

Working Paper No. 171

Do electoral handouts affect voting behavior?

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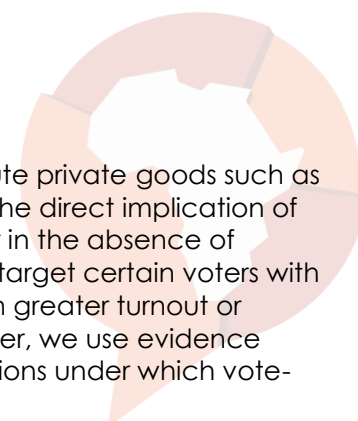
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Abstract

The literature on vote-buying often assumes a complete transaction of cash for votes. While there is ample evidence that candidates target certain voters with cash handouts, it is unclear whether these actually result in higher turnout and vote shares for the distributing party. Empirically, using different matching techniques and accounting for district-level factors, we find that cash handouts have little to no effect on either turnout or vote shares during the 2011 presidential election in Benin. We cross-validate these results with additional surveys from four other African countries (Kenya, Mali, Botswana, and Uganda). Results suggest that vote-buying is better explained as an incomplete transaction between candidates and voters and that handouts from multiple parties as well as district-level traits (e.g. patronage, public goods) may account for the null effects observed.

Acknowledgements

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1 Introduction

Vote-buying is defined as a transaction whereby candidates distribute private goods such as cash and gifts in exchange for electoral support or higher turnout.¹ The direct implication of this definition is that vote shares and turnout would have been lower in the absence of electoral handouts. While there is ample evidence that candidates target certain voters with cash handouts, it is unclear whether these handouts actually result in greater turnout or higher vote shares in favour of the distributing candidate. In this paper, we use evidence from Benin and other African democracies to investigate the conditions under which vote-buying, as defined above, is likely to take place.

We first use a theoretical framework developed in other studies to examine when the exchange of bribes for votes might become the preferred course of action for parties and politicians. As argued elsewhere (Stokes, 2005; Nichter, 2008), in the presence of low monitoring by political parties, cash in exchange for votes cannot become an equilibrium in one-shot interactions. However, we show that even in the case of repeated interactions, low monitoring renders the bribes or handouts to be paid for votes prohibitively high. The situation is worsened when more than one party is bribing to obtain votes. Under these conditions, complete cash-for-votes transactions are very unlikely.

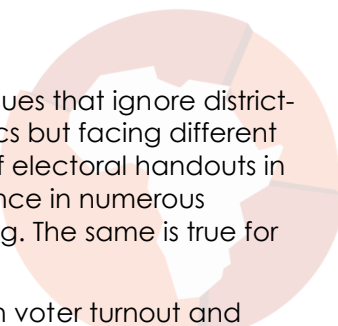
After outlining the theoretical framework, we investigate empirically whether electoral handouts lead to visible differences in individual vote choices or turnout. We use three sets of surveys to investigate the effectiveness of handouts: an original survey conducted after the 2011 presidential election in Benin (see Wantchekon, 2012), Afrobarometer Round 3 surveys conducted in 2005/2006 across 18 African countries, and the Afrobarometer Round 5 survey conducted in 2011 in Benin. While the 2011 Afrobarometer survey in Benin has the unique feature that it measures whether handouts were offered by one or more parties, the Afrobarometer Round 3 data allow for additional robustness checks of our results. That is, the similarity in the Afrobarometer questions across countries helps us cross-validate our findings in different contexts.

This paper is motivated by the fact that monitoring by political parties in African countries is actually quite low: 81% of respondents² across 34 African countries surveyed in Round 5 of Afrobarometer report it to be very to somewhat unlikely for powerful actors to find out how they voted. In Benin, perceptions of vote privacy are 91%. In this context, is it likely for politicians to sustain cooperation (votes, turnout) over time by targeting voters with electoral handouts?

Any compelling answer in favour of vote-buying should involve the construction of a valid counterfactual of how targeted voters would have behaved in the absence of cash handouts. For this purpose, we preprocess our data using different matching techniques to account for the non-random targeting of cash handouts and limit model dependence (Ho, Imai, King, & Stuart, 2007a, 2007b). To improve our comparisons, we only match individuals belonging to the same political unit – the electoral district. This approach incorporates the counterpart of district fixed effects within a matching framework, thus controlling for district-

¹ For example, Brusco et al. (2004, 67) defines vote-buying “as the proffering to voters of cash or (more commonly) minor consumption goods by political parties, in office or in opposition, in exchange for the recipient’s vote.” Similarly, Finan and Schechter (2012, 864) define vote-buying as “[offered] goods to specific individuals before an election in exchange for their votes.” Kramon (2009, 4) defines it as “the distribution of particularistic or private material benefits with the expectation of political support.” Nichter (2008, 20) defines vote-buying (as opposed to “turnout-buying”) as “exchanging rewards for vote choices.” Banerjee, Kumar, Pande, & Su (2011, 14) consider vote-buying as any instance by which “cash, liquor, food, clothes or milk/refreshments [are distributed] as enticement [to vote or mobilize].” Finally, Stokes et al. (2013) have recently labeled as vote-buying the situation in which “political machines may treat or bribe to persuade people to vote for them.”

² This estimate excludes missing observations and those who responded with “Don’t know.”



level factors that may influence electoral behavior. Matching techniques that ignore district-level factors may pair off individuals with similar personal characteristics but facing different (district-level) political conditions. For instance, the marginal impact of electoral handouts in districts already heavily targeted with spending – a common occurrence in numerous democracies³ – might be much smaller than in those with less spending. The same is true for other district-wide factors such as close elections.

Following this approach, we estimate the effect of cash distribution on voter turnout and electoral choices using both matched and unmatched data. Consistent with the current literature, when using unmatched data, we find that cash distribution increases votes and turnout (Brusco et al., 2004). However, the use of preprocessed data and district-level fixed effects reveals no statistically significant difference in the behavior of individuals who received cash handouts and those who did not. This result suggests that district-level factors might influence (either enhance or mitigate) the effect of cash handouts on individual behavior.

We cross-validate these results using Afrobarometer Round 3 survey data for Kenya, Uganda, Mali, and Botswana. We chose these cases based on regression results (with unmatched data) that show a strong effect of electoral handouts on voter turnout and are likely to exhibit an effect on vote choices. However, as with our 2011 Benin survey, we find that such an effect is largely diminished when we use matched data and district fixed effects. The findings show that at least in the context of the cases we study, the vote-buying transaction is incomplete. This result is consistent with other studies in the African context (Lindberg & Morrison, 2008; Bratton, 2008; Conroy-Krutz & Logan, 2012, to mention a few).

One potential reason for the null effect of cash distribution is that a typical voter may receive multiple cash offers or be “cross-pressured from both sides of the partisan divide” (Bratton, 2008, 622), thus weakening the *quid-pro-quo* nature of electoral handouts. In fact, when we compare the electoral effect of single vs. multiple offers in Benin, we find that these often run in opposite directions. Specifically, while those targeted by one party are less likely to favour the opposition party, the converse is true for those targeted by more than one party. Such a divergence reduces the potential impact that targeting by a single party might have.

This paper contributes to the current literature in several ways. First, it builds on the vote-buying literature by closely examining the actual effect of cash handouts on voting behavior. Although numerous studies have documented the targeting strategies of politicians to “purchase” votes (Stokes, 2005; Nichter, 2008; Finan & Schechter, 2012; Calvo & Murillo, 2004; Brusco et al., 2004), there is little discussion of how these handouts may translate into actions.⁴ We empirically explore whether vote-buying actually “buys” votes instead of assuming that whoever receives electoral handouts will choose to vote for the distributing candidate. This is important to corroborate in contexts of poor monitoring where there is little to no “cost” of voting for the party of one’s choice.

Second, our approach improves extant estimates of the effect of cash handouts by providing a formal treatment of counterfactuals in the context of the cash-for-votes literature. Specifically, we ask, “*Would voters who receive cash behave differently if they had not?*” In addition, by focusing on the average treatment effect on the treated via matching and accounting for common political conditions at the district level – such as strategic spending or campaigning – we can provide a better estimate of the potential role of vote-buying.⁵

³ For instance, see Magaloni, 2006; Herron & Theodos, 2004; Dahlberg & Johansson, 2004; Cornelius, 2004; Diaz-Cayeros, Estevez, & Magaloni, 2006, among many others.

⁴ One exception is Cantu (2016), which examines the distribution of grocery gift cards and votes for the incumbent in the 2012 Mexican election.

