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Enforcing Vote-Buying: Experimental Evidence from Angola

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Abstract
How do politicians enforce vote-buying agreements when ballots are secret? This asymmetric information problem lies at the heart of the vote-buying literature, yet existing research has largely described and examined individual enforcement strategies in isolation, rather than their relative effectiveness. We address this gap and compare the effectiveness of five common enforcement strategies in a randomized vignette experiment in Angola: turnout buying, questioning ballot secrecy, machine politics, reciprocity, and a novel signaling mechanism. We find that only tangible enforcement strategies—monitoring turnout and casting doubt on ballot secrecy—significantly increase voting intentions. By contrast, clientelistic enforcement strategies based on promises of future benefits have no effect and even backfire among politically interested voters. Our results suggest that the effectiveness of vote buying depends on the type of enforcement employed and that strengthening ballot secrecy and voter information may reduce the effectiveness of vote-buying. A complementary list experiment confirms the prevalence and provides robustness for our measurement of vote-buying.

Keywords
Vote-buying; Enforcement Strategies; Vignette Experiment; Angola; Electoral incentives; Political accountability; Clientelism

JEL classification
D72; C93; O17

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1 Introduction

Illicit influence during elections, prominently in the form of vote-buying — the exchange of money or gifts for a promised vote around election dates — is pervasive worldwide. Although precise measurement is inherently difficult, recent data from the World Values Survey provides a useful benchmark: on average, 61% of respondents in Africa, 47% in Asia, and 58% in Latin America reported that voters in their country are bribed (often or very often) during elections.[1] Further, vote-buying is shown to have negative consequences, such as reduced accountability, perpetuation of corruption and clientelism, risking worse public-good provision (Khemani, 2015). Yet, despite its prevalence, harmful consequences, and a growing body of research, the central vote-buying puzzle remains unanswered: why is vote-buying effective despite the secret ballot, when voters could accept a payment and still vote for whomever they prefer?

Central to this puzzle is asymmetric information: under the secret ballot, votes are unobservable, creating uncertainty about whether payments translate into votes. Vote buyers use enforcement strategies to overcome this information asymmetry. The importance of such strategies has been recognized since the early literature on vote-buying (e.g., Brusco et al., 2004), and theoretical models examine a range of possible enforcement strategies (Gans-Morse et al., 2014; Keefer and Vlaicu, 2017). Empirical evidence on these strategies, however, remains limited. Finan and Schechter (2012) study reciprocity as an enforcement mechanism in isolation, while Stokes (2005) and Nichter (2008) provide correlational evidence on machine politics and turnout buying, respectively. Hence, existing empirical work does not fully capture the strategic environment faced by vote buyers, who can choose among multiple enforcement mechanisms rather than rely on a single mechanism.

We address this limitation by comparing the relative effectiveness of several enforcement strategies within a common experimental framework. This study draws on a vignette experiment conducted in Angola's Huambo province. All respondents are presented with a fictitious politician's statement promising public goods and asking for their vote. Respondents are then randomly assigned to one of six conditions varying the enforcement strategy: five treatment arms offering a fictitious monetary amount in exchange for the respondent's vote, each paired with a distinct enforcement strategy, and one control arm without a promised payment or enforcement strategy. These strategies include those most prominently discussed in the literature as well as a novel signaling mechanism, which frames the payment as a signal of the politician's goodwill and future intentions rather than as a transaction tied to future patronage. We group them into two categories: tangible enforcement strategies, which have immediate consequences, and clientelistic enforcement strategies, which convey promises of future benefits. Respondents then state their voting intentions on a scale from one to seven.

Tangible enforcement strategies include 1) Turnout Buying, which involves surveilling voter

[1] Figures are calculated as the mean share across the 91 countries participating in the World Values Survey Wave 7, 2017–2022 (Haerpfer et al., 2022), that included question Q227: "In your view, how often does the following thing occur in this country's elections: Voters are bribed?" Response options "very often" and "fairly often" were combined, and the country-level percentages were then averaged by region.

presence on election day, and 2) Questioning Ballot Secrecy, which involves insinuating that secret votes can be observed. Clientelistic enforcement strategies comprise 3) Machine Politics, emphasizing party influence on daily life and future benefits; 4) Signaling, presenting payment as an expression of care and an indication of beneficial future policies; and 5) Reciprocity, invoking norms of reciprocal exchange between money and a favorable vote. Comparing each of the five vignettes to the control yields an estimate of the effectiveness of each enforcement strategy. In a second experiment, we use a list experiment to estimate the prevalence of vote-buying.

We study these enforcement strategies using a representative sample of ex-combatants from both sides of the Angolan civil war (1975 to 2002). Post-conflict and consolidating democracies such as Angola provide an informative setting for this question: weak state delivery, pervasive corruption, and limited credibility of political promises are widespread features of environments in which vote-buying is most prevalent. Ex-combatants are a particularly relevant population in such settings: Angola's civil war involved mass mobilization on both sides, and decades on, former combatants and their households still make up a substantial share of the adult population in provinces such as Huambo, making their political behavior directly consequential for electoral outcomes rather than a marginal subgroup.

Our results show that enforcement strategies matter. Only tangible enforcement strategies, Turnout Buying and Questioning Ballot Secrecy lead to an increase in voting intentions. By contrast, clientelistic enforcement strategies are ineffective and do not alter voting intentions. The heterogeneity analysis reveals that among politically interested individuals, clientelistic enforcement strategies backfire. Despite offering money, these strategies lead to a decrease in voting intentions. Based on our list experiment, we estimate that 38% of our sample experienced vote-buying in the past election, consistent with our direct measurement.

1.1 Related Literature

Existing research on vote-buying has made substantial progress in explaining whom politicians target with vote-buying offers.[2] Brokers play an important role and tend to occupy central positions in local networks, allowing them to identify politically uncommitted or otherwise receptive voters (Cruz, 2019; Duarte et al., 2026). In particular, brokers can identify voters with reciprocal preferences, and targeting these voters increases electoral returns (Finan and Schechter, 2012). Yet explaining who is targeted is distinct from understanding how politicians convert payments into electoral support and sustain compliance. Existing empirical studies generally examine individual mechanisms—such as reciprocity, monitoring turnout, or machine politics—in isolation, or provide correlational evidence on the conditions under which vote-buying succeeds (Stokes, 2005; Nichter, 2008). Providing direct evidence on how payments generate compliance despite the secret ballot, Hicken et al. (2018) use a field experiment to show that accepting money creates a sense of indebtedness and increases voters' willingness to reciprocate in the voting booth. Our main contribution

[2]See Mares and Young (2016) and Graham et al. (2026) for a broader overview of the vote-buying and recent clientelism literature.

is to move beyond such mechanism-specific evidence by comparing several enforcement strategies within a common experimental framework. This allows us to assess whether strategies based on the mechanics of secret voting differ systematically from those relying on relationships, reciprocal obligations, or future benefits.

Our second contribution concerns the measurement of vote-buying. Because vote-buying is illegal and stigmatized, direct survey questions are vulnerable to social desirability bias and fear of sanctions (Blair et al., 2020). List experiments mitigate these concerns by protecting individual responses while allowing researchers to estimate prevalence at the sample level (Blair and Imai, 2012). The difference can be substantial: in Nicaragua, only 2% of voters directly reported accepting electoral gifts, whereas a list experiment produced an estimate of 24% (González-Ocantos et al., 2012). We apply this method to provide the first list-experiment estimates of vote-buying in Angola. In doing so, we implement five variants of the design that vary list length, item tone, the inclusion of a placebo sensitive item, and the level of reference, contributing new evidence on the sensitivity of list-experiment estimates to these design choices.

Quantifying the prevalence of vote-buying and understanding the effectiveness of enforcement strategies also has policy implications. Interventions against vote-buying have so far produced mixed results: appeals encouraging voters to take payments but vote independently can weaken vote-buying (Vicente, 2014) but may prompt a strategic reaction to expand targeting or raise payments (Cruz et al., 2021; Duarte et al., 2026), whereas radio and civic-oversight campaigns proved both effective and cost-effective (Schechter and Vasudevan, 2023; Garbiras-Díaz and Montenegro, 2022). More broadly, vote-buying can become self-reinforcing: voters who accept payments may demand less accountability (Leight et al., 2020) and politicians may find it more cost-effective to engage in vote-buying, rather than programmatic policies (Cruz et al., 2024). Taken together, these findings highlight the complexity of the forces that sustain vote-buying and suggest that designing effective interventions requires a better understanding of the enforcement mechanisms that perpetuate it.

The rest of the paper is structured as follows. Section 2 reviews the enforcement mechanisms of interest. Section 3 describes the Angolan context. Section 4 details data collection. Section 5 describes the experimental design and the empirical strategy. Section 6 discusses the results. Section 7 concludes and draws policy implications.

2 Enforcement Mechanisms

Vote-buying can be conceptually divided into two parts. First, it comprises a monetary transaction: the exchange of money or goods for a promise to vote for the candidate. Second, it often also includes enforcement mechanisms; these are actions, promises or threats, that the vote buyer (politician) uses to overcome the asymmetric information problem and ensure that on the election day, the voter casts the vote for the buying politician. These include direct or indirect monitoring, but crucially also the framing of the exchange, which determines the effectiveness of vote-buying and

voters' compliance on election day.

We test the most commonly observed and theoretically discussed enforcement strategies and propose a novel distinction between two categories. The first category, which we call 'tangible enforcement strategies', i.e., acts related to vote secrecy that carry potentially immediate consequences during or on election day. The second category, which we call 'clientelistic enforcement strategies', entails promises of future benefits conditional on voting for the desired party. Next, we describe the enforcement mechanisms used in our experiment.

Tangible Enforcement Strategies: Turnout Buying. Turnout buying is an enforcement strategy in which vote buyers monitor whether targeted individuals appear at the polling station. This makes abstention observable, but also increases the mental cost of voting for another candidate. Nichter (2008) argues that turnout buying reduces monitoring cost because brokers need only observe whether an individual votes, rather than infer the content of the ballot. The authors predict that turnout buying targets moderate supporters, who would abstain without additional incentive. In practice, it may involve facilitating participation, for example, by organizing private transport to the polling station (Larreguy et al., 2016).

