

Diminishing the Effectiveness of Vote Buying: Experimental Evidence from a Persuasive Radio Campaign in India*

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Abstract

During the 2014 Indian general elections, we carried out a large-scale field experiment to evaluate the electoral effects of an information campaign to persuade voters to reject politicians who engaged in vote buying. We broadcast ads on randomly selected radio stations, emphasizing the incentives of politicians who distribute “gifts” to voters and the likely economic consequences of electing them. The ads appealed to voters to act in their economic self-interest and urged them to renege on any promises to vote for such politicians. By strategically timing the broadcasts after the prescribed electioneering period but before voters went to the polls, we are able to estimate the electoral effects of educating and persuading voters while holding politician behavior fixed. Prior to the announcement of the election results we interviewed approximately 400 journalists, asking them to identify parties that had engaged in vote buying in different areas. Using official electoral data, we find that exposure to the radio campaign significantly decreased the vote share of these putative vote-buying parties, with estimates ranging from 4 to 7 percentage points (depending on specification) and had a small negative but statistically insignificant effect on the voter turnout rate.

JEL Classification: D72, D83, K42, O12.

Keywords: Vote Buying, Elections, Corruption, Voters, Persuasion, Radio, Mass Communication, Field Experiment, Randomized Experiment, India.

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1 Introduction

“Vote buying” refers to the distribution of cash or other material benefits by politicians before elections to persuade voters to vote for them. From a welfare perspective, there is growing evidence that vote buying is accompanied by under-provision of public goods (Baland and Robinson, 2007; Khemani, 2015). Its prevalence across the world (Schaffer, 2007b) seems to run counter to the predictions of standard rational choice models, which suggest that vote buying should fail to generate votes for the buyer because the “contract” between buyers and sellers is unenforceable under the secret ballot. Two mechanisms have been proposed in the economics literature to explain how vote buying could influence voters despite ballot secrecy.

One is that vote buying is effective because the informal agreement plays on voters’ feelings of moral obligation to reciprocate a “gift” from a political party or candidate by voting for them. Even relatively weak norms of this kind may be sufficient to sustain high levels of compliance with informal agreements (MacLeod, 2007). Using experimental measures of reciprocity along with survey data from Paraguay, Finan and Schechter (2012) find that vote buyers target gifts towards individuals with greater propensity to reciprocate.

The other is that parties may use pre-electoral transfers to signal competence in providing post-electoral benefits (Drazen and Eslava, 2010; Rogoff, 1990; Rogoff and Sibert, 1988). In contrast to reciprocity, which is backward looking, under the signaling mechanism vote buying is effective because rational forward-looking voters update their beliefs about the post-electoral pay-off from voting for each party after observing the vote-buying behavior of the competing parties.

There is growing interest from governments, civil-society organizations, and multilateral agencies in devising cost-effective strategies to curb vote buying and to diminish its influence on electoral outcomes. Law enforcement agencies spend significant resources to curb vote buying by targeting the supply side. India, for example, deployed approximately 120,000 federal police troops during the 2014 general election to prevent the procurement and transport of resources for vote buying and to enforce various measures such as bans on sale of liquor,

limits on cash withdrawals, checking vehicles at roadblocks, putting airports and railway stations under police surveillance, and even sending videographers to shadow campaigning candidates (Biswas, 2014; Ford, 2014). Voter information campaigns could complement these expensive law enforcement efforts by diminishing the influence of “gifts” or “bribes” on voter behavior. In the long run this could reduce the incentives of politicians to engage in vote buying.

What kinds of messages would be most effective at persuading voters to reject vote-buying politicians? Anti-vote-buying information campaigns have typically used moralistic or legalistic appeals to urge voters to refuse bribes and to vote their conscience.¹ The hypothesized mechanisms of vote buying effectiveness (signaling and reciprocity) suggest that the message most likely to be effective are those that reduce voter expectations of post-electoral performance (provision of public goods) by vote buyers and those that diminish voters’ perceived moral obligation to reciprocate to them.²

Vicente (2014) conducted a randomized experiment in São Tomé and Príncipe to evaluate the effectiveness of moralistic and legalistic appeals. Canvassers distributed door-to-door leaflets underscoring the illegality of vote buying and verbally urged voters to cast their ballots in accordance with their “conscience” even if they go on to accept cash from politicians. Using survey data, he finds that the campaign reduced the prevalence of bribes and diminished their effect on voter behavior. Using electoral data, he finds that the campaign increased the vote share of the incumbent party by 4 percentage points and reduced the voter turnout rate by 3 to 6 percentage points. He argues *ex post* that the intervention increased the vote share of the incumbent party because it asymmetrically reduced the ability of the challenging party to buy votes while preserving the pre-existing advantage of the incumbent party. The lack of corroborating evidence for this interpretation underscores the importance of measuring, however indirectly, which parties are actually engaging in vote buying.

¹Schaffer (2007a) provides a survey of such campaigns from across the world but argues that such messages may not be effective among poor voters – those considered most susceptible to vote buying – because they typically do not share the same norms regarding vote buying as the middle-class reformers who make those appeals.

²Schaffer similarly hypothesizes that convincing poor voters that voting for vote buyers is neither in their own nor their community’s economic interest is far more likely to be effective.

These results lend plausibility to the hypothesis that information campaigns may affect electoral outcomes, but as a practical matter, door-to-door campaigning is infeasible in large countries. Moreover, the finding that this type of intervention hurts incumbents may not be generalizable to other contexts, such as India, where incumbents seem to enjoy no electoral advantage (Uppal, 2008). Further, given the timing and nature of the intervention, it may have affected the vote-buying behavior of parties directly. It is therefore unclear how much of the observed effect can be attributed to changing voter attitudes versus changing party behavior.

Our experimental design builds on Vicente (2014) in several ways – 1) by evaluating an information campaign to educate voters about the incentives of politicians to engage in vote buying and the negative economic consequences of voting for them (in contrast to moralistic/legalistic appeals), 2) by using large-scale mass media in a large democracy to deliver the information campaign “at-scale,” 3) by strategically timing the campaign to influence voter attitudes and expectations without simultaneously influencing politician behavior, and 4) by measuring, area by area, which parties reputedly engaged in vote buying.

We selected radio as the mass medium to deliver the information campaign for several reasons. Radio reaches a wider and more economically diverse audience than television. Radio ads can be quickly and cheaply scaled up, and they may be readily adapted for use in other developing countries. Importantly, they are less susceptible to interference by vested interests than on-the-ground campaigns (e.g., those that distribute leaflets or posters) and are therefore safer to implement in regions where violence and intimidation are a concern.

The rest of the paper is organized as follows. We first lay out the conceptual framework of voter behavior in the presence of vote buying and formally model how an anti-vote-buying information campaign might persuade voters to change their behavior. Next, we elucidate the experimental design by describing the electoral setting; the information campaign including the content, timing and broadcasting media used; and the random allocation of the information campaign to different areas. This section also describes in detail how information about vote-buying behavior by parties was gathered from interviews with a large and

diverse sample of journalists. We then present the econometric specification used to analyze the randomized experiment, which addresses complications arising from geographically clustered assignment, with overlapping radio coverage areas. We find substantial effects on voting but not on turnout. We conclude by discussing the implications of these findings and suggest avenues for future research.

2 A Simple Model of Voter Persuasion

Our information campaign consisted of radio ads which dramatized the message that politicians distribute “gifts” not out of kindness but rather to buy their way into office. By characterizing vote buying as a selfish act rather than one that expresses generosity, we hoped to diminish voters’ sense of moral obligation or reciprocity towards such politicians. Further, the ads suggested that politicians incur such expenditure with an objective of recouping it from public funds, after election, through corruption. The ads highlighted the hardship (under-provision of public goods) voters are likely to experience if they vote for such politicians. The goal of this argument was to undermine the favorable assessments of politicians that had engaged in vote buying. A key feature of the information campaign was its timing – it was delivered after electioneering (including vote buying) by politicians had ended but before voters cast their ballots. Since politicians were unable to take any further action after our ads aired, the effect of the ads on electoral outcomes is solely through changes in voter attitudes and expectations.

In this section we present a stylized representative agent model to formalize the two channels – undermining reciprocity towards vote-buying politicians and undermining their favorability – through which our information campaign is assumed to persuade voters. We solve the model to derive qualitative predictions of the net effect of the information campaign on electoral outcomes of interest – the vote share of politicians that engaged in vote buying and the voter turnout rate. The setup of the model draws on relevant aspects of the institutional setting and experimental design, which we discuss in detail in Section 3.

Consider an economy with an electorate and a political regime. We assume that the

electorate is homogeneous and can be modeled by a representative agent. The political regime, on the other hand, is not homogeneous and is composed of two “factions.” One of the factions gives a “gift” to the agent whereas the other one does not.³ The vote-buying behavior of the two factions is a given feature of the election and occurs before our information campaign is delivered. Let the agent be endowed with one unit of time, which can be converted without cost into a vote for the vote-buying faction or the non-vote-buying faction. Let time and votes be perfectly divisible. The voter turnout rate in the election is $t = v + w$, and the vote share of the vote-buying faction is $s = \frac{v}{t}$, where v and w are the votes received by the vote-buying and non-vote-buying factions, respectively. Since there are only two factions, the vote share of the non-vote-buying faction is $(1 - s)$. The agent consumes the time remaining after voting as leisure (l).