⁵ Naturally, individual unobservable traits remain a concern not addressed by matching and district-level fixed effects.

Finally, we show theoretically and provide some empirical evidence that the presence of multiple electoral handouts renders successful vote-buying highly unlikely due to the relative “expensiveness” of “purchasing” votes. While numerous studies rely on the assumption of a single political machine (usually the incumbent), this is not the case in Benin. In fact, individuals who received money from more than one party actually outnumber those who received money from a single source – a scenario that has been labeled “empirically unusual” (Stokes, 2005; Nichter, 2008) but is consistent with findings in the theoretical literature on legislative vote-buying (Dekel, Jackson, & Wolinsky, 2008). Future studies need to explicitly incorporate the potential for numerous electoral handouts when modeling and empirically examining the impact of vote-buying.

Ultimately, the paper underscores the problems of attributing an electoral effect to the mere distribution of cash handouts. As exhaustively discussed with respect to other topics, such as campaign contributions and political favours, conclusive evidence of actual “wrongdoing” involves showing that an individual's actions were *changed* due to the distribution of money. The mere presence of cash, while raising questions, does not show that favours or votes are exchanged for money.

The paper is organized as follows. In Section 2 we present the relevant literature. Section 3 uses extant theoretical models to analyze vote-buying in the presence of low monitoring and multiple handouts. In Section 4 we provide an overview of the data and methodology. In Section 5 we present the empirical results. Finally, Section 6 discusses the results and concludes.

2 Vote-buying in the literature

The literature on vote-buying has focused on the strategic targeting of cash handouts but has devoted less attention to voters' response to these. For example, Stokes (2005) thoroughly documents the distributional patterns of those who receive material gifts in Argentina, finding that those who are mildly opposed to the distributing candidate and those with low incomes are more likely to be targeted. Similar results are found by Kramon (2009) in Kenya, where swing voters and those with low incomes are more likely to be targeted for mobilization purposes. Brusco et al. (2004) and Calvo and Murillo (2004) also provide evidence that political parties target low-income individuals. In contrast, Nichter (2008) finds that political parties target passive constituencies to increase their vote share – a result corroborated by Nichter and Palmer-Rubin (2014) – while Finan and Schechter (2012) provide evidence of how party operatives target reciprocal individuals to ensure their compliance at the polling station. However, rather than follow a single strategy, parties often rely on a combination depending on the circumstances (Gans-Morse, Mazzuca, & Nichter, 2014).

Yet studies on vote-buying often rely on two implicit assumptions. The first is that brokers or party operatives are able to correctly identify voters' political inclinations through their social networks or personal interactions. However, this task is actually very difficult to fulfill, and brokers may be no better than a coin toss in correctly detecting co-partisans (Schneider, 2016). In addition, correctly targeting voters is even harder during campaigns, when political inclinations are fluid, further reducing the precision of targeted efforts (Greene, 2016). The second assumption is that those targeted with handouts behave in a way that would not have happened otherwise. As shown in Uganda (Conroy-Krutz & Logan, 2012), Mexico (Simpser, 2012), Taiwan (Wang & Kurzman, 2007), and Nigeria (Bratton, 2008), the mere presence of handouts is not sufficient to argue that electoral outcomes were changed.

A second group of empirical studies instead relies on experimental frameworks to establish the causal effect of cash handouts on voting behavior (Banerjee et al., 2011; Vicente, 2014; Kramon, 2016). However, natural experiments on the topic are scarce, and experimental designs that directly randomize cash handouts to influence voting behavior may raise ethical concerns. Due to these constraints, field experiments typically randomize some aspect of the voting-decision process rather than the direct distribution of electoral handouts. For example, Vicente (2014) randomizes the distribution of anti-corruption (e.g. anti-vote-buying)

information to assess indirectly the effect of cash handouts on electoral behavior. Similarly, Kramon (2016) randomly provides voters with information on whether a given politician engages in vote-buying to assess subsequent electoral support. However, such approaches introduce an additional layer of complexity (e.g. individual perceptions of the negativity of vote-buying, effectiveness of information campaigns) that makes a straightforward interpretation of its electoral impact on behavior difficult.

Finally, a third group of empirical studies departs from the traditional explanations for why parties distribute electoral handouts (e.g. to purchase votes) to explore alternative accounts. One set of explanations put forward focuses on enhancing credibility (Schaffer, 2002; Keefer & Vlaicu, 2008) or commitment to future redistribution (Kramon, 2016). According to these studies, handouts by politicians need not have an effect on the specific targeted voter but rather should signal to the entire population the credibility of their campaign promises (Banegas, 2002; Nugent, 2007; Schaffer & Schedler, 2007). Extending this logic, even if voters do not directly receive money, they need only be aware of the vote-buying activities as credible proof of the politicians' good intentions. For instance, Kramon (2016) finds that spreading information that randomly attributes vote-buying activities to politicians actually bolsters electoral support for them, even if voters have not benefited directly. Although we do not directly investigate this question, our theoretical framework provides some reasons for why politicians might provide handouts even if these fail to actually purchase votes. For instance, political parties might still find it in their interest to distribute handouts, particularly in the presence of other parties doing the same thing (Chauchard, 2016).

In terms of theoretical models, the literature on vote-buying has mostly focused on conditions under which bribes may sustain cooperation (e.g. votes) in repeated-interaction settings. One assumption underpinning this analysis is that parties are able to monitor vote choices (Stokes, 2005) or at least observe turnout (Nichter, 2008). Under these conditions, there exists a bribe level that will satisfy the voter and guarantee that s/he votes in favour of the machine or distributing candidate. Relying on this framework, we relax the monitoring assumption – consistent with a lack of political machines across sub-Saharan Africa (Van de Walle, 2007; Bratton, 2008) – and increase the number of actors distributing handouts during an election.

3 Theoretical framework

To motivate the empirical analysis, we present a stylized framework to understand individual vote choices in the presence of electoral handouts. Unlike in other models of vote- or turnout-buying (Stokes, 2005; Nichter, 2008), the focus here is on the vote choices of individuals, rather than on the strategies of political parties. In this framework, voter i obtains a utility U_i^x for voting for party x equal to the sum of her intrinsic preferences for that party (V_i^x) plus the handout given by party x to vote in its favour (p_i^x). Similarly, the individual's utility of voting for party y can be represented by $U_i^y = V_i^y + p_i^y$. To sway voters, the utility derived from handouts must surpass that derived from intrinsic attachments to parties/candidates. Now, let's suppose that only party y is engaging in vote-buying whereas party x is not ($p_i^x = 0$). In this case, if party y finds out that voter i has reneged, it punishes her with a one-time cost d . From the perspective of voter i , the expected utility from voting for each party is:

$$U_i^x = m(V_i^x - d + p_i^y) + (1 - m)(V_i^x + p_i^y) \quad (1)$$

and

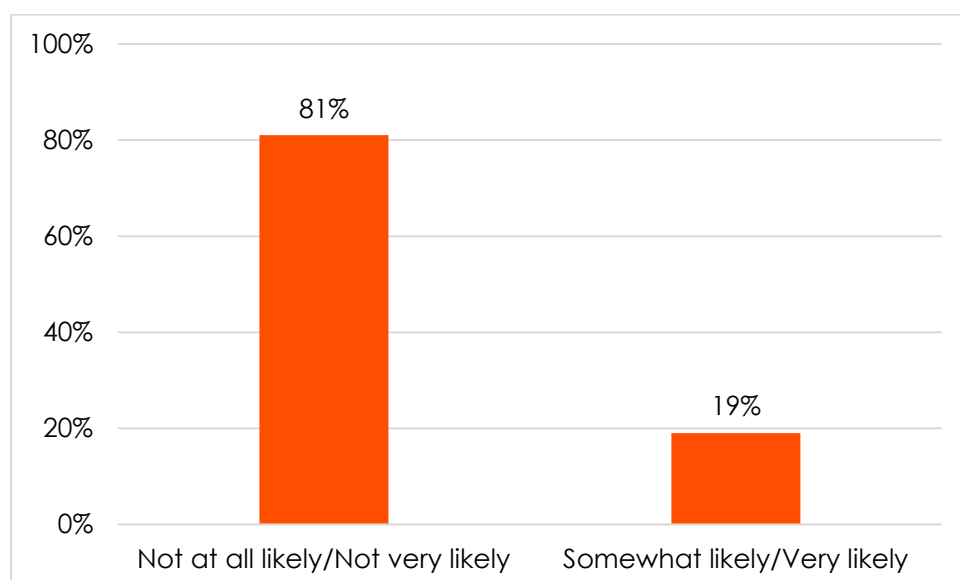
$$U_i^y = V_i^y + p_i^y \quad (2)$$

where m is the probability that party y finds out how she voted (monitoring). In equilibrium, voter i will choose the distributing party y whenever the expected punishment is greater than the difference in the valuations for each party:

$$md > V_i^x - V_i^y$$

The point to highlight is that the relevant determinants of vote choice are the intrinsic valuations and the expected punishment if caught, but not necessarily the size of the payment given by party y . The reason for this is that for one-time interactions, payments are given regardless of the action taken. This is the case in many African countries where party machines are either absent or short-lived. As an illustration, in the Afrobarometer Round 5 surveys, the question was asked, “How likely do you think it is that powerful people can find out how you voted, even though there is supposed to be a secret ballot in this country?” The answer across 34 African countries in 2011/2013 is that 81%⁶ of respondents report it either not at all likely or not very likely, while only 19% find it somewhat likely or very likely (Figure 1).