Questioning Ballot Secrecy. Questioning ballot secrecy is an enforcement strategy in which vote buyers claim or imply that they can observe how individuals vote, raising doubts about the de facto secrecy of the vote. Such claims may create fear of negative consequences if they do not vote for the vote-buying party. This strategy may be particularly viable in countries with weak electoral oversight or authoritarian governments, where concerns about the integrity of the voting process prevail (Stokes, 2005). In fact, subjective beliefs in ballot secrecy are decisive and influence turnout (Gerber et al., 2013). This strategy is widely used because it can create doubts about ballot secrecy at low cost and with minimal actual monitoring.

Clientelistic Enforcement Strategies: Machine Politics. Machine politics uses party organizations and state resources to secure electoral support by making employment opportunities and other forms of economic participation contingent on vote choice. Unlike turnout buying, which operates at the individual level, machine politics leverages broader patronage networks to enforce political loyalty (Stokes, 2005). These networks combine promises of future party-mediated benefits with implicit threats to withdraw existing ones. Brusco et al. (2004) highlight that machine politics embeds voters in networks of dependence, thereby fostering persistent relationships beyond one-shot vote-buying exchanges. Its effectiveness depends on voters' beliefs that the party will be able to provide or withdraw these benefits after the election.

Signaling. We introduce signaling as a novel enforcement mechanism, informed by observations in the field. We use the term here to describe how a politician presents a payment as evidence of caring for the recipient and as an indication that the politician and party will act in the recipient's interest if elected. Although this mechanism resembles machine politics, it emphasizes the perceived qualities and intentions of the individual politician rather than the organizational capacity of the

party. In addition, being a good politician does not necessarily entail patronage: it could be about doing well through public policy and public good provision.

Reciprocity. This enforcement mechanism treats vote-buying as a balanced exchange, where receiving material benefits creates a social obligation to return the favor through voting. It is portrayed as an equal exchange, *I do something for you, you do something for me.* Note that vote-buying is common and widely accepted. In contrast to Signaling, this enforcement strategy appeals to, and exploits, the individual norm of reciprocity, present in communities with strong social ties (Finan and Schechter, 2012).

3 Context

Angola experienced more than four decades of conflict, beginning with the war of independence from Portuguese colonial rule (1961–74) and continuing after independence in 1975 as a civil war between the MPLA (People's Movement for the Liberation of Angola) and UNITA (Union for the Total Independence of Angola), supported by competing foreign powers during the Cold War. The conflict ended in 2002 with an MPLA victory, establishing the party as Angola's dominant political force. The end of the war coincided with rapid, oil-driven growth: real GDP grew by an average of 8.1% between 2003 and 2013, although poverty remained widespread and inequality high. Huambo province, where our data were collected, lies in Angola's agricultural Central Highlands, historically the country's breadbasket. Its economy is dominated by smallholder agriculture and informal activity, with little formal industrial employment (World Bank, 2020). Huambo city, with roughly 600,000 inhabitants in 2014, serves as the regional economic hub, with non-agricultural employment concentrated in low-paying commerce, services, and transport.

Politically, the post-war period combined weak institutions and widespread corruption with strong initial support for the MPLA, which was closely associated with the return of peace. The party won 81.6% of the vote in the first post-war general election in 2008. However, by the 2012 election dissatisfaction with corruption, patronage, and the distribution of the gains from the post-war boom had become more visible, even as MPLA dominance remained strong. Our study population consists of ex-combatants from both the MPLA and UNITA. Both organizations imposed compulsory military service on young men in territories under their control. In Huambo, where territorial control shifted repeatedly during the war, conscription therefore depended heavily on geography and timing rather than political preferences (Justino and Stojetz, 2025).

4 Data Collection

Our study takes place in the province of Huambo in the Angolan highlands, see Figure 1, approximately seven months after the August 2012 general election. This timing ensures that electoral experiences remained salient when respondents answered the vignette and list experiment modules. Data collection was part of a larger mixed-methods research project on ex-combatants in Angola

(Spall, 2020; Justino and Stojetz, 2025; Stojetz and Brück, 2023). Informed consent was obtained from all participants. Given the sensitivity of questions on vote-buying and political behavior, the experimental module was placed after trust had been established through earlier household-level modules covering well-being, family structure, and economic activities.

4.1 Sampling Strategy

To achieve the most representative sample, the survey employed three levels of randomization, where the first two involved the primary sampling unit (PSU) and enumeration area (EA) level. PSUs (bairro) were comunas (the lowest tier formal administrative unit) in rural areas, and boroughs in urban areas. In each sampled PSUs, we drew EAs, which were one settlement or multiple very small settlements in rural areas, and equivalent to neighborhoods in urban areas. In the absence of systematic and reliable veteran population data, we used general population data from the ongoing Angolan census to randomly draw PSUs and EAs, with weights proportional to the population. At each survey site, we engaged with relevant stakeholders, local administrations, chiefs and coordinators to produce listings of all former soldiers residing in the EA. The resulting lists were double- and cross-checked that they credibly captured the local veteran population. Provided that the general population is a reliable proxy for the ex-combatant population in the first sampling stages, the sampling strategy is self-weighting and yields representative geographic coverage across Huambo province. Assuming that we did obtain complete lists of ex-soldiers, the EA-level sample is representative of the EA-level veteran population.

4.2 Descriptives

Our sample (see Table 1) is made up entirely of men, all heads of the household, average age 49.7, with sociodemographic characteristics broadly similar to the general male population of Huambo of similar age (Table A.1). Most reside in rural areas (60%) and work informally, with only 14% in formal employment but 87% holding land. Despite moderate political interest (2.8/5), voter registration and turnout are near-universal (98% and 97%), and, among those reporting a party choice, 81% voted for the ruling MPLA– exceeding the officially reported 2012 provincial results of 68.2% (Comissão Nacional Eleitoral, 2012). While 45% have heard of prior war veteran demonstrations, many declined to report past or potential future participation, suggesting sensitivity around political questions. Comparing baseline characteristics of individuals in each vignette (Table A.2), across a range of demographic and household characteristics there are virtually no significant differences between vignette arms, suggesting that randomisation across vignettes was achieved. The only significant differences are observed in education, as individuals in the Turnout Buying and Ballot Secrecy vignettes are on average 0.8 and 0.7 years more educated.

5 Experimental Design

5.1 Vignette Experiment

Our main experimental component is a vignette-based survey experiment, embedded in data collection. Each respondent was randomly assigned to one of six vignettes presenting a short statement by a fictitious political candidate. To avoid confounding responses with pre-existing partisan attachments, respondents were explicitly told that the candidate could belong to any party, including the MPLA, UNITA, or another party. The baseline vignette featured a speech emphasizing broadly supported policy goals, such as improving education and health care provision, and requested electoral support without mentioning material inducements or enforcement strategies. In the five treatment vignettes, the politician, in addition to this baseline statement, offered money in exchange for voting for the candidate. The monetary amount in each hypothetical scenario was 1,000 Kwanza (approximately 10 USD), equivalent to roughly 20% of the median monthly household income in our sample. Each vignette conveys one enforcement mechanism. Four of these enforcement mechanisms correspond to well-documented strategies in the literature: 1) Turnout Buying, 2) Questioning Ballot Secrecy 3) Machine Politics and 4) Reciprocity. We also introduce a novel fifth mechanism, 5) Signaling, which is observed in practice, but has not been experimentally evaluated. The original questionnaire is shown in the appendix D.

After hearing the general statement of the politician, all treatment vignettes proceed with a call to action: *As proof that I care for your well-being I am leaving you with 1000 Kwanza [..].* Each vignette then varies the stated enforcement: 1) Turnout Buying: *[..] Since I know you are my supporter I will be waiting to see you near the polling station on the day of the election.*; 2) Questioning Ballot Secrecy *[..] This is done in confidence that you will vote for me on the day of the election. It is not difficult for me to know for whom you have voted*; 3) Machine Politics: *[..] Since I know you are my supporter I hope you will be able to take the effort of voting for me and continue supporting our party in the future.*; 4) Reciprocity *[..] Today I am doing the best for you, I hope you do the best for me in the future.* and 5) Signaling: *[..] This is a sign that my candidacy and my party really care about you individually and will do the best for you once I win the elections.*

This design allows us to isolate the causal effect of exposure to different enforcement strategies on self-reported voting intentions. Immediately after hearing the assigned vignette, respondents were asked to rate, on a scale from 1 to 7, how likely they would be to vote for the candidate in the next election if the situation were real. Random assignment ensures comparability across groups and the use of standardized scripts minimizes interviewer discretion. In this setting, vignette experiments offer a viable approach to elicit preferences, with prior work validating that well-designed vignettes capture reactions reflecting real-world behavior (Hainmueller et al., 2015; Boas et al., 2019). The near-universal turnout (97%) and voter registration (98%) in our sample further support this interpretation, indicating that the relevant decision margin is the choice between candidates rather than the decision to participate.

5.2 List Experiment

We employ a list experiment to validate our direct estimate of vote-buying prevalence and to mitigate concerns about social-desirability bias in direct responses. With this, we provide the first experimental, indirect estimates of the prevalence of vote-buying in Angola. In a list experiment, respondents are randomly assigned either to a control group, which receives a short list of non-sensitive statements, or to a treatment group, which receives the same list plus a sensitive item. The difference in mean counts between treatment and control identifies the prevalence of the sensitive behavior (Blair and Imai, 2012).

The interviewer introduces the exercise as follows: *"I am going to read you a list of some of the things that people have told us happened to them during the 2012 campaign. I am going to read you the whole list, and then I want you to tell me how many of the different things happened to you. Please do not tell me which of the things happened to you, just how many. If you would like me to repeat the list, I will do so."*

In our main specification, respondents receive four neutral control items. These ask whether they: (i) accepted putting campaign posters on their house; (ii) listened to the radio every day to learn about the campaign; (iii) met a campaigner to discuss their ideas for the country; and (iv) discussed the campaign with a *soba* (village chief). Respondents assigned to the treatment group receive an additional sensitive item: *"You received money or a gift from a party or politician"*. An additional discussion of the list experiment design is presented in the appendix C and the original questionnaire can be found in appendix D.

