The Effect of Information and Local Intermediaries on Vote Choice: A Field Experiment in Bihar

Pre-Analysis Plan

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1 Introduction

This project proposes a large field experiment in the Indian state of Bihar to be conducted in the fall of 2015, before, during and after state elections (Dates for the election have not been officially announced, but it will likely take place in September and October of 2015). The project is part of a broader comparative project funded by the Experiments in Governance and Politics (EGAP). For more information on this broader project, please see here.

The project seeks to assess the role of information dissemination on the selection of political candidates by voters. It builds on a series of recent field experiments about this question that have been conducted around the world over the past decade (Humphreys and Weinstein, 2010; Banerjee et al., 2011; de Figueiredo, Hidalgo and Kasahara, 2011; Chong et al., 2011; Casey, Glennerster and Bidwell, 2015).\(^1\) The goal of these field experiments is to understand whether providing voters with information about candidates leads them to choose "better" politicians. In India, our focus is on whether voters choose candidates with fewer criminal charges once we directly provide them with a summary of criminal charges against each candidate in the constituency.\(^2\)\(^3\) In this project we are not only interested in the effect of information, but also the identity of those delivering the information about politicians. In particular, we vary the identity of the individual providing information to the voter.

The study will take place in 600 polling booths across the state of Bihar, North India, ahead of much-anticipated state-level elections scheduled for October-November of 2015 (in several phases). State assembly elections are organized every five years, unless a government is dissolved early, and set the stage for considerable electoral battles among the main political parties. Because states play a key role in the Indian federal system regarding the distribution of benefits (Chandra, 2004), and because regional parties have increasingly become important in India, state assembly elections are now often seen as the most important elections in India.

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\(^1\)Note that Banerjee et al. (2011) provides evidence from India.

\(^2\)Candidates are required to publicly declare all criminal cases (with corresponding penal code) at the time of filing for candidacy. While candidates charged in criminal charges may perform as well or better than candidates than are not accused – we do not assume that they are low-quality representatives –, their presence in an elected assembly does not send voters a positive signal about the nature of their regime. Besides, a pilot study strongly suggested that voters associated criminal charges with poor service and poor performance.

\(^3\)It is probably the case that some charges are not well-founded and that some others are politically driven. We are however not worried about potential ethical and legal repercussions insofar as publicizing this information is already something the Election Commission of India already engages in (though with insufficient means). All we are doing here is expending on the work of the institution in charge of ensuring fairness in Indian elections.
While the specific polling booths in which the study will take place remain to be determined, there are several important reasons why it makes sense for a field experiment on information and accountability to focus on the state of Bihar. Bihar is often seen – along with the eastern part of the adjacent state of Uttar Pradesh – as a hotbed for "criminal politics" and corruption in the country (Witsoe, 2005; Berenschot, 2008; Michelutti, 2010; Vaishnav, 2012). According to Vaishnav (2012), "on a percentage basis, Bihar sends the largest number of politicians facing pending criminal indictments to its state assembly of any state in India." As can be ascertained from publicly available electoral affidavits, this alarming trend is not specific to a single party. In the last assembly elections in 2010, all the main political parties in Bihar fielded candidates facing major criminal charges in the Assembly elections, with the then ruling JD(U)/BJP coalition topping the list with 72 such nominees. An informational intervention with a stated objective to decrease the number of winning candidates facing criminal charges is thus particularly relevant to the political context of Bihar, as several local civil society organizations – especially the Association for Democratic Reforms (ADR) – have in the past attempted to mount awareness campaigns that echo our efforts in this proposal. A field experiment of the type we describe in this proposal allows us to build on these efforts and test what various types of informational interventions, and the various types of politically relevant individuals that may be the bearers of this information, can do to increase accountability against "criminal types."

2 Research Design: General Principles and Scope

In this experiment, we compare the reactions of different groups of voters that have been exposed to different treatments. As mentioned above, our treatments all take place at the polling-booth level. We vary treatments across but not within polling booths. That is, our design is a cluster-randomized experiment, with the cluster defined to be the polling booth.

There are three different experimental groups: a control group and two different interventions. In the first intervention, we provide information on criminality using our survey research team. In the second intervention, we provide the same information while enlisting locally influential individuals to disseminate the information. Details on the format, the scope and the content of these interventions are discussed below.
When measuring reactions to these treatments, we are interested in two types of outcomes:

- Official polling-booth level electoral results and polling-booth level turn-out data
- Voters’ self-reported attitudes and preferences, as retrieved as part of a post-election survey.

In order to make sense of both electoral statistics and post-election attitudes, we also run a baseline survey prior to our interventions and prior to the election.