In this economy, the election is the political process that produces a composite public good, which we interpret broadly to include both local public goods (e.g., water, electricity, sanitation, health, education, and law and order) and welfare benefits (e.g., subsidies and transfers). The agent has a mental representation of this political process. She recognizes that electoral outcomes influence the post-electoral level of the public good (x). She also recognizes, however, that there is uncertainty associated with the political process. We model the agent’s mental representation of the political process with voter turnout and the vote shares of the two factions as inputs, along with stochastic parameters which capture the uncertainty in the voter’s mental representation.

The agent’s mental representation of the political process of public goods production is:

$$x(s, t) = t^h s^{h_1} (1 - s)^{h_2} \tag{1}$$

where $h_1 > 0$ and $h_2 > 0$, are parameters that capture the productivity of the vote-buying and non-vote-buying factions, respectively. The influence of voter turnout on the political process is mediated by the overall productivity of the political regime which is captured

³In our setting, both the vote-buying and non-vote-buying factions are comprised of multiple political parties.

by the parameter h . Since the political regime is composed of the two factions, the overall productivity parameter h is a function of the productivity parameters of the two factions. For simplicity we assume a basic additive relationship, i.e., $h = h_1 + h_2$. The productivity parameters are stochastic and reflect the agent’s uncertainty about the political process.

The agent has preferences over reciprocating to the vote-buying faction, consuming leisure, and consuming the public good. Let the agent’s preferences be given by the utility function:

$$U(v, l, x) = \alpha \log(v) + \beta \log(l) + \gamma \log(x) \tag{2}$$

where $\alpha > 0$, $\beta > 0$, and $\gamma > 0$ are preference parameters associated with reciprocity, leisure, and public goods, respectively. Note that the reciprocity parameter should be interpreted as characterizing the agent’s preference for reciprocating to the vote-buying faction given the level of vote buying.

Just prior to making the electoral decisions, the agent is exposed to our ads with intensity m . We parameterize exposure as a continuous rather than a binary variable for ease of mathematical exposition. Further, we assume that all functions in this model are differentiable with respect to m . The timing of the ads relative to vote buying is crucial – in this model all electioneering (including vote buying) has already occurred prior to the ads and no further action, by either faction, is possible.

Our ads affect voter behavior through two channels – by decreasing voter reciprocity towards the vote-buying faction and by changing voter expectations of the productivity of the two factions. Formally, it affects the reciprocity preference parameter, $\alpha = \alpha(m)$, and the expectations of the productivity parameters, $\mathbf{E}(h_1|m) = h_1(m)$ and $\mathbf{E}(h_2|m) = h_2(m)$. Given the content of our message, discussed in detail in Section 3.2, we assume that the ads can only decrease reciprocity, i.e., $\alpha'(m) \leq 0$, and that it can only decrease voter expectation of the productivity parameter of the vote-buying faction, i.e., $h'_1(m) \leq 0$, and increase that of the non-vote-buying faction, i.e., $h'_2(m) \geq 0$.

The agent maximizes her expected utility, $\mathbf{E}_m U(x, v, l)$ subject to the resource constraint

$v + w + l = 1$. The agent's maximization problem in terms of the variables of interest s and t and the parameters is:

$$\underset{1 \leq s, t \leq 0}{\text{maximize}} \quad \alpha(m)\log(st) + \beta\log(1 - t) + \gamma h(m)\log(t) + \gamma h_1(m)\log(s) + \gamma h_2(m)\log(1 - s) \quad (3)$$

Assuming that an interior solution exists, the first order conditions of the maximization problem give the following expressions:

$$s(m) = \frac{\alpha(m) + \gamma h_1(m)}{\alpha(m) + \gamma h(m)} \quad (4)$$

$$t(m) = \frac{\alpha(m) + \gamma h(m)}{\alpha(m) + \beta + \gamma h(m)} \quad (5)$$

We can now predict the effect of greater exposure to the ads on the vote share of the vote-buying party and on the voter turnout rate.

Differentiating equation 4 with respect to m :

$$s'(m) = \frac{\gamma h_2(m)}{(\alpha(m) + \gamma h(m))^2} \alpha'(m) + \frac{\gamma^2 h_2(m)}{(\alpha(m) + \gamma h(m))^2} h_1'(m) - \frac{\gamma(\alpha(m) + \gamma h_1(m))}{(\alpha(m) + \gamma h(m))^2} h_2'(m) \quad (6)$$

The first term represents the decrease in vote share of the vote-buying faction from a decrease in voter reciprocity; the second term represents a decrease in vote share from a decrease in voter expectation of the vote-buying faction's productivity; and the third term represents a decrease in vote share from an increase in voter expectation of the non-vote-buying faction's productivity. Thus, the total effect of greater exposure to the ads is an unambiguous decrease in the vote share of the vote-buying faction. Note that even without the reciprocity component, the vote share effect is unambiguously negative.

For turnout results, we differentiate equation 5 with respect to m :

$$t'(m) = \frac{\beta}{(\alpha(m) + \beta + \gamma h(m))^2} \alpha'(m) + \frac{\gamma \beta}{(\alpha(m) + \beta + \gamma h(m))^2} h'(m) \quad (7)$$

The first term represents a decrease in turnout from decreased voter reciprocity. The second

term represents the shift in turnout from the changed expectation of overall productivity of the political regime. We cannot, however, sign the second term since the sign of $h'(m) = h'_1(m) + h'_2(m)$ depends on the relative magnitudes of the changes in the expectation of productivity of the two factions. If the increase in pessimism about the vote-buying faction is greater than the increase in optimism about the non-vote-buying faction, this component of the effect on turnout will be negative and reinforce the reciprocity component. On the other hand, if there is a large increase in optimism about the non-vote-buying faction relative to the increase in pessimism about the vote-buying faction, this component of the effect on turnout will be positive and go in the opposite direction of the reciprocity component. Therefore, the net effect of the ads on voter turnout rate will be positive only if the increase in optimism about the non-vote-buying faction is large enough – not only to counteract the increased pessimism about the vote-buying faction but also to counteract the decreased reciprocity towards the vote-buying faction. Formally, the voter turnout rate increases if:

$$h'_2(m) > -h'_1(m) - \frac{\alpha'(m)}{\gamma} \quad (8)$$

In sum, communication of the type we envision is expected to decrease the vote share of the vote-buying faction but has ambiguous consequences for voter turnout.

3 Field Experiment Design

3.1 General Elections

The setting for this experiment was the 2014 Indian general elections, in which 8251 candidates contested elections to 543 seats in the national parliament. Each parliamentary seat represents an electoral district known as a parliamentary constituency (PC).⁴ A candidate can either represent a political party or be unaffiliated (referred to as an independent). Party candidates appear on the ballot along with their party name and party symbol. Across the

⁴A candidate wins by receiving a plurality of the votes cast.

country, 464 parties participated in the elections, out of which 6 were officially recognized as “national parties” and 39 as “state parties.”⁵ The coalition led by the Bharatiya Janata Party (BJP) won a decisive victory over the long-dominant coalition led by the Indian National Congress (INC). Although rising prices topped voter concerns, scandal and corruption were important as well and were cited by one in seven voters (DNA, 2014).

Allegations of widespread vote-buying were leveled by many media observers (Choudhury, 2014; Ford, 2014; Mandhana and Agarwal, 2014). Cash bribes reportedly ranged from Rs. 1000 (\$17) to Rs. 2500 (\$43) (Chilkoti, 2014). Police working in collaboration with the ECI seized around \$50 million in cash and 30 million liters of liquor, and arrested more than two million people in connection with election-related violations (Election Commission of India, 2014a).

For security reasons, the ECI conducted the election in staggered phases, with polling held on nine different dates.⁶ Polling in PCs in the first phase was held on April 10, 2014 and in PCs in the last phase on May 12, 2014.⁷ The ECI enforced a ban on opinion and exit polling during this entire period. Further, it enforced a three-day ban on electioneering and liquor sales, starting two days before the polling date in each phase.⁸ Election results for all phases were released on May 16, 2014.⁹

⁵All parties participating in the election are required to register with the Election Commission of India (ECI), which designates parties as national, state or unrecognized based on past performance in national and state elections. National and state parties receive benefits such as subsidized airtime on government-run television and radio stations for election campaigning.

⁶This is comparable to the presidential primary elections in the US.

⁷The five phases represented in this study have polling dates approximately a week part.

⁸Broadcast of mass media ads, because of their geographically unrestricted nature, was prohibited throughout the country during each phase’s three-day window. The legality of speeches or statements made in non-restricted PCs that are nevertheless publicized in restricted PCs is ambiguous. Rules governing electioneering practices evolve based on the interpretation of election law in individual cases by the ECI which has quasi-judicial status under the constitution. See Election Commission of India (2014b) for a list of various electoral offenses and corresponding penalties.