We additionally implement four variants of the list experiment to assess the consistency of the estimates. The first reduces the number of control items to three, testing sensitivity to list length. The second replaces the sensitive item with a placebo item of zero expected prevalence, following Riambau and Ostwald (2021), providing a falsification check. The third varies the tone of the control items toward strongly worded formulations (e.g., "You were threatened to vote a certain way"; "You heard news on the radio about political violence"), keeping the sensitive item unchanged, to test sensitivity to control-item framing. The fourth asks both control and sensitive items at the village level (e.g., "Most people in your village discussed the campaign with a *soba*"), shifting the level of reference. Respondents answer all list-experiment variants in the same, non-randomized order and, for each variant, are independently re-randomized between the control and treatment lists.

5.3 Estimation

We estimate the effect of the experimental treatments on respondents' stated voting intentions using an ordinary least squares regression. The outcome variable is the likelihood of voting for the candidate, measured on a scale from one to seven.

$$Y_i = \beta_0 + \sum_{k=1}^{5} \beta_k T_{ik} + \gamma X_i + \lambda_v + \varepsilon_i, \tag{1}$$

where Y_i denotes respondent i's reported voting intention and T_{ik} is an indicator for assignment to treatment arm k: Turnout Buying, Ballot Secrecy, Machine Politics, Reciprocity, or Signaling. The control vignette is the omitted category, so each β_k captures the treatment effect relative to the control group. The vector X_i includes individual-level controls for age, years of education, and household size, while λ_v denotes village fixed effects. The error term is denoted by ε_i. Standard errors are clustered at the village level. Given the 34 clusters, we use the Bell-McCaffrey small sample correction (Imbens and Kolesar, 2016).

6 Results

6.1 Vignette Experiment

Our preferred specification in column (5) of Table 2 with controls for age, education, household size, and village fixed effects and with standard errors clustered at the village level, shows that two of the five vignettes shift voting intentions relative to the control. Both are tangible enforcement strategies and have a statistically significant and positive effect. Individuals in the "Turnout Buying" vignette raised voting intentions by 0.54 points (on a scale from 1–7), which is equivalent to a 21.6% increase over the unconditional control group mean of 2.5 and significant at the 5% level. Questioning ballot secrecy leads to a greater increase in voting intentions of 0.705 points, or 28.2% compared to the control group. The results are robust to alternative variance estimators (Table B.1) and to model specifications (Table B.2) reported in the appendix.

In contrast, clientelistic vignettes: "Machine Politics", "Signaling" and "Reciprocity" do not significantly alter voting intentions. The "Machine Politics" vignette suggests negative effects, but becomes insignificant after including village fixed effects. Individuals only change their voting intentions when presented with tangible enforcement strategies, namely the threat to surveil turnout, or questioning ballot secrecy. Clientelistic enforcement strategies, such as promising to deliver future benefits or appealing to positive traits of politicians such as reciprocity and signaling, contrary to our hypothesis, do not influence voting decisions. Part of this could be explained by a salient lack of credibility of the ruling party when promising benefits for the future.

6.2 Heterogeneity Analysis

We next examine heterogeneity along covariates that are linked to susceptibility to vote-buying: political interest (Cruz et al., 2021), network embeddedness (Cruz, 2019), partisanship (Finan and Schechter, 2012), rural-urban location (Vicente, 2014), and income (Bobonis et al., 2022). We classify respondents as politically interested if they report a value of four or above on a 1–5 scale; network embeddedness is proxied by participation in one or more local groups; partisanship is captured by the party voted for (MPLA or UNITA) in the 2012 election; location distinguishes rural and urban respondents; and income is measured relative to the median in our sample. We compute unconditional differences in means relative to the control group and report 95% confidence intervals.

To increase power, we group the enforcement strategies into tangible and clientelistic categories; heterogeneity results for each enforcement strategy are separately reported in the appendix.

Political interest moderates the effects of vote-buying strategies (see Figure 2). Among respondents interested in politics, clientelistic enforcement strategies—"Machine Politics" and "Reciprocity"—significantly reduce voting intentions relative to the control group (Figure B.5). Among respondents not interested in politics, by contrast, the estimated effect of these strategies shows no effect. Tangible enforcement strategies significantly increase voting intentions among respondents not interested in politics. Overall, pairing a monetary offer with clientelistic promises appears to backfire among politically interested respondents, reducing their voting intentions relative to the control group despite the additional payment.

The remaining heterogeneity analysis reveals a consistent, yet surprising pattern: individuals systematically increase their voting intentions when subject to tangible enforcement strategies and do not change their voting intentions when facing clientelistic enforcement. This is consistent for differing network size, proxied by participation in groups, location (rural or urban), income, as well as, vote choice in the past election - although by the small number of observations the confidence intervals for UNITA are high. The results for the binary classification (Figures B.1–B.4) and the complete vignette analysis (Figures B.5–B.9) are reported in the appendix.

In general, we find that the choice of enforcement strategies matters. The perceived credibility of enforcement strategies seems to be the decisive factor in determining the effectiveness of enforcement strategies. Although tangible enforcement strategies increase voting intentions across all categories, clientelistic strategies remain ineffective. For the politically interested, these even backfire and lead to a reduction of voting intentions.

6.3 List Experiment Results

We find evidence that vote-buying, despite its high prevalence, remains a sensitive topic. Among respondents who answered the direct individual-level question, 44% reported having been offered money or a gift in exchange for their vote (see Table 1). The reported prevalence increases to 58% when respondents are asked about vote-buying at the village level. However, only 18% of the full sample answered the individual-level question, while the remaining respondents refused to respond. High prevalence and non-response justify indirect elicitation. Figure 3 reports the list-experiment estimates across all five variants. Our preferred specification (4 control + 1 sensitive questions) estimates vote-buying prevalence at 38%. This estimate is close to the 44% obtained from the direct individual-level question and further indicates that vote-buying attempts are widespread in our sample. The remaining four variants serve as robustness exercises, testing sensitivity to list length, item tone, level of reference, and the validity of the design itself.

The robustness exercises yield a mixed but informative picture. The placebo sensitive item is statistically insignificant, as expected under a well-functioning list-experiment design, while the strong-control variant produces estimates close to the baseline. Both findings support the internal consistency of list experiment. The shorter (3+1) list, however, yields a prevalence estimate of

17%, and the village-level reformulation produces a lower and statistically insignificant estimate than the individual-level questionnaire, contrary to expectations and findings in the prior literature. We interpret these differences cautiously. Sequential exposure to multiple list experiments may generate fatigue or learning effects, behavioral channels that remain underexplored in the literature beyond the well-studied double-list design (Glynn, 2013). The divergence of village-level results similarly underscores that list-experiment estimates can be sensitive to design choices or ordering, informing the broader literature on measurement of sensitive behavior. The divergence does not undermine our main estimate, which remains unaffected by these concerns. Recent methodological innovations, such as the ballot-bag survey method (Crépon et al., forthcoming), provide promising complementary approaches to address the bias-variance trade-off in measuring sensitive behavior.

7 Conclusion

Our study sheds light on enforcement strategies as a potential explanation for how the vote-buying puzzle is overcome. This puzzle stems from asymmetric information, voters could accept money in exchange for their promised vote, but proceed to vote for whomever they wish. However, vote-buying remains prevalent. We are the first to empirically test and compare the relative efficacy of the most widely discussed enforcement strategies. Using a representative survey of war veterans in Angola and a randomized vignette design, we tested five enforcement strategies linked to vote-buying, including a novel signaling mechanism.

We find that the choice of enforcement strategies matters. Only tangible enforcement strategies, namely monitoring voter turnout or questioning ballot secrecy, significantly increase self-reported voting intentions. By contrast, clientelistic enforcement strategies, including promises of patronage or appeals to reciprocity, have no detectable effect on stated support. This pattern is consistent across subgroups defined by social networks, location (rural versus urban), past vote-choice (UNITA or MPLA) and income, all dimensions expected to influence the effectiveness of vote-buying. Political interest is the notable exception: among politically interested respondents, adding clientelistic enforcement strategies to a promised payment significantly reduces voting intentions.

In addition, our list experiment provides the first indirect estimate of the prevalence of vote-buying in Angola, indicating that vote-buying attempts are common: we estimate that 38% of our sample were offered money or gifts for votes. Although the list experiment confirms the results of our direct vote-buying question, marginally varying the design of the questions yields varying estimates and raises concerns about the internal consistency of the methodology. These differences highlight the sensitivity of list-experiment estimates to design and ordering, informing the broader literature on the measurement of sensitive behavior.

These results reinforce the idea that compliance in clientelistic exchanges relies on credible enforcement and that these strategies matter. In contexts where citizens have observed the failure to deliver promises, clientelistic strategies are ineffective. Among informed and politically interested voters, these strategies can even backfire, consistent with evidence that additional political

information may reduce electoral support (Ahmed et al., 2024; Cruz et al., 2021). Only tangible enforcement mechanisms, such as turnout buying and questioning ballot secrecy, affect intentions to vote. These results holds important policy implications: On the one hand, strengthening (the belief in) ballot secrecy and a safe voting environment may reduce the effectiveness of vote-buying. On the other hand, political knowledge or increased interest may decrease the effectiveness of enforcement strategies.

Ethics approval

The survey protocol received IRB approval (IRB Services Protocol USIP-070-10F).

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Declaration of competing interests

The authors declare no competing interests.

Declaration of generative AI and AI-assisted technologies in the manuscript preparation process.

During the preparation of this work the authors used Claude (Opus 4.6) in order to control for grammatical errors, refining the language and debugging of code. After using this tool/service, the authors reviewed and edited the content as needed and take full responsibility for the content of the published article.

Data availability

Data will be made available upon request.