There are, thus, three phases in this field experiment:

1. A baseline survey a month before the election in which we survey 20 voters in each polling booth selected to be part of the experiment (including those assigned to the control condition).

2. An intervention phase (in 2 out of our 3 groups).

3. A post-election survey in which we survey the same voters in each polling booth that is part of the experiment (including those assigned to the control condition). This will be conducted before the election results are announced.

The study will take place in 600 polling booths (200 in each of the three experimental groups) stratified over 25 assembly constituencies across Bihar (which mean that we will be sampling 24 polling booths in each of 25 constituencies). There are, on average, 250 polling booths per constituency so we should confidently be able to sample polling booths without serious concerns of spillovers (we highlight a strategy to minimize spillovers below). The 25 assembly constituencies within which we select these polling booths are selected due to their likelihood of having criminal candidates in the upcoming election. The precise details of the sampling strategy are included in section 4. In each polling booth, 20 randomly selected voters (as per the most recent voting list) will be surveyed. As this is a panel study, we will attempt to interview the same respondents in the baseline and post-election surveys. If a respondent cannot be reached in the post-election survey, another respondent will be randomly drawn to reach 20 survey respondents per polling booth.4

4In agreement with our Bihar-based implementing partner, SUNAI, which has consulted local authorities such as the police and the election commission, we have decided to exclude from the sample 25 constituencies (out of a total of 243). The reason behind these exclusions from our sampling frame varies. Some constituencies are extremely
3 Interventions

The two interventions (hereafter referred to as treatment 1 and treatment 2) share a common component. In treatment 1 and treatment 2, we will disseminate publicly available information on pending criminal charges against each candidate in the constituency. In each intervention, $\frac{2}{3}$ of the households in each selected polling booth area will receive a flyer/calendar providing information on all of the candidates running in the relevant assembly constituency. Voter lists are typically split into neighborhoods, and all neighborhoods will be reached. This flyer will mention the number of serious, minor and total criminal cases faced by all candidates on the ballot, as well as the specific charges brought against these candidates. In order to provide recipients of the flyer with a benchmark, each flyer will in addition provide information about the average number of serious, minor and total criminal cases faced by candidates in other constituencies in the same subdivision of the state. The information on the flyer will also be explained orally to each household using a standardized prompt. Finally, the enumerator will ask respondents to keep this prompt visible in their household up until after the election.

These flyers will present the information in a simple yet visually appealing way incorporating the number of criminal charges, and serious criminal charges, faced by each candidate. Importantly, departing from the usual practice in the aforementioned studies, the persons delivering these prompts will not simply drop this material in front of the respondent’s home. They will be tasked with explaining it to every household member they manage to meet.

One of the core aims of the EGAP comparative project is to isolate the impact of providing "good" and "bad" information to voters. Our design allows us to test not one but two natural def
initions of good and bad news. Voters may be sensitive to the extent of criminal charges among candidates in the constituency, in which case we expect those candidates displaying more criminality than the mean/median candidate in the constituency to be penalized by voters more in the elections. Alternatively, voters may only be concerned about whether a candidate is broadly a good or bad candidate with respect to his/her level of criminal behavior as compared to similar constituencies. In this case, voters will primarily respond to the benchmarks provided in the flyer.

Polling booths selected to be part of our control group will not receive these flyers. Polling booths selected to receive either treatment 1 or treatment 2 will receive these flyers. The main difference between treatment 1 and treatment 2 is that the identity of the person delivering these flyers and explaining them to voters.

In treatment 1, enumerators from our implementing partner in Bihar (SUNAI) deliver this information. To the best of our knowledge, interventions have so far relied on such a delivery mechanism (i.e., using survey teams and/or NGO workers that rarely belong to the community to which voters themselves belong). While this may not be a problem if the treatment is constituted of materials anonymously delivered at voters’ houses, this should matter in the context of a door-to-door campaign.