⁹This is unlike the US presidential primaries where results become available immediately after polling in each phase, potentially affecting voter behavior in subsequent phases. We registered our pre-analysis plan for the experiment described below at the American Economic Association (AEA) registry on May 15, 2014, before the results were released.

3.2 Radio Campaign

The national scope of this election allowed us to make use of a large number of All India Radio (AIR) stations, enhancing statistical power. AIR, also known as *Akashvani*, is the national public radio broadcaster. AIR’s transmitters cover 95% of the country by area and 99% by population (Prasar Bharati, 2008). As of 2014, it operated 194 stations that broadcast external advertising. Compared to other mass media, AIR has the highest audience reach in rural areas, where it has a statutory monopoly on radio broadcasting, television viewership is constrained by unreliable electricity, and newspaper readership is low because of high rates of illiteracy. Regular listenership of AIR comprises 55% of all households in rural areas and 50% in urban areas (Prasar Bharati, 2008).

Of the 194 AIR stations, 57 had high-power transmitters and therefore had large coverage areas that overlapped with those of several neighboring radio stations. In order to minimize such overlap, we eliminated these high-power stations from our sample. Further, for budgetary reasons we excluded 30 radio stations with prohibitively high advertising rates. In all, AIR broadcasts in the 22 nationally recognized languages and several additional languages recognized by individual states. Since it was not feasible to record messages in so many languages, we restricted our sample to stations broadcasting in five languages that could be understood by the largest listener populations. This restriction eliminated 47 additional stations. The selection process resulted in a final list of 60 stations. These radio stations belong to 10 of the major states in India,¹⁰ which contain 67% of the country’s population and 62% of the parliamentary constituencies (PC).

We produced three 60-second ads in Hindi and four regional languages – Kannada, Marathi, Oriya, and Telugu. Each radio ad was a dramatized vignette that involved a conversation between a “naive” voter who had just received a “gift” from a politician and a “sophisticated” voter who understands the corrupt incentives of vote-buying politicians and the consequences of voting for them. The sophisticated voter conveys the message that politicians give gifts to buy their way into office and that once elected vote-buying politicians

¹⁰A new state of Telangana was carved out of the Andhra Pradesh, one of the states in our sample, after the election.

are likely to steal public money in order to recoup their expenditure, instead of providing public services like schools and electricity. Further, the sophisticated voter argues that it is quite likely that politicians who trade cash for votes, will not fulfill their official obligations without bribes. In the vignettes we portrayed the naive voter deciding to no longer honor their promise to vote for the vote-buying politician, having realized that doing so may not be in his or her economic self-interest. Each ad ended with an appeal by an announcer urging voters to teach vote buyers a lesson by voting instead for honest candidates. The ads did not mention any particular candidate or party. English translations of the scripts of the ads are presented in Appendix A.

Reciprocity involves individuals being kind to those who were kind to them in the past. By framing the “gift” as an act of greed rather than that of kindness we hoped to diminish the voter’s sense of moral obligation, i.e., reciprocity, towards the vote buyer.¹¹ Our second goal in crafting this message was to diminish voter expectation about the performance of the vote-buying politicians. We sought to convey the intuitive argument that if elected such politicians would try to recoup their substantial illicit expenditure by stealing public funds,¹² at the cost of providing the promised public goods.¹³ The message indirectly implied that non-vote-buying parties are likely to be less corrupt in the future.¹⁴

In order to reveal the effect of the ads on the behavior of voters, purely through changes in their attitudes and expectations, we have to rule out the possibility of endogenous changes in the interaction between bribe givers and voters in response to our ads. The ideal strategy for this purpose is to deliver the ads after all the vote buying has already occurred and parties are no longer able to interact or communicate with voters. This suggests broadcasting the ads as close to day of polling as possible. A conflicting goal however is to ensure high voter exposure to the ads, which requires a campaign period that is sufficiently long. Based on these considerations we timed the campaign to occur during the three days of the pre-election

¹¹This is equivalent to decreasing the voter’s reciprocity preference parameter (α) in our theoretical model.

¹²In the Indian context, there is abundant evidence suggesting that office holders accumulate wealth at an exceptionally high rate (Fisman et al., 2014).

¹³This is equivalent to decreasing the expectation of the vote-buying faction’s productivity parameter (h_1).

¹⁴This is equivalent to increasing the expectation of the non-vote-buying faction’s productivity parameter (h_2).

window, when electioneering is prohibited by law.¹⁵

Vote buying is a massive logistical exercise involving transfers of large amounts of resources and coordination of thousands of agents, requiring careful planning and execution to evade detection by police, political rivals and anti-corruption activists. Much of the vote buying occurs under the guise of legitimate electioneering – during political rallies, “celebrations,” and door-to-door canvassing. Media reports suggest vote buying starts several weeks before the day of elections (Hiddleston, 2011). By the time our ads aired most of the vote buying would have already taken place. Vote-buying behavior exogeneity would be violated if substantial vote buying occurs during the window period. While we cannot rule it out completely, anecdotal evidence suggests that the quantum of vote buying during this period is far smaller compared to that during the extended electioneering period prior to it because intensive police monitoring under the supervision of the ECI.

3.3 Random Assignment of the Radio Campaign

Our unit of randomization was the radio station and its accompanying coverage area. Prior to the randomization, we assigned a schedule for broadcasting the ads to each of the 60 radio stations. The broadcast schedule consisted of a total of 48 airings, 24 airings during popular prime-time programs and 24 additional airings during regular non-prime-time programs, spread over three consecutive days.¹⁶ The third day of the scheduled airings coincides with the election date for the station’s central location¹⁷ of the station’s coverage area. Because of the irregular boundaries of election phases¹⁸ and the circular station coverage areas, the central location’s election date does not match the election date for every area covered by the station. The goal behind using the central location’s election date to dictate a station’s campaign timing was to maximize the proportion of the station’s covered area where the

¹⁵All India Radio found our ads to be exempt from this prohibition on campaigning because they are strictly non-partisan and mentioned neither candidates nor parties, and are therefore not considered electioneering.

¹⁶The distribution of the airings was: 18 on the first day, 18 on the second day, and 12 on the third day.

¹⁷The central location is the town or city where the station is based. We do not have the exact location of the station’s transmitter within the city; and therefore use the coordinates of the city center. All the stations in our sample are based in small cities.

¹⁸Recall that an election phase is the set of regions that hold elections on the same day.

campaign timing would coincide with the pre-election window described previously.¹⁹ We divided the 60 radio stations into 4 groups corresponding to the election dates.²⁰ Exactly half of the radio stations in each group were randomly selected to broadcast the radio campaign according to the pre-assigned schedule.

We hired an advertising agency²¹ to develop and translate the scripts, produce the ads, and purchase the air-time on the specified radio stations for designated dates.²² The total cost of the radio campaign, including producing and airing the ads on the 30 stations, was \$23,000, or \$750 per radio station.

3.4 Unit of Analysis

Each PC is divided into several assembly constituencies (AC), typically 5 to 10, each containing 150,000 to 250,000 registered voters.²³ We choose the AC as the unit of observation for analysis for two reasons: they are typically quite small compared to a station’s coverage area and they provide the most easily accessible disaggregated official electoral data. We consider an AC to be covered by a radio station if its geographic centroid lies within the radio’s coverage area.²⁴ We obtained the typical coverage radii for different levels of transmitter power from information available for a few stations on the AIR website.²⁵ There are

¹⁹The stations in our sample had campaign end dates of April 10 (N = 8), April 17 (N = 30), April 24 (N = 12), April 30 (N = 5), and May 7 (N = 5). Henceforth, we will use these end dates to identify campaign timing. We had incorrect election dates for the central locations of three stations, which were later than the correct dates. The assigned campaign timing for these three stations would allow delivery of only post-election ads to their covered areas. This decreases our experiment sample but adds to the placebo sample, discussed in Section 3.4.

²⁰We pooled the stations with April 30 and May 7 campaign timing into one group.

²¹Super Ads Pvt. Ltd., New Delhi.

²²The additional non-prime-time airings were part of a promotional scheme of the AIR that consisted of one free non-prime-time airing for each prime-time airing. We received broadcast certificates from the radio stations only for the paid prime-time ads, not for the free non-prime-time ads. Broadcast certificates are available from the authors on request.

²³Each AC represents a seat in a state’s legislature known as an assembly.

²⁴We use a simple model of radio coverage based on transmitter power alone; an alternative approach would be to model each station’s propagation zones as a function of terrain. The correspondence between transmitter powers and coverage radii are – 1 kW: 25 km, 3 kW: 50 km, 5 kW: 65 km, 6 kW: 75 km, 10 kW: 100 km, and 20 kW: 125 km. An alternate approach might use the proportion of the AC area covered. We use the centroid approach because it is computationally straightforward and ACs are typically small enough to be entirely covered by radio stations.

²⁵<http://www.allindiaradio.gov.in>

a total of 751 ACs that are geographically covered by the sample radio stations.