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Tables and Figures

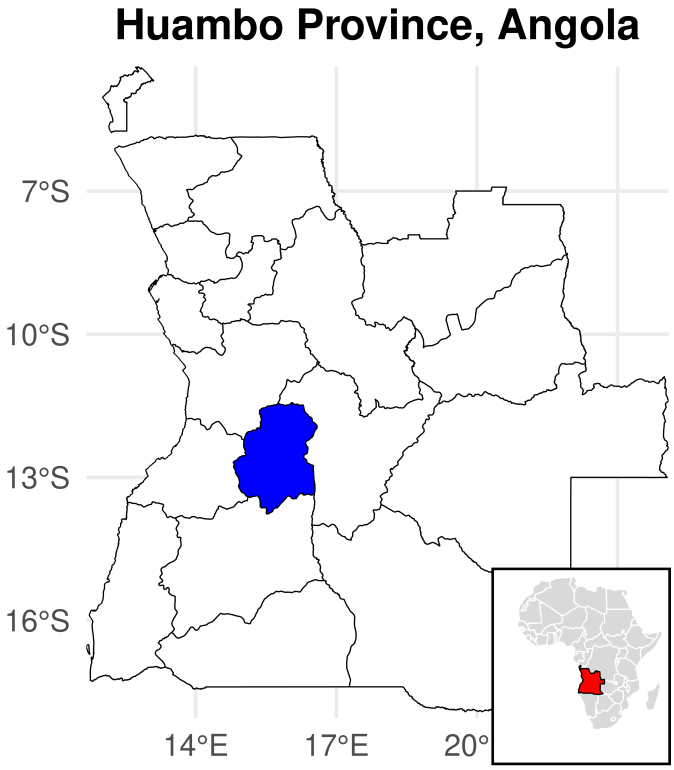


Figure 1: Map of Angola

Table 1: Descriptive Statistics

Variable	Mean	SD	Min	Max	N
Age	49.71	9.85	25	86	759
Household Size	7.05	2.87	1	17	759
Head of Household	1	0	1	1	759
Married	0.41	0.49	0	1	759
Formal Job	0.14	0.35	0	1	759
Land Access	0.87	0.33	0	1	759
Political Outcomes					
Interest in Politics (1–5)	2.81	1.62	1	5	756
Registered to Vote	0.98	0.14	0	1	759
Voted (2012 Election)	0.97	0.17	0	1	758
Voted for MPLA	0.81	0.39	0	1	642
Heard of Demonstrations	0.45	0.50	0	1	759
Attended Demonstration	0.11	0.32	0	1	325
Will Attend Future Demonstrations	0.20	0.40	0	1	640
Vote Buying					
Received Gift	0.44	0.50	0	1	133
Received Gift: Village Level	0.58	0.49	0	1	156

Note: "Heard of Demonstrations" refers to ex-combatant protests in Luanda and Huambo preceding the 2012 elections. "Received Gift" indicates an affirmative response to whether the respondent was offered a gift by a party or politician during the 2012 election. The village-level variant asks whether most people in the respondent's village received such gifts. "Voted for MPLA" is conditional on having voted and answering this question.

Table 2: OLS Regression: Enforcement Mechanisms & Voting Intentions

	Dependent variable:				
	Voting Intention				
	(1)	(2)	(3)	(4)	(5)
Turnout Buying	0.594**	0.593**	0.529**	0.534**	0.539**
	(0.264)	(0.264)	(0.252)	(0.250)	(0.256)
Ballot Secrecy	0.804***	0.805***	0.749**	0.758***	0.705**
	(0.284)	(0.284)	(0.274)	(0.273)	(0.272)
Machine Politics	-0.364*	-0.362	-0.400*	-0.393*	-0.312
	(0.213)	(0.216)	(0.210)	(0.210)	(0.225)
Reciprocity	-0.048	-0.045	-0.085	-0.095	0.002
	(0.200)	(0.201)	(0.208)	(0.205)	(0.210)
Signaling	0.106	0.109	0.091	0.087	0.098
	(0.248)	(0.247)	(0.244)	(0.244)	(0.243)
Constant	2.500***	2.429***	2.123***	1.897***	1.686**
	(0.177)	(0.496)	(0.477)	(0.561)	(0.626)
Age		✓	✓	✓	✓
Schooling			✓	✓	✓
Household Size				✓	✓
Village FE					✓
Observations	759	759	759	759	759
R^2	0.030	0.031	0.040	0.041	0.090

Notes: OLS regression results of the vignette experiment. The dependent variable is the self-reported likelihood to vote for the candidate (1–7 scale). The independent variables are indicator variables for the randomly assigned enforcement mechanism; the reference category is "Control", where no money in exchange for the vote is offered. The unconditional mean voting intention in the control group is 2.5. For the exact vignette wording and the balance test, see Appendix. Standard errors in parentheses, clustered at village level (34 clusters) with CR2 small-sample correction. * p<0.1, ** p<0.05, *** p<0.01.

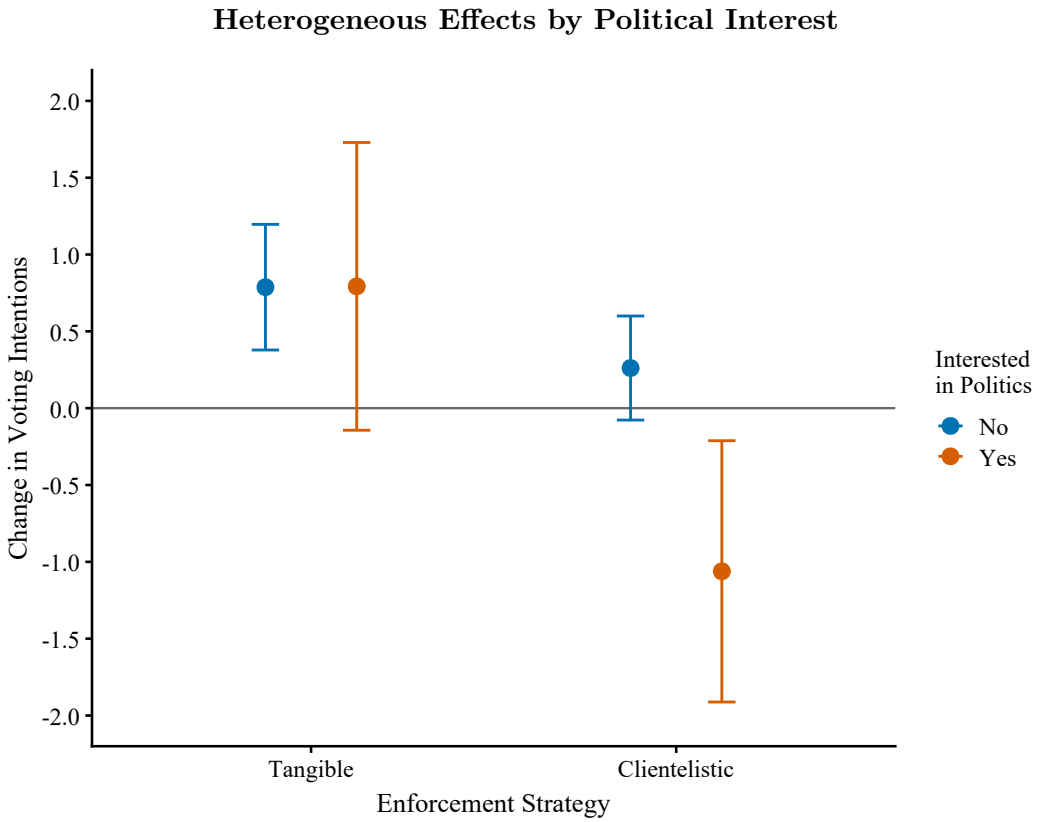


Figure 2: Differences in Voting Intentions to Control Group

Note: The sample is split into individuals according to self-stated interest in politics measured on a scale of 1 to 5. "Politically Interested" individuals are those who choose 4 or 5 vs. those reporting 1–3. In our sample, there are 260 individuals who are characterized as politically interested. The graph reports the unconditional difference in mean likelihood of voting for a candidate, relative to the respective control group. Bars report 95% confidence intervals. Tangible enforcement strategies include *Turnout Buying* and *Ballot Secrecy* vignettes. Clientelistic enforcement strategies include *Machine Politics*, *Signaling* and *Reciprocity*.

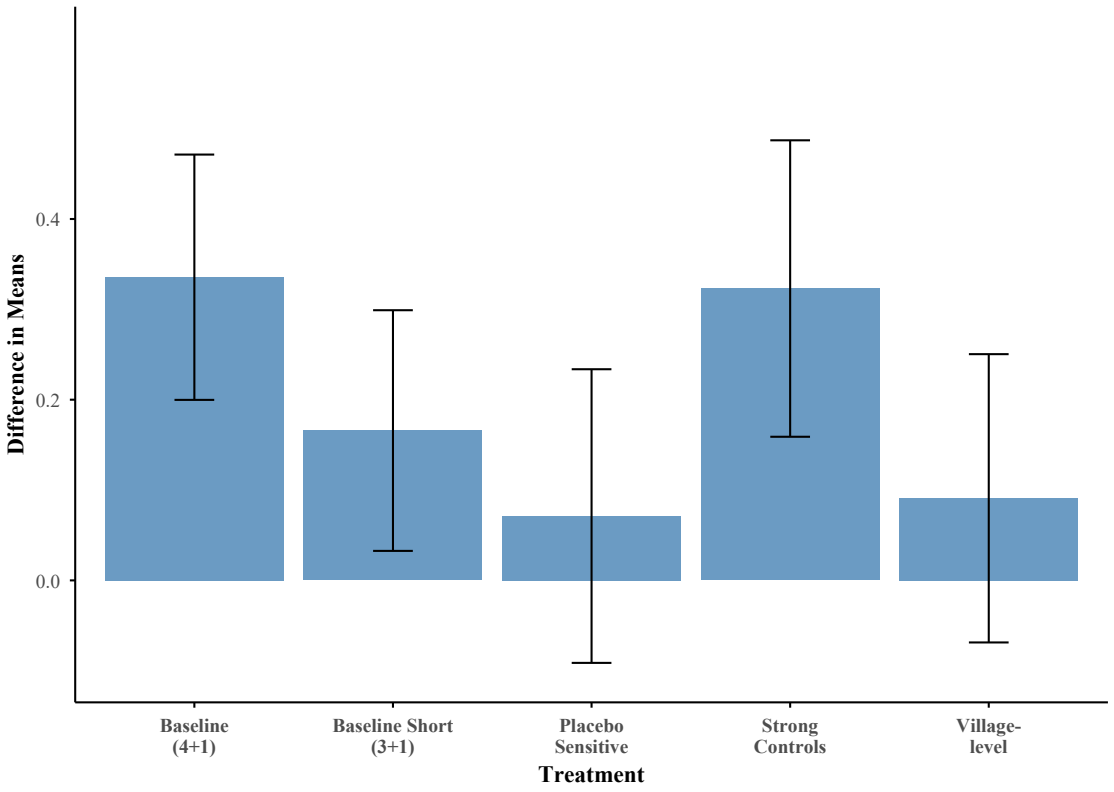


Figure 3: List Experiment Results

Note: The figure shows the estimated prevalence of vote-buying using a list experiment design. Bars represent the difference in means between the treatment group (receiving the sensitive vote-buying item) and the control group (without the sensitive item). Error bars indicate 95% confidence intervals. "Baseline (4+1)" includes five items (four control plus one sensitive); "Baseline Short (3+1)" includes four items (three control plus one sensitive); "Placebo Sensitive" replaces the sensitive item with a placebo question; "Strong Controls" uses a different set of control items while keeping the sensitive item unchanged; "Village-level" asks the sensitive and control questions at the village level ("most people in your village") instead of at the individual level.