Accordingly, in treatment 2, we enroll locally recruited intermediaries and/or other locally influential citizens from the polling booth area in order to disseminate this same information. Successful civil society and political campaigns often conduct door-to-door activities in India, with parties and organizations usually enlisting a variety of locally influential notables, intermediaries or "brokers" in order to carry a message (and promises, including promises in kind) to voters (Kruks-Wisner, 2011; Bussell, 2015). The way in which campaigns unfold in India suggests that door-to-door campaigns may be more efficient when it comes to convincing voters. This is, for instance, the way in which the Aam Aadmi Party (AAP) reportedly won the state elections in Delhi in December 2013 and February 2015 on a clear anti-corruption platform. Rather than flooding voters with flyers and written ads, the AAP understood fairly early that they had to enlist locally relevant intermediaries who already have a relationship with voters (either because they mediate relations between voters and the state, or because they run a successful NGO, or both).
Enlisting "messengers" that are locally known to voters may be important for at least two reasons. First, respondents may not trust or even be willing to listen to information communicated to them by outsiders. Second, even if they listen and accept the information delivered by outsiders as true, they may not reject "criminal candidates" if they depend on these politicians (for clientelistic reasons) or on other opinion leaders more than they depend on the individual delivering this information. In a clientelistic system, it is not entirely surprising that voters would be more loyal to their "bad" politicians than to an outsider participating to a one-time campaign in favor of clean politics. Even if politics is not particularly clientelistic, voters may follow the preferences of "opinion leaders," such as local caste or religious leaders, who have ties to certain politicians. Given that voters depend on local intermediaries for many of their interactions with state officials, the theoretical intuition here is that we may be able to overturn their loyalty to bad politicians if we convince local and influential intermediaries to take part in the intervention. Alternatively, we may get larger effects from the treatment when the information on the flyers support the preferences of these local intermediaries.

We identify these local intermediaries through our baseline survey (see last set of questions in attached baseline survey), in which we ask people to name the 3 most important intermediaries in their polling booth area. Our implementing partner (SUNAI) will then contact the 2 most frequently named social workers in each polling booth area and provide them with an opportunity to work for us for a few weeks in the lead up to the election.\footnote{Take up can be endogenous to political characteristics or to the treatment. But this is only a problem to the extent that it adds heterogeneity to our sample of influencers. What we are really interested in is the comparison between enumerators, who are outsiders and do not have a relationship with voters and ?influencers?, who are insiders and have an ongoing relationship with villagers.} In practice, we will ask each respondent for three local (i.e., in the sampled or adjacent polling booth area) influential individuals as an answer to the following question:

\begin{quote}
In India, poor people often need help accessing state benefits and documentation. When people need help to access documents (for instance: aadhar card, BPL, caste certificate, etc.) or to gain admission to a hospital in this/village/block/hamlet, who do they seek help from?
\end{quote}

We will then tabulate the names across all of the respondents in the polling booth area and select two influential individuals, attempting to hire the most named individuals and working down a list in descending order of popularity if these individuals refuse to work with us. These...
individuals will also be asked to conduct a door-to-door campaign during which they will be asked to target \( \frac{2}{3} \) of the households in the polling booth area, subject to weekly unannounced monitoring from partnering organization.\(^{11}\) In order to incentivize the individuals, we will reward them with Rs. 2500. \(^{12}\) Besides, the post-treatment survey will ask informational questions, corresponding to the information in the flyer, to our respondents, allowing us to evaluate the extent to which the treatment was effectively received when these intermediaries delivered it. When targeted by our survey team, we will keep track of exactly which voters have been reached, but this will be more difficult in the case of intermediaries. We will also keep track of which households have kept our calendar as a proxy for delivery of message across all treatments.

We also believe we will gain many important insights about local politics in India from these local intermediaries. Accordingly, we will conduct a survey of these individuals. See appendix D for the draft survey for these local intermediaries. Altogether, this design implies a total of 3 experimental groups (of equal size):

I. Control (no intervention)

II. Door-to-Door campaign led by a member of our partnering organization (SUNAI) – Enumerators will reach \( \frac{2}{3} \) of households in a polling booth area in a door-to-door campaign.

III. Door-to-Door campaign led by a locally influential individual – Two local campaigners will be selected from the baseline survey. They will be enlisted to reach \( \frac{2}{3} \) of households in a polling booth. The campaigner will receive an incentive salary, be monitored, and her performance (i.e. her efficiency in informing voters of the polling booth area) will be measured through a post-treatment survey.

\(^{11}\)Hired influencers attend a 5-day-long meeting at the block level prior to the intervention, during which they receive clear instructions about this target, and about the message to convey (they take a brief survey administered by the supervisor at this time). The message conveyed will include an instruction to display and keep the calendar/flyer in the home. To monitor the efforts of these locally hired intermediaries, we assign a supervisor from our implementing partner (SUNAI) to monitor the works of these intermediaries in each polling booth. We also incentivize the work of these intermediaries by measuring, in our post-election survey, the percentage of polling booth residents who are aware of the information contained on the flyers. To ensure that this amount of households is reached by the intervention and to ensure that the treatment is delivered properly, we combine two monitoring strategies. We first have our supervisors drop by unannounced in the polling booth area twice during the 7 days during which the intervention takes place. This visit should include an accounting of which households have been visited until that point (as well as a random check for a small percentage of households).