Figure 1: Likelihood that powerful people can find out how you voted | 34 African countries | 2011/2013

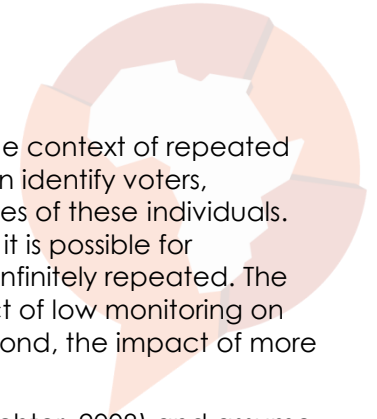


Respondents were asked: How likely do you think it is that powerful people can find out how you voted, even though there is supposed to be a secret ballot in this country?

Source: Afrobarometer Round 5 (2011-2013) (data available at www.afrobarometer.org). Countries: Algeria 2013, Benin 2011, Botswana 2012, Burkina Faso 2012, Burundi 2012, Cameroon 2013, Cape Verde 2011, Côte d'Ivoire 2013, Egypt 2013, Ghana 2012, Guinea 2013, Kenya 2011, Lesotho 2012, Liberia 2012, Madagascar 2013, Malawi 2012, Mali 2012, Mauritius 2012, Morocco 2013, Mozambique 2012, Namibia 2012, Niger 2013, Nigeria 2012, Senegal 2013, Sierra Leone 2012, South Africa 2011, Sudan 2013, Swaziland 2013, Tanzania 2012, Togo 2012, Tunisia 2013, Uganda 2012, Zambia 2012, Zimbabwe 2012.

Moreover, when looking at the case of Benin, respondents who were targeted with electoral incentives are not significantly more likely than those who were not targeted to think that powerful people can find out how they vote. Put together, this suggests that monitoring by political parties, or by any powerful actor, might actually be quite low or imperfect, making it difficult to enforce an exchange of cash for votes. In addition, the lack of compulsory voting may render vote-buying a less effective strategy in the African context, as suggested by Gans-Morse et al. (2014). But is it possible to achieve complete vote-buying transactions in settings with repeated interactions?

⁶ Excluding “missing” and “Don’t know” categories



3.1 Vote-buying with repeated interactions

First, let's examine what would be the vote choice of individuals in the context of repeated interactions where there is a permanent political "machine" that can identify voters, distribute private benefits, and at least partly monitor the vote choices of these individuals. Unlike one-shot interactions, where enforcing transactions is difficult, it is possible for complete vote-buying transactions to take place if interactions are infinitely repeated. The purpose of this exercise is to highlight two key aspects: first, the effect of low monitoring on the size of the bribes necessary to purchase all future votes, and second, the impact of more than one political party distributing private rewards.

We follow the approach advanced in the literature (Stokes, 2005; Nichter, 2008) and assume an infinitely repeated prisoner's dilemma in which the two sides follow a grim-trigger strategy to investigate the conditions necessary to induce cooperation toward the party distributing handouts (y). Similar to the previous case, we assume that the ability to monitor voters is given by m while the discount factor of the future is given by β . Under this characterization, the distributing party y can sustain cooperation from voter i whenever the following is true:

$$\frac{1}{1-\beta} (V_i^y + p_i^y) \geq V_i^x + p_i^y + \frac{\beta}{1-\beta} m[(V_i^x) + (1-m)(V_i^y + p_i^y)] \quad (3)$$

where the left side of this inequality depicts the benefits to voter i from voting for y at each stage. The right side reflects the benefits of defecting at any given stage plus the discounted value of the rewards to be obtained if a) with probability m she is caught and only obtains the intrinsic value of voting for the opposition party x in all subsequent stages or b) with probability $1 - m$ she is not caught but continues voting for party y indefinitely.

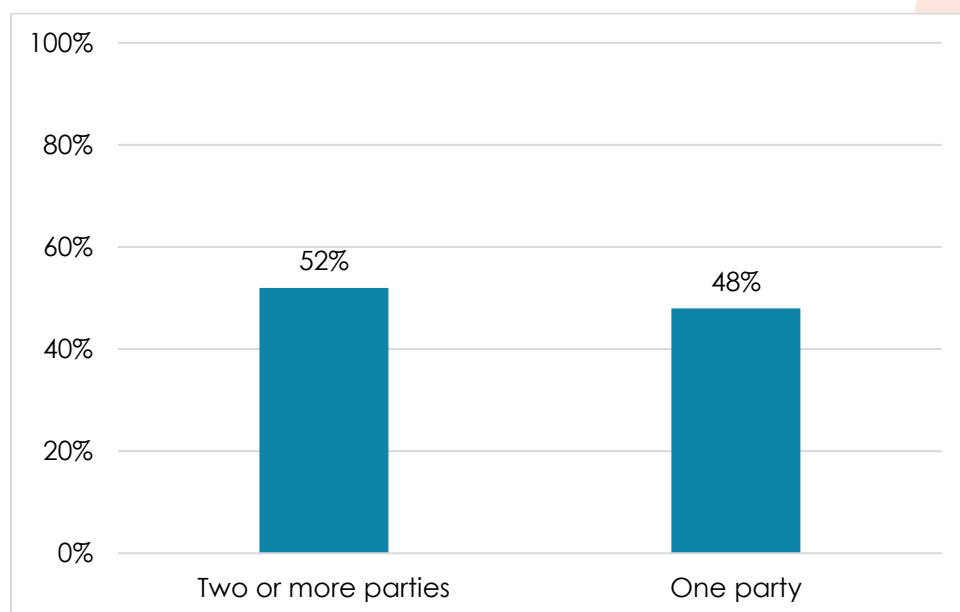
Simplifying (3) we obtain:

$$p_i^y \frac{1}{\Phi} \geq (V_i^x - V_i^y) \quad (4)$$

where $\Phi = \frac{1-\beta+m\beta}{m\beta}$. This result yields similar comparative statics to those of Stokes (2005) and Nichter (2008) in terms of intrinsic preferences: The size of the bribe is directly correlated with the difference in the intrinsic value for these parties. However, the implication we want to highlight is that when the probability of detection is low, the price to be paid for each vote p_i^y becomes quite large. Since parties prefer to pay the least amount possible, let \bar{p}_i^y be the level of bribes such that (2) holds at equality. In this case, it is straightforward to notice that as $m \rightarrow 0$ the size of the bribe offered ought to be much larger. The implication is that for the distributing party to sustain cooperation, the value of the handout has to be significantly higher when monitoring is poor than when the party can monitor better. Although the machine may only pay what a single vote is worth from its perspective, this might not be enough to guarantee cooperation from the voter.

3.2 Vote-buying competition

Having shown that the size of the bribe needs to be very high to sustain cooperation from voter i in the presence of low monitoring, we now consider the case in which party x also distributes payments p_i^x to voter i in exchange for her vote. In contrast to the focus of the literature on a single "machine," handouts are often distributed by more than one party, further limiting the completeness of the vote-buying transaction. For instance, a majority of individuals who reported being offered "electoral incentives" said they were offered handouts by more than one party during the 2011 Benin election (Figure 2).

Figure 2: Is there only one “machine”? | Benin | 2011

Respondents were asked: Did a single political party or more than one political party offer you a gift in exchange for your vote?

