Vote buying and redistribution

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Abstract

Vote buying is a form of political clientelism involving pre-electoral transfers of money or material benefits from candidates to voters. Despite the presence of secret ballots, vote buying remains a pervasive phenomenon during elections in developing countries. While prior literature has focused on how vote buying is enforced by parties and political candidates and which types of voters are most likely targeted, we know much less about the behavioral spillover effects of vote buying on citizens' demand for redistribution and contributions to the provision of public goods. In this paper, we provide evidence on how vote buying causally affects voters' candidate choice, support for redistribution, and public goods provision. Using data from a laboratory experiment in Kenya, we find that vote buying is a double-edged sword for candidates using clientelist strategies: it attracts votes from those who were offered money and accepted it, but it also leads to negative reactions from those who rejected the offer as well as those who were not offered money. In line with its effect on voting behavior, vote buying has negative effects on subjects' evaluations of the vote-buying candidate. Vote buying significantly reduces individuals' stated preferences for more government spending on police and law enforcement—yet, surprisingly, not on other welfare areas such as unemployment benefits or health. We also find that open ballots—but not vote-buying campaigns—reduce individuals' willingness to contribute to public goods provisions.

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1 Introduction

Political parties often use vote buying to mobilize support during elections in developing countries. Vote buying is a type of distributive politics where parties and candidates hand out money or material benefits to voters in exchange for votes or political support on election day (Stokes, 2007). In theory, the secret ballot is supposed to militate against the use of vote buying as an electoral strategy, because it allows voters to conceal their vote choice from parties and candidates employing clientelist strategies (Cox & Kousser, 1981; Mares, 2015; Morgan & Várdy, 2012; Teorell et al., 2017). However, evidence from around the world suggests that the practice is pervasive—even in contexts with nominal ballot secrecy (Cruz et al., 2021; Gonzalez-Ocantos et al., 2012; Kramon, 2016a, 2016b, 2017; Stokes et al., 2013). This occurs particularly—but not exclusively—in developing countries, including Argentina (Brusco et al., 2004), Mexico (Cantú, 2019), Nicaragua (Gonzalez-Ocantos et al., 2011), Paraguay (Finan & Schechter, 2012), and some countries in Africa, such as Ghana (Jensen & Justesen, 2014), Nigeria (Bratton, 2008), São Tomé and Príncipe (Vicente, 2014), and Kenya (Kramon, 2016b).

While vote buying is mostly considered an electoral strategy with implications for voters' party choice and candidate support (Baghdasaryan et al., 2019; Guardado & Wancthekon, 2018; Kramon, 2016b, 2017; Vicente, 2014), it may also have adverse effects on programmatic redistribution and limit the provision of public goods (Robinson & Verdier, 2013; Vicente & Wantchekon, 2009). Yet, we lack clean evidence on the causal effects of vote buying on redistribution and public goods provision. To our knowledge, this paper is the first to examine how electoral vote buying causally affects voters' choice of political candidates differing in their redistributive profiles, as well as voters' candidate evaluations, contributions to public goods, and stated preferences for government spending in different areas including health, unemployment benefits, and police and law enforcement. To this end, we leverage evidence from a laboratory experiment conducted in Nairobi, Kenya—a country where vote buying is common during elections (Kramon, 2017, 2016a, 2016b). The experiment examines the effects of vote buying on two behavioral dimensions—candidate choice and public goods contributions—and two sets of stated preferences—candidate evaluations and preferences for programmatic redistribution. Given the prominent role of the secret ballot in shaping the nature of electoral clientelism (Mares & Young, 2016, 2019; Teorell et al., 2017), we also examine whether the effect of vote buying is conditioned by the presence (or absence) of the secret ballot.

Our findings show that vote buying has the potential to attract votes from subjects who are offered money and accept the offer. However, vote buying also causes negative reactions from those who reject the offer, and especially from those who are "left out"—who punish the vote-buying candidate by supporting the competing candidate. These results corroborate the findings of Leight et al. (2020), who show experimentally that—when vote buying is known to take place—voters who do not receive vote payments are more likely to punish vote-buying politicians. Our findings add to this by showing that vote-buying politicians are also punished by voters who receive offers of money in return for their votes—but reject the offer.

In line with results from the candidate choice game, we find that vote buying has negative effects on subjects' evaluation of vote-buying candidates—especially in terms of leadership and competence, and corruption and cheating.

Importantly, while vote buying matters for candidate choice and evaluations, we also show that it has implications for the demand for public goods provisions. Indeed, our

findings further reveal a negative effect of vote buying on stated preferences for more government spending on police and law enforcement—yet not in other welfare areas such as health, defense, and unemployment benefits. We further show that individuals' willingness to contribute to public goods is negatively affected by open ballots, but not by vote buying.

In doing so, our paper contributes to a growing literature examining the implications of vote buying—and political clientelism more broadly—for public goods and redistributive policy. While a large literature engages with questions of which voters are targeted by vote-buying campaigns (Carreras & İrepoğlu, 2013; Jensen & Justesen, 2014; Matsubayashi & Sakaiya, 2021; Stokes et al., 2013) and how vote buying and other forms of electoral clientelism are enforced in the shadow of the secret ballot (Guardado & Wantchekon, 2018; Larreguy et al., 2016; Nichter, 2008; Stokes, 2005), we know much less about how vote buying affects voter preferences for programmatic redistribution and public goods provision. We contribute to filling this gap by providing evidence from a laboratory experiment designed to examine how vote buying by political candidates causally affects voter support for programmatic redistribution—and how this effect is moderated by shifting the electoral environment from open to secret ballots. By zooming in on voter *demand* for programmatic redistribution and how it is shaped by clientelist politics, we expand upon the existing literature, which mainly focuses on how clientelism affects the *supply* of programmatic redistribution and public goods (Anderson et al., 2015; Cruz et al., 2021; Kemani, 2015; Robinson & Verdier, 2013).

Indeed, the literature on clientelism often highlights that clientelist distribution can generally be used to limit the supply of—and access to—distributive goods and welfare benefits to supporters of the distributing party or to attract the support of swing voters (Albertus, 2013; Mares & Young, 2019; Nichter, 2008; Robison & Verdier, 2013; Stokes, 2005; Stokes et al., 2013). The existing literature also suggests that vote buying has adverse effects on broad-based public policies and programmatic redistribution (Aidt & Jensen, 2017; Baland & Robinson, 2012; Vicente & Wantchekon, 2009; Keefer & Vlaicu, 2008, 2017; Stokes, 2007), but the empirical evidence is still limited. Khemani (2015) provides evidence of a negative relationship between vote buying and pro-poor public policies at the municipal level in the Philippines. Baland and Robinson (2012) show that patron-client relations can be used by economic elites to extract rents, but that rent extraction is reduced by the transition from open to secret voting. Anderson et al. (2015) similarly show that the use of vote trades by political elites in India results in lower supply of pro-poor policies. The use of pre-electoral transfers may be particularly pronounced when political parties are weakly organized and find it difficult to commit to future redistribution (Keefer & Vlaicu, 2008, 2017). In such contexts, voter confidence in electoral promises is low and may lead political parties to increase their use of pre-electoral vote buying—at the expense of post-electoral redistribution. This idea matches the work of Robinson and Verdier (2013) showing that patronage—the distribution of jobs in exchange for political support—is a credible mode of clientelist distribution, but also results in inefficiencies in the supply of public goods.

While the clientelism literature emphasizes that political parties often use particularistic clientelist distribution as a substitute for broad-based programmatic policies, a related body of work suggests that voter evaluations of candidate performance are also affected by the use of clientelist distribution. For instance, using evidence from a laboratory experiment in Kenya and the USA, Leight et al. (2020) show how vote buying makes voters less inclined to punish politicians for rent extraction. Similarly, survey experimental work by Bøttkjær and Justesen (2021) provides evidence that the distribution of patronage attenuates voter punishment of corrupt politicians.

Overall, this points to a trade-off between different modes of distributive politics: where vote buying and electoral clientelism are widespread as strategies of pre-electoral distributive politics, post-electoral programmatic redistribution is limited. We provide direct evidence on this proposition—focusing on how vote buying shapes candidate choice and evaluation, contributions to public goods, and stated preferences for programmatic redistribution.

In the bigger picture, our paper adds to our understanding of how democracy works in low-income countries where political parties find it difficult to commit to future redistribution (Keefer & Vlaicu, 2017). In theory, democratic elections are supposed to serve as mechanisms for channeling voter preferences for public policy and redistribution into the political systems (Besley, 2006). A key part of democracy is that voters elect political candidates with a mandate to represent and translate their preferences and interests into public policy (Besley, 2006; Powell & Vanberg, 2000). In contexts with pervasive poverty and high levels of inequality—such as Kenya—democratic elections should therefore pave the way for candidates campaigning on widespread pro-poor redistribution (Acemoglu & Robinson, 2006; Meltzer & Richard, 1981). Yet, in new democracies around the world, this pattern often fails to appear (Keefer, 2009). Our findings suggest that one piece of the answer to this puzzle is that the use of clientelist distribution by parties and candidates during elections crowds out voter demand for programmatic redistribution. Although vote-buying candidates may be punished by voters, the mere fact that vote buying is used during election campaigns may weaken demand for programmatic redistribution. These findings complement the existing literature, and highlight that the use of clientelism not only shapes incentives of incumbents and parties to alter the supply of programmatic redistribution but also lowers voter demand for such policies, suggesting that voters view clientelist distribution as a substitute for—rather than a supplement to—programmatic redistribution.

