number of hypotheses is large.

While our primary outcome is Intend to vote, we will also explore effects on all other variables measuring political participation, interest, and preferences. Some of these are secondary outcomes and some of them will be used more to dig into the mechanisms behind possible effects (e.g., the internal and external efficacy variables). Some variables will also be used to measure changes over time in the liberalization efforts that have been rolled out in Ethiopia. This may be important as political participation may be differentially affected by employment under more or less autocracy. For instance, we will use whether the network exists in the area to investigate the changes over time in government run networks. In particular “One-to-five networks” and “Development teams” were closely linked to the Government control structures before the liberalization. By investigating participation in such networks before and after the liberalization we can gain insights into how meaningful vs mandated participation is affected differently. We also ask about the meaningfulness of meetings now and retrospectively.

We will test the effects of Treatment on various measures of employment and earnings. The most important measures will be *Any\_factory\_job* (Dummy variable equal to one if the respondent has worked in a factory job for at least one month since the first interview) *Months\_factory\_job* (in months), and *Any formal wage job (last 6 months)* a dummy variable based on earnings from any wage job (where 1 equals positive earnings).

When we find reduced form effects of treatment on our outcomes we will also scale the effects using an IV estimation to account for imperfect compliance. We use an IV model of the following form:

$$Any\ factory\ job_{it4} = aTreated\_Job_i + c_l + e_{i,t} \quad (2)$$

$$y_{i,t4} = Predicted(Any\ factory\ job)_{it4} + c_l + e_{i,t} \quad (3)$$

We will also test the IV regressions with the variables, *Months\_factory\_job* and *Any formal wage job (last 6 months)* to explore whether these are relevant channels.

In order to test mechanisms we will also regress treatment on income, time use, empowerment, and attitudes toward gender equality. If there is an effect on these variables we can include them as controls in the intention to treat analysis to investigate the effect of treatment over and above the effect it has on the intermediate variables. Such type of mediation analysis should not be seen as causal, however, as there are likely to be many variables correlated with the mediators

that are not included in the analysis. For the first six months follow up we know that there were effects on income and time use, but not on empowerment or attitudes. By interacting treatment with the baseline characteristics included as controls and with the mechanism variables at baseline, we also explore the extent to which the effect differs across different types of women. In the process of testing mechanisms we will code some new variables.

*Income and income shares:*

We will create a measure from the survey section on intra household income where we have net income for the respondent from:

1. <i>Factory job employment</i>
2. <i>Other wage employment</i>
3. <i>Self-employment</i>
4. <i>Remittances</i>
5. <i>Government or NGO transfer</i>
6. <i>Other (specify).....</i>

Adding up 1 and 2 will be useful for assessing whether treatment affected earnings from wage employment during the last 6 months. Investigating 1 will also be informative for indicating how much the treatment affected earnings from factory jobs in total.

The questions about incomes is also asked about the woman’s partner and we will create a variable of her share of couple income by adding 1-3 for her and him and then take her income divided by their total income.

*Empowerment*

We have 15 different questions in the survey on intra-household decision making. Not all questions apply to all people in the sample, however. For example, the decision to send a child to school has missing values for all individuals that do not have children. To create an empowerment measure we restrict ourselves to the 12 measures that have fewer missing responses (03-15 below). For each of these measures we create a dummy variable for whether the partner has the final say (i.e., it is reverse coded empowerment). As seen, this corresponds

to the value of 2 in code B. We code it as one if the partner has the final say, and also if the partner decides together with some other member of the household. If the respondent woman decides together with her partner we code the variable as zero only if she has “a lot” of input into the decision (i.e. category 4 on the J1B questions) and otherwise as 1. All other values of J1A are coded as zero on the partner has the final say variables. We then add the 12 variables together and divide by 12 to get an index ranging between 0 and 1.

## Section J. Intra-household Decision Making and Domestic Responsibility Allocation

### J1. Who in your household usually has the final say about the following decisions?

Decision-Making		J1A Household Member  <i>Use Code B</i>	J1.B if she is not a sole decision maker, How much input does the respondent have in this decision? Code : 1=no input 2=little 3=some 4=a lot
01	Whether to send or not send children to school		
02	What to do if a child falls sick		
03	What to do if the respondent falls sick		
04	Whether to have children or to have more children		
05	Which family planning methods to use		
06	Whether or not you should earn money outside the house		
07	Whether you can visit your family or relatives?		
08	The use of the wife's earned income		
09	The use of the man's /husband's earned income		
10	Purchase of small daily food purchases		
11	Purchase of bulk or expensive food items		
12	Large purchases of items like furniture, cattle, TV, or other assets		
13	Purchase of children clothing and shoe		
14	Whether to open bank account or borrow money		
15	Whether to start a new business		

Code B: 01 = Respondent 02=husband/partner 03=other female member 04=other male member 12=respondent and husband jointly 13=respondent and other female jointly 14=respondent and other male member 23=husband and other female jointly 24=husband and other male jointly 34=other male and female member

### *Attitudes towards gender equality:*

The survey also includes questions on a wider set of attitudes toward gender equality. The 11 statements are given below and we code them so that 1 is non-gender equal (e.g. 1 or 2 on statement 1 and 3 or 4 on statement 2). We will investigate whether employment affects these

variables individually as well as together by creating an index where we add the dummies together and divide by 11.