Online Appendix

Enforcing Vote-Buying: Experimental Evidence from Angola

Andrej Smirnov Wolfgang Stojetz Pedro C. Vicente

A Additional Descriptives and Summary Statistics

Table A.1: Sample Characteristics: Survey Experiment vs. Huambo Population

Variable	Huambo Population (N=14,673)	Survey Experiment (N=759)
Age (years)	44.31 (9.58)	49.71 (9.85)
Married	0.50 (0.50)	0.41 (0.49)
Urban	0.45 (0.50)	0.40 (0.49)
Born in Municipality	0.80 (0.40)	0.74 (0.44)
Any Education	0.82 (0.38)	0.85 (0.36)
Years of Education	5.75 (3.23)	3.82 (2.78)
Household Size	6.13 (2.86)	7.05 (2.87)
Survey experiment only		
Land Access	–	0.87 (0.33)
Agricultural HH Activity	–	0.85 (0.35)
Formally Employed	–	0.14 (0.35)

Notes: Mean (SD) reported. The Huambo Population column is based on the 10 % census sample of the 2014 Angola Population Census (Instituto Nacional de Estatística, 2016), restricted to 30–66 year old male heads of the household. The selection on age represents the 95% range of age in the survey. The Survey Experiment column reports characteristics of the analytic sample of war veterans surveyed in Huambo in 2013. The three variables in the lower panel were collected only in the survey and have no population-level analogue.

Table A.2: Balance Across Enforcement Vignettes (T-tests)

	Control	Turnout Buying	Machine Politics	Signaling	Reciprocity	Vote Secrecy
Observations	128	128	125	127	126	125
Demographics						
Age	50.60 (9.98)	50.90 (9.44)	49.32 (10.06)	48.59 (10.37)	48.73 (8.98)	50.12 (10.13)
		[0.807]	[0.310]	[0.116]	[0.118]	[0.704]
Married	0.39 (0.49)	0.41 (0.49)	0.44 (0.50)	0.43 (0.50)	0.42 (0.50)	0.37 (0.48)
		[0.799]	[0.428]	[0.576]	[0.628]	[0.712]
Urban	0.34 (0.47)	0.41 (0.49)	0.38 (0.49)	0.43 (0.50)	0.40 (0.49)	0.45 (0.50)
		[0.198]	[0.428]	[0.143]	[0.258]	[0.068]
Born in Municipality	0.76 (0.43)	0.77 (0.42)	0.73 (0.45)	0.74 (0.44)	0.75 (0.44)	0.70 (0.46)
		[0.769]	[0.589]	[0.746]	[0.829]	[0.272]
Education						
Any education	0.82 (0.39)	0.91 (0.28)	0.83 (0.38)	0.82 (0.39)	0.83 (0.38)	0.86 (0.34)
		[0.027]	[0.807]	[0.977]	[0.916]	[0.343]
Schooling (yrs)	3.37 (2.50)	4.16 (2.79)	3.85 (2.71)	3.61 (2.84)	3.88 (2.70)	4.07 (3.08)
		[0.017]	[0.143]	[0.476]	[0.117]	[0.047]
Household						
Household size	6.95 (2.85)	6.94 (2.83)	6.86 (2.85)	7.27 (2.84)	7.52 (2.79)	6.78 (3.03)
		[0.982]	[0.821]	[0.367]	[0.109]	[0.663]
Land access	0.91 (0.29)	0.86 (0.35)	0.87 (0.34)	0.87 (0.33)	0.83 (0.38)	0.90 (0.30)
		[0.245]	[0.387]	[0.412]	[0.059]	[0.952]

Notes: Means are reported with standard deviations in parentheses. Values in square brackets are p-values from two-sample t-tests comparing each treatment arm to the Control condition. "Born in Municipality" asks if the individual was born in the same municipality as currently residing in.

B Vignette Experiment

B.1 Alternative Specifications

Table B.1: Enforcement Mechanisms: Comparison of Standard Error Methods

	(1) No Cluster	(2) HAC	(3) Village Cluster	(4) Village Cluster (small-sample)
Turnout Buying	0.539**	0.539**	0.539**	0.539**
	(0.285)	(0.296)	(0.262)	(0.256)
Ballot Secrecy	0.705**	0.705**	0.705**	0.705**
	(0.287)	(0.312)	(0.278)	(0.272)
Machine Politics	-0.312	-0.312	-0.312	-0.312
	(0.286)	(0.263)	(0.230)	(0.225)
Reciprocity	0.002	0.002	0.002	0.002
	(0.284)	(0.260)	(0.215)	(0.210)
Signaling	0.098	0.098	0.098	0.098
	(0.285)	(0.297)	(0.249)	(0.243)
Observations	759	759	759	759
R^2	0.090	0.090	0.090	0.090
Controls	✓	✓	✓	✓
Village FE	✓	✓	✓	✓
Clusters	–	–	34	34

Notes: Standard errors in parentheses. *p<0.1, **p<0.05, ***p<0.01. Significance levels based on specification (4). The dependent variable is voting intentions measured on a 1–7 scale. The omitted category is the control vignette. Controls include age, years of schooling, and household size. Column (1) reports conventional OLS standard errors assuming independent, homoskedastic errors. Column (2) reports heteroskedasticity-consistent (HC3). Column (3) reports cluster-robust standard errors at the village level. Column (4) reports cluster-robust standard errors with the Bell-McCaffrey small-sample correction.

Wild cluster bootstrap p-values for our preferred specification (4) with Rademacher weights and B=10000 yield: Turnout Buying: p=0.028; Ballot Secrecy: p=0.007; Machine Politics: p=0.159; Reciprocity: p=0.992; Signaling: p=0.684.

Table B.2: Alternative Model Specifications

	(1) OLS (0–1 Scale)	(2) Probit Y≥4	(3) Probit Y≥5	(4) Probit Y≥6	(5) Probit Y=7
Turnout Buying	0.090**	0.136***	0.104**	0.077	0.049
	(0.043)	(0.050)	(0.044)	(0.047)	(0.043)
Ballot Secrecy	0.118**	0.144***	0.143***	0.111**	0.088*
	(0.045)	(0.042)	(0.044)	(0.049)	(0.046)
Machine Politics	-0.052	-0.072	-0.021	-0.050	-0.063
	(0.037)	(0.060)	(0.051)	(0.048)	(0.050)
Reciprocity	0.000	-0.041	-0.060	-0.043	-0.044
	(0.035)	(0.046)	(0.049)	(0.050)	(0.047)
Signaling	0.016	0.013	0.057	0.033	0.032
	(0.041)	(0.043)	(0.047)	(0.046)	(0.045)
Observations	759	759	759	759	759
Controls	✓	✓	✓	✓	✓
Village FE	✓	✓	✓	✓	✓
Clusters	34	34	34	34	34
Outcome Mean	0.280	0.273	0.217	0.190	0.171

Notes: Cluster-robust standard errors (village level) in parentheses. *p<0.1, **p<0.05, ***p<0.01. Column (1) reports OLS estimates with the outcome rescaled to 0–1 (where 1=0 and 7=1 on the original scale). Columns (2)–(5) report average marginal effects from probit models with binary outcomes defined as Y≥4, Y≥5, Y≥6, and Y=7 respectively. Controls include age, years of schooling, and household size. The omitted category is the control vignette.

B.2 Heterogeneity Analysis (Binary)

Network: Group Participation

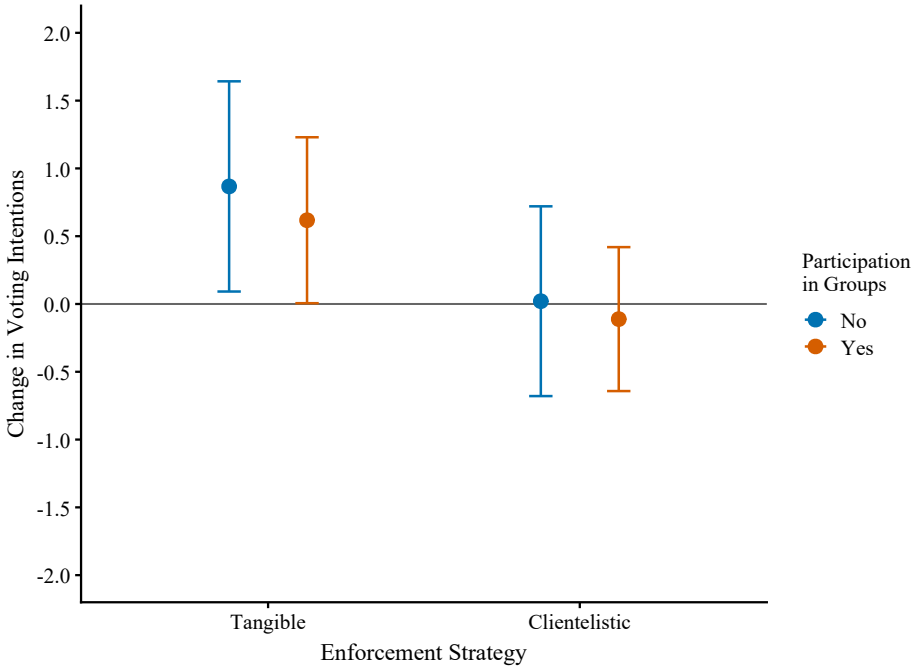


Figure B.1: Participation in Groups

Note: The sample is split into individuals who participate in any local, political, religious, or cultural group and those who do not. The graph reports the unconditional difference in mean likelihood of voting for a candidate, given the presented vignette and group-participation status, relative to the respective control group. Bars report 95% confidence intervals.