Source: Afrobarometer Round 5 (2011) survey in Benin

Similar to the previous case, we assume that party x follows a grim-trigger strategy if voters defect. Therefore, to induce cooperation, now party y has to guarantee that the benefits of cooperating are equal to or greater than those from defecting after receiving an electoral handout p_i^x from party x . In such a scenario, we have:

$$\frac{1}{1-\beta} (V_i^y + p_i^y) \geq V_i^x + p_i^y + \frac{\beta}{1-\beta} m[(V_i^x) + (1-m)(V_i^y + p_i^y)] \quad (5)$$

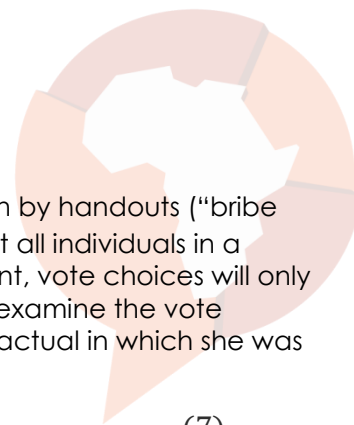
which simplifies to:

$$(p_i^y - p_i^x) \frac{1}{\psi} \geq (V_i^x - V_i^y) \quad (6)$$

where $\psi = \frac{1-\beta+m\beta}{m\beta}$. In the case of more than one party competing for votes, the difference in the offers from each party has to be greater than the difference in the intrinsic valuation of parties weighted by ψ . Comparing (4) to (6), we observe that if $p_i^x > 0$ and we assume that $\Phi = \psi$ and the inequality in (4) is binding – i.e. parties would prefer to pay the minimum necessary to sustain cooperation – it follows that for the same intrinsic valuations, the bribe offered by party y to sustain cooperation is higher under competition than when no competition is present. This finding suggests that in the presence of multiple offers, budget-constrained political parties may be unable to meet such “prices” and voters might just take the bribe and vote their conscience.

To summarize: First, in contexts with no prospects of future interactions and low monitoring, vote choices will be driven by intrinsic preferences. Second, even in infinitely repeated games in which parties follow a grim-trigger strategy, low monitoring actually drives up the size of the handout needed to sustain cooperation. The situation is aggravated in the presence of competition by other parties and reduces the likelihood that parties can purchase sustained loyalty.

The presence of more than one party distributing handouts may also explain why parties actually continue doing so even if these actually do not purchase votes: The prisoner's dilemma structure of the game prevents each party from stopping (Chauchard, 2016).



4 Data and methodology

4.1 Estimation strategy

Based on our theoretical framework, vote choices V_{ic}^x, V_{ic}^y will be driven by handouts (“bribe payments”) and monitoring (likelihood of punishment). Assuming that all individuals in a given constituency face the same level of monitoring and punishment, vote choices will only be driven by electoral handouts. The main empirical challenge is to examine the vote choice of individual i when given a handout relative to the counterfactual in which she was not.

$$E(\text{vote}_{ic}^y | p_{ic}^y = 1) - E(\text{vote}_{ic}^y | p_{ic}^y = 0) \quad (7)$$

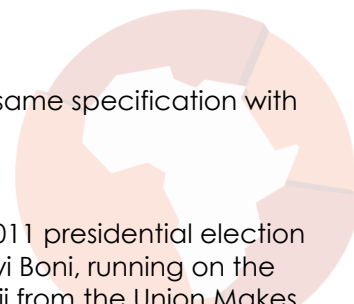
Since we cannot observe both conditions, our analysis relies on different matching techniques. The idea is to account for the non-random exposure to handouts, that is, that those who received cash from politicians differ from those who did not in ways that may be correlated with voting behavior. For instance, an endogeneity concern arises if partisan individuals attend more political rallies, thus increasing their likelihood of being offered a reward *and* of turning out to vote. In this case, it is not the reward driving turnout, but rather a prior interest in politics. Although matching is not a solution for potential unobservables influencing both the treatment (cash handouts) and the outcome (vote behavior), it can improve regression approaches in the estimation of the average treatment effect among the treated – our question of interest – by weighting more heavily the characteristics of those who are “treated” (Angrist & Pischke, 2008).

Yet a key concern with matching is utilizing the appropriate variables to predict the likelihood of receiving the treatment, particularly those variables influencing whether an individual is likely targeted to receive a cash handout, such as the degree of partisanship and income level. For example, Nichter (2008) and Stokes (2005) find that the level of support or partisanship will influence the odds of being targeted with a handout.⁷ The concern with accounting empirically for partisanship is that it is often only measured post-treatment and thus strongly correlated with vote choice. The same is true of other outcomes considered important in the vote-buying literature, such as attitudes toward democracy and citizenship (Carlin & Moseley, 2015), the secrecy of the vote, or politics in general. Therefore, in our analysis we will distinguish those specifications where we include party identification measures from those in which we exclusively match on pre-treatment characteristics.

In terms of economic variables, it is generally hypothesized that individuals with fewer economic resources are likely to be targeted since their votes are cheaper to purchase (Nichter, 2008; Dixit & Londregan, 1996; Stokes, 2005; Brusco et al., 2004; Kramon, 2009). We therefore include two measures of income, as available. The first is an objective poverty index based on home ownership, property size (number of rooms), water and electricity services, and roofing material. The second is an index based on the frequency with which an individual reports going without enough food, enough water, medical care, or enough cooking fuel. Additional socioeconomic variables capture individuals’ educational attainment and employment status.

In addition to the above, we include (as available) a measure of exposure to campaign propaganda such as T-shirts and calendars. This variable allows us to compare the effect of money from that of normal campaign paraphernalia and information. Other variables, such as the level of reciprocity (Finan & Schechter, 2012), are not directly controlled for, but since these would act against the hypothesis of a zero effect for vote-buying, they are less of a concern for our estimates. Finally, we include other demographic covariates in the matching

⁷ The authors disagree on which degree of partisanship is more likely to be targeted. Stokes (2005) argues that those weakly opposed are most likely to be approached by political machines to ensure their support. In contrast, Nichter (2008) and Stokes et al. (2013) argue that passive supporters are most likely to be targeted as well.



equation, such as ethnicity, gender, and age. The goal is to use the same specification with different country data to cross-validate the results.⁸

4.2 Data sources

Our main data set originates from a post-electoral survey after the 2011 presidential election in Benin. This election opposed three top candidates: incumbent Yayi Boni, running on the Cowry Forces for an Emerging Benin (FCBE) ticket; Adrien Houngbedji from the Union Makes the Nation (UN) coalition of parties, who had run in the previous election as the candidate of the Party for Democratic Renewal (PRD); and Abdoulaye Bio Tchane (ABT), an economist and former director of the Africa Department at the International Monetary Fund. The 2011 campaign started on February 10 and ended on March 12, 2011. Yayi was re-elected in the first round of voting with 53% of the vote.

A particularity of our survey is that it was part of a broader research agenda to evaluate the effect of different campaign strategies – town-hall meetings vs. traditional rallies – on voter behavior (Wantchekon, 2012). To avoid capturing changes in voting behavior induced by the intervention, we always include in the analysis a village-level indicator of where the intervention occurred, thus matching individuals from the same electoral district (commune) and treatment status.

Our post-electoral survey includes 150 villages, each with approximately 30 randomly selected respondents (N = 4,491). The survey captures electoral outcomes in the aftermath of the election as well as standard demographic, socioeconomic, and partisan information. The main explanatory variable is an indicator for whether the individuals report receiving “the money,” which in this context implies being offered a handout and accepting it. Unlike other surveys, this is a conservative measure of the dependent variable, which minimizes overstatements of the actual prevalence of vote-buying. In fact, it is likely that this measure might lead to an underestimation of our effects due to social desirability bias. Therefore, in the robustness section we examine an alternative wording provided by the Afrobarometer data.

To cross-validate the results obtained from our survey, we use Round 3 of the Afrobarometer series in Kenya, Uganda, Mali, and Botswana, which contains a related battery of questions on vote-buying.⁹ This survey wave captures electoral behavior and opinions of individuals in the last national election prior to 2005 (2002 in Kenya, Uganda, and Mali, 2004 in Botswana). The conditions surrounding these elections are quite different from those prevailing in Benin; therefore, they would make for a good test case of our initial findings.

Moreover, unlike our own post-electoral survey data, the Afrobarometer Round 3 data include a direct measure of partisanship (not only party membership) and subjective measures of poverty and, more importantly, allow us to test the sensitivity of results to a different measure of the independent variable. With these data, we reconstructed as closely as possible the specification used in our own survey to test the robustness of our initial results.

Finally, we also use Afrobarometer Round 5 (2011) data from Benin to examine the effect of electoral handouts offered by a single party vs. multiple parties. Since this question was posed only in the Benin survey, it presents a unique opportunity to examine a) how frequently parties target the same individuals and b) what are the electoral consequences for vote choice and turnout. The only drawback is that this survey does not contain a district indicator (only regional ones), hence the estimates should be interpreted with caution.

