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Do gifts buy votes?

Evidence from sub-Saharan Africa

Jenny Guardado¹ and Leonard Wantchekon²

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Abstract: Vote-buying—or the pre-electoral distribution of private goods in exchange for support at the ballot box—is often blamed for the poor economic performance of many sub-Saharan countries. For instance, vote-buying may undermine accountability and the implementation of sound development policies by pressuring individuals to vote against their own interests. Yet, these effects depend on vote-buying leading to electoral outcomes that would not have occurred otherwise. In this paper, we use Afrobarometer survey data from 17 sub-Saharan elections between 2000 and 2005 to show that, despite its widespread prevalence, vote-buying has limited electoral impact: in only 6 out of 17 elections are gifts-for-votes practices associated with higher turnout than would be expected; yet, in only one election (Nigeria in 2003) did this higher turnout visibly translate into electoral advantage for a particular party. This contrasts with common perceptions about the effectiveness of electoral handouts and the apparent low quality of sub-Saharan African elections.

Key words: national elections, electoral gifts, vote-buying, sub-Saharan Africa, survey analysis

JEL classification: C83, D72, O55

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¹ Georgetown University, Washington, DC, USA, corresponding author: jennyguardado@gmail.com; ² Princeton University, Princeton, NJ, USA

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Katajanokanlaituri 6 B, 00160 Helsinki, Finland

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1 Introduction

The exchange of private goods such as cash and gifts for electoral support or higher turnout on election day is a common occurrence in developing democracies. According to Afrobarometer Round 3¹, an average of 18.3 per cent of respondents were offered an electoral handout (gift, money) in exchange for their vote in the previous year's elections across 17 countries in sub-Saharan Africa (SSA). For many observers and academics, these activities amount to 'vote-buying'²—a practice that differs from 'pork' clientelism³, patronage, or electoral fraud but that has emerged as an important concern in new democracies.

In particular, gifts-for-votes exchanges could reduce accountability and lead citizens to vote against their own interests (Banerjee et al. 2011; Djankov et al. 2010). That would be the case if vote-buying led to policies targeted along ethnic lines and not based on objective needs (Chandra 2007; Horowitz 1985), leading to worse public outcomes. Alternatively, vote-buying may enable the concentration of power of certain elites by keeping them in office (Acemoglu et al. 2013). In this sense, the distribution of electoral gifts may disproportionately affect the poorest by denying them a voice in the design of policies that affect them.

Yet, such negative outcomes rest on the premise that these practices effectively change an individual's political behaviour in a way that would not have occurred otherwise. Do we actually *know* that these handouts lead to higher political support and turnout in a *quid-pro-quo* fashion that would not have happened otherwise? In this paper, we explore this question explicitly by looking at the connection between gifts and the likelihood of turning out to vote and expressing support for a particular political party.

Despite the importance of establishing the electoral impact of these exchanges, it is only recently that some studies have attempted to explore how they translate into political behaviour. Rather, most of the attention in the literature has centred on the distribution or targeting of handouts by parties and politicians. For instance, seminal papers have found that politicians do not distribute these gifts at random; instead they follow distribution strategies, such as targeting poorer voters (Dixit and Londregan 1996) or those indifferent to political options (Stokes 2005). Other evidence points to politicians targeting voters with a higher propensity to turn out to the ballot box (Nichter 2008), with more extensive social networks (Cruz 2019), or exhibiting intrinsic reciprocity (Finan and Schechter 2012; Lawson and Greene 2014). An implicit assumption in this literature is that the non-random distribution of these handouts must have 'bought' some votes; otherwise, rational political actors would be wasting scarce time and financial resources in giving them.

In this paper, we bring this assumption to the data: how do electoral gifts impact, on average, political mobilization and political support for a political party? To answer this question we use survey data from 17 African elections provided by Afrobarometer R3 (2005/06) on the prevalence of electoral handouts, people's voting history, and their current political preferences. The advantage of these data is that they field the same question across very different contexts, thus helping us cross-validate and generalize our findings—avoiding the limitation of single-case

¹ Available at www.afrobarometer.org. Afrobarometer data cover Benin, Botswana, Cape Verde, Ghana, Kenya, Lesotho, Madagascar, Malawi, Mali, Mozambique, Namibia, Nigeria, Senegal, South Africa, Tanzania, Uganda, and Zambia. Round 3 relates to the year 2005/06.

² This definition is consistent with the one used in Stokes et al. (2013).

³ Or a collective's exchange of public goods for support.

studies. These surveys have the added benefit of including electoral district identifiers, thus allowing us to control for time-invariant contextual variables, such as the level of political competition in a district as well as specific country characteristics (e.g. electoral systems, level of development). Furthermore, these cases represent elections in newly democratic settings, which scholars suggest may be more amenable to this type of practice—representing a ‘most likely’ scenario.

Our priors from interviews with politicians and other contextual evidence is that handouts have little to no impact, *on average*, on either the extent of political mobilization or the distribution of political support. Furthermore, there is limited evidence that voters in numerous SSA elections think that their vote choices can be monitored—a key condition for enforcing these transactions. Likewise, qualitative interviews with Beninese politicians suggest that handouts are not intended in a *quid-pro-quo* manner but rather to incentivize participation at rallies or to signal viability, for example.

Nonetheless, to adjudicate between these views our empirical approach compares the voting history and political preferences of individuals offered electoral incentives with those who were not. Our premise is that a significant correlation (positive or negative⁴) between electoral gifts and turnout or political support would indicate that handouts have a plausible impact on voters’ political behaviour. For instance, while controlling for other factors, if the share of those who vote for a particular party ‘matched’ the share of those who received a handout, this would provide some evidence in this direction. In contrast, a small and weak correlation—not driven by effects cancelling out in the aggregate—would suggest that other factors (not handouts) play a stronger role in determining voters’ behaviour. If that is the case, we need to worry less about the potentially deleterious down-the-road effects of handouts on electoral accountability, public policy, and democratic consolidation in general.