The objective of this experiment is to estimate the effect of a voter information campaign on voter behavior when party behavior is exogenous. As argued before, the conditions for party behavior exogeneity are most likely to be met if the ads are broadcast during the short regulated pre-election window. Our “treatment” is receiving *any* anti-vote-buying radio ads during the three-day pre-election window. Our experimental sample of ACs follows quite naturally – all ACs that are eligible to receive the ads during their pre-election window but not before.²⁶ Although all radio stations have the same probability of being assigned to broadcast the ads, an AC’s probability of being treated depends on the number of radio stations from which it is eligible to receive the ads during the pre-election window.²⁷ We therefore exclude ACs that are eligible to receive any ads earlier than the three-day window.²⁸ Additionally, we exclude the 86 ACs that are eligible to receive only post-election ads. We will however utilize these 86 ACs as a sample for placebo tests (henceforth, placebo sample).

Our experiment sample therefore comprises of 615 ACs that are eligible to receive the treatment.²⁹ Based on the realization of the random selection of radio stations, 312 ACs were treated (referred to as treatment ACs) and 303 ACs were untreated (referred to as control ACs). Figure 1 depicts various aspects of the experiment design (experiment sample, excluded ACs, placebo sample, and treatment probability for the experiment ACs) and the outcome of the random selection of radio stations (realized treatment status for the experiment sample)

²⁶An AC is ineligible if there is no randomization under which it could receive the treatment.

²⁷The probability of assignment of a radio station to broadcast the ads is 0.5. Thus, the treatment probability for an AC which is eligible to receive the ads from one station is 0.5, from two stations is $1 - 0.5 * 0.5 = 0.75$, and from three stations is $1 - 0.5 * 0.5 * 0.5 = 0.875$.

²⁸39 ACs are eligible to receive only early ads and 11 ACs are eligible to receive both early ads and treatment ads.

²⁹Although we used 60 stations, only 57 stations are relevant for delivering the treatment ads to the experiment sample because of the late campaign timing of the 3 stations. Out of the 57 stations, 29 broadcast the ads.

3.5 Vote-Buying Parties

In order to estimate the effect of the treatment on electoral support for parties that engaged in vote buying, we need to first identify those parties. Vote buying is an illegal transaction between a bribe giver and a bribe taker that is rarely observed by outsiders in any systematic fashion. There is no direct method for documenting which parties engaged in vote buying, let alone for documenting spatial variation in vote buying by parties.³⁰ We therefore rely on the impressions of experts to identify vote-buying parties in different areas.³¹

Our research team conducted phone interviews with journalists covering elections across the 10 states, asking them a standardized set of questions to shed light on which party or parties were engaging in vote buying in regions they were covering. We drew our sample of journalists primarily from names listed in directories of registered reporters in each of the ten states. We also asked respondents to suggest other journalists we could interview. Our sample of journalists is diverse and includes reporters for English, Hindi and local language newspapers, correspondents at national and state-level news channels, and freelancers. Since electoral contests for the *Lok Sabha* elections occur at the parliamentary constituency (PC) level, we asked questions pertaining to specific PCs the journalists were covering. The ACs in our experiment sample belong to 144 PCs, and ACs in the placebo sample belong to 34 PCs.³² In all we received responses from 426 journalists³³ regarding PCs relevant for our experiment and placebo sample, 82% of whom identified at least one vote-buying party in the PC(s) they were covering.

Of the 144 PCs relevant for our experiment sample, we obtained at least one journalist response for 138 PCs.³⁴ The questionnaire (presented in Appendix B) made no mention of

³⁰ECI reports statistics related to confiscation of suspected vote-buying resources and police arrests in connection with election-related violations. These reports do not identify the parties connected with the illicit resources or the arrests. This is comparable to the inability of law enforcement authorities in the US to identify mafia bosses with drug busts. This attests to the highly organized nature of vote buying in India. Further, the statistics are only reported at the state level.

³¹Large-scale voter surveys are an alternate approach for identifying vote-buying parties. In our case this was not practically feasible.

³²There are 21 PCs in common between the two sets of PCs.

³³All except three journalists were interviewed before elections results were released.

³⁴10% of the 138 PCs had one respondent, 51% had two respondents, 23% had three respondents and 16% had four or more respondents.

our radio campaign. Consensus among respondents about which parties were engaging in vote buying is fairly high. In 83% of the PCs with two respondents, both agreed on at least one party.³⁵ In 97% of PCs with three respondents, two respondents agreed on at least one party, and in 58% all three agreed on at least one party.³⁶

In addition to identifying which parties were buying vote, we also asked respondents to describe the kinds of bribes distributed by parties and the categories of voters they typically targeted with such bribes. Of the 426 respondents, 67% identified at least one type of bribe; within this subset, cash (82%), liquor (28%), and food (15%) were the most commonly identified kinds of bribes. Within the subset (63%) of respondents who identified at least one category of voters that was targeted by parties, lower castes and religious minorities (40%), the poor (29%), all voters (19%), slum residents (14%), the youth (14%), and rural voters (14%) were the most commonly stated categories.

We coded the putative vote-buying parties based on journalist responses to the question “Which party/parties seem to be spending the most money secretly (such as on distribution of liquor, cash or other gifts)?”³⁷ The lowest level at which we can code the vote-buying parties is the PC (henceforth, specification 1). Under specification 1 we designate all parties identified by at least one respondent for a PC as vote buyers in that PC. It is our impression that journalists are much more likely to omit reporting one or more vote-buying parties due to incomplete information than to spuriously misreport non-vote-buying parties as vote-buying parties. By fully incorporating the differences in opinion between journalists in identifying vote-buying parties we are able to minimize the Type I error in their responses. Because we lack journalist respondents for 6 PCs, we are unable to designate vote-buyers for approximately 4% of the ACs using this coding scheme.³⁸

In order to designate vote-buying parties for the entire sample and to further reduce the Type I error, we aggregate the journalist responses to the state-election phase (henceforth

³⁵84% of respondents report only 1 party, 14% report 2 parties and 2% of respondents report 3 parties.

³⁶87% of respondents report only 1 party, 12% report 2 parties and 1% of respondents report 3 parties.

³⁷Candidates unaffiliated with any party are quite marginal and cannot realistically compete with the vote-buying resources of parties.

³⁸Out of the 34 PCs relevant for our placebo sample, we obtained at least one journalist response for 24 PCs.

specification 2). Parties designated as a vote buyer in at least one PC are designated as a vote-buying party for the entire state-election phase. Our experiment sample belongs to 21 state-election phases.³⁹ The assumption underlying this coding scheme is that vote buying by parties is similar among the PCs within a given state and election phase. While aggregation of responses to higher levels further reduces the Type I error, it is likely to also increase the Type II error. If there is excessive Type II error relative to the Type I error in the vote-buyer specification, it could lead to attenuated estimates of the effect of the ads on the vote share of the “true” vote-buying parties. To assess robustness we will present estimates for both specifications.

3.6 Data Sources

We estimate the impact of the treatment on two main outcomes – the vote share of the vote-buying parties and the voter turnout rate. To calculate vote share, in each AC we sum the votes received by all the parties identified as vote-buyers and divide this sum by the total votes cast in that AC. Of the ten states in our sample, two states – Andhra Pradesh and Odisha – had state legislative elections simultaneously with the parliamentary elections. In order to maintain consistency in the outcome variable across states, we use votes cast for the parliamentary candidates for all states. The voter turnout rate is calculated by dividing the total votes cast in an AC by the total registered voters in that AC. All electoral data were obtained from the Election Commission of India (2014c). Data for demographic variables are from the 2011 Census geocoded by ML Infomap (2012).⁴⁰

³⁹Few examples of state-election phases are: Andhra Pradesh-30 April, 2014; Andhra Pradesh-7 May, 2014; Jharkhand-17 April, 2014; and Karnataka-17 April, 2014.

⁴⁰The demographic data is at the census block-level. We calculate the demographics of an AC based on those of the census block which contains the AC centroid. Calculations based on the average of block demographics weighted by the proportion of AC area overlapping with the census block yield almost identical values.

4 Empirical Specification

We seek to estimate the AC-level average treatment effect (ATE) on the outcome variables of interest – the voter turnout rate and the vote share of the putative vote-buying parties. Ordinarily, one may obtain a consistent estimate of the ATE from the difference in mean outcomes in the treatment and control groups, respectively. In our case, overlap among radio coverage areas introduces varying treatment probabilities for different ACs, as discussed in Section 3.4. Difference in means will therefore not provide a consistent estimate of the ATE since the treatment probability of an AC is correlated with its treatment status and may also be correlated with unobserved characteristics of the AC that affect electoral outcomes.

Fortunately, this is a case of selection on observables, where an AC’s treatment probability is known. Weighting the data by inverse probability weights (IPW), which for an AC is the inverse of the probability of it being treated if treated and the inverse of the probability of it being untreated if untreated,⁴¹ provides consistent estimates of the ATE (Horvitz and Thompson 1952; Wooldridge, 2010). The weights (% of the sample) are 1.14 (0.15%), 1.33 (7.97%), 2 (91.28%), and 4 (0.60%).

