Repairing Information Underload: The Effects on Vote Choice of Information on Politician Performance and Public Goods in Uganda

Addendum to Pre-Analysis Plan

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Timing

This addendum to our pre-analysis plan is being registered after we collected all data, but prior to any analysis or hypothesis testing. It is a minor modification to our main pre-analysis plan (version 2, registered February 17, 2016) that outlines additional hypotheses with respect to election irregularities and reinforcing treatments across information arms that we generated in the process of collecting outcome data.

Theory

Effects Conditional on Electoral Irregularities
Some subjects were living in locations where there were greater accusations of vote fraud and voter intimidation in the general election. In affected polling-station areas, we expect to see lower voter turnout and less sincere voting, particularly among opposition supporters (Hafner-Burton, Hyde and Jablonski 2015). We also expect that violence or fraud in earlier rounds of voting may serve as an additional negative signal of candidate type (Magalon 2006). If so, we expect, first, that this reduction in sincere voting will result in a weaker treatment effect of all types of news among supporters of non-NRM candidates. Second, even in non-exposed areas, we may also see a reduction in sincere voting among non-NRM voters due to national media reporting of violence and fraud during the presidential election. Third, we expect that exposure to violence, by sending a bad signal of candidate type, will shift performance priors downward, thereby reducing the saliency and effect of bad news information relative to control. We will perform statistical tests of these hypotheses on relevant subgroup partitions using randomization inference through difference-in-means and model-assisted randomization inference with covariates.

Reinforcing Treatment Effects
Some subjects in the study were eligible to a reinforcing treatment from both arms of the experiment. That is, a subset of subjects were eligible to receive good news about how their district performed with respect to budget accountability and also good news about the quality of the public service — roads, schools, health clinics, or water access — that they deemed most important. Others were eligible to receive reinforcing bad news. Subjects treated with reinforcing news can be compared to subjects in the same villages and that were eligible for the reinforcing news but instead were assigned to control for one or both treatment arms. In the case where not enough subjects are available for this test within a single village, we will collapse village blocks to the politician level and then to the district level. We hypothesize that subjects treated with reinforcing news for budget accountability and public services should be more inclined to update their beliefs for candidate effort, candidate integrity and to vote or not for the incumbent candidate according to the type of news. Effects on voter uncertainty about candidate quality and
effects on turnout should be consistent with what was written in the main pre-analysis plan. We will therefore perform statistical tests on these subgroup partitions using randomization inference difference-in-means analysis and model-assisted randomization inference with covariates.

**Hypotheses**

**Electoral Irregularities**

**H12:** Information effects – both positive and negative – on turnout will be weaker for voters that align with the opposition party and were exposed to local electoral irregularities.

**H13:** Information effects – both positive and negative – on votes for incumbent will be weaker for voters that align with the opposition party and were exposed to local electoral irregularities.

**H14:** Information effects – both positive and negative – on turnout will be weaker for voters that align with an opposition party and were exposed to national electoral irregularities.

**H15:** Information effects – both positive and negative – on votes for the incumbent will be weaker for voters that align with the opposition party and were exposed to national electoral irregularities.

**H16:** The effect of bad news on turnout will be weaker for voters who were exposed to electoral irregularities committed by the incumbent’s party.

**H17:** The effect of bad news on votes for incumbent will be weaker for voters who were exposed to electoral irregularities committed by the incumbent’s party.

**Reinforcing Treatments**

As a secondary analysis, we plan to retest H1a, H1b, H3a, H3b, H4a, H4b, H7a, H7b, H8a, H8b outlined in our main pre-analysis plan among the subgroup of subjects eligible for either good news or bad news in both budget accountability and public services treatment arms. More formally, \( \ell^+ \) becomes the subgroup eligible for good news on both treatment arms and \( \ell^- \) becomes the subgroup eligible for bad news on both treatment arms. We hypothesize that assignment to the reinforcing treatment-treatment condition will have a larger effect on the outcomes of interest in the hypotheses listed above, as compared to assignment to either placebo-treatment or placebo-placebo treatment assignment for those subjects in both \( \ell^+ \) or \( \ell^- \).