⁸ Given the multiplicity of matching techniques available and the different criteria for pairing off observations, it is important for us to show results from different techniques that may achieve better balance of the covariates included. We will focus on results using “genetic matching” (Diamond & Sekhon, 2013) as well as data pre-processing using nearest-neighbour matching (Ho et al., 2007a, 2007b).

⁹ We focus on Afrobarometer Round 3 because its surveys contain district-level information not available in subsequent rounds.

4.3 Descriptive statistics

Table 1 below presents descriptive statistics for the variables included in the analysis. Panel A reports statistics from our 2011 survey, in which 30% of individuals report being offered money, while 38% report receiving other gifts (T-shirts or calendars) from a candidate during the campaign. It is worth noting the similarity in the self-reported prevalence of vote-buying and the rates found in other contexts, even after using list experiments (Gonzalez-Ocanto, de Jonge, Melendez, Osorio, & Nickerson, 2012). This might suggest that social desirability bias is a lesser concern in this context. In fact, taken at face value, these descriptives suggest a high prevalence of vote-buying in the 2011 Benin elections.

In addition, Panel A of Table A.1 in the Online Appendix describes the data from Afrobarometer Round 5 in Benin and Round 3 in Botswana, Kenya, Uganda, and Mali. The key variable in these surveys is an indicator taking the value of 1 if the respondent was offered an "electoral incentive" and 0 otherwise. While this wording does not exactly capture whether an electoral incentive was actually accepted, it is helpful in reducing social desirability bias (Carlin & Moseley, 2015). Moreover, because this measure likely overestimates the real extent of vote-buying, it runs against our hypothesized effect.

Table 1: Descriptive statistics: Benin 2011 post-electoral survey

Variable	Mean	Std. dev.	Min.	Max.	N
Panel A - 2011 Benin post-electoral survey					
Turnout %	94.4	0.23	0	100	4489
Received money %	29.6	0.456	0	100	4489
Campaign propaganda %	38	0.485	0	100	4489
Party – UN*	3.2	0.177	0	100	4489
Party – ABT*	.6	0.074	0	100	4489
Party – FCBE*	3.7	0.189	0	100	4489
Age	38.239	14.764	18	96	4489
Employed %	66.4	0.473	0	100	4489
No formal education %	45.1	0.498	0	100	4489
Voted Yayi %	23.2	0.422	0	100	4489
Voted ABT %	6.1	0.239	0	100	4489
Voted UN	34.9	0.477	0	100	4489
Female %	43.3	0.495	0	100	4489
Log (objective poverty index)	0.757	0.798	-1.609	3.219	4489
Ethnicity Fon** %	.42.9	0.495	0	1	4489
Treated village %	47.2	0.499	0	1	4489
Panel B - 2011 Benin official village results					
Turnout %	86.801	12.025	11.5	100	150
% votes Yayi	55.872	22.708	10	100	150
% votes ABT	6.897	12.534	0	80.92	149
% votes UN	29.387	25.895	0	89.68	150
% votes opposition (ABT and UN)	36.34	24.402	1	90.21	149

* UN = Union Makes the Nation (coalition of opposition parties); ABT = Abdoulaye Bio Tchane; FCBE = Cowry Forces for an Emerging Benin (incumbent Yayi Boni) ** Majority ethnic group in Benin.

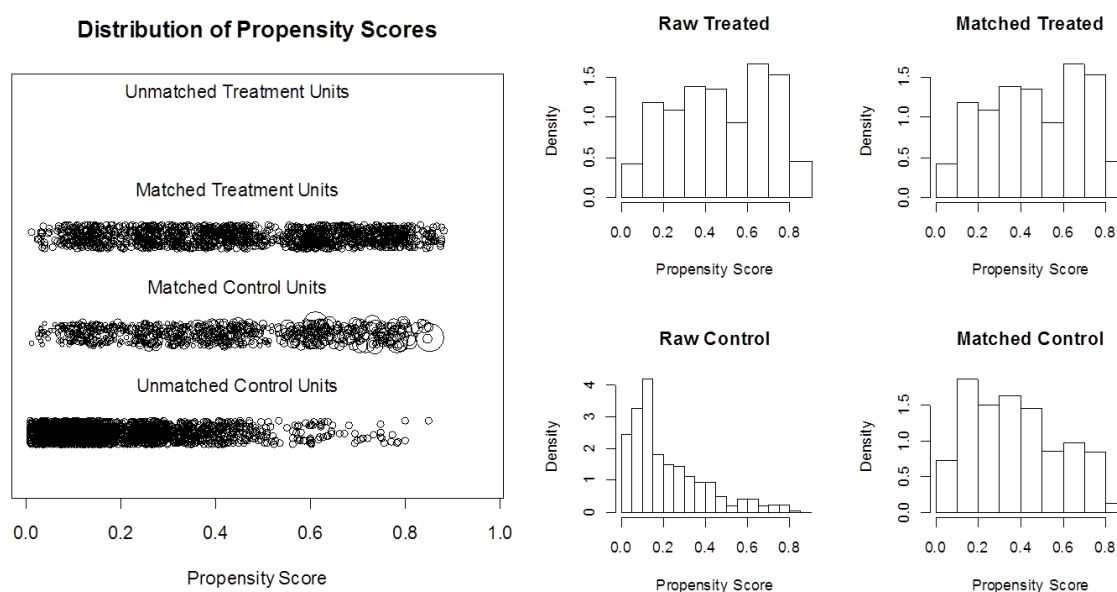
For the specific case of Benin, Afrobarometer fielded a follow-up question asking how many parties or candidates made offers to the respondent. As shown in Panel A of Table A.1, around 37% of surveyed individuals reported being offered “electoral incentives.”¹⁰ Of that proportion, 18% received offers from only one party, while 19% received offers from more than one party. This simple statistic shows the prevalence of multiple rewards in the context of the 2011 election.

In the case of other African countries, the Afrobarometer Round 3 data present wide-ranging variation in the extent to which electoral incentives are reportedly offered. For instance, 46% of surveyed Kenyans report receiving an electoral handout; this proportion is 38% in Uganda, 31% in Mali, and only 2% in Botswana.

In terms of vote choice, our 2011 post-electoral survey closely follows the actual vote share of the opposition (UN and ABT) but tends to underestimate preferences for the incumbent (Yayi) and overestimate turnout when compared to the official results reported in Panel B above. For instance, our survey estimates turnout at around 94%; officially, the actual turnout rate was around 86% (Panel B).

Since we use different matching techniques¹¹, a crucial aspect of these data is whether they allow us to construct a valid counterfactual of the electoral behavior of individuals targeted with cash handouts. We therefore examine whether – after matching – those individuals receiving handouts are similar (in a number of traits) to those who didn't, provided they are from the same district. Figure 3 below presents the overall distribution of the propensity scores after pre-processing of the data with nearest-neighbor matching, and how it varies across treatment and control groups. As shown in the figure, data pre-processing increases the similarity in the distribution of treatment and control groups compared to the raw (unmatched) data.