The rest of the paper is organized as follows: Sect. 2 presents the design after a brief overview of the experiment; Sect. 3 describes the procedure and the subject pool; Sect. 4 reports the results; Sect. 5 provides a discussion of the findings and concludes.[1]

2 Experimental design

Our experiment is conducted in collaboration with the Busara Center for Behavioral Economics in Nairobi, Kenya. Kenya has held regular competitive multiparty elections since their introduction in 1992. Elections for parliament are candidate-centered, with voters voting for candidates in single-member districts. Strategies of electoral clientelism—like vote buying—are commonly used by political parties during election campaigns, and parties disproportionately target vote-buying campaigns at poor people (Kramon, 2016a, 2016b, 2017). These features of the Kenyan context fit well with our experimental setup, which

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[1] The online supplementary material includes Appendix A (additional tables), Appendix B (experimental instructions), and additional material. Specifically, Appendix A contains additional tables. Appendix B contains the protocol used to welcome participants; the instructions for the "Vote Buying, Open Ballots" treatment of the candidate choice game; the instructions for the public goods game; and the post-experiment survey questions related to candidate evaluation and preferences for redistribution. Additional tables and replication files—including the complete set of the experiment instructions—are available in the supplemental material.

simulates an environment where vote buying occurs and is targeted at poor people in candidate-based elections.

Subjects participating in the experiment were recruited chiefly from the Kibera area in Nairobi—a large urban informal settlement,[2] where poverty is prevalent (for a detailed background on the Kibera area, see Marx et al., 2019). Recruitment was done by the Busara Center according to their standard recruitment procedure, where participants are randomly selected through the laboratory database, and invited via text messages (more details in Sect. 3).

Each experimental session started with a candidate choice game consisting of two identical rounds (Sect. 2.1), followed by a public goods game consisting of six identical rounds (Sect. 2.2).[3] Each experimental game was independent of the others: individual earnings in each game did not affect earnings in the other games. In addition to the observed behavior in the experimental games, in a post-experiment survey we also asked questions about individuals' evaluation of candidates and preferences for redistribution (Sect. 2.3).[4]

At the beginning of each session, participants were randomly divided into two groups: three fourths of participants received a low endowment of 100 tokens ("the poor"), and one fourth of participants received a high endowment of 800 tokens ("the rich"), where 1 token stands for 0.5 Ksh (100 Ksh are approx. 0.909 USD).[5] The roles of poorly and richly endowed subjects remained fixed throughout the entire session. Participants were informed about the proportions of "poor" and "rich" subjects in the laboratory.

Subjects were paid 200 Kenyan shillings (Ksh) for showing up on time, and in addition they earned an extra amount of money from the games, depending on their own choices as well as on the other participants' choices. During the session, subjects' earnings were denominated in tokens and converted at the end into Kenyan shillings at a rate of 1 token for 0.5 Ksh.

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[2] The true population of the slum is unknown, with estimates ranging from about 170,000 to over one million (Marx et al., 2013).

[3] In the candidate choice game, we used two rounds for two reasons: (i) to evaluate the effect of vote-buying strategy on individuals' choices in the second round of elections; (ii) for comparability purposes with Leight et al.'s (2020) experimental design. Similar to other finitely repeated public goods games (for references, see Ledyard, 1995; Plott & Smith, 2008; Ambrus & Pathak, 2011), in our public goods games we used round repetitions to evaluate the evolution of average contributions over periods.

[4] This study is part of a larger experiment which comprises three main parts: the candidate choice game, the public goods game, and the trust game. The order of the experimental games remained fixed over all sessions, and each experimental game was independent of the others; i.e., individual earnings in each game did not affect earnings in the other games of the experiment. This large experiment has two distinct aims: to assess the effects of vote buying on (i) redistribution preferences and (ii) interpersonal (social) trust. Here, we specifically focus on one of these subprojects, the one on redistribution. Hence, we do not report data for the trust game, which was designed to address a different issue (social trust). In the interest of completeness and transparency, we have included the experiment instructions for the trust game in the supplemental material.

[5] We chose the experimental parameters according to the statistics on average household income in Kenya. The endowment of 100 tokens (50 Ksh.) approximates the average income per person and per day in the poorest 50% of households. The endowment of 800 tokens (400 Ksh.) is approximately ten times the lowest endowment, and roughly corresponds to the ratio of the richest 20% to the poorest. Sources: Standard media (https://www.standardmedia.co.ke/business/article/2001276202/what-majority-of-kenyan-households-earn-in-a-month) and United Nations (UN) statistics (http://www.un.org/en/development/desa/population/publications/pdf/popfacts/PopFacts_2017-2.pdf); Last accessed: April 2021.

2.1 The candidate choice game

The candidate choice game consisted of two identical rounds, where individuals' earnings in the first round did not influence their earnings in the second round. All subjects acted as voters.[6] In every round, each participant was asked to cast a vote via computer for one of two competing political candidates simulated by the computer—Candidate A and Candidate B—with no option to abstain from voting. We experimentally manipulated the way the two political candidates would redistribute resources from a common pool, if elected (more details in the next paragraph). The common pool was computed as 50 tokens multiplied by the number of participants in a session. A candidate would win the election if the majority voted for him/her. The redistribution of the common pool took place at the end of each round, immediately after the result of the election. In the event of a tie, no one won the election and there was no redistribution.

We experimentally manipulated the voting conditions along two treatment dimensions: vote buying and ballot secrecy. Specifically, we conducted a 2 (Vote Buying versus No Vote Buying) × 2 (Secret Ballots versus Open Ballots) between-subject design, as summarized in Table 1.

In the "Vote Buying" treatments, participants were given the choice between two candidates: Candidate A campaigning on a platform of programmatic redistribution versus Candidate B campaigning on a mix of vote buying and programmatic redistribution. Specifically, Candidate A (if elected) would redistribute the entire common pool equally among participants. The campaign platform of Candidate A effectively simulates a promise of post-electoral programmatic redistribution (cf. Stokes et al., 2013). In contrast, Candidate B first used some tokens of the common pool to cover the cost of electoral expenses—*including offering some voters money in exchange for their votes*—and then (if elected) would redistribute the remaining part of the common pool equally among participants. The campaign platform of Candidate B, therefore, simulates a mix of pre-electoral transfers—vote buying—and promises of post-electoral programmatic redistribution (cf. Keefer & Vlaicu, 2017). Specifically, before the election, Candidate B randomly targeted three fourths of the "poor" participants. Each of the targeted voters was offered a sum of money equal to one fourth of their endowment (25 tokens).[7] Vote-buying offers were common knowledge; i.e., all subjects were informed from the beginning that—in each stage of the game—money was offered to some participants by Candidate B. For the sake of anonymity, they did not know *who* received offers (i.e., the specific identity of those who received offers was not disclosed).[8]

The targeted voters could accept or reject the offer. If a participant accepted the offer, the money would immediately be transferred into his/her individual earnings before the election took place and independently of his/her final vote. This corresponds to the fact that vote buying is a pre-electoral transfer from candidates to voters (Keefer & Vlaicu, 2017). If

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[6] This is different from Leight et al. (2020), where subjects made choices as *both* the voter and the politician.

[7] We chose those parameters according to the statistics on vote buying from the Kenya Afrobarometer. Source: https://afrobarometer.org/countries/kenya-0 (Last accessed: April 2021).

[8] See the experiment instructions in the supplemental material, which clearly informed all participants that "In each round, before the election, Candidate B will randomly target more than half of participants among those with 100 tokens as initial endowment. Each of these targeted participants will be offered 25 tokens in exchange for votes. The offers are anonymous: no one will ever know who has received an offer."

Table 1 Treatments in the candidate choice game

	No vote buying	Vote buying
Secret ballots	Candidate A would redistribute the entire common pool equally among participants if elected Candidate B uses some tokens of the common pool to cover the cost of electoral expenses, and would redistribute only the remaining part of the common pool if elected Individual votes are secret and cannot be traced by the political candidates	Candidate A would redistribute the entire common pool equally among participants if elected Candidate B uses some tokens of the common pool to cover the cost of electoral expenses, *which include offering some voters money in exchange for their votes*, and would redistribute only the remaining part of the common pool if elected Individual votes are secret and cannot be traced by the political candidates
Open ballots	Candidate A would redistribute the entire common pool equally among participants if elected Candidate B uses some tokens of the common pool to cover the cost of electoral expenses, and would redistributes only the remaining part of the common pool if elected Individual votes are *not secret* and can be traced by the political candidates	Candidate A would redistribute the entire common pool equally among participants if elected Candidate B uses some tokens of the common pool to cover the cost of electoral expenses, *which include offering some voters money in exchange for their votes*, and would redistribute only the remaining part of the common pool if elected Individual votes are *not secret* and can be traced by the political candidates

a participant rejected the offer, the money would be returned to the common pool of Candidate B before the election took place and independently of his/her final vote.