We obtain the ATE by using weighted least squares to estimate the regression equation:⁴²

$$y_i^{2014} = \alpha + \beta T_i + \gamma y_i^{2009} + \delta' phase_i + \epsilon_i \quad (10)$$

where y_i^{2014} is the outcome variable of interest, T_i is the binary variable indicating the treatment status, and y_i^{2009} is the lagged outcome variable, for AC i . $phase_i$ is a vector of dummies indicating four of the five election phases. We use the vote share of the putative vote-buying party or parties in the previous general elections (in 2009)⁴³ as a covariate in the

⁴¹Formally, the weight for AC i is:

$$w_i = \begin{cases} \frac{1}{p_i} & \text{if AC } i \text{ is treated} \\ \frac{1}{1-p_i} & \text{if AC } i \text{ is not treated} \end{cases} \quad (9)$$

⁴²This was the regression envisaged in our pre-analysis plan.

⁴³*Illustration for specification 1:* Let parties A and B be the vote-buying parties in a PC. Suppose that only party A contested that PC in 2009. Then for each AC in that PC, the 2014 vote-share is the sum of the 2014 vote-shares of parties A and B in the given AC and the 2009 vote-share is the 2009 vote-share of

vote-share regression and the voter turnout rate in 2009 as a covariate in the turnout-rate regression. The WLS estimate, $\hat{\beta}$, is a consistent estimate of the ATE, where the average is over the ACs in the experiment sample.

We test the sensitivity of our estimates to the controlling for other covariates – the number of vote-buying parties, demographic variables from the 2011 Census (the percentage of the population that is literate, percentage living in rural areas, and percentage belonging to the Scheduled Caste (SC)/Scheduled Tribe (ST) category), and an indicator variable of whether the state had simultaneous elections to the state legislature.

Randomization by radio station means that ACs are assigned to treatment or control groups in geographic clusters. However, because of the overlap in the coverage areas of the radio stations, the error variance-covariance matrix does not have a block-diagonal structure. Since the cross-sectional dependence has a known structure, where the error term may be correlated between ACs covered by the same radio station but independent of ACs covered by other radio stations, we estimate a customized error variance-covariance matrix using the residual variance-covariance matrix but setting the covariance terms for ACs that do not belong to the same radio station to zero. This provides consistent estimates of the standard errors.⁴⁴

Under the sharp null hypothesis of no effect on any AC, the data reveal each AC’s treated and untreated potential outcomes. Reproducing the known randomization procedure an arbitrarily large number of times, a procedure known as randomization inference, the distribution of the test statistic under the null can be approximated with a high degree of precision, which in turn can be used to test hypotheses.⁴⁵ Motivated by the predictions of

party A in the given AC. Note that this coding scheme is agnostic about whether vote-buying parties in 2014 also bought votes in 2009. *Illustration for specification 2:* Let parties C, D, and E be the vote-buying parties in an election-phase or state. Suppose that parties C and D contested a given PC in 2014, but only parties C and E contested in 2009. Then for each AC in that PC, the 2014 vote-share is the sum of the 2014 vote-shares of parties C and D in the given AC, and the 2009 vote-share is the sum of the 2009 vote-shares of parties C and E in the given AC.

⁴⁴See Barrios et al. (2012) for a discussion.

⁴⁵We do not present the asymptotic p -values, which can be inferred from an inspection of the asymptotic standard errors. The asymptotic p -values are smaller, sometimes substantially, than those obtained from randomization inference. See Young (2016) for a discussion of the merits of p -values obtained from randomization inference over those obtained from asymptotic approximations.

our theoretical model, we specified a one-tailed hypothesis test for the anticipated negative average treatment effect on vote share and a two-tailed hypothesis test for the ambiguous effect of treatment on the turnout rate.⁴⁶

5 Results

All summary statistics and regression results presented in this section are estimated using IPW, discussed in detail in Section 4. Further, all p -values are obtained from randomization inference with 1000 iterations.

5.1 Balance Check

Table 1 presents the summary statistics of electoral and demographic characteristics and differences between the treatment and control ACs. The number of vote-buying parties, under specification 1, which aggregates journalist responses by PC, ranges from one to four; and under specification 2, which aggregates journalist responses by state-election phase, ranges from one to six. The mean number of vote-buying parties across ACs is 1.70 (SD = 0.71) under specification 1 and 2.88 (SD = 0.94) under specification 2. There is no statistically significant difference in the number of vote-buying parties between the treatment and control groups under either specification. ACs have on average 14.36 candidates (SD = 5.49) and 8.77 party candidates (SD = 2.42).

According to the Census of India (2011), 73.0% of India’s population is literate, 68.8% is rural, and 25.2% belonged to the Scheduled Castes (SC) or Scheduled Tribes (ST) category. By comparison, our sample is on average less literate (59.52%), more rural (79.15%) and has a greater proportion of the population belonging to the SC/ST category (31.77%). The sample is balanced along all observed electoral characteristics. The only demographic variable that differs appreciably between the treatment and control groups is percentage population rural. The mean percentage of population rural in the control group is 76.03; 6.30 percentage points

⁴⁶These are the hypothesis tests specified in our pre-analysis plan.

lower than the treatment group ($p = 0.04$). Recall that we cannot code vote-buying parties under specification 1 for 4% of the experiment sample (23 ACs). Only 1% of control ACs are missing under specification 1 compared to 7% of treatment ACs ($p = 0.05$).

The null hypothesis that coefficients from a regression of treatment status on a large set of covariates⁴⁷ are jointly equal to zero cannot be rejected ($F(14,42) = 0.60$, $p = 0.36$).

5.2 Treatment Effects

Table 2 presents the main results of this study – the effect of the treatment (receiving radio ads during the three-day pre-election window) on the vote share of vote-buying parties. Panel A presents the results for vote share calculated using specification 1 and Panel B for vote share calculated using specification 2. Electoral support for vote-buying parties is high – the mean for ACs in the control group is 67.42% (Specification 1) and 90.73% (Specification 2). Column 1 presents the estimates for our baseline regression, which suggests that the treatment decreased the vote share of vote-buying parties by 7.14 percentage points ($p = 0.04$, Specification 1) and 5.99 percentage points ($p = 0.02$, Specification 2), respectively. The next four columns assess the robustness of our baseline estimates by controlling for various additional covariates. The specification 1 (Panel A) estimate with all the covariates (Column 5) is not significant at conventional levels ($p = 0.12$); all other estimates are significant and do not vary appreciably. The vote-share results are not sensitive to level of aggregation at which vote-buying parties are classified (i.e., parliamentary constituency versus state-election phase). Overall, our estimates suggest that the treatment decreased the vote share of vote-buying parties by 4 to 7 percentage points.

Table 3 presents the estimates of the treatment effect on the voter turnout rate. The baseline result suggests a small decrease in the voter turnout rate of 0.16 percentage points. The mean turnout rate in the control group is 68.20%. The estimated effect, however, is statistically insignificant ($p = 0.96$) and remains weak regardless of whether we control for covariates. Further, the magnitude of the effect is negligible compared to the shift in vote

⁴⁷All variables from 1 except the two specification 1 variables are included. The regression is estimated using WLS with IPW.

share, suggesting that voters primarily responded to the ads by shifting their votes rather than abstaining altogether.

Assuming that 150,000 voters cast ballots in a given AC, ⁴⁸ even the most conservative estimate of 4 percentage points reduction in vote share implies that vote-buying parties received 6000 fewer votes (assuming no effect on turnout). Across the 312 treatment ACs, this estimated effect implies the radio messages drew close to two million votes away from the putative vote-buying parties.

5.3 Placebo Tests

Recall that 86 ACs were eligible to receive only post-election ads, i.e, under no random assignment of radio stations could they receive the ads before their election. We utilize this collection of ACs to perform a set of placebo tests. Out of the 86 ACs, 39 ACs received post-election ads whereas the remaining 47 ACs received no ads. The results of the placebo tests in which the true effect of receiving any post-election ads on the outcome variables is known to be zero, are presented in the Appendix (Tables C1 and C2). Some of the vote-share coefficients are positive, others are negative, but all are statistically insignificant. The turnout rate coefficients follow a similar pattern.

5.4 Additional Outcomes

Table 4 presents the effect of receiving the ads on the vote share of the incumbent party, vote share of candidates unaffiliated with any party (independents), and the vote share that went to the “none of the above (NOTA)”⁴⁹ option. One concern is that the ads could have been perceived as generic anti-corruption messages directed at the incumbent party. We find that the ads had a negative but statistically insignificant effect (-1.98 percentage points, $p = 0.43$) on the vote share of the incumbent party (control mean = 39.53 percentage points). The vote share of candidates unaffiliated with any political party increased by just

⁴⁸The mean turnout in the control group is 150,807.

⁴⁹The 2014 general elections was the first in which voters were given the option of rejecting all the candidates on the ballot.

0.64 percentage points. Although this estimate is large relative to the control mean of 2.85 percentage points, it falls well short of statistical significance ($p = 0.44$). There is similarly only limited evidence of an effect on the NOTA vote share. The effect is an increase of 0.15 percentage points relative to a control mean of 1.01 percentage points. In addition to being statistically insignificant ($p = 0.31$), the effect is not robust to inclusion of controls. These results suggest that the radio ads neither induced blind anti-incumbent voting nor blanket disaffection with all political parties.