Geographic Location: Urban vs. Rural

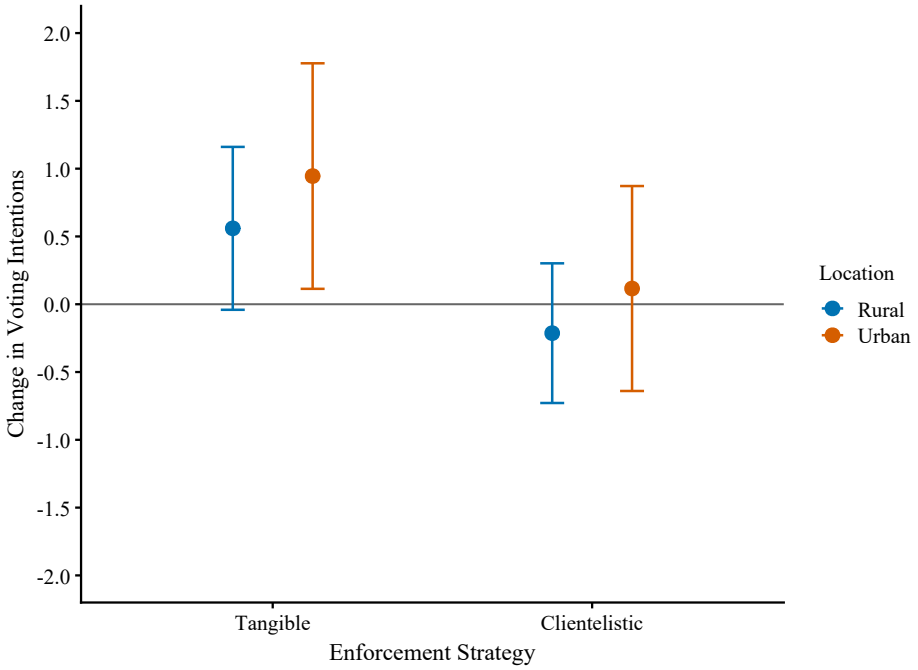


Figure B.2: Urban vs. Rural Location

Note: The sample is split by geographic location (urban vs. rural). The graph reports the unconditional difference in mean likelihood of voting for a candidate, relative to the respective control group. Bars report 95% confidence intervals.

Differences in Voting Intentions Based on the Income Level

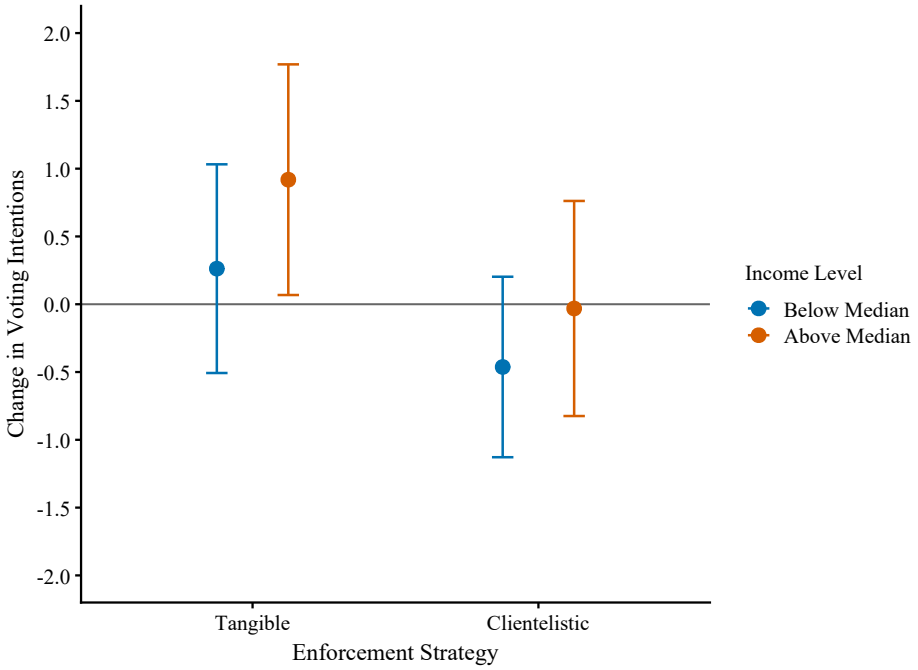


Figure B.3: Above vs. Below Median Income

Note: The sample is split at median monthly per-capita income. The graph reports the unconditional difference in mean likelihood of voting for a candidate, relative to the respective control group. Bars report 95% confidence intervals.

Vote Choice (2012)

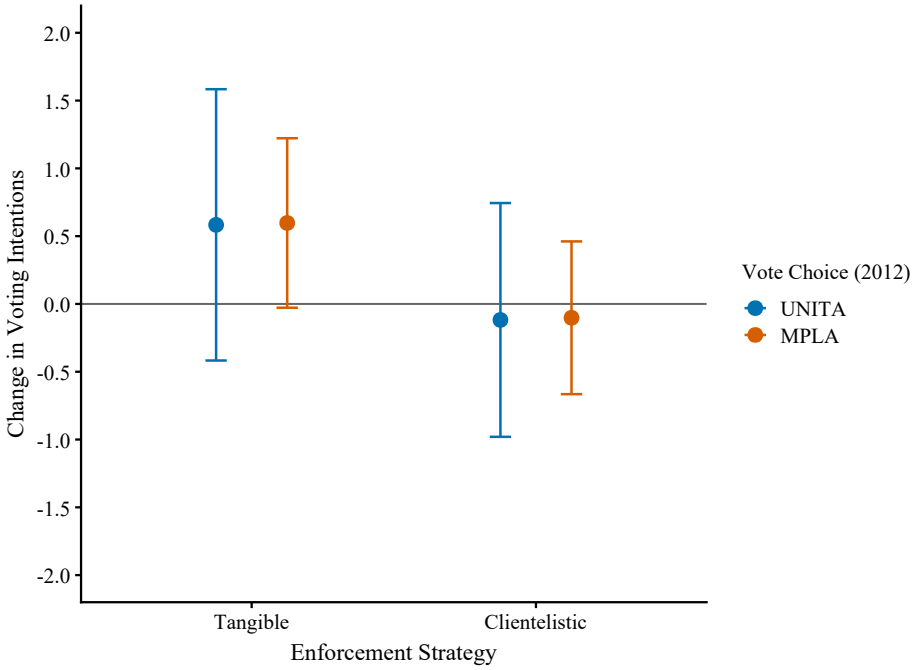


Figure B.4: Past Vote Choice (2012)

Note: The sample is split by self-reported vote choice in the 2012 elections (MPLA vs. UNITA). The graph reports the unconditional difference in mean likelihood of voting for a candidate, relative to the respective control group. Bars report 95% confidence intervals.

B.3 Heterogeneity Analysis

Political Interest

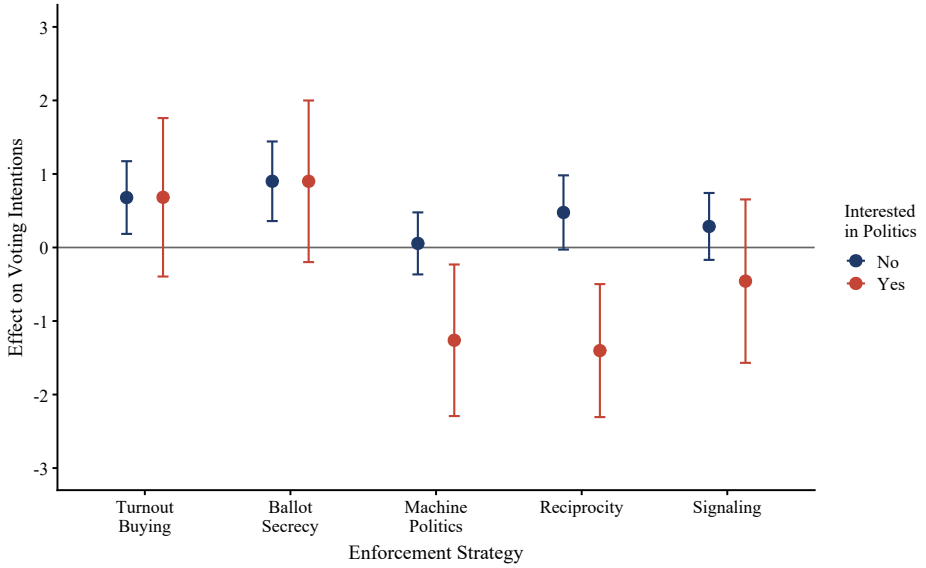


Figure B.5: Interest in Politics

Note: The sample is split into individuals according to self-stated interest in politics measured on a scale of 1 to 5. "Politically Interested" individuals are those who choose 4 or 5 vs. those reporting 1-3. In our sample, there are 260 individuals who are characterized as politically interested. The graph represents the unconditional difference in the mean likelihood of voting for a candidate, given the presented vignette and his binary interest in politics, compared to the respective group in the control vignette. The bars report the 95% confidence level.