In the "No Vote Buying" treatments, Candidate A (if elected) would redistribute the entire common pool equally among participants, whereas Candidate B would first use some tokens of the common pool to cover the cost of electoral expenses, and then (if elected) would redistribute the remaining part of the common pool equally among participants. To ensure consistency with the "Vote Buying" treatments, in the "No Vote Buying" treatment, the amount Candidate B used to cover electoral expenses was equal to one fourth of the endowment of three fourths of poor participants.

In the "Secret Ballots" treatments, participants were informed that their votes were secret—also for the political candidates. In other words, individual votes could not be traced. In practice, this means that in the "Secret Ballots" treatments, we (the experimentalists) are unable to link each participant's vote to their computer ID: because ballots are kept secret, we can only observe the aggregate, final number of votes for each candidate. In contrast, in the "Open Ballots" treatments, participants were informed that their individual votes were not secret and could be traced by the political candidates. In practice, this means that we (the experimentalists) can link each participant's vote to their own computer ID.

In all treatments, *at the end of each round*, subjects received information about the final number of votes for each candidate, the winning candidate, and their own earnings.

Importantly, in the "Vote Buying" treatments, all participants were also informed of the percentage of individuals who accepted/rejected the offer from Candidate B.

The optimal choice in each treatment for all participants was to vote for Candidate A. The optimal choice for subjects who were offered money was to accept the money from Candidate B but to vote for Candidate A. Only in the "Vote Buying" treatments—and in the case where *all* participants rejected the offer from Candidate B—were individual payoffs equal when voting for either Candidate A or Candidate B.

The payoff structure implies that the experimental environment is relatively benign: candidates cannot punish voters for voting "the wrong way," and voters have incentives to take the vote-buying offer from candidate (B) but still vote for the competing candidate (A). The experimental design, therefore, assumes that the capacity of parties and candidates to monitor and enforce vote-buying exchanges is weak. This feature of the experimental environment fits the Kenyan context well. For instance, Kramon (2016b) argues that vote buying in Kenya is often unmonitored and that parties in Kenya generally have weak organizational capacity. Work from other countries in Africa similarly emphasizes that parties often lack the capacity to orchestrate large-scale monitoring of vote-buying transactions (Lindberg, 2010; Vicente & Wantchekon, 2009; Bratton, 2008). Our design, therefore, matches the context of the experiment quite well. It mainly mimics contexts of relatively weakly organized parties that find it difficult to commit to post-electoral redistribution (Keefer & Vlaicu, 2017), rather than contexts where electoral clientelism is orchestrated by well-organized political machines (Stokes, 2005; Woller et al., 2022).

2.2 The public goods game

After the candidate choice game, subjects were engaged in a public goods game consisting of six identical rounds, where individuals' choices and earnings in one round did not influence their choices and earnings in any of the other rounds. In each round, subjects were randomly and anonymously clustered into groups of four members—always composed of three "poor" subjects and one "rich" subject. At the beginning of each round, new groups were randomly formed in such a way that no participant interacted with the same group composition more than once. The initial endowments assigned at the beginning of the session remained fixed: the same three fourths of participants kept their initial endowment of 100 tokens, and the same one fourth of participants kept their initial endowment of 800 tokens. All aspects of the game were common information.

Our design here followed the basic elements of standard public goods games (Camerer & Fehr, 2004; Chaudhuri, 2011; Vesterlund, 2014): each participant's task was to decide how much of his/her tokens to divide between a common project and his/her own wallet. More specifically, each subject was asked to decide to put either 0%, or 30%, or 70%, or all of his/her tokens in the common project, while keeping the remaining tokens in his/her wallet. The total tokens put in the common project were doubled and then equally divided among the four group members. In every round, each participant's earnings were computed as the sum of the tokens in his/her own wallet and the tokens received from the redistribution of the common pool.

At the end of each round, each subject was informed about the tokens kept in the wallet and those put in the common project by each member of the group as well as the overall number of tokens that the group put in the common project.

2.3 The post-experiment survey

At the end of the experiment, before receiving their final payment, subjects were asked a series of questions—with no consequences for their earnings—about their assessments of the political candidates from the candidate choice game, and their preferences for more government spending on different areas (such as health and unemployment benefits). We use these questions to test whether the vote buying and secret ballots treatments sway participants' beliefs about candidate characteristics (such as corruption), and their stated preferences for redistribution.

Regarding candidates' evaluation, we asked subjects which candidate they believed was best in terms of leadership and competence (political leadership; competence; public services provision); support for people in need (help for poor people; help getting a job; help for people facing economic distress); honesty and trust (most honest and trusted candidate); and corruption and cheating (involvement in corruption and tendency to use fraud during an election to win political office). More specifically, subjects were asked the following ten questions—for each of which possible answers were either "Candidate A" or "Candidate B."[9]

Which candidate do you think:

- … is most competent?
- … is most honest?
- … would be the best political leader?
- … is most likely to help poor people?
- … is most likely to help you get a job?
- … is most likely to help people like you when they face economic distress?
- … is most likely to provide public services like clean water and electricity to your local community?
- … you would trust the most?
- … is most likely to be involved in corruption?
- … is most likely to cheat during an election in order to win political office?

Regarding preferences for redistribution, we asked subjects to state whether there should be more—or less—government spending in the following areas: health, unemployment benefits, defense, old age pensions, social grants/welfare benefits, business and industry, and police and law enforcement. Answers were given on a scale from 1 ("much less than now") to 5 ("much more than now"), with the option of selecting "Don't know."

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[9] In the experiment instructions (available in Appendix B and the supplemental material) another possible answer was "Don't Know." However, due to a technical problem, this answer was not implemented in the experiment software—hence not actually available on participants' screens. While we preferred to keep the original instructions in the interest of transparency, we shall again clarify that there were only two possible answers here: either "Candidate A" or "Candidate B."

3 Experimental procedure

We conducted 16 experimental sessions (four per treatment) in January 2019 in collaboration with the Busara Center for Behavioral Economics at their laboratory facilities in Nairobi, Kenya.[10] Each session consisted of 16 or 20 subjects and lasted approximately two hours, including welcome and exit from the Busara Center. Overall, 288 subjects participated in the experiment (72 subjects per treatment), of which 216 were assigned the role of "poor" and 72 the role of "rich."[11] Each subject participated in only one session, hence only one treatment (between-subject design).

3.1 Laboratory protocol

Subjects gave their signed consent to participate in a session and were given the right to drop out at any point during the study. As is standard in economics experiments, the consensus form—available in the supplemental material—informed participants that their choices during the experiment would be kept anonymous, with no possibility of linking them to their identity. Computer clients at the Busara Center are partitioned to further ensure confidentially and avoid any communication between participants.

The experiment was computerized using z-Tree (Zurich Toolbox for Ready-made Economic Experiments) software (Fischbacher, 2007), and participants performed all the experimental tasks via computer. The instructions—available in Appendix B and the supplemental material—were displayed on each participant's screen, in both English and Swahili, and read out loud in Swahili. We designed a user-friendly interface that largely relied on graphical elements with intuitive tasks,[12] which we tested in several pilots. To make their choices, subjects simply had to touch the screen, and there was no need to use the keyboard or the mouse. Exemplary screenshots of decision and feedback screens as they appeared to participants are available in Appendix B and the supplemental material.

Subjects were allowed to ask questions at any time by raising their hands and speaking to the laboratory assistant in private, but they were not allowed to communicate with one another or to take breaks. At the end of the instructions for each game, participants were asked to complete a quiz—with no consequences for their earnings—aimed at checking their comprehension of the task and allowing them to familiarize themselves with the screen interface. In the case of wrong answers, participants were provided with immediate feedback. Each game started when all participants finished the related quiz. In addition, in the post-experiment survey, participants were asked whether the instructions were clear. Most of them answered "very clear" (62.15%) or "fairly clear" (30.21%).

Participants were directly compensated for their participation through the M-Pesa mobile phone money transfer system. The compensation included a show-up fee of 200

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[10] For more information, see: https://www.busaracenter.org/ (Last accessed: April 2021). To test the experiment, we conducted 12 dry pilot sessions without participants, and two pilot sessions with participants. After the pilot sessions, we changed the instructions slightly to make them more comprehensible fee for participants.

[11] Our per-session/treatment sample size is comparable to other related experiments on vote buying. See, for example, Leight et al. (2020) with 61 Kenyan subjects per treatments (12 or 18 subjects per session).

[12] In programming the interfaces, we took inspiration from the experiments conducted in the first wave at the Internet Laboratory for Experimental Economics (iLEE) at the University of Copenhagen. For more information, see: http://www.econ.ku.dk/cee/ilee/description/ilee1/ (Last accessed: February 2021).

Kenyan shillings (approx. USD 1.94), including coverage of transport and a bonus of Ksh 50 for on-time arrival. Participants also gained additional earnings during the experiment, depending on their own and other participants' choices.[13] Subjects' earnings ranged from Ksh. 352.5 (approx. USD 3.44) to Ksh. 1525.5 (approx. USD 14.88), with the average earnings being Ksh. 703.26 (approx. USD 6.86) including the show-up fee.