Our main empirical challenge is that the distribution of handouts is, according to the literature, purposefully non-random. However, we argue that this type of bias actually runs against our hypothesized effect—a null or very small impact of election handouts. As thoroughly documented in the targeting literature across many different contexts, politicians recurrently target the types of voters who would yield the highest electoral gains—for example, those with lower incomes and less information and education;⁵ those with (or without) a political preference, in a bid to make them switch allegiances (or just turn up to vote); those who may better reciprocate (Finan and Schechter 2012; Lawson and Greene 2014); or those with large social networks (Cruz 2019). The overall purpose of these strategies is to select individuals who would be most likely to behave in a way beneficial to the politician (or party). In the light of these strategies, we expect the targeting of electoral handouts to run against finding a null result.⁶

⁴ If positive, it would suggest that handouts induce greater turnout and increase the declared preference for a given party. However, some scholars also point to electoral strategies in which handouts may have a negative impact on mobilization—via abstention buying—or lead to a switching of political allegiances.

⁵ The argument is that such people would be cheaper and easier to manipulate.

⁶ A particular distributing strategy, whereby politicians purposefully target those who are less likely to vote or support the party, seems at odds with the existing literature. We find only one empirical example of this particular case, in Cox and Kousser (1981), relating to elections in New York in the late 19th century. Other activities, such as electoral violence or intimidation, may seem more effective than gift-giving in de-mobilizing groups of voters, particularly in SSA. Nonetheless, in the final section of the paper we also discuss the possibility that electoral gifts may not be distributed with electoral behaviour in mind.

Finally, other individual reporting biases inherent in surveys, such as social desirability bias or overstating one's voting history, would might run against our hypothesized effect, be unlikely to differ among those receiving handouts or not, or be minimized by the specific wording of the Afrobarometer surveys.

Our estimates show that, while handouts have some effect on mobilization in a third of the 17 elections studied, in only one of these cases—that of the Nigerian election of 2003—could it have plausibly translated into visible differences in political behaviour. All estimates control for pre-election individual sociodemographic characteristics⁷ as well as for constituency-fixed effects that capture local institutional traits and development levels, local political competition, and the electoral system, among many other possibilities. We interpret these findings as showing how, consistent with the qualitative and contextual characteristics of these settings, handouts have little detectable statistical impact on overall electoral behaviour.

Importantly, these results appear not to be driven by the inadequate targeting of voters. For instance, those targeted were generally self-reportedly poorer on average—a key trait in the targeting literature. Given that these individuals' votes are reputedly 'cheaper' to purchase, this suggests that efforts are being made to achieve the highest electoral yield at the lowest monetary cost. Yet, we find little evidence that these efforts bear any fruits electorally speaking.

Similarly, these effects are unlikely to be driven by the cross-pressure of parties distributing handouts to different constituencies such that they cancel out in the aggregate. If that were the case, we would expect to see larger turnout effects as well as an overall reduction in the number of individuals reporting no political preference for any party, neither of which is supported by the data. However, it is possible that the null effect we document is driven by the fact that the same individuals receive handouts from multiple parties, such that they feel a lower pressure to turn out to vote or choose a particular party and might simply follow their own intrinsic preferences. If that were the case, the behaviour of recipients and non-recipients would look very similar, serving as a potential explanation for the weak association between handouts and political behaviour that we document.

Finally, it is possible that handouts do not have an impact on presidential/national vote choices but do so at the local level. Although we do not include an explicit question about political support for local politicians, we find that the presence of handouts has no positive impact on the evaluation of local-level officials. If anything, those targeted have a more negative opinion of the performance of local (as well as national) officials, consistent with their small impact on political support at the ballot box.

In sum, these estimates suggest that, despite handouts being distributed in a way that might maximize their yield, they do not translate into statistically detectable differences in voting behaviour or political support. We think that these results are due to a misplaced focus on the *quid-pro-quo* role of handouts, a view in line with recent revisionist accounts (e.g. Hicken and Nathan 2020). Instead, these results strongly suggest that handouts may instead play other roles, such as that of an 'entry fee' into politics, an inducement to attend political rallies, a payment for mobilization services rendered, or a form of publicity.

⁷ Their exclusion has little impact on the turnout results and leads to large standard errors in the political support analysis.

This paper contributes to a small but growing literature on the electoral impacts of handouts (Brusco et al. 2004; Cantu 2019; Diaz-Cayeros et al. 2009). Like Cantu (2019) and Diaz-Cayeros (2009), we find a very small impact of handouts on average—unlikely to sway electoral results one way or another. This is not to say that handouts never have an impact—for example, Brusco et al. (2004) do find a relationship in Argentina—and our own data show that this was also the case in the 2003 Nigerian election, widely characterized in the media as fraudulent. Yet, in our results, taken from an array of cases, a null effect appears to be the rule rather than the exception.

Our paper also contributes to recent revisionist accounts in the literature on clientelism (e.g. Hicken and Nathan 2020) by providing empirical evidence on the lack of commitment to political parties (in general) on the part of voters induced by electoral offers. In this sense, this paper reaches the same conclusion as those authors—namely, that the literature focus on such commitment problems is a red herring—but for a different reason. While Hicken and Nathan (2020) focus on the lack of evidence of monitoring or enforcement of vote-buying exchange, we base our case on the lack of results at the ballot box.

Finally, in relation to previous work, this paper expands on the results of Guardado and Wantchekon (2018) by providing further evidence of a small impact of electoral handouts in a wider set of contexts and a ‘lower’ statistical bar to meet the definition of ‘vote-buying’.

The rest of the paper is organized as follows: Section 2 reviews the existing literature and presents the theoretical framework. Section 3 provides information on the context of these elections. Sections 4 and 5 describe the data and the results of the analysis, while Section 6 discusses the results and suggests some avenues for future research.

2 Related literature

Political scientists have long worried about the prevalence of cash handouts in democratic elections. According to a first set of seminal papers, the strategic targeting of cash handouts provided evidence of turnout- or vote-buying. In this group of papers, the distribution of cash handouts by political machines was seen in effect to ‘purchase’ votes or turnout in a *quid-pro-quo* fashion. Even in the presence of a secret ballot, these machines would be able to monitor and enforce compliance as well as to strategically target handouts to sway swing voters, mobilize passive supporters, or purchase ‘abstention’ votes to obtain the desired electoral outcome (Gans-Morse et al. 2014). Evidence from Argentina (Brusco et al. 2004; Nichter 2008; Stokes 2005), Mexico (Magaloni 2006), Paraguay (Finan and Schechter 2012), and Philippines (Cruz 2019) supports the idea of strategic targeting by politicians. The implication is that the presence of handouts, particularly when directed at strong, swing, reciprocal, or poor voters, strongly indicates vote-buying and will lead to visible differences in behaviour, *all else being equal*.