\(^{12}\)Rs. 2500 was chosen to ensure high take-up. Given incomes in rural Bihar, rs. 2500 is an extremely strong incentive to participate for most individual referred to as influential and to continue participating until the end of the intervention.
3.1 Prompt in Treatment 1 and Treatment 2

As noted above, each voter targeted by either of our interventions (treatment 1 or treatment 2) will receive a flyer containing information about the criminal charges faced by political candidates in her/his constituency.

This information treatments differ from information treatments delivered in previous experiments of this type on at least three dimensions: 1) we focus on criminal charges currently brought against candidates; 2) the kind of candidates we provide information about (we provide information about all candidates, and not only about incumbents); and 3) the presentational style of these interventions (we move away from a pure report-card to present the information in a hopefully catchier way).

We propose a shorter, more attractive and cognitively more accessible informational intervention. In our design, respondents in both our treatment conditions are presented with information regarding a single topic: criminal cases against candidates. Our flyer will include information about serious cases, those involving financial improprieties and those involving serious violence (e.g., murder and rape), and other cases. We assume this to be the most basic and natural indicator of the quality of representation, vis-à-vis criminality, to voters.

While the final version of this document is not yet available (it will only be ready when we know the names of candidates in each constituency), its general format has already been decided (and validated by our implementing partner in PATNA, Sunai). See appendix D for the draft of this prompt.

As can be seen from the doc, the prompt contains a general slogan in the header which reads, "the more honest your representative, the more work will be done for you."\textsuperscript{13} It also contains information about the number of criminal cases in which each of the candidates running in the constituency is potentially accused on the right. In order to be visually appealing, and in the hope that targeted voters would not readily discard the flyer after the interviewer explains it to them (and potentially place it on a wall), the document also includes a 2016 calendar mentioning holidays in the state.

A mention at the bottom of the document explains that this is a independent, non-partisan
campaign sponsored by an American University and a Delhi-based Think-tank, and implemented by a Bihar-based partner (SUNAI). It will also provide respondents with a website and a local phone number from which they will be able to obtain further information on the project.

4 Sampling

The research team will select 25 assembly constituencies (ACs) from which the sample will be drawn. Within each AC, 24 polling booths\textsuperscript{14} will be randomly selected according to a protocol described below.\textsuperscript{15} Within each polling booth, 20 respondents will be randomly selected for survey. The randomization at the level of the polling booth and the voter will follow the protocol described below.

Since the efficacy of the treatment is predicated on disseminating information about criminals, we do our best to maximize the probability of selecting ACs with serious criminal candidates who are competitive. Unfortunately, since candidates are not known ahead of time, and incumbency rates are low, we have to rely on educated guesses. Comparing 2005 to 2010 electoral results, we however found that if a constituency had its top candidates as serious criminals in 2005, it had over an 80\% chance of having a competitive candidate who was a serious criminal in 2010. We further found that if just the winner was a serious criminal in 2005, then the constituency had a 68\% chance of having a competitive candidate who was a serious criminal. Accordingly, we rely on the number of charges brought against candidates in the 2010 elections in order to select the constituencies in which the experiment will take place.

In other words, we priorily sample constituencies which had candidates with serious criminal cases in 2010. Because elections in Bihar are typically conducted in six or seven phases, our selection process is however slightly more complex than this. This is because we have to avoid selecting constituencies in which voting will take place during the last phase, as selecting these constituencies (in which voting will take place right before the results for all constituencies are announced) would not leave our partner enough time to complete the post-survey, which we

\textsuperscript{14}If at all possible, we want to stick to the initial polling booths selected from the randomization. If, however, a polling booth cannot be reached or surveyed for some reason, replacement polling booths will be provided.

\textsuperscript{15}Note that 8 polling booths each will be selected for each of three treatment conditions (control, treatment 1, treatment 2). However, the actual selection of polling booths and households to be targeted in treatment 1 and treatment 2 will be conducted after the baseline survey results are provided. This is to allow for blocking on pre-treatment covariates at the village level before treatment delivery.
want to complete before results are announced. Given this complication, we have so far relied on 2010 numbers to choose not 25 (our target) but 33 ACs, with the understanding that 8 of these will be dropped when the phases for the upcoming election are announced.

Concretely, in order to select our constituencies, we took every constituency (in which our partner deemed it was safe to work) where both competitive candidates were serious criminals in 2010. This yielded 22 constituencies. We selected the other 11 constituencies by selecting constituencies with the 11 highest vote shares for serious criminals where the winner was also a serious criminal. We believe this maximizes the likelihood that our treatments will provide salient information to voters.