3.2 Sample descriptives

Table A1 in Appendix A reports summary statistics for our sample. Most subjects (82.99%) stated that they had voted in the 2017 Kenyan general elections, and 35.42% of participants, overall, reported that they had received an offer (like money, food, or a gift) from someone from a political party in return for their vote.

The age of participants ranged from 18 to over 70 years old, with most participants (73.61%) being younger than 39 years old; 42.36% of the subjects were male. Most participants (33.68%) had completed secondary school,[14] and reported being unemployed (41.67%), self-employed (20.49%), or working part-time (17.36%). Only a few subjects were students (6.94%), housewives or similar (3.82%), and only one was retired. Most participants declared they had two children (22.57%), followed by those with no children (19.44%) and one child (18.75%).

Table A2 in Appendix A provides summary statistics of participants' households. Subjects reported an average household monthly income of Ksh. 7826.92 (approx. USD 75.80), and most of them (69.44%) declared that they were the main income earner. Most participants (42.71%) reported their own household's living conditions as "just below middle income," and the same response was given by most subjects (32.29%) when asked about their parents' living conditions (when they were children). Most participants (67.01%) indicated mud to be the main wall material of their house; 38.25% and 50.18% of participants reported that they had zero or only one sleeping room, respectively, with a median of one sleeping room. The majority of subjects declared that they owned a radio (70.83%) and a television (54.86%), but only a few of them a refrigerator (4.51%), a microwave (5.56%), or a bicycle (7.99%), and almost none a car (1.39%, corresponding to four participants) or a motorcycle (3.47%, corresponding to ten participants).

To substantiate the causal interpretation of our estimates, we first determine that the treatment groups are balanced in terms of the large set of individual and household characteristics mentioned above. A set of balance tables (available in the supplemental material)—with p values computed according to Chiapello (2018)—shows that there are no statistically significant differences (at 10% significant level or lower) in means or variance of personal characteristics across experimental treatments and roles ("poor" versus "rich").[15]

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[13] Specifically, at the end of the study, the computer server randomly drew one of the two rounds of the candidate choice game, one out of six rounds of the public goods game, and one out of six rounds of the trust game (omitted in the current paper) for actual payments. Participants were informed that each round had the same probability of being selected for the final payment.

[14] The education level of our sample was heterogeneous: 2.78% of subjects (corresponding to eight participants) declared that they had no formal education; 2.78% completed preschool; 6.60% started primary school; 18.06% completed primary school; 13.19% started secondary school; 10.76% started college; and 12.15% completed college.

[15] The balance tables available in the supplemental material report the means and difference in means for each variable, with standard deviations (of means) and p values (of the difference in means) in parentheses. Standard errors are clustered at the session level, and p values are computed according to Chiapello (2018).

Hence, randomization was effective in ensuring that the treatment groups were similar on average in terms of a large set of characteristics. Nonetheless, as robustness checks, we added some of the participant characteristics as listed in Tables A1 and A2 in Appendix A as controls in our regression analyses (see Sect. 4 and the supplemental material).

4 Results

In this section, we report the results of treatment effects on four outcomes: support for the vote-buying candidate (Sect. 4.1); evaluation of candidates (Sect. 4.2); contributions to public goods (Sect. 4.3); and stated preferences for programmatic redistribution (Sect. 4.4). All regression models include standard errors clustered at the session level. Results are robust to clustering standard errors at the individual level (see supplemental material).

4.1 Candidate choice

In this section, we present the results from the candidate choice game as described in Sect. 2.1. Over a total of 32 voting rounds, Candidate A won 28 times, Candidate B won three times,[16] and a tie occurred once (in the "No Vote Buying, Open Ballots" treatment). On average, Candidate B had lower vote shares than Candidate A (30% vote shares on aggregate; 33.7% in round 1; 26.4% in round 2; see Table A3 in Appendix A). The average vote shares for Candidate B were lower in the "Vote Buying" treatments than in the "No Vote Buying" treatment (24.8% versus 35.2% on aggregate; 27.8% versus 39.5% in round 1; 21.9% versus 30.9% in round 2).[17] This can be seen in Fig. 1, which plots the average vote shares for Candidate A (white bars) and Candidate B (gray bars), both by treatment and pooled across treatments. Overall, the graphs in Fig. 1 show that, by keeping ballots either secret or open, average vote shares for Candidate B were lower in the presence of vote buying ("VB") than in the absence thereof ("No VB"). The lowest vote shares for Candidate B occurred in the presence of vote buying joint with open ballots (22.2% on aggregate; 25.6% in round 1; 18.7% in round 2). On aggregate, therefore, the vote-buying candidate (B) overwhelmingly lost the vote.

To study the effect of vote buying on subjects' candidate choice, we analyze the *individual* decision to vote for Candidate B—who offered money to selected participants in return for their votes. Table 2 reports the marginal effects of probit regressions for the

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[16] Candidate B won twice in the "No Vote Buying, Secret Ballots" treatment, and once in the "No Vote Buying, Open Ballots" treatment. It is worth noting that the number of votes for Candidate B was higher than zero in the No Vote Buying treatments. A possible interpretation is that subjects positively evaluated Candidate B's investments in electoral expenses (in the absence of vote buying) as a sign of effort (see Sect. 5 for an extended discussion). However, given our data, any explanation of this result would remain a conjecture—hence we prefer to avoid any speculation and rather focus on treatment effects. We are in any case confident in excluding subjects' lack of understanding of the task as an explanation for this result, for two reasons. First, we checked subjects' understanding of the instructions in the quizzes, and provided them with direct feedback in the event of wrong answers. Here, no major issues were observed. Second, we controlled for subjects' understanding of the instructions in our regression analyses (see the control variable "Instructions Clear" in the supplemental material).

[17] Here, data are pooled at the session level; hence the number of observations per treatment (i.e., eight observations in the aggregate; four observations by round) is not sufficient to conduct any statistical test or regression analysis.
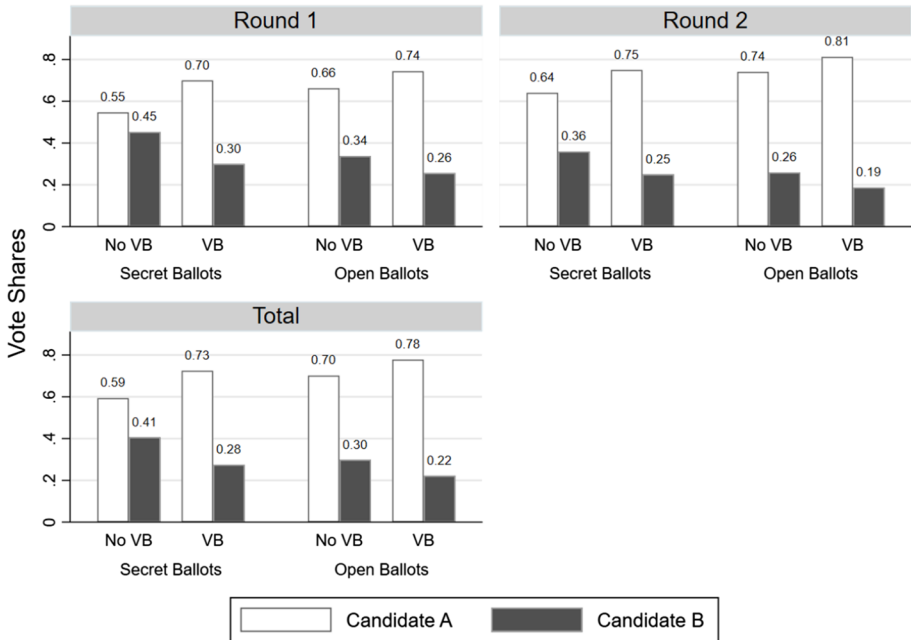
Fig. 1 Average shares of votes for Candidate A and Candidate B (the vote-buying candidate), by treatment. *Notes.* Observations pooled at the session level (32 obs. on aggregate; 16 obs. by round). Abbreviation: "VB" for "Vote Buying" "No VB" for "No Vote Buying." See Table A3 in Appendix A for related summary statistics

probability of voting for Candidate B—with observations pooled across rounds in columns ("cols.") 1–4—with round fixed effects in cols. 3 and 4; and split by round in cols. 5–8. Since participants' *individual* votes under secret ballots are not traceable, this analysis necessarily considers the observations in the "Open Ballots" treatments (288 observations on aggregate; 144 observations in each round). The dependent variable is *Voted for Candidate B*—a dummy variable equal to 1 if the participant voted for Candidate B, and 0 if the vote was for Candidate A. Columns 2, 4, 6, and 8 add controls for personal and household characteristics (see supplemental material for the estimates of all control variables). The key explanatory variable is *Vote Buying*, which is equal to 1 for the "Vote Buying" treatments, and 0 otherwise. In all model specifications, the estimates show that the coefficient of *Vote Buying* is negative but not statistically significant.

However, different candidate choices emerge between those who received an offer from Candidate B and those who did not. Let us focus on the "Vote Buying, Open Ballots" treatment. Table 3 reports summary statistics of vote shares for the vote-buying candidate (Candidate B) by role—"Poor" in cols. 1–5, and "Rich" in col. 6—with observations pooled across rounds in panel A "Aggregate" and split by round in panel B "By round." Within the "Poor" group, Table 3 further distinguishes between those who received an offer from Candidate B ("Targeted" cols. 2–4) and those who did not ("Not targeted" col. 1). Moreover, within the "Targeted" group, Table 3 discerns between those who rejected the offer ("Rejected" col. 2) and those who accepted it ("Accepted" col. 3).