While this framework offers a good explanation of some elections—mainly those where sophisticated political machines are present—a more recent and growing literature in political science has highlighted its limitations when looking at other settings. Whether it is because political machines appear to be underdeveloped or completely absent in many new democracies, particularly in SSA (Bratton 2008; Kramon 2018; Van de Walle 2007) or because the political machines that exist are unable to adequately target voters (Greene 2016; Lindberg 2010; Schneider 2014), scholars have increasingly noted that the presence of strategically targeted cash handouts is not a sufficient condition to ascertain a cash-for-votes exchange (Conroy-Krutz and Logan 2012; Guardado and Wantchekon 2018; Simpson 2012). Rather, the presence of electoral handouts is a reflection of more complex political dynamics than simple cash-for-votes transactions. The

implication is that in certain cases ‘neither the purpose [of vote-buying] nor its impact can be taken for granted’ (Conroy-Krutz and Logan 2012: 636).

Using these two competing views of the effect of cash handouts as our starting point, in this paper we seek to characterize where most of SSA elections stand: is it the case that the presence of cash handouts, particularly if targeted at certain characteristics, leads to visible differences in behaviour, as suggested in the early literature? Or, rather, are there ‘more complex dynamics’ in place such that electoral handouts do not necessarily translate into more political support and participation, as suggested in more recent accounts?

Our priors, based on qualitative accounts from politicians, contextual information, and previous findings across four SSA elections (Guardado and Wantchekon 2018), suggest that most elections in the region fall into the second category. Namely, handouts are widespread but not a major determinant of voting behaviour and are unlikely to play a significant role in electoral results. The reasons for this null effect are numerous: handouts often come from multiple sources; they are difficult to enforce in the absence of a well organized political machine; they could also simply be signalling electoral viability (i.e. candidates distribute gifts not to purchase votes but to show that they are a serious contender); or handouts could simply be a reflection of existing social norms, such as that of gift-giving by those in power or with better socioeconomic conditions, among many other possibilities. This suggests that the first wave of vote-buying studies, while useful in uncovering patterns of distribution, may be less so in identifying their effectiveness, and ultimately in answering the question of ‘why’ such activities take place.

In the next sections, we take these claims to the data in three steps. First, we examine the question: are cash handouts directed at poor voters or at supporters (swing voters)? Second, we ask: are handouts associated with greater turnout—a sign that handouts are deployed to influence electoral results? Finally, based on the targeting and turnout results, we ask: is the presence of electoral handouts correlated with political support for certain parties?

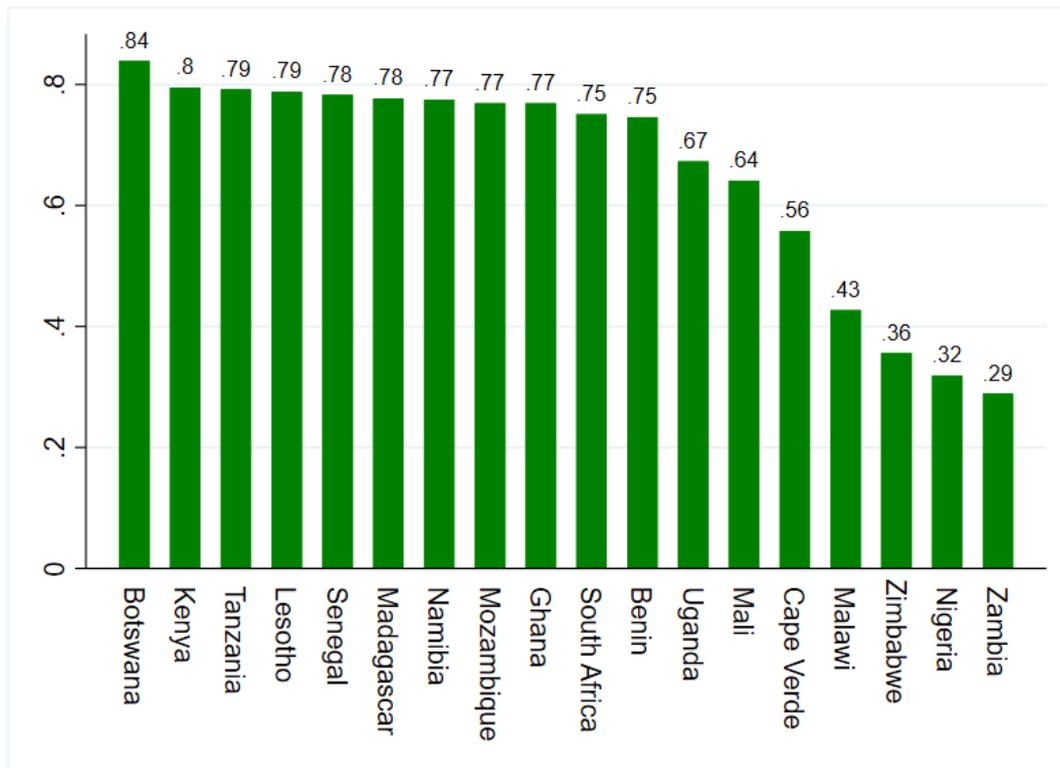
3 Context: national elections in Africa

We examine the role of electoral handouts using individual-level survey data across 17 African elections between 2000 and 2005. All of these cases represent new democracies with elections at different levels of democratic quality, at least as perceived by their citizens. For instance, Figure 1 shows how a large majority of respondents in many countries perceive their elections as ‘completely free and fair’ or at least ‘free and fair, but with minor problems’. Only in three countries, Zambia, Nigeria, and Zimbabwe, do a large majority of citizens express concerns about the functioning of their democracy.

This perception contrasts with some other measures of democratic quality—for example, the EIU democracy index, according to which very few SSA countries (in fact, only Mauritius) are considered a ‘full democracy’; rather, most of them are categorized as ‘flawed’ or ‘hybrid’ democracies.⁸ One component of the EIU index is the presence (or absence) of intimidation and (or) fraud in the electoral process.