The set of 33 pre-selected constituencies is listed in appendix F.

4.1 Protocol for Selecting Voters

Within each polling booth 20 respondents will be randomly chosen from existing and publicly available voters’ lists. We will break the population into 40 equally sized intervals (or very close), and randomly select one voter from every other interval to make sure we have geographic spread over the polling booth.

4.2 Details of Sampling Strategy

The goal of the sampling design is to draw a random sample where the polling booths are sufficiently far from each other and no two respondents are from the same household. In order to do this, we select polling booths in a way to minimize the polling booths with "polling booth numbers" close to each other (because these booths likely to be spatially clustered). To do this, we break the list of polling booths in each constituency into 24 equal sized intervals (or as close as possible using a greedy algorithm) based on polling booth number and then randomly select one polling booth from each interval.

Once a polling booth is selected, a similar strategy is undertaken for the voter list. In particular, the voter list is broken into 40 equally sized segments (or as close as possible) and one voter is randomly selected in every other interval. Because two individuals with numbers that are close in the voter list number are more likely to be in the same household, this strategy the
minimizes the chance of overlap (of course, it will still occur some of the time). This will yield slight deviations from equal probability sampling, but in ways that are unlikely to significantly impact estimates (and can be controlled for after in the analysis phase).

An example should help explain the sampling strategy. Imagine a constituency with 200 polling booths. Since we aim to select 24 constituencies out of 48 intervals, the sample is broken into 40 intervals of 4 polling booths, and 8 intervals of 5 polling booths (1-4, 5-8, 9-13, etc.) and one polling booth is selected randomly from every other interval with a probability that is proportional to its size. Once the polling booth is selected, the list of voters in that polling booth will be broken into 40 equally spaced intervals (1-50, 51-100, 101-150, etc.) and one voter will be randomly selected from every other interval. In all cases, replacement of polling booths and voters must come from the same interval.

4.3 Power Analysis

We begin by discussing our exact design and power analysis. Rather than conduct a simulated power analysis, we prefer to analytically derive our sample size. The proposed study is a cluster-randomized experiment, where 24 polling booths (clusters) are selected in each of 25 constituencies for a total of 600 polling booths. Booth sizes can vary, but usually they do not vary too much. While the Election Commission of India has guaranteed that no booth has over 1500 voters, a standard rule of thumb is 1000 voters per polling booth.

This implies that our experiment will be conducted over approximately 600,000 individuals with approximately 200,000 in each of the 3 treatment groups (since we select the groups to be of equal size). The estimated effect will be a (corrected) difference in proportions estimate between treatment groups for the dependent variables of interest (turnout and vote share).\(^\text{17}\)

The power analysis will assume that polling booths (clusters) have a sample size of \(m = 1000\). Note that since we are dealing with binary variables, a conservative estimate of the standard

\(^{16}\)In practice, a slight adjustment for number of voters in the polling booth will be required at the level of the polling booth, but logic is the same.

\(^{17}\)It is known that standard difference in means analyses of cluster-randomized experiments may yield biased estimates when clusters are of different sizes. While bias corrections exist, simple regression adjustment on cluster size may be preferred to maintain efficiency.
deviation of any probability, \( p \), is just:\(^{18}\)

\[
sd(p) = \sqrt{p(1-p)} \leq 0.5
\]

For a total sample size of \( n = 400,000 \), where \( n \) is the total of the sample sizes of two treatment groups, a conservative estimate of the standard error of difference in estimated proportions (of individuals turning out or voting for particular candidate) across two groups \( A \) and \( B \), \( \hat{p}_A - \hat{p}_B \), is given by:

\[
se(\hat{p}_A - \hat{p}_B) \leq \sqrt{\frac{1}{n_2} + \frac{1}{n_2}} = \frac{1}{\sqrt{n}}
\]

Consistent with Banerjee and Pande (2011), we aim to find turnout effects and vote share effects of at least 5%. In order to generate the industry standard of 80% power with 95% confidence intervals, we aim to select a \( \sigma \) such that \( \hat{p}_A - \hat{p}_B - 2.84\sigma \geq 0 \). Plugging in the 5% standard for the difference in proportions, yields \( \sigma = 0.0178 \). There is one final concern in the power analysis, the design effect. The design effect is the inflation factor from a "complex design" applied to the variance from resulting from simple randomization in the experimental design. In short, we account for the fact that our data are cluster-randomized and that this causes inefficiency. The design effect inflates the variance by approximately \( 1 + (m - 1)\rho \), where \( m \) is sample size within the cluster (1000) and \( \rho \) is the proportion of the variance due to across cluster variance.