Table 2 Effect of vote buying on decision to vote for Candidate B (probit marginal effects)

DV: Voted for Candidate *B* (0/1)

	Aggregate				By round			
	Baseline (B)		B + Round FE		Round 1		Round 2	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Vote buying	−.076	−.083	−.076	−.083	−.083	−.088	−.069	−.078
	(.089)	(.080)	(.089)	(.080)	(.094)	(.095)	(.097)	(.085)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Round 1 (omitted)								
Round 2			−.076*	−.075*				
			(.035)	(.035)				
Observations	288	288	288	288	144	144	144	144
Pseudo R^2	.006	.037	.013	.044	.007	.066	.006	.038
BIC	343.610	361.569	347.107	359.387	186.022	200.419	163.975	183.849

The table reports marginal effects of probit regressions for the probability of voting for the vote-buying candidate (Candidate B). Standard errors clustered at the session level in parentheses. The regressions consider 288 observations (i.e., those related to the *Open Ballots* treatments); 144 observations per round. The dependent variable is a dummy variable equal to 1 if a participant voted for Candidate B, 0 if they voted for Candidate A

$^+p<.10$, $^*p<.05$, $^{**}p<.01$, $^{***}p<.001$

BIC Bayesian information criterion

Table 3 Summary statistics of vote shares for Candidate B in the "Vote Buying, Open Ballots" treatment, by role

	Poor					Rich
	Not targeted	Targeted			Pooled	
		Rejected	Accepted	Pooled		
	(1)	(2)	(3)	(4)	(5)	(6)
(A) Aggregate						
Mean	.152	.107	.382	.258	.213	.278
SD	(.363)	(.315)	(.493)	(.441)	(.411)	(.454)
Frequency	*46*	*28*	*34*	*62*	*108*	*36*
(B) By round						
Round 1						
Mean	.261	.200	.313	.258	.259	.278
SD	(.449)	(.414)	(.479)	(.445)	(.442)	(.461)
Frequency	*23*	*15*	*16*	*31*	*54*	*18*
Round 2						
Mean	.043	.000	.444	. 258	. 166	.278
SD	(.208)	(.000)	(.511)	(. 445)	(. 376)	(.461)
Frequency	*23*	*13*	*18*	*31*	*54*	*18*

The table provides the mean, standard deviation (in parentheses), and frequency (in italics) of vote shares for the vote-buying candidate (Candidate B) by role ("Poor" and "Rich"), in the "Vote Buying, Open Ballots" treatment (144 observations in panel A "Aggregate"; 72 observations in panel B per round)

Table 4 Mechanisms behind decision to vote for Candidate B (probit marginal effects)

DV: Voted for Candidate *B* (0/1)

	Baseline				Baseline + Round fixed effects			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Poor (not targeted)	−.130	−.147	−.125	−.146+	−.134	−.150	−.129	−.151+
	(.163)	(.096)	(.155)	(.082)	(.163)	(.097)	(.154)	(.084)
Targeted	.113	.088			.116	.091		
	(.101)	(.095)			(.102)	(.099)		
Rejected offer			−.061	−.098			−.066	−.103
			(.062)	(.067)			(.060)	(.068)
Accepted offer			.269**	.296***			.279**	.307***
			(.083)	(.078)			(.094)	(.088)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Round 1 (omitted)								
Round 2					−.074*	−.079**	−.089*	−.094**
					(.031)	(.026)	(.040)	(.034)
Observations	144	144	144	144	144	144	144	144
Pseudo R^2	.016	.058	.023	.069	.118	.173	.127	.187
BIC	167.491	160.986	166.354	159.279	151.652	143.122	150.176	140.883

The table reports marginal effects of probit regressions for the probability of voting for the vote-buying candidate (Candidate B). Standard errors clustered at the session level in parentheses. The regressions are conducted over 144 observations—i.e., those related to the "Vote Buying, Open Ballots" treatment. The dependent variable is a dummy variable equal to 1 if a participant voted for Candidate B, 0 if they voted for Candidate A

+$p < .10$, *$p < .05$, **$p < .01$, ***$p < .001$

BIC Bayesian information criterion

On aggregate (panel A), vote shares for Candidate B were higher among "poor" subjects who received an offer from Candidate B compared with those who were not offered money (25.8% versus 15.2% vote shares). Within the "Targeted" group, vote shares for Candidate B were higher among those who accepted the offer than among those who rejected it (38.2% versus 10.7%). The vote shares for Candidate B by "rich" participants remained constant at 27.8% (col. 6).

Looking at observations by round (panel B) reveals a notable response to Candidate B's vote-buying strategy by the poor subjects in the second round of elections: poorly endowed subjects who were offered money but rejected it did not vote for Candidate B at all (col. 2, round 2), whereas 44.4% of those who accepted the offer did vote for Candidate B (col. 3, round 2). A substantially lower percentage of "poor" subjects who were not offered any money voted for Candidate B in round 2 compared with those in round 1 (4.3% versus 26.1%; col. 1). This result may be related to a learning effect between rounds. Indeed, after the first round of elections, all subjects received feedback about the percentage of individuals who accepted/rejected the offer, and this likely affected individuals' choices in the second round.[18]

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[18] This result may also be explained by individuals having a better understanding of the game in the second round—on top of the effects of receiving feedback about the first round.

To better understand individuals' candidate choices, we conduct regression analysis. Table 4 reports the marginal effects of probit regressions for the probability of voting for Candidate B in the "Vote Buying, Open Ballots" treatment. Observations are pooled across rounds in cols. 1–4, and round fixed effects are added in cols. 5–8.

The dependent variable is *Voted for Candidate B*—a dummy variable equal to 1 if the participant voted for Candidate B, and 0 if they voted for Candidate A. The key explanatory variables are *Poor* (*Not Targeted*), which is equal to 1 if the participant was assigned to the role of "poor" (low endowment), and 0 if assigned to the role of "rich" (high endowment); and *Targeted*, which is equal to 1 if the participant was assigned to the role of "poor" *and* was offered money by Candidate B (and 0 otherwise). In cols. 3–4 (and cols. 7–8, which add round fixed effects), we operationalize the variable *Targeted* as split into two separate variables—*Rejected Offer* and *Accepted Offer*—each of which is equal to 1 if the participant was assigned to the role of "poor," was targeted by Candidate B, *and* rejected or accepted the offer, respectively.

For the interpretation of the results, in all columns the coefficient for *Poor* is the difference in likelihood between a non-targeted poor and a rich subject; in columns 1–2 and 5–6, the coefficient for *Targeted* is the difference in likelihood between a targeted and non-targeted poor; in columns 3–4 and 7–8, the coefficient for *Rejected Offer* (*Accepted Offer*) is the difference in likelihood between a targeted poor who rejected (accepted) the offer and a targeted poor who accepted (rejected) it.

The estimates show that—on aggregate and across all specifications—the coefficient of *Accepted Offer* is positive and statistically significant. This reveals that, among "poor" subjects who were targeted by Candidate B, those who *accepted* the offer were approx. 27% more likely to vote for that Candidate B compared with those who rejected it.

To better investigate the dynamics between rounds, we conduct additional regressions by each round separately. Similar to Table 4, Table 5 reports the marginal effects of probit regressions for the probability of voting for Candidate B in the "Vote Buying, Open Ballots" treatment, but now the sample is split by round (72 observations per round). The dependent variable and covariates are similar to those in Table 4—but now the regression models in column "Round 2" include controls for individuals' choices in round 1. Columns 2, 4, 6, and 8 also include controls over personal and household characteristics (see supplemental material for the estimates of all control variables).

The estimates show that in round 1 (cols. 1–4), the probability of voting for Candidate B was not affected by any independent variable. Instead, in round 2 (cols. 5–8), being "poor" and targeted by Candidate B significantly increased the likelihood of voting for Candidate B by approx. 27% (see the coefficient of *Targeted in R2* in col. 5), whereas not being targeted (in either round) significantly decreased that likelihood by approx. 29% (see the coefficient of *Poor* (*Not Targeted*) in cols. 5–6). The results in cols. 7–8 further show that among "poor" subjects who were targeted by Candidate B in round 2, those who accepted the offer were significantly more likely to vote for that candidate—as shown by the coefficient of *Accepted Offer in R*2, which is positive and significant—while those who rejected the offer were significantly less likely to do so—as shown by the coefficient of *Rejected Offer in R*2, which is negative and significant. Interestingly, this suggests that the "poor" subjects who were "left out" from Candidate B's vote-buying campaign were less likely to vote for that candidate in the second round—as shown by the coefficient of *Poor* (*Not Targeted*) in cols. 5–8, which is negative and significant.