Network: Group Participation

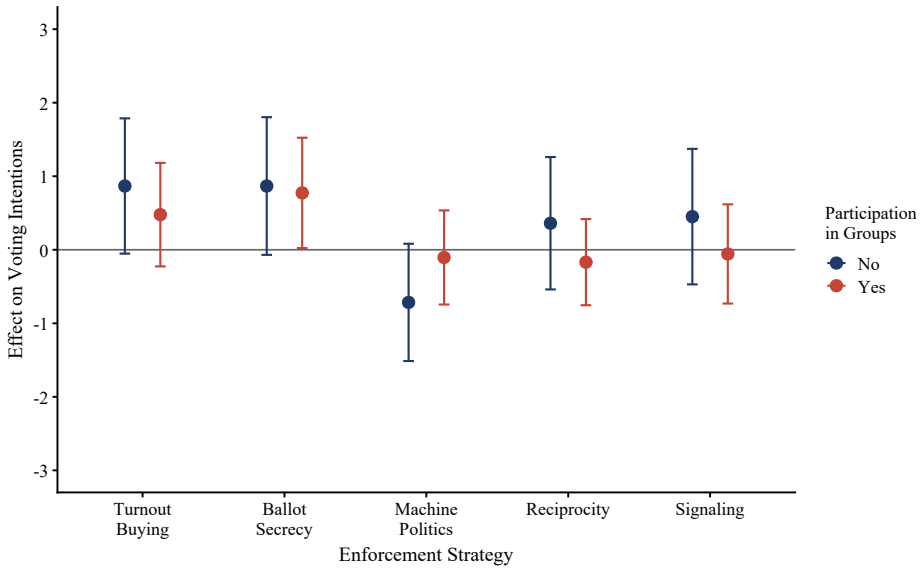


Figure B.6: Participation in Groups

Note: The sample is split into individuals who participate in any local, political, religious, or cultural group and those who do not. The graph reports the unconditional difference in mean likelihood of voting for a candidate across all five enforcement strategies, given the presented vignette and group-participation status, relative to the respective control group. Bars report 95% confidence intervals.

Geographic Location: Urban vs. Rural

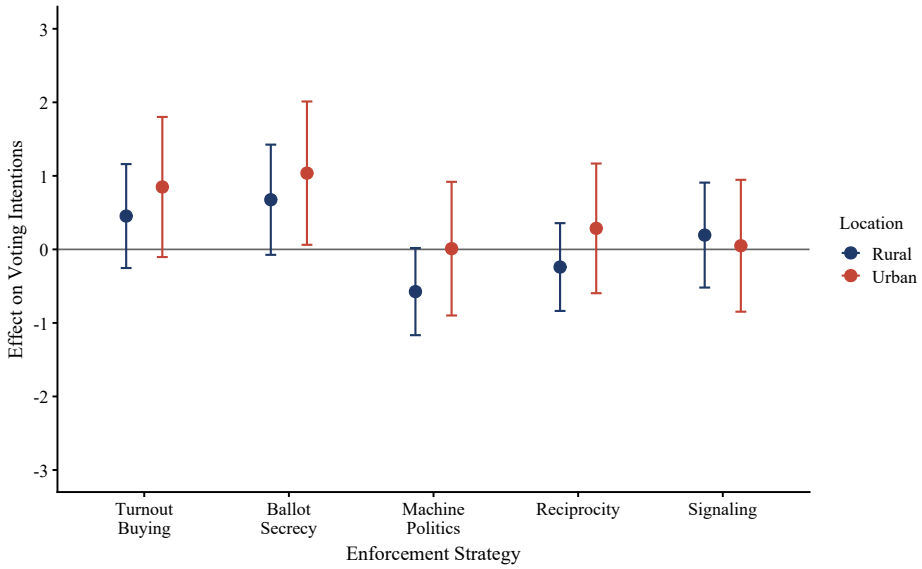


Figure B.7: Urban vs. Rural Location

Note: The sample is split by geographic location (urban vs. rural). The graph reports the unconditional difference in mean likelihood of voting for a candidate across all five enforcement strategies, relative to the respective control group. Bars report 95% confidence intervals.

Differences in Voting Intentions Based on the Income Level

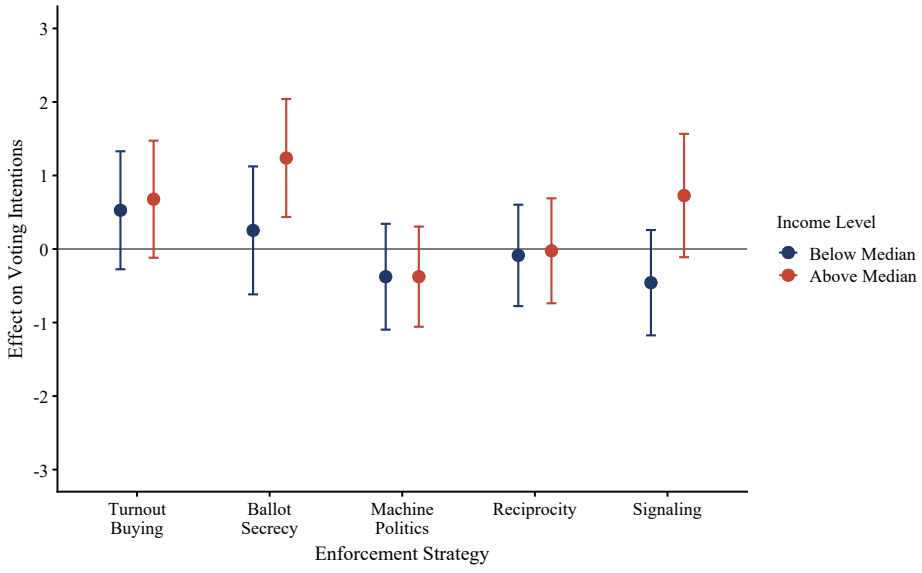


Figure B.8: Above vs. Below Median Income

Note: The sample is split at median monthly per-capita income. The graph reports the unconditional difference in mean likelihood of voting for a candidate across all five enforcement strategies, relative to the respective control group. Bars report 95% confidence intervals.

Vote Choice (2012)

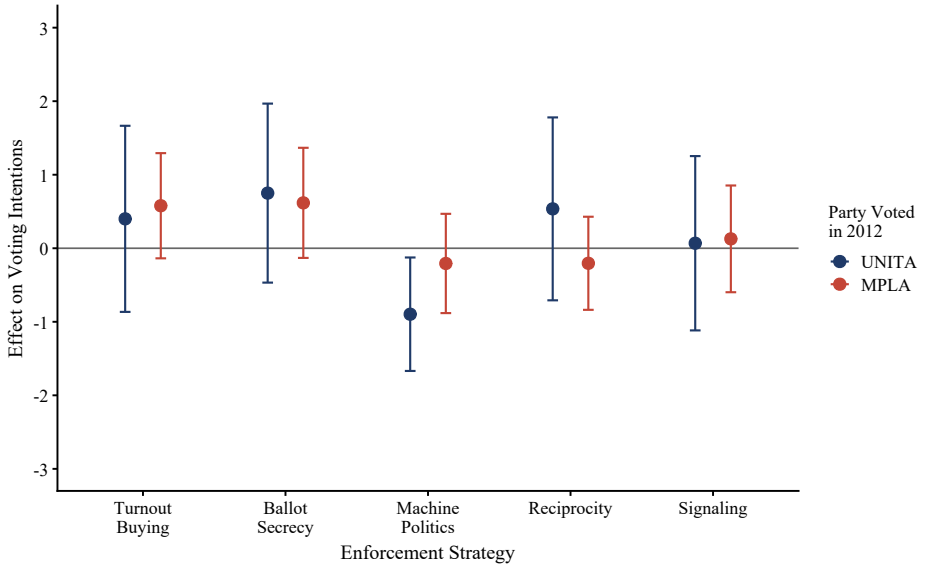


Figure B.9: Past Vote Choice (2012)

Note: The sample is split by self-reported vote choice in the 2012 elections (MPLA vs. UNITA). The graph reports the unconditional difference in mean likelihood of voting for a candidate across all five enforcement strategies, relative to the respective control group. Bars report 95% confidence intervals.

C List Experiment

C.1 Experimental Design

The list experiment design follows the standard approach of Blair and Imai (2012), with design choices tailored to our setting. Respondents were randomly assigned to a control or treatment list with equal probability, with randomization implemented at the question level through physical ball draws by the enumerator. This individual-level randomization ensures balance in expectation and avoids spillovers from sharing assignment within enumeration areas.

Identification assumptions. Identification rests on two assumptions. First, the *No Design Effect* assumption requires that adding the sensitive item does not change respondents' answers to the control items. Second, the *No Liars* assumption requires respondents to incorporate the sensitive item truthfully into their reported count. The selection of control items and the enumerator protocol were designed to reduce threats to both assumptions.

Item selection. The four control items were selected to minimize both floor and ceiling effects, which are the main sources of bias in list experiments and essential to keep anonymity and conduct a viable experiment (Glynn, 2013). Floor effects arise when too few respondents engage in any of the listed activities, allowing those who do not vote-sell to be identified by a count of zero; ceiling effects arise when most respondents engage in all control items, allowing vote-sellers to be identified by a count equal to the list length. To minimize these risks, we paired high-prevalence items ("You listened to the radio every day to learn about the campaign") with low-prevalence items ("You discussed your campaign with a Soba"). We further selected control items that were unrelated to vote-buying, but related to politics, along observable dimensions, reducing the risk of design effects from item correlation (Blair and Imai, 2012).

Enumerator protocol Enumerators read a scripted preamble emphasizing that the respondent should only report the *number* of items that applied, never which specific items, and explicitly offered to re-read the list.

D Original Questionnaire

SECTION 14: GOVERNANCE AND VOTING

Now, I would like to talk to you about politics.

14.1 VIGNETTES

Instruction: Randomly draw <u>one</u> vignette for the respondent, i.e. draw 1 of the 6 balls in the glass (each representing a different vignette). Ignore <u>all the other</u> vignettes and fill in 14-Q7.

Vignette 1: Turnout buying

Interviewer read: Suppose Mr X here (show the photo of Mr X) [name and photo should be as neutral as possible] is a politician running in the next election in Angola. Mr X can be of any party, MPLA, UNITA, or any others – we do not know that. I am going to give a little speech from Mr X.

'Angolans need to be given an opportunity for development. As politicians we have the responsibility to provide education and health to all Angolans. We need to work hard to build and maintain well-functioning schools and health centers close to all villages of this country. This electoral campaign is very important. I am asking you to vote for me in the next election.

As proof that I care for your wellbeing I am leaving you with 1000KZ. Since I know you are my supporter I will be waiting to see you near the polling station on the day of the election.'