Since the data are binary at the individual level, we expect there to be more significant variation within clusters than across clusters (see Chakraborty et al. (2009) for this point). There aren’t any good industry standards for estimating the proportion of variance across clusters. As a conservative estimate, we let \( \rho = 0.125 \) (usually we think of \( \rho \leq 0.1 \) as low). In order to calculate the required sample size, we now solve (for \( n \)), the equation:

\[
(1 + (m - 1)\rho) \frac{1}{n} \leq \sigma^2
\]

Solving through yields \( n = 397,283 \approx 400,000 \), or 200,000 per treatment group, as we suggest. In short, we conduct our power analysis using conservative estimates of variance and clustering for

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\(^{18}\)We should also note that the variance is presumably decreased since we are stratifying on constituency, but we are not factoring this into our power calculations, so our estimates are likely conservative in that sense as well.
the outcomes of interest and find that approximately 200,000 individuals per treatment group reaches 80% power with 95% confidence intervals when assuming a 5% effect in both turnout and vote share dependent variables.¹⁹

## 5 Hypotheses and Pre-Registered Analysis

For a list of variables that will be pre-registered for this analysis, please see the spreadsheet in appendix C. In our analysis, we consider six dependent variables, four at the level of the polling booth and two at the individual level. In our analysis, we define a criminal as any individual who is charged in criminal cases, as per the publicly available affidavits provided by candidates. In general, we trust the polling booth level data more as it is calculated from official data and does suffer the biases involved in survey response. Our polling booth level measures focus on the extent to which voters demonstrate support for criminals, either by explicit support or by showing up at the polls (or failing to do so). The four dependent variables at polling booth level can be calculated from official data after the 2015 elections:

- **D1.** Whether a criminal candidate won in the polling booth (binary)
- **D2.** Whether a criminal candidate was competitive, i.e., top two (binary)
- **D3.** Aggregate vote share for criminal candidates (between 0 and 1)
- **D4.** Aggregate turnout

The two dependent variables at the individual level will be collected from survey data in the post-survey:

- **D5.** Vote choice for a criminal (binary)
- **D6.** Turnout in election (binary)

¹⁹Note that while we have provided conservative estimates in a number of ways, we have not accounted for variation in polling booth size, which is likely to generate some increase in variance.
5.1 Aggregate Impacts of Treatment

In this subsection, we discuss our hypotheses and proposed analysis.20

At the most fundamental level, before exploring mechanisms, we seek to determine whether the treatments had an overall impact on vote choice for criminals and on turnout. In the following discussion, the word "treatment" will mean an indicator capturing the difference between two treatment groups (we have three in the analysis), unless otherwise stated, and there are three such differences in each analysis. Thus, for instance, the "treatment" may capture the difference between the survey team and an influencer spreading information. In this case, we would restrict the analysis to these two groups and put an indicator for whether an influencer disseminated the information. For the rest of the analysis plan, it should be assumed (unless otherwise stated) that we are testing these three treatment group differences in each analysis.

Our most robust analysis will take the form of a set of suitable predictors (discussed for each hypothesis below) and a set of random effects for the constituency level for the polling booth level analysis, and a set of random effects at both the constituency and polling booth level for the individual analysis. Let \( y_{i,t} \) be an outcome of interest for individual \( i \) in treatment group \( t \in \{0, 1, 2\} \), and let \( X_{i,t} \) be a set of control variables of interest. In each of our models the set of predictors will be identical in each treatment group. We run three versions of each model, without random effects, with just a constituency-level random effect, and with constituency-level and polling booth random effects. The last model should be viewed as the most robust, as it properly addresses the design effects.

In the analysis, we estimate a parameter vector \( \beta_t \), and random effects \( \alpha_{j[i],t}^{\text{const}} \) and \( \alpha_{k[i],t}^{\text{pb}} \) where \( j[i] \) and \( k[i] \) refers to constituency \( j \) including individual \( i \) denotes polling booth \( k \) including individual \( i \):

\[
y_{i,t} = X_{i,t} \beta_t + \alpha_{j[i],t}^{\text{const}} + \alpha_{k[i],t}^{\text{pb}} + \epsilon_{i,t}
\]

(5.1)

\[
\alpha_{j,t}^{\text{const}} \sim N(0, \sigma_{\text{const},t}^2)
\]

\[
\alpha_{k,t}^{\text{pb}} \sim N(0, \sigma_{\text{pb},t}^2)
\]