Table 5 Mechanisms behind decision to vote for Candidate B (probit marginal effects by round)

DV: Voted for Candidate *B* (0/1)

	Round 1 ("R1")				Round 2 ("R2")			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Poor (not targeted)	−.017	−.012	−.017	−.013	−.288[+]	−.530**	−.257*	−.425***
	(.195)	(.127)	(.194)	(.124)	(.174)	(.166)	(.123)	(.083)
Targeted in R1	−.003	−.027			.011	−.052		
	(.104)	(.157)			(.047)	(.080)		
Rejected offer in R1			−.065	−.109			.131	.106
			(.059)	(.105)			(.102)	(.102)
Accepted offer in R1			.115	.145			−.345*	−.197
			(.171)	(.162)			(.173)	(.174)
Targeted in R2					.266[+]	.493***		
					(.149)	(.079)		
Rejected offer in R2							−.749***	−.645***
							(.188)	(.121)
Accepted offer in R2							1.174***	1.147***
							(.137)	(.068)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	72	72	72	72	72	72	72	72
Pseudo R^2	.000	.105	.007	.115	.085	.381	.335	.566
BIC	95.906	87.230	95.390	86.358	77.722	56.709	64.243	47.863

The table reports marginal effects of probit regressions for the probability of voting for the vote-buying candidate (Candidate B). Standard errors clustered at the session level in parentheses. The regressions are conducted over 72 observations, i.e., those related to the "Vote Buying, Open Ballots" treatment—per round. The dependent variable is a dummy variable equal to 1 if a participant voted for Candidate B, 0 if they voted for Candidate A

[+]$p < .10$, *$p < .05$, **$p < .01$, ***$p < .001$

BIC Bayesian information criterion

4.2 Evaluation of candidates

We now expand upon the experimental analysis by examining whether vote buying affected individuals' evaluations of the two candidates. To this end, we use data from the post-experiment survey as described in Sect. 2.3, where we asked participants ten questions about their perceptions of the candidates.

For each treatment and each of the ten questions, Fig. 2 shows the fraction of subjects who selected the vote-buying candidate (Candidate B) *instead of Candidate A*. Overall, each panel in Fig. 2 shows that the percentage of participants selecting Candidate B remained below 50%, meaning that most subjects were more favorable toward Candidate A. This is particularly true in the presence of vote buying—especially if combined with open ballots—under which the positive evaluations of Candidate B dropped substantially.

Panel (a) "Leadership and Competence" of Fig. 2 shows that in the absence of vote buying ("no VB" in Fig. 2)—under secret ballots—36% of subjects chose Candidate B instead of Candidate A as the best leader (white bars), 39% as the most competent candidate (gray bars), and 43% as the candidate most likely to provide public services to the local community (black bars). In the presence of vote buying ("VB" in Fig. 2), those percentages
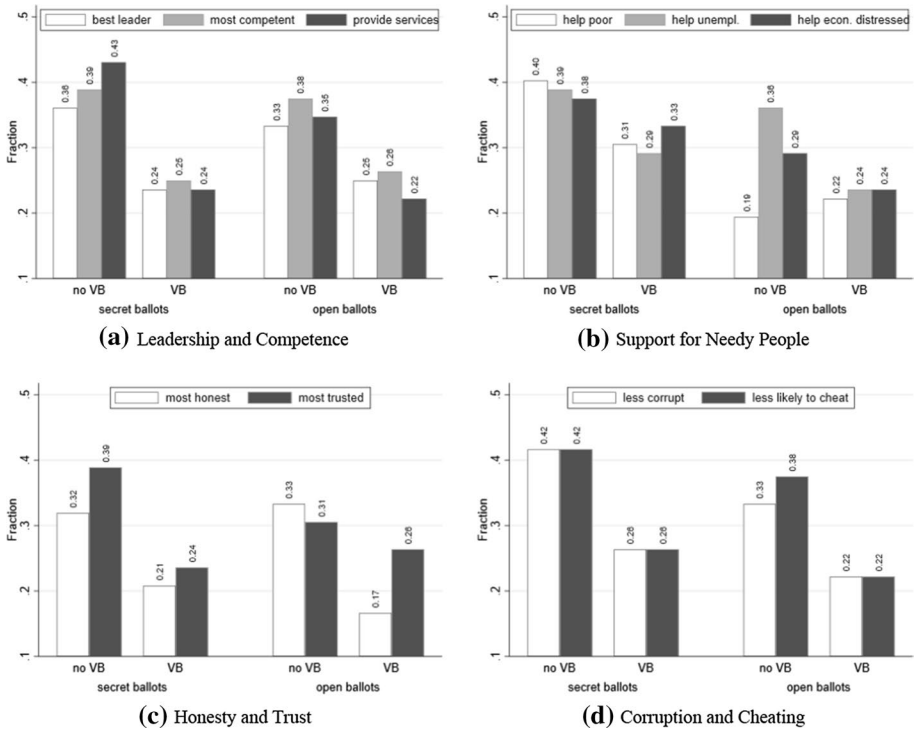
Fig. 2 Evaluation of Candidate B (the vote-buying candidate) vis-à-vis Candidate A, by treatment. Abbreviations: "VB" for "vote buying"; "unempl." for unemployed; "econ." for economically

dropped to 24%, 25%, and 24%, respectively. A similar negative effect of vote buying on Candidate B's evaluation is revealed in the other panels of Fig. 2. Panel (b) "Support for Needy People" shows that under secret ballots, in the absence of vote buying, almost 40% of subjects chose Candidate B instead of Candidate A as the best leader in providing support for people in need—including help for poor people (white bars), help for the unemployed in getting a job (gray bars), and help for people facing economic distress (black bars). In the presence of vote buying, those percentages dropped to approx. 30%.

The effect of vote buying on individuals' evaluation of Candidate B is especially pronounced in panel (c) "Honesty and Trust" and panel (d) "Corruption and Cheating." Under secret ballots, the percentage of subjects selecting Candidate B instead of Candidate A as the most honest (white bars) and the most trusted (black bars) candidate dropped from 39% in the absence of vote buying to 24% in the presence of vote buying. Similarly, in the secret ballot environment, the percentage of subjects selecting Candidate B instead of Candidate A as less likely to be involved in corruption (white bars) or less likely to cheat during an election to win political office (black bars) dropped from 42% in the absence of vote buying, and to 26% in the presence thereof. Similar effects—albeit generally lower in magnitude—hold under open ballots.

To test for statistical significance and better understand individuals' candidate evaluations, we conducted a set of regression analyses. For each question on which candidates are evaluated, Table 6 reports the marginal effects of probit regressions with standard errors

Table 6 Treatment effects on evaluation for Candidate B (probit marginal effects)

DV: Selected Candidate B (0/1)

(a) Leadership and competence

	Best leader				Most competent				Provide services			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Vote buying (VB)	−.104		−.104	−.124	−.124[+]		−.124[+]	−.138	−.158*		−.157*	−.186
	(.093)		(.093)	(.140)	(.073)		(.073)	(.129)	(.077)		(.076)	(.132)
Open ballots (OB)		−.007	−.007	−.026		−.000	.000	−.013		−.049	−.048	−.075
		(.099)	(.093)	(.151)		(.082)	(.073)	(.127)		(.091)	(.078)	(.115)
VB×OB				.041				.028				.059
				(.182)				(.141)				(.149)
Observations	288	288	288	288	288	288	288	288	288	288	288	288
Pseudo R^2	.011	.000	.011	.011	.014	.000	.014	.015	.024	.002	.027	.027
BIC	357.008	356.762	362.657	368.174	366.961	372.161	372.624	378.221	358.803	366.681	363.673	369.036

(b) Help needy people

	Help poor				Help unemployed				Help economically distressed			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Vote buying (VB)	−.035		−.033	−.086	−.110		−.111	−.094	−.049		−.049	−.039
	(.072)		(.061)	(.066)	(.085)		(.084)	(.116)	(.073)		(.069)	(.072)
Open ballots (OB)		−.144*	−.144*	−.202*		−.042	−.042	−.026		−.090	−.090	−.080
		(.060)	(.059)	(.083)		(.090)	(.085)	(.139)		(.070)	(.068)	(.123)
VB×OB				.118				−.034				−.020
				(.123)				(.166)				(.138)
Observations	288	288	288	288	288	288	288	288	288	288	288	288
Pseudo R^2	.001	.022	.023	.027	.011	.002	.013	.013	.002	.008	.010	.010
BIC	353.115	345.904	351.173	355.570	368.057	371.586	373.126	378.691	366.681	364.723	369.575	375.203

Table 6 (continued)

DV: Selected Candidate B (0/1)

	Most honest				Most trusted			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(c) Honesty and trust								
Vote buying (VB)	−.138		−.138	−.108	−.097		−.096	−.149
	(.089)		(.089)	(.147)	(.093)		(.092)	(.145)
Open ballots (OB)		−.014	−.015	.012		−.028	−.027	−.077
		(.099)	(.090)	(.141)		(.098)	(.093)	(.156)
VB×OB				−.061				.107
				(.170)				(.176)
Observations	288	288	288	288	288	288	288	288
Pseudo R^2	.022	.000	.023	.024	.009	.001	.010	.013
BIC	332.207	339.479	337.782	343.090	359.243	362.238	364.657	369.316