5.5 Heterogeneous Effects

We might reasonably expect the effect of our ads to vary by demographic characteristics of ACs. Two noteworthy dimensions along which ACs are often classified are ruralness and literacy. Radio listenership, vote-buying levels, norms of reciprocity, and preference for public goods are thought to vary by ruralness and literacy. Because of these multiple channels, it is difficult to make *ex ante* predictions about how the effect is likely to vary by these demographic characteristics. We therefore view this analysis of treatment effect heterogeneity as exploratory. To investigate the heterogeneity of the effects along these characteristics, we partition the experiment sample of ACs into four groups based on whether the AC has lower than median % population rural and whether it had lower than median % population literate. The subgroup-wise treatment effects are presented in the Appendix (Table C3).

The apparent effect on vote share is smaller for ACs in the lowest rural and lowest literacy category. The estimated vote-share effect in the other three groups is substantially larger than the first group but does not exhibit any obvious pattern. We cannot reject the joint null hypothesis that the effects for the four groups is the same ($p = 0.38$). Turnout effects, too, do not vary significantly across the four categories of ACs. The ads decreased the turnout rate in lowest rural and lowest literate ACs but increased the turnout rate in highest rural and highest literate ACs. Again, however, we cannot reject the null of equal treatment effects across the four groups ($p = 0.67$). In sum, it appears that treatment effects do not seem to

vary appreciably along two salient demographic dimensions. This finding may reflect a lack of power to detect subgroup differences, or it may have a substantive interpretation having to do with broad receptiveness to the message conveyed by the ads.

6 Discussion

Vote buying in India is believed to take place on such a grand scale that it is difficult to contain solely through law enforcement. Educational messages directed at voters through mass media represent a complement to law enforcement efforts. This study represents the first systematic attempt to evaluate the effects of educational radio messages on voter behavior and one of the few randomized trials to evaluate anti-vote-buying campaigns.⁵⁰ In keeping with other experimental evaluations of radio dramatizations dealing with subjects such as ethnic tensions or HIV infection (Paluck, 2009; Paluck and Green, 2009) as well as recent quasi-experimental studies of the political influence of radio (Adena et al., 2015; DellaVigna et al., 2014; Enikolopov et al., 2011; Ferraz and Finan, 2008; Larreguy et al., 2015; Yanagizawa-Drott, 2014), our results suggest that anti-vote-buying vignettes can be surprisingly effective, shifting large numbers of votes away from putative vote-buying parties.

The magnitude of the apparent effect – a 4 to 7 percentage point reduction in vote share of vote-buying parties – is substantial.⁵¹ Importantly, exogenous party behavior implies that our estimated effect is attributable to changes in voter attitudes and expectations, and therefore a rigorous estimate of the persuasive effects of mass communication. We calculate the implied persuasion rate using the framework put forth by DellaVigna and

⁵⁰For a thorough review of the literature on social-economic and political-economic effects of mass media see DellaVigna and La Ferrara (2016) and Strömberg (2016), respectively.

⁵¹It is comparable to the 4 percentage point reduction in the vote share of the challenging party in Vicente (2014).

Gentzkow (2010).⁵² Even a 4 percentage point effect implies a persuasion rate of 7 to 24%.⁵³ For comparison, the persuasion rate in DellaVigna and Kaplan (2007)⁵⁴ is 11.6%, 7.7% in Enikolopov et al. (2011)⁵⁵, 2% in Chiang and Knight (2011)⁵⁶, and 19.5% in Gerber et al. (2009).⁵⁷

The causal mechanisms underlying this effect are not easily disentangled. It is not possible to say how much of the effect was from diminished voter reciprocity and how much was from increased pessimism about the post-electoral productivity of vote-buying parties. Further, in our theoretical model we do not specify how voters decide which party/parties have engaged in vote buying. In reality, voters are likely to make judgments based on both first-hand experiences (of receiving gifts) as well as from second-hand information (e.g., reputation or hearsay).

Based on our model, the ads will reduce voter reciprocity only towards parties that actually gave them gifts. The effect of the ads on expectations is more complicated. There is a possibility of voters holding biased beliefs about the vote-buying behavior of parties.⁵⁸ The ads will, therefore, increase pessimism about parties that they (potentially incorrectly) believe to be vote buyers. It is unclear if voters indeed hold biased beliefs. These nuances cannot be studied using aggregate electoral data.

One way to disentangle the mechanisms underlying the effect would be to collect individual survey data on whether a voter received a bribe, from which party, which parties they believe to be vote buyers, whether they had heard the radio messages, whether they had

⁵²Persuasion rate in their framework is the share of the audience that is convinced to change their behavior because of the message and would not have in its absence. The persuasion rate is calculated as

$$f = \frac{ATE}{\Delta Exposure} * \frac{1}{(1 - \bar{y}_c)}, \quad (11)$$

where f is the persuasion rate, ATE is the estimated average treatment effect, $\Delta Exposure$ is the difference in exposure between the treatment and control groups, and \bar{y}_c is the mean in the control group.

⁵³Assuming no exposure in the control group, and 50% exposure in treatment group (the average daily listenership), and control means of 91 percentage point (Specification 2) and 67 percentage point (Specification 1).

⁵⁴Availability of Fox News on U.S. Republican presidential vote share.

⁵⁵Availability of anti-Putin TV station on vote share of anti-Putin parties.

⁵⁶New York Times endorsements on support for U.S. presidential candidate, Gore.

⁵⁷Free subscription to *The Washington Post* on the U.S. Democratic vote share.

⁵⁸For example, if they have little first-hand experience and/or a lot of inaccurate second-hand information.

voted, and for whom. One of the many challenges with this approach is social desirability bias, as respondents may misreport their behavior to conform to social norms or to the perceived expectations of the interviewer. List experiments have been used recently to elicit truthful responses to sensitive questions such as vote buying (Gonzalez-Ocantos et al., 2012), but are practically infeasible for identifying the vote-buying parties in our context given the large number of parties, regional variation in vote buying, and the large geographic scope of the experiment.

A more fruitful way to augment our field experimental design may be to test whether other messages that do not address vote buying per se have similar electoral effects. For example, do encouragements to vote against corrupt or venal candidates work as well as messages that focus specifically on vote buyers? Banerjee et al. (2014) suggest not. The use of “report cards” and other forms of performance-related information to deflect voter support away from corrupt or criminal candidates have met with mixed success (Banerjee et al., 2011; Chong et al., 2015; de Figueiredo et al., 2014; Ferraz and Finan, 2008; Humphreys, 2009). A head-to-head competition between these different types of messages, deployed on radio or other mass media, has yet to be undertaken.

Complementing the study of the mechanisms is the investigation of whether our results apply to anti-vote-buying campaigns in other contexts. Further research is needed to assess whether the effects we observe in India’s national election apply as well to its state and local elections, where vote buying is reputed to be even more widespread. Another question is whether and how vote-buying parties respond to these kinds of media campaigns over time. One (undesirable) possibility might be that as this type of intervention raises the cost-per-vote for vote buyers, parties engaging in this activity will go to greater lengths to monitor voters’ compliance (e.g., by having voters use their cell phones to take photos of their ballots inside the voting booth) or perhaps reallocate some of their resources to other forms of illicit electioneering or electoral misconduct, including voter intimidation.

7 Conclusion

Rapid growth in the use of field experimentation to study both mass media (Green et al., 2014) and corruption (Serra and Wantchekon, 2012) has opened new lines of research and provided rigorous scientific evidence for potentially useful policy interventions. This study shows that messages about vote buying and the economic consequences of voting for vote buyers can be surprisingly effective in deflecting votes from parties that engage in vote buying. The challenge ahead is to refine and optimize this type of messaging both in the Indian context and elsewhere, as well as to study its impact on actual provision of public goods and related socioeconomic outcomes.