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20All analysis will account for inclusion probabilities based on the estimated population size of polling booth and the probability of inclusion. Note that because of unequal probabilities, and errors in the voting lists, it is not feasible to calculate the without replacement estimator. Accordingly, we use the with replacement estimator.
\[ \varepsilon_{i,t} \sim N(0, \sigma^2_t) \]

where the \( \sigma^2_{\text{const},t} \), \( \sigma^2_{\text{pb},t} \), and \( \sigma^2_t \) terms are variances for the random effects at constituency level, random effects at the polling booth level and data-level error, respectively, for each treatment group. Our estimates of an average treatment effect can be given from differences in averages of the predicted values in each treatment group, and variances of the estimates can be calculated from simulation. In effect, these regressions calculate "response curves" for each of the treatment groups.

**H1. The treatment increased/decreased support for criminals.**

**Analysis:** At the polling booth level, we test the hypothesis with D1, D2, and D3. Each linear regression will have the value of the dependent variable in 2010 coded from official data. At the individual level, D5 will be tested in a linear regression with the value of the dependent variable in the baseline. We may further consider varying effects of all coefficients by the constituency level in the multilevel model framework. We will also look at the simple difference in means of the different treatment groups with random effects, but this is not our core specification.

**H2. The treatment increased/decreased turnout.**

**Analysis:** At the polling booth level, we test the hypothesis with D4. Each linear regression will have the value of the dependent variable in 2010 coded from official data. At the individual level, D6 will be tested using a difference in means by treatment group (i.e., set of predictors is just a constant) and appropriate random effects.

We envision an underlying voter model where each individual has a prior on salient characteristics, over which individuals evaluate their utility of supporting each candidate. The treatment can affect this utility function in two ways. It may cause voters to update the prior by providing "good" or "bad" news, where good (bad) news makes the individual more (less) likely to vote for the candidate. The treatment may also directly impact the utility function by changing the salience of various characteristics.
5.2 Impact of Good and Bad News

In our study, candidates will not be known at the time of the baseline or will have just been announced and news will not have spread far. Furthermore, incumbency rates are very low in India. As such, we anticipate that voters have very weak priors on the level of criminality of each candidate in their constituency. Accordingly, we will define good and bad news according to two benchmarks. We will define providing good news as providing information that a candidate has less than the mean number of serious cases per candidate in the constituency ("good candidates"). Bad news will be defined news as providing information that a candidate has more than the mean number of serious cases per candidate in the constituency ("bad candidates"). An alternate measure of good and bad candidates will be defined with respect to the benchmark provided on the flyer (the flyer provides information about the average number of crimes faced by candidates in the same geographically clustered election phase in Bihar). Since each election phase is geographically-clustered and goes to the polls at the same time, this is a meaningful number to voters. We define four new dependent variables for this analysis, closely related to those above:

D1’. Whether a bad candidate won in the polling booth (binary)

D2’. Whether a bad candidate was competitive, i.e., top two (binary)

D3’. Aggregate vote share for bad candidates (between 0 and 1)

One dependent variable at the individual level will be collected from survey data in the post-survey:

D5’. Vote choice for a bad candidate (binary)

Using this basic theoretical framework, we have the following main hypotheses:

H3a. Positive information – which we define as information suggesting that the politician is relatively "less criminal", compared to alternatives – increases voter support for politicians.
H3b. Negative information – which we define as information suggesting that the politician is relatively "more criminal", compared to alternatives – decreases voter support for politicians.

Analysis: The analyses from H1 will be repeated with the above transformed dependent variables.

Another way good and bad news might affect voter behavior is that it might make individuals more or less willing to turn out to the polls. For these purposes, we require a measure of good and bad news that aggregates across all candidates. For this purpose, the within constituency benchmark will not make much sense. We define good and bad news with respect to the benchmark provided in the flyer. Using this framework, we test the following hypotheses at the booth and individual level:

**H4a. Bad news decreases voter turnout.**

**H4b. Good news increases voter turnout.**

Analysis: At the polling booth level, we test the hypothesis with D4. Each linear regression will have the following predictors: a) the value of the dependent variable in 2010 coded from official data; b) a polling booth measure of good news; and c) all interactions between a) and b). At the individual level, D6 will be tested in a linear regression with a measure of good news at the individual level.

### 5.3 Salience

The treatment might also impact the salience of criminality in a voting decision. In order to test this psychological relationship, we ask respondents in the baseline and post-survey whether they would support criminal candidates, given other factors, through a vignette (Q27 in the baseline). Lower levels of support in the post-survey as compared to the baseline suggest increased salience of criminality, and the opposite suggests decreased salience. Since the answer to the vignette is at the individual level, a polling booth level predictor will be constructed by taking respondent averages in the baseline and post-survey, and compared between baseline and post-survey. Accordingly, we test the following two hypotheses:
H5a. Increased salience for criminality decreases support for criminal politicians.