**Update on Sampling**

We endeavored to use a nationally representative sample of districts (LC V) and then polling stations using a hierarchical sampling procedure. We sought to use the sample of 28 districts selected by Twaweza’s Uwezo project to measure educational outcomes based on independent
testing of school children. The sample of districts is nationally representative and stratified by region of the country. However, because Residential District Commissioners in two of the districts — Namutumba and Moyo — were unwilling to grant our field team leaders approval to conduct research, we were not able to work in these districts. We had time to replace Namutumba with Kamuli, which contains a similar tribal and linguistic makeup and has produced educational data from an earlier round of Uwezo. Because the RDC in Kyegegwa was very late in providing approval, there is a smaller number of respondents from this district, compared to the others. Within each of these 27 districts, we identify approximately 30 polling stations that fall within the randomly-selected enumeration areas in the 2014 Uwezo study. These enumeration areas are based on blocks used for the 2012 census. See our implementation report for more details.

**Estimation Summary**

*Table 1. Summary of estimation strategy for hypotheses outlined in addendum*

<table>
<thead>
<tr>
<th>Hypothesis</th>
<th>Outcome Measure</th>
<th>Treatment</th>
<th>Analytical Method</th>
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</thead>
<tbody>
<tr>
<td>H12</td>
<td>Difference between reported turnout and stated pre-treatment intention to turnout, such that $Y \in {-1, 0, 1}$.</td>
<td>Good/bad news defined by individuals’ prior. Partisanship defined as the self-reported party alignment in baseline survey. Irregularities are defined as below.</td>
<td>Randomization Inference on (4) for $\tau_2$ for the relevant subset of subjects.</td>
</tr>
<tr>
<td>H13</td>
<td>Difference between vote for incumbent and stated pre-treatment intention to vote for the incumbent, such that $Y \in {-1, 0, 1}$.</td>
<td>Good/bad news defined by individuals’ prior. Partisanship defined as the self-reported party alignment in baseline survey. Irregularities are defined as below.</td>
<td>Randomization Inference on (4) for $\tau_2$ for the relevant subset of subjects.</td>
</tr>
<tr>
<td>H14</td>
<td>Difference between reported turnout and stated pre-treatment intention to turnout, such that $Y \in {-1, 0, 1}$.</td>
<td>Good/bad news defined by individuals’ prior. Partisanship defined as the self-reported</td>
<td>Randomization Inference on (4) for $\tau_2$ for the relevant subset of subjects.</td>
</tr>
<tr>
<td>H15</td>
<td>Difference between vote for incumbent and stated pre-treatment intention to vote for the incumbent, such that $Y \in {-1, 0, 1}$.</td>
<td>Good/bad news defined by individuals’ prior. Partisanship defined as the self-reported party alignment in baseline survey. Irregularities are defined as below.</td>
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</tbody>
</table>

**Measuring Electoral Irregularities**

Electoral irregularity events will be measured using reports from Ugandan media and other regional news outlets. We will also incorporate reports from election monitors and civil society NGOs such as Human Rights Watch as available. We will code both electoral intimidation/violence events and electoral fraud events. Violence/intimidation events will include acts of violence, threats of violence and arrests of party agents or candidates. These acts can be committed by opposition officials, government officials, party agents or citizens. Fraud events will include visible behavior that violates Ugandan electoral law or behavior noted as irregular by election monitors, including ballot stuffing, ballot box tampering, inordinate delays in polling, inflated registration figures, and “ghost” voters. For the purposes of analysis we will create an electoral irregularity variable which will be a count of all fraud or violence/intimidation events in a respondent’s district between 30 days prior to the Presidential and Parliamentary election on 18 February until one day after the sub-county elections on 9 March 2016. We will measure and
estimate both the direct exposure to electoral irregularities as well as the spillover effect from national irregularities.