⁸ See [DEMOCRACY_INDEX_2007.indd](#) (economist.com)

Figure 1: Freedom and equity of last election

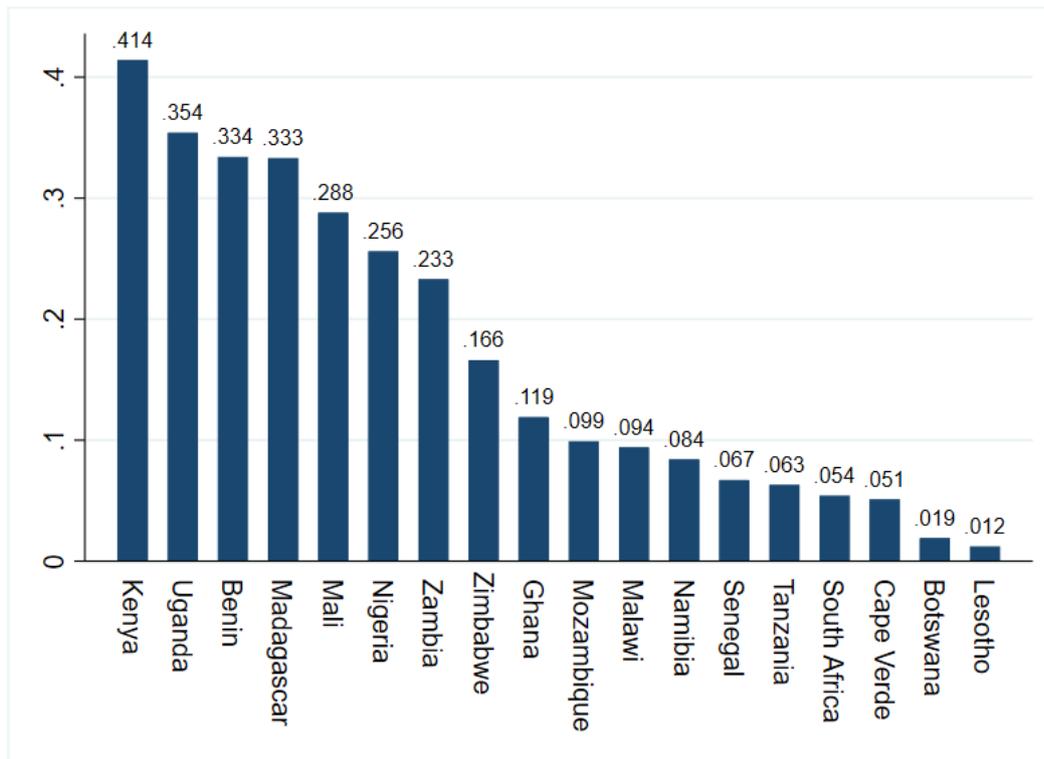


Source: authors' construction based on Afrobarometer Round 3 data. Combination of the categories 'Completely free and fair' and 'Free and fair, but with minor problems' in answer to the question: 'All in all, how would you describe the freedom and equity of the last national elections of 20xx⁹?'

As highlighted in Figure 1, respondents across most countries generally perceived their elections as free and fair. However, this sense of 'fairness' coexisted with a seemingly large presence of electoral handouts, as shown in Figure 2. In other words, politicians offered food and/or gifts in return for votes. Even in countries with the (self-reported) most free and equitable elections—Kenya, Benin, Madagascar—there is a non-negligible amount of offers in exchange for citizens' votes. In contrast, some of the cases with the worst perceived elections do not exhibit such a high prevalence of this type of behaviour (namely, Cape Verde and Malawi). This could be due to underreporting, although it is more likely that the perceived freedom and fairness of an election is unrelated to the presence of these incentives, and other factors such as violence or intimidation play a bigger role.

⁹ The specific election year varies by country, see footnote 17 for details.

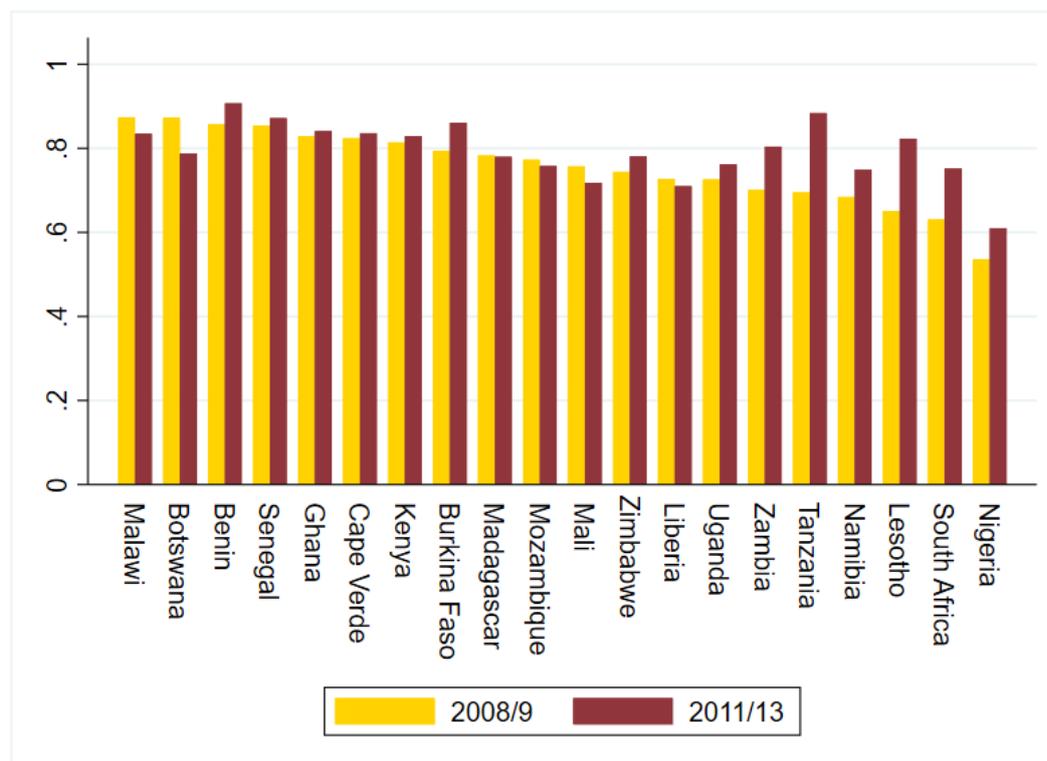
Figure 2: Frequency of electoral incentives offered: any offer



Source: authors' construction based on Afrobarometer Round 3 data. Combination of the categories 'Once or twice', 'A few times', and 'Often' in answer to the question 'And 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?'

