Appendix D

Further Details, Chapter 5

D.1 SMS AVERAGE TREATMENT EFFECTS

In the main text, I show difference-in-differences estimates comparing the effects of foreign aid on changes in incumbent support. This estimate does not necessarily identify the effect of providing the SMS treatment because (1) not all respondents were eligible to receive SMS messages, and (2) the treatment effect on the treated could have plausibly spilled over onto untreated individuals.

To estimate the effect of the SMS message directly and independently of the effect of aid, we use the following estimation approach:

$$y_{ijt} = \beta_1 m_j + \beta_2 y_{ijt-1} + \epsilon_{ij} \tag{D.1}$$

Here, $m_j = 1$ if a respondent was sent an SMS treatment message and 0 if a respondent was sent an SMS placebo message. Standard errors are clustered at the school level (the level of treatment assignment).

Since m_j was randomly assigned, β_1 should identify the causal effect of receiving an SMS message within each aid-eligible group in Table 5.1.

One important thing to note is that, for power reasons, we did not randomize the SMS treatment within the aid-recipient group (Group A). So for this group, we estimate the following equation:

$$y_{ijt} = \beta_1 m_i^* + \beta_2 y_{ijt-1} + \epsilon_{ij} \tag{D.2}$$

Unlike above, m_j^* equals one if a respondent was eligible to receive an SMS message (i.e., provided a valid phone number). It equals zero if a respondent was ineligible. Since there was no random assignment and

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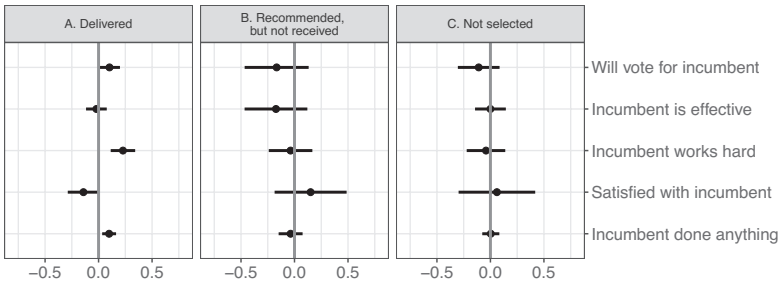


FIGURE D.1 Effect of SMS messages on outcome variables (95 percent CI)
Note: This plot shows the change in survey responses from baseline among those receiving an SMS treatment message compared to those receiving an SMS placebo message (except in the delivery group as described above).

respondents with phone numbers differ in important ways from those without, there are reasons to doubt that this is a causal estimate. For one, the SMS treatment effect is only identified under the assumption of parallel trends in outcomes across eligible and noneligible respondents. As I discuss in the main text, this is a plausible assumption; however, it cannot be confirmed directly. Additionally, since SMS recipients often communicated with others in their community, it is likely that estimates for this group violate the stable unit treatment value (SUTVA) assumption. This spillover likely biases the treatment effect estimate downward.

The results in Figure D.1 are consistent with the predictions in the main text. We have comparatively weak statistical power in these tests (Ns = 575, 155, and 255 for Groups A, B, and C); so the 95 percent confidence intervals overlap with the null for some outcomes. However, in Group A, we can reject the null that the messages had no effect on voting intention or beliefs about incumbent performance. In Group C, we observe a negative effect on voting intention, though the estimate does not reach standard levels of statistical significance ($p = 0.13$). Consistent with the predictions, the results in Group B are consistent with the null.

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