H5b. Decreased salience for criminality increases support for criminal politicians.

Analysis: The analysis in H1 is extended by adding a predictor for increased salience of criminality at the polling booth and individual levels, and all possible interactions. Note that the coefficients on the treatment are biased (as difference in salience is a post-treatment predictor).

5.4 Testing Other Effects and Hypotheses

H6. Positive (negative) information increases (decreases) voter beliefs in candidate integrity.

H7. Positive (negative) information increases (decreases) voter beliefs that candidate is hard-working.

Contrary to some of the other EGAP projects, we will not be able to test this from candidate-specific questions. Since candidates will not be known by the time the baseline takes place, we cannot measure the evolution of beliefs about specific candidates. We however have a party-specific question about the image of each of the main parties in the baseline and post-survey. This allows us to test the effect of positive (negative) information on voters’ beliefs about parties.

H8. Politicians mount campaigns to respond to negative information.

To test this hypothesis, we will directly ask whether politician mounted a campaign to respond to our information (Q45 in the post-survey). One may also wonder if those candidates who have less criminality mount campaigns to further disseminate the information.

5.5 Heterogeneous Effects

We now declare hypotheses on various heterogeneous effects of interest. The hypotheses H9-H16 can be tested in a large regression, including those predictors to the specifications for H1, H2, H3a, H3b, H4a, and H4b, and the predictors described in this subsection. For individual-level predictors, we will take averages by polling booth for polling booth level predictors.

H9. Information effects are more positive for voters that do not share ethnic identities with the politician.
We collect information on the caste and religion of our respondents. We will also code the same for the candidates, once the lists are available. Co-ethnicity, using major political faultlines, will be coded as: Yadav, Koeri, Kurmi, Dusadh/Paswan, Ravidas, Other Dalit, Muslim, Brahmin, Other. Using these categorizations, a respondent will be considered co-ethnic if he/she has the same ethnic category as a candidate. We can use number of co-ethnic candidates in the constituency.

H10. Information effects are more positive for voters with weaker partisan identities.

Q28 in the baseline asks about whether the respondent is a long-time supporter of any party. Straightforwardly, we will say someone has stronger partisan identity if he/she answers "yes" to this question.

H11. Information effects are more positive for voters who have not received clientelistic benefits from any candidate.

In the post-survey, we ask whether respondents received clientelistic benefits using Q48-Q51. We will test the robustness of these effects with Q48 and Q50 (an answer of "yes" will be viewed as engaging in clientelistic exchange).

H12. Informational effects are stronger in informationally weak environments.

We calculate exposure to information using Q18-Q20 in the baseline at the individual level. We use this to build a polling booth level measure of the information environment. Straightforwardly, this will be the average (or item-response constructed scale) on Q18.2, Q19.2, and Q20.2.

H13. Informational effects are stronger in more competitive elections.

Ex-post, we will code the margin of victory between the top two candidates in each constituency.

H14. Informational effects are stronger in settings in which elections are believed to be free and fair.

We use Q41.1 in the baseline to determine whether the voter believes the election to be free and fair. An answer of "agree" will be viewed as a belief in fairness of the election.
H15. Informational effects are stronger the more the information relates directly to individual welfare.

We use the 4 question in Q26 to construct an index with an average and item response.

H16. Informational effects are stronger the more reliable and credible is the information source.

This is directly tested from the experiment. We can also use the answers to Q21 to understand the credibility of our delivery mechanism (see options 3 and 4).

H17. Information effects–both positive and negative–are stronger when the gap between voters’ prior beliefs about candidates and the information provided is larger.

This is difficult to test because of low incumbency rates and because candidates will not be known at the baseline. We do, however, have party-specific information in the baseline and post-survey.

H18. Informational effects are stronger when information is provided in public settings.

This is not tested. It is not feasible to fully isolate individuals during treatment delivery. We will, however, collect data on who was present at the time of treatment delivery.

H19. Hawthorne effects do not drive informational effects.

We can use the difference between individual response in the post-survey and official data at the polling booth level (which is more trustworthy) to determine if we are seeing strong Hawthorne effects.
References


**Enclosed**

Appendix A: Baseline Survey
Appendix B: Post-Survey
Appendix C: Connection to MetaPAP
Appendix D: Intermediary Survey
Appendix E: Draft Flyer
Appendix F: Selected Constituencies