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Figure 1: Experiment Design and Randomization

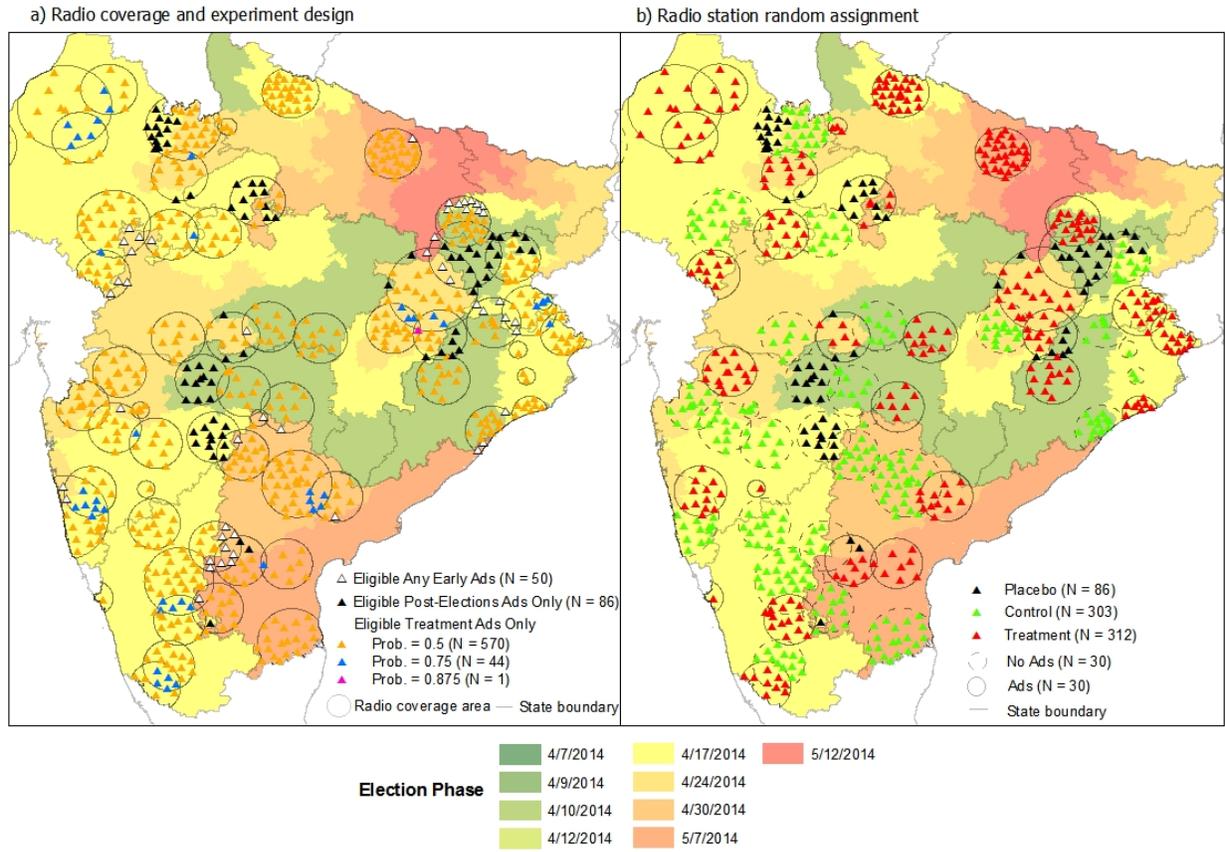


Table 1: Summary statistics of electoral and demographic characteristics

Variable	Mean	SD	Obs.		Control Mean	Diff.	<i>p</i> -value
			T	C			
<i>Parliamentary elections 2009</i>							
Lagged turnout (%)	57.85	12.25	312	303	59.21	-2.74	0.38
Lagged vote share of vote-buying parties (%)							
Specification 1	54.30	25.84	291	301	57.03	-5.72	0.19
Specification 2	79.00	20.50	312	303	77.88	2.25	0.62
<i>Parliamentary elections 2014</i>							
Number of vote-buying parties							
Specification 1	1.70	0.71	291	301	1.79	-0.19	0.17
Specification 2	2.88	0.94	312	303	2.94	-0.12	0.66
Registered voters	235,434	53,230	312	303	222,738	25,681	0.15
All candidates	14.36	5.49	312	303	14.77	-0.83	0.47
Party candidates	8.77	2.42	312	303	8.73	0.09	0.86
Election phase							
April 10 (=1)	0.14	0.35	312	303	0.12	0.04	0.33
April 17 (=1)	0.44	0.50	312	303	0.43	0.02	0.83
April 24 (=1)	0.20	0.40	312	303	0.21	-0.02	0.78
April 30 (=1)	0.10	0.30	312	303	0.14	-0.09	0.42
May 7 (=1)	0.12	0.33	312	303	0.10	0.05	0.63
State election (=1)	0.25	0.44	312	303	0.31	-0.12	0.30
<i>Demographics (Census 2011)</i>							
Pop. Literate (%)	59.52	9.63	312	303	60.69	-2.37	0.31
Pop. Rural (%)	79.15	23.69	312	303	76.03	6.30	0.04
Pop. SC/ST (%)	31.77	17.70	312	303	29.83	3.92	0.34
Missing specification 1	0.04	0.19	312	303	0.01	0.06	0.05

Notes: Specification 1 codes vote-buying parties by parliamentary constituency. Specification 2 codes vote-buying parties by state-election phase. Mean, SD, Control Mean, and Difference estimated using WLS with IPW. *p*-values of Difference are obtained from randomization inference with 1000 iterations.

Table 2: Effect of receiving treatment radio ads on vote share of vote-buying parties (%)

	(1)	(2)	(3)	(4)	(5)
<i>A. Vote-buying parties coded by parliamentary constituency (specification 1)</i>					
Control mean	67.42	67.42	67.42	67.42	67.42
Treatment	-7.14	-5.71	-6.98	-5.37	-3.73
	(3.44)	(3.41)	(3.37)	(2.57)	(2.36)
One-tailed p -value	0.04	0.09	0.05	0.06	0.12
R^2	0.54	0.64	0.66	0.58	0.68
N (Control)	301	301	301	301	301
N (Treatment)	291	291	291	291	291
<i>B. Vote-buying parties coded by state-election phase (specification 2)</i>					
Control mean	90.73	90.73	90.73	90.73	90.73
Treatment	-5.99	-5.88	-5.92	-5.14	-4.78
	(2.53)	(2.42)	(2.52)	(2.13)	(2.01)
One-tailed p -value	0.02	0.03	0.03	0.07	0.10
R^2	0.47	0.49	0.49	0.52	0.57
N (Control)	303	303	303	303	303
N (Treatment)	312	312	312	312	312
<i>Covariates</i>					
Lagged vote share	X	X	X	X	X
Num. vote-buying parties		X			X
Demographics			X		X
State election				X	X

Notes: Control mean and Treatment coefficient estimated using WLS with IPW. All columns include election phase fixed effects. Demographics include percent population rural, SC/ST, and literate. Standard errors robust to heteroskedasticity and cross-sectional dependence given in parentheses. p -values are obtained from randomization inference with 1000 iterations.

Table 3: Effect of receiving treatment radio ads on voter turnout rate (%)

	(1)	(2)	(3)	(4)
Control mean	68.20	68.20	68.20	68.20
Treatment	-0.16	-0.37	0.05	-0.19
	(0.87)	(0.70)	(0.86)	(0.69)
Two-tailed p -value	0.95	0.90	0.99	0.95
R^2	0.79	0.81	0.80	0.82
N (Control)	303	303	303	303
N (Treatment)	312	312	312	312
<i>Covariates</i>				
Lagged turnout	X	X	X	X
Demographics		X		X
State election			X	X

Notes: Control mean and Treatment coefficient estimated using WLS with IPW. All columns include election phase fixed effects. Demographics include percentage population rural, SC/ST, and literate. Standard errors robust to heteroskedasticity and cross-sectional dependence given in parentheses. p -values are obtained from randomization inference with 1000 iterations.

Table 4: Effect of receiving treatment radio ads on additional outcomes (%)

	Incumbent vote share		Independent vote share		NOTA vote share	
	(1)	(2)	(3)	(4)	(5)	(6)
Control mean	39.53	39.53	2.85	2.85	1.01	1.01
Treatment	-1.98	-2.32	0.64	0.55	0.15	0.03
	(2.19)	(2.19)	(0.59)	(0.63)	(0.14)	(0.10)
Two-tailed p -value	0.43	0.39	0.44	0.51	0.31	0.86
R^2	0.40	0.42	0.09	0.10	0.09	0.42
N (Treatment)	304	304	290	290	312	312
N (Control)	292	292	302	302	303	303
<i>Covariates</i>						
Lagged outcome	X	X	X	X		
Demographics		X		X		X
State election		X		X		X

Notes: In 19 ACs the incumbent party did not stand for re-election. In 23 ACs there were no independent candidates. Control mean and Treatment coefficient estimated using WLS with IPW. ‘Demographics’ includes percent population rural, SC/ST, and literate. ‘Election dummies’ includes indicators for the election phases and simultaneous state election. Standard errors robust to heteroskedasticity and cross-sectional dependence given in parentheses. p -values are obtained from randomization inference with 1000 iterations.

A Radio Ad Scripts

A.1

It is a village setting. We hear birds chirping, the distant rumble of a motorbike and the faint noise of distant conversations. Kamala (grandmother, around 70 years old) comes to Ramesh's (male, around 50 years old) shop to make a purchase.

Kamala: Namaste, Ramesh Bhaiyya!

Ramesh: Namaste! How are you Amma?

Kamala: I am really happy today. Give me three of your best school bags. I am buying them as gifts for my grandchildren.

Ramesh: School bags (puzzled)? But our area doesn't even have a school!

Kamala: Then we'll get one soon (laughing). I was just at an election rally where the candidate promised to build a school if he wins.

Ramesh: Oh, really?

Kamala: Yes, and he was handing out cash as well. And all we have to do in return is to vote for him. That's it!

Ramesh: That's it (rhetorical)? Don't you get it? He is trying to buy your vote with money.