14-Q1	Interviewer read: How likely would be that you vote for this candidate in the next election, if this situation would be real?	Extremely unlikely – Extremely likely (7-point scale): |____| **-> 14-Q7**

Vignette 2: Machine politics

Interviewer read: Suppose Mr X here (show the photo of Mr X) [name and photo should be as neutral as possible] is a politician running in the next election in Angola. Mr X can be of any party, MPLA, UNITA, or any others – we do not know that. I am going to give a little speech from Mr X.

'Angolans need to be given an opportunity for development. As politicians we have the responsibility to provide education and health to all Angolans. We need to work hard to build and maintain well-functioning schools and health centers close to all villages of this country. This electoral campaign is very important. I am asking you to vote for me in the next election.

As proof that I care for your wellbeing I am leaving you with 1000KZ. Since I know you are my supporter I hope you will be able to take the effort of voting for me and continue supporting our party in the future.'

14-Q2	*Interviewer read:* How likely would be that you vote for this candidate in the next election, if this situation would be real?	Extremely unlikely – Extremely likely (7-point scale): |____| **-> 14-Q7**

Vignette 3: Signaling

Interviewer read: Suppose Mr X here (show the photo of Mr X) [name and photo should be as neutral as possible] is a politician running in the next election in Angola. Mr X can be of any party, MPLA, UNITA, or any others – we do not know that. I am going to give a little speech from Mr X.

'Angolans need to be given an opportunity for development. As politicians we have the responsibility to provide education and health to all Angolans. We need to work hard to build and maintain well-functioning schools and health centers close to all villages of this country. This electoral campaign is very important. I am asking you to vote for me in the next election.

As proof that I care for your wellbeing I am leaving you with 1000KZ. This is sign that my candidacy and my party really care about you individually and will do the best for you once I win the elections.'

14-Q3	Interviewer read: How likely would be that you vote for this candidate in the next election, if this situation would be real?	Extremely unlikely – Extremely likely (7-point scale): |____|

Vignette 4: Reciprocity

Interviewer read: Suppose Mr X here (show the photo of Mr X) [name and photo should be as neutral as possible] is a politician running in the next election in Angola. Mr X can be of any party, MPLA, UNITA, or any others – we do not know that. I am going to give a little speech from Mr X.

'Angolans need to be given an opportunity for development. As politicians we have the responsibility to provide education and health to all Angolans. We need to work hard to build and maintain well-functioning schools and health centers close to all villages of this country. This electoral campaign is very important. I am asking you to vote for me in the next election.

As proof that I care for your wellbeing I am leaving you with 1000KZ. Today I am doing my best for you. I hope you do the best for me by voting in my candidacy/party on the day of the election.'

14-Q4	*Interviewer read:* How likely would be that you vote for this candidate in the next election, if this situation would be real?	Extremely unlikely – Extremely likely (7-point scale): |____| **-> 14-Q7**
Vignette 5: Secret voting questioned		
Interviewer read: Suppose Mr X here (show the photo of Mr X) [name and photo should be as neutral as possible] is a politician running in the next election in Angola. Mr X can be of any party, MPLA, UNITA, or any others – we do not know that. I am going to give a little speech from Mr X. 'Angolans need to be given an opportunity for development. As politicians we have the responsibility to provide education and health to all Angolans. We need to work hard to build and maintain well-functioning schools and health centers close to all villages of this country. This electoral campaign is very important. I am asking you to vote for me in the next election. **As proof that I care for your wellbeing I am leaving you with 1000KZ. This is done in confidence that you will vote for me on the day of election. It is not difficult for me to know for whom you have voted.**		
14-Q5	*Interviewer read:* How likely would be that you vote for this candidate in the next election, if this situation would be real?	Extremely unlikely – Extremely likely (7-point scale): |____| **-> 14-Q7**
Vignette 6: Control		
Interviewer read: Suppose Mr X here (show the photo of Mr X) [name and photo should be as neutral as possible] is a politician running in the next election in Angola. Mr X can be of any party, MPLA, UNITA, or any others – we do not know that. I am going to give a little speech from Mr X. 'Angolans need to be given an opportunity for development. As politicians we have the responsibility to provide education and health to all Angolans. We need to work hard to build and maintain well-functioning schools and health centers close to all villages of this country. This electoral campaign is very important. I am asking you to vote for me in the next election.		
14-Q6	*Interviewer read:* How likely would be that you vote for this candidate in the next election, if this situation would be real?	Extremely unlikely – Extremely likely (7-point scale): |____| **-> 14-Q7**
14-Q7	**FOR THE INTERVIEWER: Which of the vignettes was implemented? (1-6)**	

14.2 EXPERIMENT

Instruction: Ask <u>all</u> questions. For <u>each</u> question: randomly draw one of the two versions, i.e. draw 1 of the 2 balls in the glass (each representing a different version). Ask <u>this</u> version, ignore the other one and fill in the version used after each question.

1. Individual Vote-buying		
	Interviewer read: I am going to read you a list of some of things that people have told us happened to them during the 2012 campaign. I am going to read you the whole list, and then I want you to tell me how many of the different things happened to you. **Please do not tell me which of the things happened to you, just how many.** If you would like me to repeat the list, I will do so.	[The baseline version of the question includes the following four items:] 1. You participated in the campaign by accepting to put up posters on your house. 2. You listened to the radio every day to learn about the campaign. 3. You met a campaigner to discuss his or her ideas for the country. 4. You discussed the campaign with a soba. **[The sensitive version of the question adds the following item:]** 5. You received money or a gift from a party or politician.
14-Q8	**FOR THE INTERVIEWER:** *Which version of the list did you* use?	|_____|
2. Individual Vote-buying with different number of options		
14-Q9	*Interviewer read:* I am going to read you a list of some of things that people have told us happened to them during the 2012 campaign. I am going to read you the whole list, and then I want you to tell me how many of the different things happened to you. **Please do not tell me which of the things happened to you, just how many.** If you would like me to repeat the list, I will do so.	[The baseline version of the question includes the following four items:] 1. You participated in the campaign by accepting to put up posters on your house. 2. You listened to the radio every day to learn about the campaign. 3. You discussed the campaign with a soba. **[The sensitive version of the question adds the following item:]** 4. You received money or a gift from a party or politician.

14-Q10	**FOR THE INTERVIEWER:** *Which version of the list did you use?*	|_____|
3. Individual Vote-buying with placebo option		
14-Q11	*Interviewer read*: I am going to read you a list of some of things that people have told us happened to them during the 2012 campaign. I am going to read you the whole list, and then I want you to tell me how many of the different things happened to you. **Please do not tell me which of the things happened to you, just how many.** If you would like me to repeat the list, I will do so.	[The baseline version of the question includes the following four items:] 1. You participated in the campaign by accepting to put up posters on your house. 2. You listened to the radio every day to learn about the campaign. 3. You met a campaigner to discuss his or her ideas for the country. 4. You discussed the campaign with a soba. **[The sensitive version of the question adds the following item:]** 5. You talked with the President JEDS at his home about the election.
14-Q12	**FOR THE INTERVIEWER:** *Which version of the list did you use?*	|_____|
4. Individual Vote-buying with different strength of options		
14-Q13	*Interviewer read:* I am going to read you a list of some of things that people have told us happened to them during the 2012 campaign. I am going to read you the whole list, and then I want you to tell me how many of the different things happened to you. **Please do not tell me which of the things happened to you, just how many.** If you would like me to repeat the list, I will do so.	[The baseline version of the question includes the following four items:] 1. You were threatened to vote in a certain way. 2. You or any of your family members was physically attacked by campaigners. 3. You heard news in the radio about political violence. 4. You discussed the campaign with a soba. **[The sensitive version of the question adds the following item:]** 5. You received money or a gift from a party or politician.
14-Q14	**FOR THE INTERVIEWER:** *Which version of the list did you use?*	|_____|
5. Village Vote-buying		
14-Q15	*Interviewer read:* I am going to read you a list of some of things that people have told us happened to them during the 2012 campaign. I am going to read you the whole list, and then I want you to tell me how many of the different things happened to you. **Please do not tell me which of the things happened to you, just how many.** If you would like me to repeat the list, I will do so.	[The baseline version of the question includes the following four items:] 1. Most of the people in your village accept to put up posters on their houses. 2. Most of the people in your village listened to the radio every day to learn about the campaign. 3. Most of the people in your village met a campaigner to discuss his or her ideas for the country. 4. Most of the people in your village discussed the campaign with a soba. **[The sensitive version of the question adds the following item:]** 5. Most of the people in your village received money or a gift from a party or politician.
14-Q16	**FOR THE INTERVIEWER:** *Which version of the list did you use?*	|_____|
6. Effects of Individual Vote-buying on voting		
14-Q17	*Interviewer read:* I am going to read you a list of some of things that people have told us happened to them during the 2012 campaign. I am going to read you the whole list, and then I want you to tell me how many of the different things happened to you. **Please do not tell me which of the things happened to you, just how many.** If you would like me to repeat the list, I will do so.	[The baseline version of the question includes the following four items:] 1. You participated in the campaign by accepting to put up posters on your house. 2. You listened to the radio every day to learn about the campaign. 3. You met a campaigner to discuss his or her ideas for the country. 4. You discussed the campaign with a soba. **[The sensitive version of the question adds the following item:]** 5. You voted for a party or politician because they gave you money or a gift during the campaign.

14-Q18	**FOR THE INTERVIEWER:** *Which version of the list did you use?*	|_____|
7. Effects of Village Vote-buying on voting		
14-Q19	*Interviewer read:* I am going to read you a list of some of things that people have told us happened to them during the 2012 campaign**.** I am going to read you the whole list, and then I want you to tell me how many of the different things happened to you**. Please do not tell me which of the things happened to you, just how many.** If you would like me to repeat the list, I will do so**.**	[The baseline version of the question includes the following four items:] 1**.** Most of the people in your village accept to put up posters on their houses**.** 2**.** Most of the people in your village listened to the radio every day to learn about the campaign**.** 3**.** Most of the people in your village met a campaigner to discuss his or her ideas for the country**.** 4**.** Most of the people in your village discussed the campaign with a soba**.** **[The sensitive version of the question adds the following item:]** 5**.** Most of the people in your village voted for a party or politician because they gave them money or gifts during the campaign.
14-Q20	**FOR THE INTERVIEWER:** *Which version of the list did you use?*	|_____|