Another reason for the disconnect between the perceived quality of elections and the presence of electoral handouts is that the latter are rarely seen as part of an enforceable *quid-pro-quo* exchange of gifts for votes. As shown in Figure 3, in most countries, around 70–80 per cent of respondents believe that it is 'not at all likely' or 'not very likely' that powerful people could find out how they voted. That is, there appears to be a widespread belief that the vote is secret, potentially complicating efforts to enforce any handout-for-votes transaction. This is important as politicians may manipulate perceptions of ballot secrecy to induce compliance in gifts-for-votes exchanges (see Ferree and Long 2016).

Figure 3: Perceived secrecy of the vote



Source: authors' construction based on Afrobarometer Round 4 and Round 5 data. Combination of the categories 'Not at all likely' and 'Not very likely' in answer to the question: '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?'

Altogether, these stylized facts suggests that, despite the widespread nature of electoral handouts, there are not many countries where these are expected to impact political behaviour. Not only do most citizens perceive their vote to be secret, but they also mostly view their democracy positively, suggesting that the prospect of electoral handouts threatening vote secrecy and subverting democracy is not a major factor in their calculation. However, in the following section we examine this possibility systematically using individual-level data and accounting for country-level differences in the analysis.

4 Data and empirical approach

4.1 Data

Our data come from the Afrobarometer Survey Round 3 conducted in 2005/06, which includes questions about the latest national elections conducted prior to the survey.¹⁰ This survey fields the same questionnaire across different countries, providing us with a larger sample and enabling us to assess the generalizability of our findings.

¹⁰ The countries included are: Benin, Botswana, Cape Verde, Ghana, Kenya, Lesotho, Madagascar, Malawi, Mali, Mozambique, Namibia, Nigeria, Senegal, South Africa, Tanzania, Uganda, and Zambia. Zimbabwe was included in the survey but did not field our questions of interest.

The main variables of interest are: first, the prevalence of offers of gifts in exchange for support during the latest election;¹¹ second, whether the individual voted in the last election;¹² and which party do they support politically.¹³ As shown in Table 1, across our sample, 18.3 per cent of respondents report being offered a gift in exchange for political support and 80 per cent report having voted in the last election. Finally, around 22 per cent of respondents report no preference for a political party if a presidential election were held tomorrow. If handouts affect political support in a lasting manner, we would expect fewer voters to exhibit ‘no preference’.

In addition to the above variables we include in the analysis other controls, such as whether politicians frequently offer gifts; respondents’ ‘closeness’ to any political party; and respondents’ sociodemographic characteristics (gender, age, subjective poverty index, objective poverty index, employment status, belonging to the ethnic majority group in their country, and formal education) (Table 1).

Table 1: Afrobarometer Round 3 descriptives

	Mean	Std. Dev.	Min.	Max.	N
Voted in last election?	0.808	0.394	0	1	21,838
Offered gift?	0.183	0.387	0	1	23,431
No vote preference	0.216	0.411	0	1	23,431
Age	36.543	14.765	18	103	23,415
Subjective poverty index	4.31	3.833	0	16	23,405
No closeness to party	0.377	0.485	0	1	23,211
Employed	0.376	0.484	0	1	23,633
Female	0.499	0.5	0	1	23,694
Objective poverty index of majority ethnic group	0.313	0.464	0	1	23,694
Formal education	0.394	0.489	0	1	23,612

Source: authors’ construction based on Afrobarometer Round 3.

4.2 Reporting biases

The way these variables are measured has some advantages and disadvantages vis-à-vis the existing literature in its capacity to capture the prevalence of electoral handouts and electoral behaviour. An important advantage of our main measure of electoral incentives is that it reduces concerns of social desirability bias. Because it only asks whether gifts were offered (not necessarily accepted), there is less stigma attached to reporting (Carlin and Moseley 2015) and therefore might be providing us with an upper bound of their prevalence.¹⁴ However, in terms of political participation and preferences, there is some potential for overstatement of an individual’s voting history. In particular, respondents may over-report whether they voted in the past election or be more likely to report that they supported the winner of the election—a form of confirmation bias. Yet, these factors are likely to vary across the board, not only among those receiving handouts.

¹¹ Responses to the question ‘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?’

¹² Responses to the question ‘Did you vote in the most recent 20xx national election?’

¹³ Responses to the question ‘If a presidential election were held tomorrow, which party’s candidate would you vote for?’ For reasons of space, we only report in Table 1 whether the respondent has no preference (or does not know).

¹⁴ Gonzalez-Ocantos et. al. (2012) find a large degree of desirability bias when asking individuals whether they were given a gift or done a favour by a party activist, which is nonetheless different from the wording in the Afrobarometer surveys used here.

However, if for some reason being offered a gift induces respondents to overstate their voting history or misstate their political preferences (e.g. in favour of the winner), it would only run against our hypothesis of no relationship.

4.3 Sample

In terms of the sample included, although the original Afrobarometer survey includes 18 countries (see footnote 11), in the data used there is no information about Zimbabwe, so it is excluded from the sample. Furthermore, the fact that some of the elections were conducted up to five years prior to the survey raises questions about the accuracy and relevance of the results: not only is it hard to remember one's preferences and behaviour from five years ago, but the analysis of whether an individual was targeted with handouts and an individual's political support at the moment only makes sense if these events are not far apart.¹⁵ For this reason, when examining the effect of handouts on political preference we include only those elections conducted in the previous two years.¹⁶

4.4 Empirical approach

Our analysis proceeds in three steps. Following the targeting literature, we start by examining the individual determinants of electoral handouts: are handouts driven either by existing political support or by socioeconomic and educational status? If so, that would suggest some degree of intentionality, namely, that handouts are distributed to the individuals most likely to respond to them. If this is the case, we would expect handouts to have a larger impact on political behaviour than in cases where handouts are (or appear to be) distributed randomly. For this initial analysis, we estimate Equation (1) using OLS:

$$Handout_{idc} = \alpha_{dc} + X_{idc} + e_{idc} \quad (1)$$

Where $Handout_{idc}$ is the individual's reported offers of handouts in the last election (once or twice, a few times, or often); α_{dc} is a constituency-fixed effect for each district d from country c ; X_{idc} represents a vector of individual-level controls (age, poverty perceptions, employment status, ethnic group affiliation, employment status, and gender); and e_{idc} is the error term.