<b>Enumerator:</b> “I will read some statements about men and women. Please say whether you strongly disagree, disagree, agree or strongly agree with these statements.”		
(1=strongly agree      2=agree      3=disagree      4=strongly disagree)		
1	It is better to send a son to school than it is to send a daughter	<input type="checkbox"/>
2	It is okay for women to work outside of the home	<input type="checkbox"/>
3	It is okay for women to earn more money than men.	<input type="checkbox"/>
4	Women have a right to decide what to do with the money they earn.	<input type="checkbox"/>
5	It is okay for women to travel or to leave the house for several nights to do business.	<input type="checkbox"/>
6	Men should be responsible to help with childcare when his wife is busy with business or factory job	<input type="checkbox"/>
7	Men should be responsible to help with domestic duties when his wife is busy with business or factory job	<input type="checkbox"/>
8	The important decisions of the family should be made by the men of the family only.	<input type="checkbox"/>
9	A wife should tolerate being beaten by her husband/partner to keep the family together	<input type="checkbox"/>
10	Woman should seek help if she encounters sexual harassment	<input type="checkbox"/>
11	Woman should seek legal recourse if she encounters sexual harassment	<input type="checkbox"/>

### *Time use*

There is a question about time use: “How many hours did you or your family member spend on the following activities over the last seven days?”

1. Paid work /income generating activities
2. Work outside home but unpaid (Apprenticeship, work at family business & farm etc.)
3. Work inside the home(unpaid)*
4. Sleeping
5. Eating and drinking
6. Personal care
7. School (include homework)
8. Travel time
9. Social and religious activities
10. Leisure time(watching TV, reading magazine, playing, exercising, recreation etc.)

In particular, we will use the values for 3 (household work) 9 (social and religious) and 10 (leisure) to see how they are affected.

### *Longer term effects and further heterogeneity*

We will also do the analysis separately for our different areas by splitting them into over and below median levels number of women working at baseline. We will here also incorporate contextual information on female employment from the DHS surveys conducted within buffer zones of 50 km from the factories. Geocoded data on protests will also be added.

Data collection is intended to continue over time in order to follow the same sample. The analysis at these later points will be similar albeit the first stages are likely to be weaker.

## **4. Threats to the design and power analysis**

### *Threats to the design*

Attrition: Attrition from the sample will be investigated. In particular, we will check whether attrition is related to treatment status by the following regression:

$$Attrition_i = aTreated\_Job_i + c_l + e_{i,t}$$

Where *Attrition* is dummy equal to 1 if individual *i* is not answering the fourth (or later) survey. Unless the difference between treatment and control is significantly different from zero at the 5 percent significance level, all estimations will proceed without any adjustment for attrition. If there is a statistically significant difference we will employ Lee bounds (Lee 2009). We will do a new test of balance with the sample from follow up 4 by regressing *Treated\\_Job<sub>i</sub>* on the variables one by one and together while controlling for the blocking variable *c<sub>l</sub>*. As many variables are tested we do not necessarily expect all of them to be statistically insignificant. We will also include all variables at the same time and we will deem the randomization as successful if the F-test shows that the control variables cannot predict treatment status together.

Limited variation: In order to limit noise caused by variables with limited variation, questions for which 95 percent of the observations have the same value within the relevant sample (for both treatment and control together) will be omitted from the analysis. If there is limited variation in the main outcome variable, vote intention, we will have *Protest last year* as our main outcome variable.

Missing values on independent variables: If we have missing values on variables we will code the variables as zero and include dummy variables controlling for missing status so that we do not lose observations.

Possibly weak first stages and heterogeneity in the first stage relationship: We are genuinely uncertain about the strength of the first stage in our study. It may furthermore be the case that the first stage is stronger in some areas than in others due to e.g. less other options. It is difficult to come up with a decision to handle this ex-ante but we will check the first stage in each of the 5 regions and run a separate analyses in the region where the first stage is strongest as well as one where we drop the region with the weakest first stage. Other eventualities are likely to come up during the analyses and we will have to highlight them separately in the paper.

### *Power*

Our main dependent variable will be *Intend to vote* and our main specification is the ITT with only the blocking variables as controls. We want to ensure that our sample is large enough to detect quite small effects and we also want to assess the sensitivity of the project with respect to sample size and number of factories. We therefore calculate a set of minimal detectable

effects (MDE). Such calculations hinge on the number of women surveyed and the amount of variance we can control for with the help of covariates.

We expect to have between 1000-1300 women still included in the study with around 50 percent assigned to treatment. As vote intention is a binary variable we have to make some assumptions about the share intending to vote in the control group. We expect this level to be relatively high at around 85 percent. With a total of 1000 women we will then have an MDE of 6 percentage points. If the share intending to vote in the control group is instead 90 percent we get an MDE of 5 percentage points.

## **5. Archive**

The pre-analysis plan is archived before any follow up data from the fourth round is received. We archive it at the registry for randomized controlled trials in economics held by The American Economic Association: <https://www.socialscienceregistry.org/> on June 6 2019. We will start data collection for the fourth follow up after June 12 2019. The data collection from the other three rounds is still ongoing so we do not expect the fourth round to be finished until May 2020. Conditional on funding, we will continue to collect several waves of data.

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