	Less corrupt				Less likely to cheat			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(d) Corruption and cheating								
Vote buying (VB)	−.131[+]		−.131[+]	−.145	−.151*		−.151*	−.147
	(.071)		(.069)	(.096)	(.077)		(.076)	(.115)
Open ballots (OB)		−.062	−.062	−.076		−.042	−.042	−.038
		(.080)	(.070)	(.113)		(.090)	(.078)	(.124)
VB×OB				.030				−.009
				(.136)				(.151)
Observations	288	288	288	288	288	288	288	288
Pseudo R^2	.017	.004	.020	.021	.022	.002	.023	.023

Table 6 (continued)

	Less corrupt				Less likely to cheat			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
BIC	361.574	366.160	365.906	371.492	364.374	371.586	369.442	375.099

Marginal effects from probit regression with standard errors clustered at the session level in parentheses. The dependent variable is a dummy variable equal to 1 if the participant selected the vote-buying candidate (Candidate B), or 0 if they selected Candidate A. Results are robust to controls for personal and household characteristics (see supplemental material)

$^+p<.10$, $^*p<.05$, $^{**}p<.01$, $^{***}p<.001$

BIC Bayesian information criterion

Table 7 Summary statistics of relative public goods contributions, by treatment

	No vote buying	Vote buying	Total
Secret ballots	.483	.349	.416
	(.237)	(.269)	(.261)
	72	*72*	*144*
Open ballots	.305	.343	.324
	(.226)	(.256)	(.241)
	72	*72*	*144*
Total	.394	.347	.370
	(.247)	(.262)	(.255)
	144	*144*	*288*

Each cell of this table reports the average relative contribution (as ratio of a subject's endowment), the standard error in parentheses, and the number of observations in italics

clustered at the session level in parentheses. The dependent variable is *Selected Candidate B*—a dummy variable equal to 1 if a participant selected Candidate B, and otherwise 0 if Candidate A was selected. The key explanatory variables are the treatment variables *Vote Buying* (VB) and *Open Ballots* (OB), and their interaction *VB×OB*.

The regression estimates show negative coefficients of *Vote Buying* in all the columns, with statistically significant results in Table 6d (Corruption and cheating) and Table 6a (Leadership and competence) only in relation to "Most Competent" and "Provide Services." The coefficients of *Open Ballots* are negative in all columns (except col. 4 in Table 6c), but statistically significant and relevant in magnitude only in relation to "Help poor people" (Table 6b, cols. 2, 3, 4). These results also hold after controlling for personal characteristics (see supplemental material). The coefficients of the interaction variable *VB×OB* are not statistically significant. Overall, these estimates reveal that subjects were likely to evaluate Candidate B more negatively than Candidate A in the presence of vote buying—especially in terms of competence, and corruption and cheating. This result is not affected by the presence of ballot secrecy, suggesting that participants were confident expressing negative beliefs about the vote-buying candidate regardless of whether their vote choice was protected by the secret ballot.

4.3 Contributions to public goods

In this section, we analyze treatment effects on individuals' contributions to public goods. Table 7 reports the summary statistics (pooled across rounds) of *relative* contributions to public goods. Relative contributions are computed as the ratio between the average absolute contributions in the six rounds of the game, and individual initial endowment.[19] Table 7 shows that participants devoted on average 37% of their initial endowment to public goods.

A set of *t*-tests reveals that contributions significantly changed across treatments. Specifically, on aggregate, subjects contributed less to public goods in the presence of vote

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[19] Given heterogeneous endowments, it is appropriate here to consider relative (ratio of endowment)—rather than absolute—contributions to public goods. See Cherry et al. (2005).
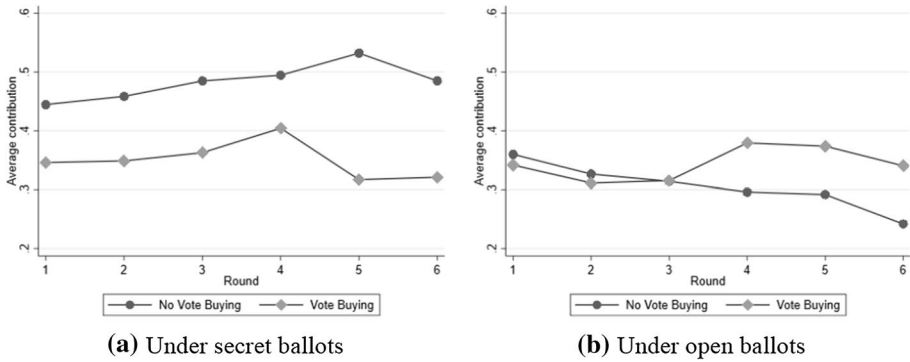
(a) Under secret ballots   **(b)** Under open ballots

Fig. 3 Average relative contribution to the public goods, by treatment and round

Table 8 Treatment effects on public goods contributions (OLS)

DV: Relative contribution to the public goods

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Vote buying	−.047	−.062			−.047	−.059	−.133	−.138+
	(.053)	(.047)			(.047)	(.044)	(.079)	(.074)
Open ballots			−.092+	−.083	−.092+	−.081	−.178***	−.161**
			(.050)	(.051)	(.047)	(.049)	(.042)	(.040)
VB × OB							.172+***	.157+
							(.082)	(.079)
Constant	.394***	.602***	.416***	.558***	.440***	.603***	.483***	.610***
	(.040)	(.129)	(.048)	(.123)	(.042)	(.124)	(.042)	(.127)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	288	288	288	288	288	288	288	288
Adjusted R^2	.005	.053	.029	.065	.035	.075	.060	.096

OLS regressions with standard errors clustered at the session level in parentheses. The dependent variable is the relative public goods contribution, computed as the ratio between the average absolute contribution across the six rounds of the public goods game, and individual initial endowment. Results are robust to tobit specifications (reported in Table A5)

$^+ p < .10, \, ^* p < .05, \, ^{**} p < .01, \, ^{***} p < .001$

buying than in the absence thereof (34.7% versus 39.4%)—but this is not statistically significant ($p = 0.116$)—and in the presence of open ballots than secret ballots (32.4% versus 41.6%; $p = 0.002$). A set of multiple comparison tests (MCT) with Tukey's method reveals an interaction effect of vote buying and secret ballots on subjects' willingness to contribute. Specifically, contributions were statistically lower in the presence of vote buying than in the absence thereof—but only under secret ballots (34.9% versus 48.3%; Tukey test $p = 0.007$). This result is stable through time, as Fig. 3 clearly shows. Figure 3 reports the average relative contributions to public goods throughout the six rounds of the game, by distinguishing between No Vote Buying (line with black circle symbol) and Vote Buying (line with gray diamond symbol) under either secret ballots (graph a) or open ballots (graph b). Both graphs show that contributions remained consistent through time. Under secret ballots (panel a), contributions were lower in the presence of vote buying than in

the absence thereof, whereas under open ballots (panel b) the two lines followed a similar pattern.

These statistics are confirmed by regression analyses. Table 8 reports ordinary least squares (OLS) estimates with standard errors clustered at the session level in parentheses. The dependent variable is the average *Relative Contribution to Public Goods*—computed as the ratio between the average absolute contributions in the six rounds, and individual initial endowment. The key explanatory variables are the treatment variables *Vote Buying* and *Open Ballots*, and their interaction *VB×OB*. Columns 2, 4, 6, and 8 add controls over personal and household characteristics (see supplemental material for the estimates of all control variables). The coefficient of *Vote Buying* is negative in all columns but not statistically significant (except in col. 8, where the interaction term and controls are added). The coefficient of *Open Ballots* is negative in all the columns but statistically significant only in col. 7—where the interaction term is included—and col. 8—where controls are added. This suggests that the sole presence of open ballots (i.e., in the absence of vote buying) significantly decreased contributions to public goods with respect to the reference case of secret ballots and no vote buying. The negative effect of open ballots on public goods contributions is slightly mitigated by the presence of vote buying. This is revealed by the positive coefficient of the interaction term *VB×OB*—although this coefficient is relatively weak in terms of statistical significance ($p < 0.10$). These estimates are robust to tobit specifications that account for the truncated nature of the dependent variable (see Table A5). We discuss this finding in Sect. 5.

4.4 Stated preferences over redistribution

In this section, we examine whether exposure to vote buying changes people's preferences for programmatic redistribution, as measured in the post-experiment survey (Sect. 2.3). To this end, we conducted a factor analysis with iterated principal axes with squared multiple correlation retaining one factor, followed by a varimax rotation. Essentially, we incorporated the group effect of government spending in welfare areas (health, unemployment benefits, defense, old age pensions, social grants/welfare benefits) into the factor *Welfare*. Similarly, we incorporated the group effect of government spending in business/industry and police into the factor *Business/Industry and Police*.