Once we have established the patterns of handout targeting, we then examine whether handouts appear to be correlated with turnout. If widespread handout distribution is having any impact at all, we would expect it to translate into systematically higher turnout (i.e. turnout-buying) or lower turnout (i.e. abstention-buying). A lack of effect could reflect either that handouts are irrelevant to this type of behaviour or that campaigns may be targeting some respondents with handouts to mobilize and others to demobilize them, which may cancel each other out in the aggregate—a possibility which seems unlikely in this context but will be discussed further below.

¹⁵ For instance, if an individual was targeted with handouts in the last election, but political parties competing in that election no longer exist or have changed significantly in the interim, it is impossible to assess the effect of the handout.

¹⁶ Because the Afrobarometer Round 3 surveys were fielded between 2005 and 2006, they covered retrospectively the following national elections: 2000 in Senegal and Tanzania; 2001 in Zambia; 2002 in Cape Verde, Kenya, Lesotho, Madagascar, Mali, and Uganda; 2003 in Benin and Nigeria; 2004 in Botswana, Ghana, Malawi, Mozambique, Namibia, and South Africa; and 2005 in Zimbabwe (but the latter did not have data on electoral incentives). To improve accuracy, the analysis is thus limited to elections between 2003 and 2005 (Benin, Nigeria, Botswana, Ghana, Malawi, Mozambique, Namibia, and South Africa).

Finally, building on the results of the above analysis, we identify the elections in which turnout-buying (or abstention-buying) activity is more likely to translate into tangible political support for a given party. For instance, in places where handouts appear to impact mobilization, we also expect them to have an impact on the proportion of individuals who report a particular political preference. This prediction is based on the observation that turnout by itself is valuable insofar as it translates into an electoral edge for a given political party.

For the latter two exercises, our analysis is based on estimates of Equation (2):

$$Y_{idc} = \alpha_{dc} + \beta_1 \text{handout}_{idc} + X_{idc} + e_{idc} \quad (2)$$

Where Y_{idc} is either mobilization (turnout) or political support for a given party (or lack thereof). All other controls are the same as those defined above, which are generally time-invariant. While this regression represents the pooled results, when analysing each country separately we drop the subscript c .

5 Results

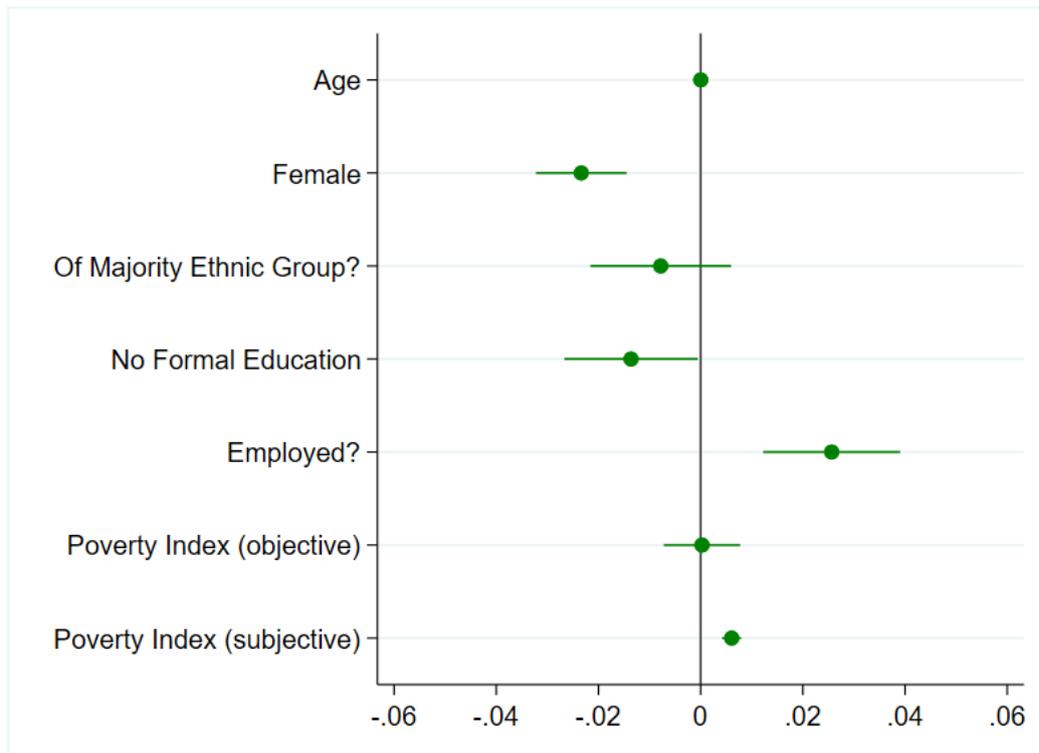
5.1 Are voters intentionally targeted?

Figure 4 presents our estimates from Equation (1) based on the sample of all respondents ($N=21,840$). District-fixed effects are included in the estimation, which accounts for both local- and country-level time-invariant differences that would capture factors such as local political competition, institutions, and levels of economic development across constituencies. On average, males appear to be more likely to be targeted than females—potentially due to their higher involvement in political and/or economic activities outside the home. The same would be true of those in the employed category. Second, those with formal schooling are more likely to be targeted, although this category includes individuals with primary education as well university graduates, so the results should be interpreted with caution; it is still possible that those with primary education are more likely to be targeted than those with university education if evaluated separately. Finally, it appears that those with self-reported higher levels of poverty—i.e. those who more frequently report going without food, cooking fuel, medical care, and water—are more likely to be offered an electoral ‘gift’. Although we observe no difference using objective measures of poverty, this is based on the characteristics of the enumeration areas, so it is still possible that targeting occurs among those most deprived within the same area.