Figure 3: Propensity score diagnostics: Benin 2011 post-electoral survey

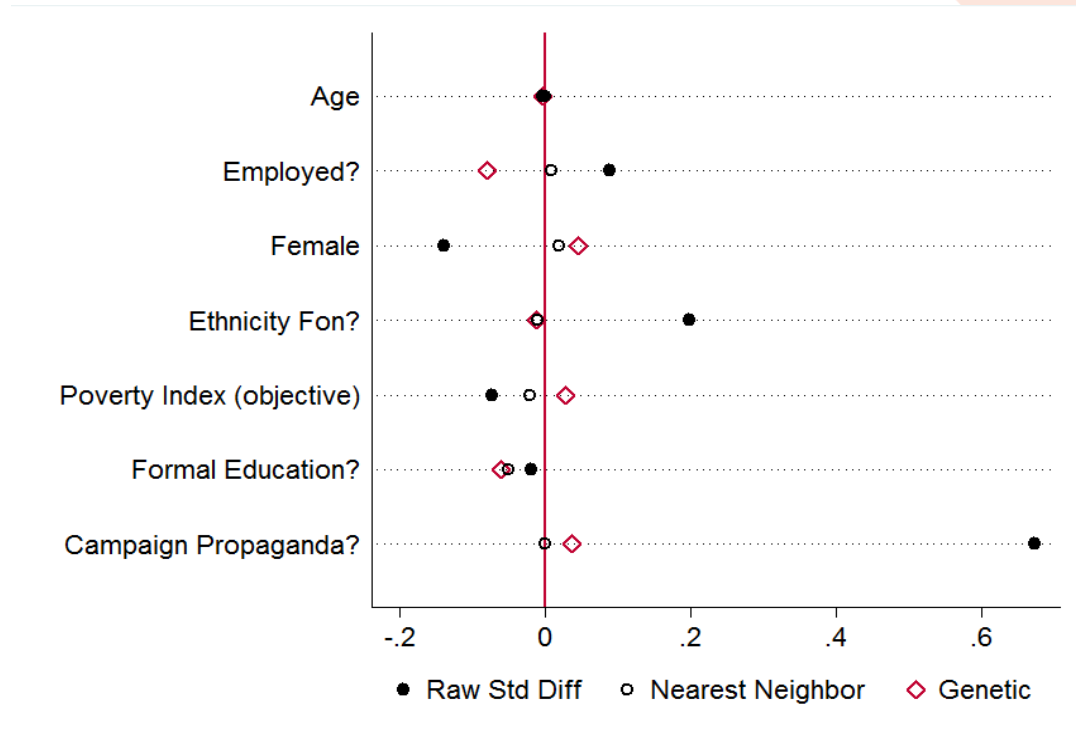


¹⁰ The question was, “Et lors des dernières élections de 2011, combien de fois est-ce qu'un candidat ou un membre d'un parti politique vous a offert quelque chose, comme des vivres ou un cadeau ou de l'argent, en échange de votre vote?”

¹¹ We pre-process our data by matching exactly by constituency and subsequently matching on individual covariates using nearest-neighbor matching as described by Ho et al. (2007a, 2007b). In addition, we use genetic matching (Diamond & Sekhon, 2013) within each constituency.

In addition, Figure 4 below shows the degree of covariate balance before and after matching *within* constituencies for the 2011 Beninese post-electoral data using both nearest-neighbor and genetic matching. Additional figures including other variables, such as partisanship, are shown in the Online Appendix.

Figure 4: Covariate balance: Benin 2011 post-electoral survey



As shown in Figure 4, the use of matched data reduces the mean standardized difference for our measures of poverty, Fon ethnicity, employment, gender, and age. More strikingly, it reduces large imbalances between those who receive propaganda and those who do not. This is important since we want to be able to claim that the effect of handouts on voting behavior is driven by money and not by campaign paraphernalia (T-shirts) or information. In general, we obtain similar balance diagnostics when using genetic matching (Diamond & Sekhon, 2013) or pre-processing data with nearest-neighbor matching (Ho et al., 2007a, 2007b).

In sum, matching techniques improve the similarity, on average, of treatment and control groups, increasing our confidence in the estimates obtained. Matching appears to correct important imbalances among key covariates. In the following section, we examine the effect of receiving an electoral handout on voting behavior.

5 Results

5.1 Unmatched data

In this section, we start by investigating the effect of electoral handouts on voting behavior. Using similar strategies as previous studies, we first rely on unmatched data with no fixed effects and then examine how the results change when we incorporate additional controls such as socioeconomic and educational background, ethnicity, gender, and exposure to campaign propaganda as well as district-level fixed effects. We also separately introduce partisanship variables – measured as party membership – due to concerns about post-treatment bias. The purpose is to identify whether receiving a cash handout has an effect on the electoral behavior of Beninese voters.

As shown in Table 2, cash handouts appear to have a statistically significant effect in the direction hypothesized by the vote-buying literature. In particular, receiving a cash handout has a positive effect on the likelihood of turning out to vote (Panel A) and some effect in choosing the UN party (Panel C). Interestingly, receiving a cash handout is negatively related to voting for the incumbent (Panel B). However, the inclusion of constituency fixed effects reduces both the size and the precision of the estimated coefficient, particularly for self-reported turnout and vote preference for the incumbent. These results suggest that fixed traits affecting all individuals within the constituency, such as institutions or electoral strategies (among others), have some influence on the relationship between handouts and voting behavior. The result is the same regardless of whether one includes measures of partisanship (columns (3) and (5)).

Table 2: Electoral handouts and voting behavior in Benin (2011 election)

| unmatched data

	(1)	(2)	(3)	(4)	(5)
	No controls	Controls	Controls & party ID	Controls & FE	Controls & FE & party ID
Panel A: Self-reported turnout					
Money	0.022*** (0.0079)	0.022*** (0.0079)	0.021*** (0.0079)	0.018** (0.0085)	0.018** (0.0085)
Observations	4,489	4,489	4,489	4,489	4,489
R-squared	0.006	0.014	0.014	0.044	0.044
Panel B: Self-reported FCBE (Yayi) vote choice					
Money	-0.11*** (0.014)	-0.082*** (0.014)	-0.082*** (0.013)	-0.0087 (0.0054)	-0.0090* (0.0054)
Observations	4,489	4,489	4,489	4,489	4,489
R-squared	0.013	0.141	0.154	0.884	0.885
Panel C: Self-reported UN vote choice					
Money	0.051*** (0.016)	0.012 (0.015)	0.012 (0.015)	0.0044 (0.013)	0.0074 (0.013)
Observations	4,489	4,489	4,489	4,489	4,489
R-squared	0.012	0.141	0.192	0.458	0.478
Panel D: Self-reported ABT vote choice					
Money	-0.0086 (0.0082)	0.0024 (0.0081)	0.0023 (0.0080)	-0.0039 (0.0067)	-0.0041 (0.0066)
Observations	4,489	4,489	4,489	4,489	4,489
R-squared	0.002	0.038	0.077	0.449	0.463

Notes: Data come from our post-electoral survey in 2011. All specifications include an indicator for whether the village was "treated" as part of the experiment reported in Wantchekon et al. (2012). Controls: indicators for whether the individual received campaign propaganda such as calendars, T-shirts, etc.; poverty index (objective); formal education; age; gender; ethnicity Fon; and employment status. Columns (3) and (5) also include self-reported membership in UN party, FCBE party, or ABT party. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

In the case of turnout, the inclusion of constituency fixed effects reduces the size of the coefficient in about 20%, but these are still positive and significant at the 5% level. This strong and positive effect for turnout would lead us to conclude that the widespread use of electoral handouts has an important mobilization effect. Handouts also appear to reduce

voting for the incumbent. Taken at face value, these results are consistent with other studies arguing that vote-buying has a positive effect on mobilization and in boosting opposition parties' vote share by undermining incumbency advantage (Vicente, 2014).

Although these findings appear encouraging to vote-buying explanations – showing some correlation between electoral incentives and turnout and voting outcomes – it is important to make sure they are not driven by the particularities of our survey. Therefore, we conduct the same analysis using Afrobarometer Round 3 data from 18 African countries. The reason to focus on this particular Afrobarometer round is that it fielded the question on electoral incentives¹² while also using subnational regional identifiers not included in the revised version of Round 5 (July 2015). In all specifications, we include a comparable set of controls to those used in our own survey and separately include measures of partisanship (“Do you feel close to any particular political party?”) unavailable in our post-electoral survey.¹³

One drawback of the Afrobarometer survey is the smaller number of observations per country. Nonetheless, the inclusion of partisanship measures will allow us to better account for competing explanations.

Table 3: Vote-buying and turnout: Afrobarometer Round 3

	(1)	(2)	(3)
Country	β Coefficient	T-stat	N
All countries	0.045***	6.33	20,143
Zambia	0.058*	1.77	965
Uganda	0.052***	3.61	2,067
Tanzania	0.001	0.050	1,071
South Africa	0.030	0.801	2,108
Senegal	0.056	1.04	972
Nigeria	0.038*	1.79	1,967
Namibia	-0.038	-1.005	968
Mozambique	0.047	1.15	802
Mali	0.107***	4.303	1170
Malawi	0.049	1.27	1,088
Madagascar	0.013	0.58	1,219
Lesotho	-0.114	-1.027	1,015
Kenya	0.101***	3.77	1,033
Ghana	0.002	0.078	1,064
Cape Verde	-0.103	-1.34	495
Botswana	0.201**	2.00	1,045
Benin	0.01	0.651	1,094

Dependent variable: “Did you vote in the last election?” Main independent variable: “During the [20xx] election, how often (if ever) did a candidate or someone from a political party offer you something, like food or a gift, in return for your vote?” (0/1) All specifications include controls for: age, gender, whether member of majority ethnic group, whether employed, whether formally educated, objective poverty index, and subjective poverty index. Further descriptive statistics for the variables included are in Table A.1 of the Online Appendix. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

¹² Which was not the case for Round 4 and Round 6.