Table 9 reports the estimates from a set of OLS regressions with standard errors clustered at the session level in parentheses. The dependent variables are the factor variables *Welfare* (cols. 1–4) and *Business/Industry and Police* (cols. 5–8). The key explanatory variables are the treatment variables—*Vote Buying* and *Open Ballots*—and their interaction—*VB×OB*. The estimates reveal that the presence of vote buying—jointly with secret ballots—significantly reduced subjects' preference for more spending in business/industry and police (col. 8). Introducing open ballots mitigated this negative effect, as revealed by the positive coefficient of the interaction term *VB×OB*—although this coefficient is relatively weak in terms of statistical significance ($p < 0.10$). The estimates further show that neither vote buying nor open ballots have a significant effect on preferences for government spending in welfare areas. The findings hold if considering additive indexes, or separate indexes for each government spending area—rather than factors; and they are robust to controls over personal and household characteristics (see supplemental material).[20]

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[20] In Table A4 in Appendix A, we report the estimates from another set of OLS regressions, where we consider *Business and Industry* and *Police and Law Enforcement* as two separate dependent variables—rather

Table 9 Treatment effects on redistributive preferences (OLS)

	Welfare				Business/industry and police			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Vote buying	.079		.079	−.070	−.112		−.113	−.270[*]
	(.110)		(.110)	(.112)	(.095)		(.094)	(.122)
Open ballots		.029	.031	−.121		−.061	−.062	−.220[*]
		(.112)	(.111)	(.078)		(.099)	(.094)	(.089)
VB × OB				.304				.320[+]
				(.201)				(.164)
Constant	−.039	−.014	−.054	.021	.056	.030	.086	.165[*]
	(.046)	(.059)	(.075)	(.057)	(.062)	(.082)	(.079)	(.077)
Observations	267	267	267	267	279	279	279	279
Adjusted R^2	−.002	−.003	−.005	−.001	.002	−.002	.000	.009

OLS regressions with standard errors clustered at the session level in parentheses. The dependent variables are the factors *Welfare* (cols. 1–4)—which is an index incorporating the spending areas of health, unemployment benefits, defense, old age pensions, and social grants/welfare benefits—and *Business/Industry and Police* (cols. 5–8)—which is an index incorporating the spending areas of business/industry, and police and law enforcement. Results are robust to controls over personal and household characteristics (see supplemental material)

[+]$p < .10$, [*]$p < .05$, [**]$p < .01$, [***]$p < .001$

5 Conclusion

A substantial body of literature has analyzed how clientelist strategies such as vote buying work in contexts of ballot secrecy (Guardado & Wantchekon, 2018; Jensen & Justesen, 2014; Larreguy et al., 2016; Nichter, 2008; Stokes, 2005; Stokes et al., 2013), and how they affect the *supply* of programmatic redistribution (Anderson et al., 2015; Cruz et al., 2021; Kemani, 2015; Robinson & Verdier, 2013). Yet, vote buying likely affects voters' behavior—including their *demand* for redistribution and public goods provision, with detrimental effects on welfare and democratic policies. However, little evidence on the causal link between vote buying and the demand for redistribution has—to our knowledge—been established so far to support that claim. This paper seeks to fill this gap by providing evidence from a laboratory experiment designed to examine how vote buying by political candidates *causally* affects voters' support for redistribution and their contribution to public goods. We further investigate whether—and how—this effect is moderated by shifting the electoral environment from open to secret ballots. In addition to behavioral dimensions, our research also delves into the effects of vote buying on individuals' stated preferences, i.e., their candidate evaluations and preferences for redistribution as measured in a post-experiment survey.

Footnote 20 (continued)

than a unique index as in Table 9—each ranging on a scale from 1 ("much less than now") to 5 ("much more than now"). Table A4 reveals that the results shown in Table 9 are driven by the negative effects of *Vote Buying* and *Open Ballots* on preferences for more government spending on police and law enforcement—not on business/industry.

Overall, our results reveal negative consequences of vote buying on voters' behavior. Specifically, vote buying strategies attract votes from those subjects who were offered money and accepted it; and it decreases individuals' stated demand for more government spending on police and law enforcement.

Perhaps more interestingly, our results unveil other implications of vote buying, especially in the presence of secret ballots. Specifically, we find that vote buying is a double-edged sword for the vote-buying candidate: while it succeeds in "buying" votes from those subjects who were offered—and accepted—money, it provokes negative reactions not only from those who rejected the offer, but also from those who were "left out" of clientelist transactions (i.e., were not offered money). Those individuals punished the vote-buying candidate by casting their vote for the competing, non-clientelist candidate.

Furthermore, we find that subjects were likely to evaluate a vote-buying candidate more negatively than a candidate running on a platform of programmatic redistribution. This result stands in contrast to parts of the literature emphasizing that clientelism may allow candidates to obfuscate normatively objectionable strategies (Mares & Young, 2019)—since it suggests that candidates cannot use clientelism to signal competence. In addition, we find that individuals were equally confident expressing negative evaluations about the vote-buying candidate regardless of whether their vote choice was protected by the secret ballot.

Our findings also show that vote buying significantly reduces individuals' stated preferences for more government spending on police and law enforcement—yet not on other welfare areas such as unemployment benefits or health. Hence, overall, there is not a robust effect of vote buying on stated preferences for redistribution.[21] Finally, we find that subjects' willingness to contribute to public goods is significantly and negatively affected by the presence of open ballots, but not significantly affected by vote-buying campaigns.

Some other unexpected findings and alternative interpretations of our results suggest directions for future research. For example, we were surprised to find support for Candidate B (in the first round of the elections) in the "No Vote Buying, Secret Ballot" treatment (Fig. 1). What might motivate this? One possible interpretation is based on framing effects. Recall that in the "No Vote Buying" treatments, the difference between Candidate A and Candidate B was that Candidate B simply did not redistribute as much back to participants, as one fourth of the common pool was kept to cover "electoral expenses." Framing Candidate B's platform as "covering the cost of electoral expenses" could have led some subjects to positively evaluate Candidate B's investments in electoral expenses (in the absence of vote buying) as a sign of effort. In the first round, this framing effect could be strong enough to make individuals vote against their self-interest (giving them a lower individual payoff). But in the second round, the increased support for Candidate A in the second round of the "No Vote Buying" treatment seems to suggest that self-interest played a stronger role: once subjects realized that Candidate A was likely to win again, they shifted to that side. Given our data, this interpretation remains a conjecture and calls for future research.

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[21] This result may be related to contributions on social desirability bias (e.g., Gonzalez-Ocantos et al., 2020) and attitude–behavior (in)consistency (e.g., Guerra & Harrington, 2018). Subjects may not be willing to truthfully report their own preferences in surveys (e.g., because they fear admitting to participating in stigmatizing or illegal behavior), but in situations where monetary incentives are at stake (e.g., our public goods game), their behavior does change in response to vote-buying campaigns.

Related to this point, a second surprise was that in both rounds, the percentage of subjects voting for Candidate A was higher in the presence of open ballots versus secret ballots (Fig. 1). Although the experimental design does not allow us to determine whether such difference is statistically significant (see footnote 17), the question arises: Why would open ballots make a difference? A possible interpretation is based on voters' perceptions of ballot secrecy. Afrobarometer data show that nearly one in four people in Africa doubts ballot secrecy. When such doubts become pervasive, ballot secrecy may backfire by boosting vote buying and intimidation strategies (Ferree & Long, 2016). Open ballots may reduce such uncertainty, hence mitigating the effects of vote buying on voters' behaviors. It would be interesting to test this interpretation in future studies, for example by asking participants post-experiment questions about their perceptions of ballot secrecy and the reasons underlying their voting choices.

A third issue concerns the effect of open ballots on public goods contributions. The regression estimates reveal that the presence of the open ballots mitigated—and did not exacerbate—the negative impact of vote buying (Table 8). Specifically, vote buying reduced subjects' willingness to contribute to public goods—but this negative effect decreased in the presence of open ballots. Although being only weakly significant (p values < 0.10 in col. 8 of Table 8), these findings suggest directions for future research. Indeed, while they may appear unexpected, there is evidence showing ambiguous effects of ballot secrecy on vote-buying practices (Cox & Kousser, 1981; Kam, 2017; Morgan & Várdy, 2012). Those contributions reveal that secret ballots changed the form of clientelism, from vote buying to turnout buying or abstention buying ("negative vote buying"). For instance, Morgan and Várdy (2012) show that ballot secrecy increased—rather than decreased—the scope of vote buying (i.e., the fraction of voters not voting according to their intrinsic preferences). Our results contribute to this literature, showing that the expected effectiveness of ballot secrecy as a countermeasure to electoral clientelism is not straightforward and deserves further investigation.

With these caveats, this paper is the first—to our knowledge—to experimentally investigate behavioral spillover effects of vote buying and ballot secrecy on a bundle of candidate choice, support for redistribution, and public goods provision. Further research is needed to understand individuals' motivation underlying voting choices and contributions to public goods, and to explore the conditions under which vote buying and ballot secrecy could affect both voters' and candidates' behavior, as well as the demand for and supply of programmatic redistribution. Fear of retaliation, voters' possibility of abstaining, citizens' perception of electoral fraud and/or ballot secrecy, and real—and not computer-simulated—political candidates, as well as different types of electoral malpractice (e.g., threat of violence, coercion, gift exchange), represent some of the possible extensions to our study as interesting avenues for future research endeavors.

Availability of data Available upon reasonable request.

Declarations

Conflict of interest No conflicts of interest/competing interests.

Ethics approval All procedures performed in this research were in accordance with the ethical standards of the institution at which the research was conducted.

Consent to participate Informed consent was obtained from all individual participants included in the study.

Pre-registration This experiment has been pre-registered on https://osf.io/9gy7b/.

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