Some of these results conform to the ones observed in the targeting literature: poorer voters may be easier to ‘buy’ (Brusco et al. 2004; Corstange 2010; Dixit and Londregan 1996). However, other patterns do not exactly align with the literature; for example, those employed and those with formal education are also more likely to be targeted. However, as mentioned, the latter traits may also reflect greater activity outside the home, thus making those people more likely to be approached by political operatives (see, for example, Brierley 2013).¹⁷

¹⁷ It should be noted that including other variables, such as whether the individual is from an urban or rural area or whether they have a higher valuation of democracy, has no impact on their likelihood of being targeted with handouts. Results available upon request.

Figure 4: Individual determinants of handout targeting



Note: 95 per cent confidence intervals. Estimates of Equation (1) using OLS. All estimates include district-fixed effects and standard errors clustered at the district level.

Source: authors' construction.

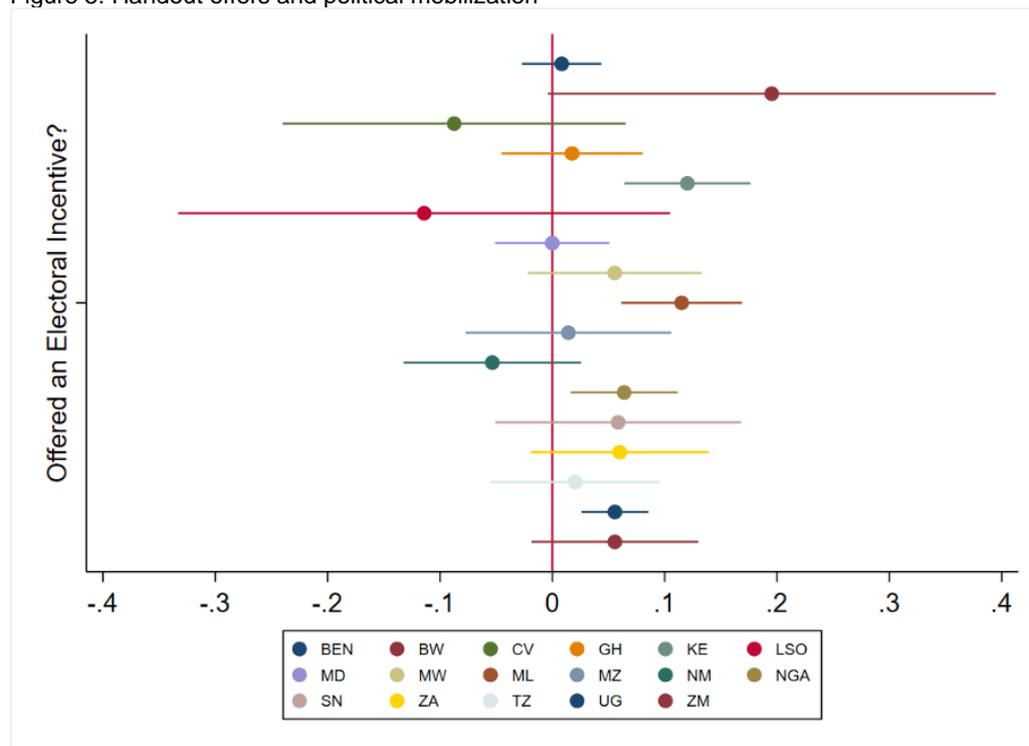
In addition to socio-demographics, it is important to explore the role of partisan affiliation or existing political preferences in the probability of being targeted, which is central to the existing literature (Nichter 2008; Stokes 2005). The difficulty with these variables is that we lack data on pre-election preferences, so that any declared affinity to a party could be driven by the distribution of handouts itself. However, we argue that this type of endogeneity bias would only run against our hypothesized result: if supporters of the party were disproportionately targeted (Nichter 2008), then handout offers and political support would be positively correlated—contrary to our claims. In contrast, if weak supporters are disproportionately targeted (Stokes 2005), and handouts sway these voters, it may also appear as if handouts are driving a positive relationship. As shown in Table A1 in the Appendix, self-reported closeness to any of the two or three major political parties in a country is not associated with a higher probability of being targeted with electoral offers. Instead, we find some evidence that those lacking a partisan affiliation are more likely to be targeted with a handout, particularly in Nigeria and Ghana (and to a lesser extent Mozambique). This correlation somewhat previews the results from the following section—which show little relationship between the distribution of handouts and expressed support for a political party. In the following section, we make this claim more systematically.

In sum, a look at the determinants of electoral handouts shows that these may not be entirely random, but targeted at those identified as the most responsive individuals—particularly those with higher reported poverty and, in two countries, those lacking partisan affiliation. Unfortunately, the survey lacks additional information to test other determinants, such as the presence of social and family networks (Cruz 2019) or their intrinsic reciprocity levels (Finan and Schechter 2012), but as these are expected to lead to higher levels of political support for the distributing party, they run against our hypothesized result.

5.2 Do handouts correlate with higher turnout?

If handouts broadly impact political mobilization and declared support for a party, we would at least expect that individuals with electoral offers would turn out to vote more than those not offered, all else being equal. This result would hold irrespective of which party distributed handouts: regardless of who they prefer, they first have to show up to vote. Figure 5 presents the results of estimating Equation (2) for each country, using self-reported participation in the last election as the dependent variable. As can be seen, of the 17 elections studied, only 6 provide some evidence of behaviour consistent with turnout-buying claims: Botswana, Kenya, Malawi, Mali, Nigeria, Uganda. In the other 11 countries, the effect is either small and/or imprecisely estimated.

Figure 5: Handout offers and political mobilization



Note: 95 per cent confidence intervals. Estimates of Equation (2) using OLS. All estimates include district-fixed effects and standard errors clustered at the district level.

Source: authors' construction.

Moreover, when we analyse the intensity of the handout offer (as opposed to *whether* any offer takes place), we see even smaller effects. Namely, receiving offers more frequently seems to increase the likelihood of turning out to vote in only 4 (as opposed to 6) of the 17 elections considered (Figure A1).