¹³ We only include party membership.

The picture that emerges from Table 3 is largely consistent with what we find in our own survey: Pooling across all countries, it appears that receiving an electoral handout increased the likelihood of turning out to vote by about 4.5%, as shown in the first row ("All countries"). Yet this effect is far from uniform across all cases. While some countries exhibit a large positive and statistically significant coefficient (Uganda, Mali, Kenya, and Botswana), other countries exhibit either very small or outright negative ones. Interestingly, the presence of electoral handouts appears to have had little effect on turnout during the 2003 Beninese election.

Furthermore, with regard to voting behavior, those countries where it is *most likely* that electoral handouts affect vote choices (Uganda, Mali, Kenya, and Botswana), we find evidence largely in line with the Benin case, particularly in Kenya and Uganda. For instance, in the Kenya case, having received electoral handouts has a positive effect on Liberal Democratic Party (LDP) vote choices and a negative effect on National Rainbow Coalition (NARC) voting. A similar pattern emerges in Uganda, where individuals offered handouts are more likely to declare a preference for the Forum for Democratic Change (FDC) while the opposite is true for the incumbent National Resistance Movement (NRM). Yet it should be noted that the FDC was created *after* the 2002 elections. Hence, those who received a handout during the 2002 elections but declared a greater preference for the FDC in 2005 cannot be interpreted as people "bought" by the party. If anything, this shows little loyalty to whichever party distributed handouts in 2002. In contrast, vote-choice results for Botswana and Mali are very imprecisely estimated, thus showing that handouts appear to affect turnout but not necessarily vote choices.

Based on these findings, one would conclude that there is *some* evidence that handouts exert a positive effect on mobilization and on the vote share of certain parties. Yet two sources of potential bias might be driving these results. First, it is important to use matching to weight more heavily observations that were actually treated and not overestimate the effect of the treatment. Second, as previewed in the case of Benin, in some cases district-level characteristics influence the observed effect and should be included in the estimation. In the next section, we follow this approach.

5.2 Turnout

In Table 4 below, we report the matching estimates relying on our own post-electoral survey. Columns (1) and (2) present the estimated effect of electoral handouts on turnout using genetic matching, while columns (3) and (4) display the same controls using nearest-neighbor matching to pre-process the data. Odd-numbered columns focus on our basic pre-treatment covariates (age, poverty index, education, gender, employment status, exposure to campaign propaganda, and ethnicity), while even-numbered ones also include measures of party membership, which could be considered post-treatment and should be treated separately. In all cases, we estimate the average treatment effect for the treated (ATT) and match on the individual characteristics within a given constituency. The aim is to understand what would have been the vote choice and turnout of these individuals in the absence of the treatment (i.e. cash handouts).

All estimates show that regardless of the matching technique used, the effect of cash handouts on self-reported turnout is extremely small (ranging from around one-tenth of a percentage point to outright negative). In every case, coefficients fail to achieve conventional levels of statistical significance.

Overall, these estimates of the impact of cash handouts on turnout suggest a small and imprecise effect. While other studies have found a positive effect of private rewards on turnout (Nichter, 2008; Kramon, 2009), once we account for potential district-wide effects in Benin and match on observable characteristics, we find no evidence that this is the case.

**Table 4: Vote-buying and turnout: Benin 2011 post-electoral survey**

	(1)	(2)	(3)	(4)
	Genetic		Nearest neighbor	
DV: Self-reported turnout				
ATT	0.005	-0.0005	0.009	0.01
SE	0.008	0.008	0.009	0.01
T-stat	0.64	-.006	0.98	1.07
P-value	0.52	0.94	.32	0.284
Districts (<i>communes</i>)	60	60	60	60

Source: Post-electoral survey Benin 2011. Columns (1) and (3) present ATT estimates using matching within electoral constituency and treatment status on the following variables: log(poverty index), age, female, formal education, exposure to campaign propaganda (T-shirts, calendars), employment status, and Fon ethnicity. Columns (2) and (4) in addition match on self-reported party membership for FCBE, ABT, and UN parties. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

But is our survey a rare instance? To further examine this possibility, we look at the Afrobarometer Round 3 data and examine how matching within constituencies may change the results obtained with unmatched data. Table 5 below presents the estimated effect of electoral handouts for the cases of Kenya, Uganda, Mali, and Botswana. We examine these cases because they exhibit a positive correlation between handouts and turnout in the unmatched data (Table 3). Once again we observe that matching on the characteristics of individuals within the same electoral district greatly reduces the size and significance of the effect of handouts on mobilization, particularly in the cases of Botswana, Kenya, and Uganda. In the case of Mali, we see a larger yet less precisely estimated coefficient when matching within electoral constituencies (Column (4)) compared to the unmatched results.

Table 5: Vote-buying and turnout: Afrobarometer Round 3 | 4 countries | 2011/2012

Nearest-neighbor matching with replacement				
DV: Self-reported turnout				
	Botswana	Kenya	Uganda	Mali
	(1)	(2)	(3)	(4)
ATT	0.15	0.097*	0.021	0.064*
SE	0.16	0.050	0.02	0.032
T-stat	0.9	1.93	1.11	1.96
P-value	0.38	0.054	0.265	0.051
Clusters	15	48	47	92
Observations	35	545	996	436

Source: Afrobarometer Round 3. Columns (1) to (4) present ATT estimates using matching within electoral constituency on the following variables: objective poverty index, subjective poverty index, age, gender, formal education, employment status, preference for a political party, and whether respondent belongs to majority ethnic group in country. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Although our results do not capture the extent to which social desirability matters, we do not believe it disproportionately affects respondents from these countries given the robustness of the results to different wordings. The next pressing question is, would vote choices be at all different in the absence of cash handouts? In the next section, we analyze whether cash distribution in the 2011 Beninese campaign had an effect on vote choices.

5.3 Vote choices

In Table 6 below, we present the matching estimates of the effect of cash handouts on vote choices using genetic and nearest neighbor matching, respectively. One limitation of our data is that we are not able to capture the identity of the distributing party, among multiple parties that were distributing handouts during the campaign. Therefore, we test the effect of handouts across a number of candidates. The idea is to examine whether, on average, the presence of handouts tended to overwhelmingly favour one of the parties.

Table 6: Vote-buying and vote choices: Benin 2011 post-electoral survey

	(1)	(2)	(3)	(4)
	Genetic		Nearest neighbor	
DV: Self-reported FCBE vote choice				
ATT	-0.001	-0.005	-0.010	-0.012**
SE	0.005	0.004	0.006	0.006
T-stat	-0.20	-1.22	-1.64	-2.03
P-value	0.84	0.21	0.1	0.042
DV: Self-reported UN vote choice				
ATT	0.010	0,006	-0.027	-0.002
SE	0.014	0.014	0.021	0.019
T-stat	0.73	0.42	-1.31	-0.08
P-value	0.46	0.67	0.192	0.93
DV: Self-reported ABT votce choice				
ATT	0.001	-0.0007	-0.004	0.012
SE	0.007	0.007	0.010	0.009
T-stat	0.25	-0.108	-0.37	1.26
P-value	0.801	0.91	0.71	0.209
Districts	60	60	60	60

Source: Benin 2011 post-electoral survey. All columns present ATT estimates using matching within electoral constituency and treatment status on the following variables: log(poverty), age, female, formal education, exposure to campaign propaganda (T-shirts, calendars), employment status, and Fon ethnicity. Columns (2) and (4) in addition match on self-reported party membership for FCBE, ABT, and UN parties. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Overall, estimates in Table 6 suggest that the effect of receiving an electoral handout does not affect the likelihood of voting for the incumbent (Yayi) or the opposition candidates (ABT or UN). That is, in almost all cases, estimates reveal that the effect of receiving an electoral reward is not statistically different from zero. The only exception is a negative effect on FCBE (incumbent) vote choice in one of the specifications in column (4) when accounting for partisanship (a potential source of bias, which means it should be interpreted with caution).

Specifically, columns (1) and (2) show results using genetic matching, while columns (3) and (4) present the results for nearest-neighbor matching. Yet the use of either matching technique shows that the effect of monetary handouts is small – outright negative in several specifications – and far from achieving conventional levels of statistical significance. We interpret these estimates as showing a lack of statistical association between electoral handouts and vote choices.