Kamala: I don't quite follow.

Ramesh: Amma, if he wins, then in order to recoup his election expenditure, he could siphon off government funds.

Kamala: Really (surprised)?

Ramesh: Yes Amma! And the school – it will remain only in the books.

Kamala: Oh! I hadn't thought of that. In that case, there is no way I am voting for him.

End scene.

Announcer: Teach vote buying leaders a lesson – use your secret ballot to vote for an honest candidate.

A.2

It is a village setting. We hear birds chirping, the distant rumble of a motorbike and the faint noise of distant conversations. Ram (male, around 40 years old) is a customer with a happy-go-lucky personality and Ramesh (male, around 50 years old) is the shopkeeper. Ram comes to Ramesh's shop to make a purchase.

Ram: Ramesh Bhai, show me the best pair of clothes you have on sale.

Ramesh: Of course. Looks like you had a good harvest.

Ram: My harvest was good. But that's not why I am buying new clothes?

Ramesh: What's the reason then?

Ram: Actually, an acquaintance of mine is contesting in the upcoming elections. He has fattened my wallet so I vote for him.

Ramesh: I see. And if he wins do you think he will fulfill his responsibilities?

Ram: Why won't he?

Ramesh: Well, why would anyone who trades notes for votes do anything for free?

Ram: Oh, I hadn't thought of that. In that case, there is no way I am voting for him.

End scene.

Announcer: Teach vote-buying leaders a lesson – use your secret ballot to vote for an honest candidate.

A.3

It is a village setting. We hear birds chirping, the distant rumble of a motorbike and the faint noise of distant conversations. Mohan (male, around 40 years old) is a passerby and Ramesh (male, around 50 years old) is a shopkeeper. Mohan happens to walk past Ramesh's shop holding an electric fan.

Ramesh: Mohan Babu, where did you buy this electric fan?

Mohan: I didn't buy it! A candidate is handing them out for free at the election rally.

Ramesh: But our village doesn't even get power.

Mohan: The candidate has promised to bring power to our village if he wins.

Ramesh: If this corrupt candidate wins then in order to recoup his election expenditure he could siphon off government funds.

Mohan: What (shocked)!

Ramesh: Yes and the promise will remain just that – a promise.

Mohan: Oh, I hadn't thought of that. In that case, there is no way I am voting for him then.

End scene.

Announcer: Teach vote-buying leaders a lesson – use your secret ballot to vote for an honest candidate.

B Journalist Interview Questions

1. Which *Lok Sabha* constituencies you are covering?

Interviewer Note: Ask the remaining questions for each constituency mentioned.

2. Which three parties have the biggest presence and what is the name of the candidate contesting from each party?
3. How are parties spending money to gain publicity and increase their vote share?
Interviewer Note: Examples used to prompt can be rallies, parades, and posters.
4. Which party/parties seem to be spending the most on campaigning?
5. Which party/parties have held the most public events (such as rallies, speeches, parades, etc.)?
6. Which party/parties have had the most visits by party leaders or “star campaigners” (such as celebrities or other well-known individuals lending support to the candidate)?
7. Which party/parties have the most volunteers or workers (largest party cadre)?

8. Which party/parties seem to be spending the most money secretly (such as on distribution of liquor, cash or other gifts)?
9. What are they spending this money on?
10. What class of voters are they trying to win by distributing gifts?
11. Which party do you think will get the largest vote share?

C Supplementary Results

Table C1: Effect of receiving post-election radio ads on vote share of vote-buying parties (%)

	(1)	(2)	(3)	(4)	(5)
<i>A. Vote-buying parties coded by parliamentary constituency (specification 1)</i>					
Control mean	64.10	64.10	64.10	64.10	64.10
Treatment	-0.95	-3.39	5.91	-4.86	3.20
	(5.38)	(5.28)	(2.92)	(5.49)	(2.51)
Two-tailed p -value	0.51	0.44	0.88	0.40	0.75
R^2	0.79	0.80	0.90	0.83	0.91
N (Control)	23	23	23	23	23
N (Treatment)	42	42	42	42	42
<i>B. Vote-buying parties coded by state-election phase (specification 2)</i>					
Control mean	80.34	80.34	80.34	80.34	80.34
Treatment	-0.88	4.33	5.59	-2.64	5.12
	(5.94)	(4.52)	(2.05)	(6.23)	(2.36)
Two-tailed p -value	0.50	0.63	0.65	0.44	0.62
R^2	0.66	0.75	0.84	0.68	0.85
N (Control)	38	38	38	38	38
N (Treatment)	47	47	47	47	47
<i>Covariates</i>					
Lagged vote share	X	X	X	X	X
Num. vote-buying parties		X			X
Demographics			X		X
State election				X	X

Notes: Control mean and Treatment coefficient estimated using WLS with IPW. The treatment group consists of ACs that receive any post-election ads and the control group of ACs that do not receive any post-election ads. The IPW of ACs is calculated from probability of receiving any post-election ads. All columns include election phase fixed effects. ‘Demographics’ includes percentage population rural, SC/ST, and literate. Standard errors robust to heteroskedasticity and cross-sectional dependence given in parentheses. p -values are obtained from randomization inference with 1000 iterations. Vote-buying parties cannot be coded for 21 ACs and 1 AC under specifications 1 and 2, respectively.

Table C2: Effect of receiving post-election radio ads on voter turnout rate (%)

	(1)	(2)	(3)	(4)
Control mean	60.76	60.76	60.76	60.76
Treatment	-0.59	0.25	-1.74	-0.71
	(2.08)	(2.52)	(1.82)	(2.38)
Two-tailed p -value	0.84	0.92	0.62	0.83
R^2	0.75	0.77	0.78	0.80
N (Control)	47	47	47	47
N (Treatment)	39	39	39	39
<i>Covariates</i>				
Lagged turnout	X	X	X	X
Demographics		X		X
State election			X	X

Notes: Control mean and Treatment coefficient estimated using WLS with IPW. The treatment group consists of ACs that receive any post-election ads and the control group of ACs that do not receive any post-election ads. The IPW of ACs is calculated from probability of receiving any post-election ads. All columns include election phase fixed effects. ‘Demographics’ includes percentage population rural, SC/ST, and literate. Standard errors robust to heteroskedasticity and cross-sectional dependence given in parentheses. p -values are obtained from randomization inference with 1000 iterations.

Table C3: Heterogeneous effects of receiving the treatment radio ads

<i>AC group:</i>	(1)	(2)	(3)	(4)
% pop. rural	Lowest [73.46]	Lowest [56.43]	Highest [96.62]	Highest [94.89]
% pop. literate	Lowest [52.23]	Highest [68.06]	Lowest [51.54]	Highest [65.82]
<i>A. Vote share of vote-buying parties coded by parliamentary constituency (% , specification 1)</i>				
Control mean	66.59	67.62	67.14	68.32
Treatment	-3.69	-7.51	-6.36	-8.45
	(2.83)	(4.09)	(5.46)	(4.23)
One-tailed <i>p</i> -value	0.16	0.06	0.22	0.03
<i>R</i> ²	0.49	0.59	0.48	0.62
N (Control)	47	115	94	45
N (Treatment)	56	84	94	57
Joint significance test: $F(3,44) = 0.04$, p -value = 0.82				
<i>B. Vote share of vote-buying parties coded by state and election phase (% , specification 2)</i>				
Control mean	87.69	91.77	90.50	91.69
Treatment	-1.09	-8.37	-6.17	-5.66
	(3.37)	(3.21)	(2.90)	(2.64)
One-tailed <i>p</i> -value	0.47	0.00	0.09	0.04
<i>R</i> ²	0.52	0.38	0.57	0.61
N (Control)	47	116	94	46
N (Treatment)	59	86	108	59
Joint significance test: $F(3,44) = 0.18$, p -value = 0.38				
<i>C. Voter turnout rate (%)</i>				
Control mean	66.91	67.07	70.25	68.22
Treatment	-1.64	-0.58	0.14	1.29
	(1.37)	(0.85)	(1.13)	(0.89)
Two-tailed <i>p</i> -value	0.58	0.54	0.92	0.23
<i>R</i> ²	0.78	0.79	0.80	0.84
N (Control)	47	116	94	46
N (Treatment)	59	86	108	59
Joint significance test: $F(3,44) = 0.12$, p -value = 0.67				

Notes: ACs are divided into four groups based on combinations of whether they have more % pop. rural than the median and whether they have more % pop. literate than the median. The IPW means for the two variables within each group are given in square brackets. Control mean and Treatment coefficient estimated with using WLS with IPW. All columns include election-phase fixed effects and the lagged dependent variable. Standard errors robust to heteroskedasticity and cross-sectional dependence given in parentheses. To formally test the presence of heterogeneous we run the baseline regression with group indicators and interactions with the treatment variable as independent variables and test the restriction that the three interaction coefficients are jointly equal to zero. For the F statistic, the denominator degrees of freedom $(44) = \text{number of radio stations } (57) - \text{number of parameters in unrestricted regression } (13)$. All p -values are obtained from randomization inference with 1000 iterations.