These results contrast sharply with the estimates using unmatched data shown in Table 5, particularly for the case of voting for FCBE (Yayi), the incumbent. This suggests that the regression results are sensitive to the statistical approach employed. Nonetheless, it should be

noted that despite the large number of variables matched and the focus on within-district results, an unobservable factor could still be driving the null findings, although this is unlikely.

5.4 Cross-validation: Afrobarometer Round 3

One concern with our results is context-specificity, as the matching results could be driven by features unique to the 2011 Beninese election. To alleviate such concerns, we utilize the same approach as before but now using Afrobarometer Round 3 data from countries that exhibit some statistical relationship between handouts and turnout as presented in Table 3 (Kenya, Uganda, Mali, and Botswana).

Once we use different matching techniques and account for potential district-level factors, there is no effect of cash handouts on individual vote choices in the case of Kenya (see panels A-D of Table A.5 in the Online Appendix). For Uganda, different matching techniques and the inclusion of constituency fixed effects greatly reduces the size and precision of the estimates. Now, the negative effect observed for the incumbent (NRMO) and the positive effect observed for the opposition FDC are only barely statistically significant. Yet as explained above, this association cannot be interpreted as voters being “bought” because the FDC wasn’t created until *after* the 2002 elections. Rather, this suggests that some previously targeted individuals shifted their allegiance from NRMO to FDC, consistent with the idea that handouts do not “purchase” permanent loyalty to a party. Finally, for countries that do not exhibit a relationship between handouts and vote choices in the unmatched data (Mali, Botswana), results using matching also lack a strong statistical association.

These estimates suggest that, in the four countries analyzed, the sheer distribution of cash handouts does not visibly “tilt” electoral results toward one particular political party. It is possible that, as explored theoretically, the low level of monitoring by political parties may be a contributing factor to the null effect observed here. Or, as argued below, these voters may be receiving multiple handouts, thus weakening the vote-buying transaction.

5.5 Single vs. multiple offers

Based on the small effects documented above, in this section we explore one potential explanation for why this might be the case: multiple sources of handout distribution. The situation has been labeled “empirically unusual” (Stokes, 2005, p. 324; Nichter, 2008, p. 31), and most analyses of vote-buying have focused on a single party distributing handouts (usually the incumbent). How likely is it that multiple parties offer handouts to the same individuals? Finan and Schechter (2012) provide some evidence of more than one party engaging in material rewards, but the authors do not examine how receiving gifts from more than one candidate might affect subsequent electoral behavior.

According to the Afrobarometer Round 5 data for Benin, it is quite common for more than one party to target the same individuals – about 52% of all electoral incentives offered.¹⁴ This renders the vote-buying transaction necessarily incomplete for at least one of the parties distributing handouts and weakens the *quid-pro-quo* aspect of vote-buying. Moreover, bidding wars may allow voters to act strategically and obtain even higher material rewards but follow their conscience when voting, as discussed in the theoretical section. If this is the case, we should expect that after matching, those who had offers from one party and those who had offers from multiple parties would exhibit electoral behavior running in opposite directions, thus explaining the small (statistically indistinguishable from zero) effect.

Only the Afrobarometer Round 5 data for Benin contain information on multiple offers. As mentioned before, this survey wave asks about electoral incentives, but the public version¹⁵ does not include sub-national district identifiers (*communes*). Hence, we will be matching

¹⁴ Conditional on being offered a handout in the first-place.

¹⁵ Data for this round were revised and published in July 2015, as noted on www.afrobarometer.org. Initial versions of this paper included data from Round 5 prior to the revision.

individuals within broad regions as defined in the survey to compare the effect of handouts from one party relative to the effect of handouts from two parties or more.¹⁶ Because of this, we should interpret the results with caution and be aware of potential constituency-level factors that may be driving the observed effects.

As shown in Table 7 below, even after matching there exists a large and significant difference in the electoral behavior of individuals who receive an offer from one party vs. those receiving offers from multiple parties. Specifically, receiving an offer from just one party is negatively associated with self-reported preferences for the major opposition party UN (which received around 30% of the vote in the election) relative to those targeted more than once. The findings suggest that the previous null results for the opposition party UN may be driven by the average of two opposing effects.

It is impossible to know whether those who were offered electoral handouts by more than one party voted their conscience or were “bought.” But the fact that electoral behavior differs depending on whether one or multiple parties offered incentives is consistent with the theoretical framework showing how offers by different parties may weaken the transactional aspect of vote-buying. Future research should examine the implications of electoral competition on vote-buying.

Table 7: Vote-buying and vote choices: Benin 2011 post-electoral survey

	(1)	(2)	(3)	(4)
Panel A: One vs. multiple offers (conditional on any offer)				
DV:	Turnout	Vote FCBE	Vote UN	Vote ABT
ATT	0.04	0.05	-0.12**	0.028
SE	0.036	0.07	0.044	0.028
T-stat	1.11	0.65	-2.7	1.00
P-value	0.26	0.52	0.007	0.318
Regions	12	12	12	12
Observations	279	279	279	279
Panel B: One vs. multiple offers (conditional on any offer)				
DV:	Turnout	Vote FCBE	Vote UN	Vote ABT
ATT	0.012	-0.002	-0.018	0.004
SE	0.029	0.062	0.032	0.024
T-stat	0.43	-0.04	-0.57	0.18
P-value	0.666	0.970	.572	0.860
Regions	12	12	12	12
Observations	281	281	281	281

Source: Afrobarometer Round 5. Columns (1) to (4) present ATT estimates using nearest-neighbor matching within region on the following variables: objective poverty index, subjective poverty index, age, gender, formal education, employment status, preference for a political party (FCBE, UN, or ABT), and whether respondent belongs to the Fon ethnic group. Panel B in addition matches on party preferences for ABT, UN, or FCBE parties. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

In sum, the statistical differences in behavior observed in Table 7 are closest to a positive result for an effect of single-party offers on vote choices for the opposition. This suggests that the widespread, indiscriminate nature of cash distribution in sub-Saharan Africa and India might weaken the vote-buying transaction, but that targeted individual handouts might be

¹⁶ Figures A.7 and A.8 in the Online Appendix show how matching improves the similarity in the distribution of propensity scores between treatment and control groups.

relevant under different circumstances, such as those prevailing in other regions (e.g. Latin America).

6 Conclusion

In this paper, we present evidence from the 2011 presidential election in Benin to investigate whether cash rewards affect voting behavior. We find that, conditional on key observables, electoral handouts have a small to null effect on vote choice and turnout. We argue that this could be due to a multiplicity of offered handouts, given that around 52%¹⁷ of individuals who were offered electoral handouts were targeted by more than one party. Finally, we provide evidence that constituency-level variables are often important to understand the effect (or lack thereof) of handouts on individuals' electoral choices and behavior.

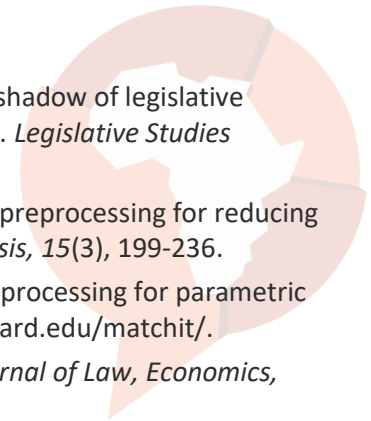
We also provide some evidence for the external validity of our findings by using Afrobarometer data from four countries. In all cases, using different matching techniques and accounting for an array of individual characteristics as well as fixed district-level factors, we find a small and imprecise relationship between electoral handouts and voting behavior. This suggests that our results are not driven by particularities of the Beninese case but might in fact be valid across other sub-Saharan African democracies. Indeed, anecdotal evidence suggests that in other parts of the world, very much like in Benin, electoral rewards tend to be distributed during large political rallies, which may limit the effectiveness of cash distribution. Yet this type of analysis may also lead to different results in Latin America, where party machines play a bigger role in targeting voters for electoral handouts.

¹⁷ Conditional on being offered a handout in the first place.



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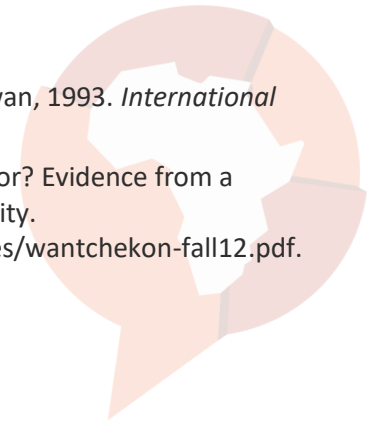
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Online Appendix

The Appendix is available online at <https://sites.google.com/site/jennyguardado/research>.

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