One explanation for the small effect on turnout may be the presence of abstention-buying, such that the mobilizing effects of some handouts are counteracted by the demobilizing effects of others. Although this possibility is not directly addressed in the survey (which only asks for gifts in exchange for votes), this type of surgical demobilization would have to happen in all 11 countries to explain away the effect. In fact, there are very few voters who reported receiving a handout and did not vote (16 per cent, N=245 out of N=3,438 respondents). Of those who did not vote despite being offered a handout, only a handful would seem prime targets for abstention-buying: namely those who were registered to vote and did not vote (N=66). Moreover, when we look at the demographics of those who received a handout but did not vote versus those who did not receive a handout and also did not vote, they are very similar. This is true of age, gender, belonging to the

ethnic majority group, having formal education, employment status, and objective poverty. The only difference lies in the self-reporting of poverty, which we know correlates with receiving a handout in general. In fact, figures in the Appendix show that the subjective poverty variable has little explanatory power on turnout by itself (Figure A2) or when interacted with the offer of electoral handouts (Figure A3).¹⁸ In other words, those who received offers and did not vote are very similar (on average) to those who did not receive offers but also did not vote.

5.3 Do those targeted express greater political support?

Given the limited impact of handout offers on self-reported turnout on average, the next question is, do these offers translate into political support for a specific party? For this analysis, we limit the sample to those elections in which the time between the election and when the survey was conducted is less than two years—as a way to increase both accuracy and the plausibility that a handout distributed in the past actually translated into political support. As explained above, this is due to the way the questionnaire is designed, which asks for handouts distributed in the last election but for political support at the time of the survey: the shorter the time between these two events, the more accurate the responses are likely to be.

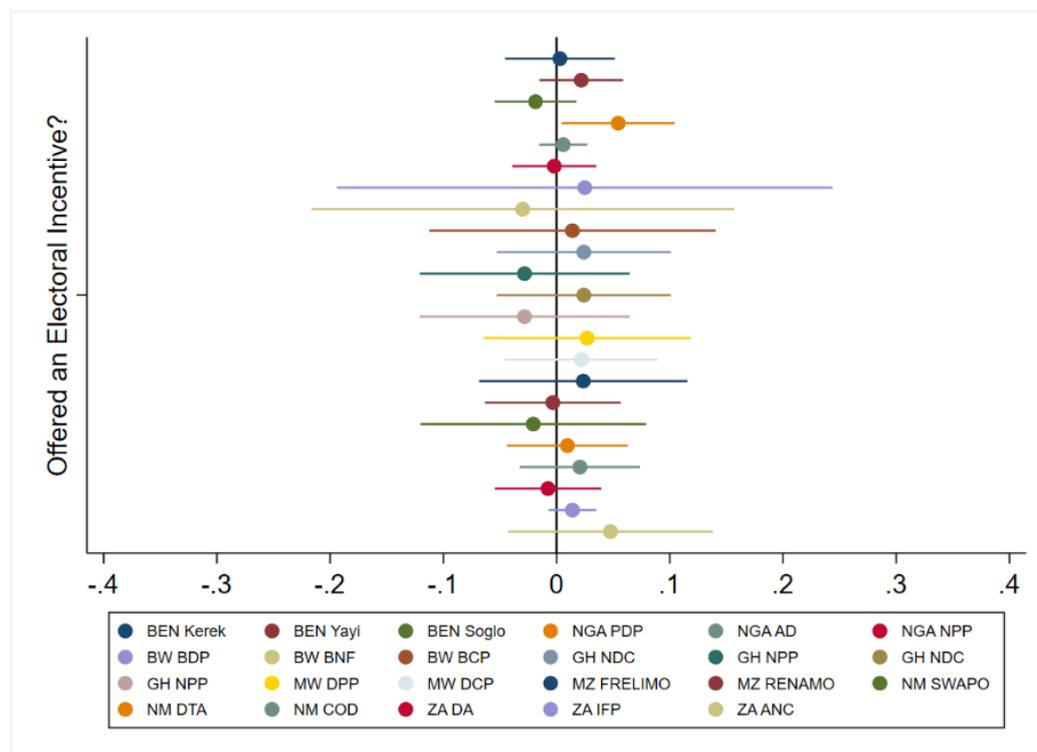
A different difficulty with this analysis is that it is not known *which* is the distributing party. In fact, there could be more than one distributing candidate, and an individual could receive offers from more than one candidate (or from the same candidate several times). Given these possibilities, the analysis examines two measures of political support: first, the extent to which offers of handouts impact support for one of the three major parties (if an election were held today); second, whether handout offers reduce the likelihood of expressing no preference (i.e. supporting ‘no one’ or ‘not knowing’). The reason to focus on the latter is that, if handouts were effective, we would expect them to reduce systematically the number of those who do not explicitly support a party.¹⁹

As shown in Figure 6, those offered a handout do not express, on average, a statistically visible preference for a particular party. The only exception is that for the People’s Democratic Party (PDP) in the 2003 Presidential Nigerian election (fourth coefficient from top to bottom). In this case alone, those who report being offered a handout are significantly more likely to express support for that party at the moment of the survey. This finding is consistent with the those from the turnout analysis, whereby electoral gifts were also associated with higher mobilization in the 2003 Nigerian election.

¹⁸ The only cases in which poverty appears to impact turnout are those where elections cannot be credibly attributed to handouts due to the length of time since the last election (for example, Cape Verde in Figure A3). Even in the case of Namibia in Figure A2, higher subjective poverty appears to lead to greater turnout, thus running in the opposite direction to what the abstention-buying strategy would predict (i.e. poorer individuals are less likely to turn out).

¹⁹ We exclude from this analysis those who ‘refuse’, since they could support a political party but simply decline to express that support.

Figure 6: Handout offers and political support for a party



Note: 95 per cent confidence intervals. Estimates of Equation (2) using OLS. All estimates include district-fixed effects and standard errors clustered at the district level.

Source: authors' construction.

In addition to the above, in Figure A4 we present the results of looking at the intensity of the handout offer, which may capture cases in which individuals are targeted multiple times by different political parties with no significant difference in the results. However, even when looking at the intensity of the handout itself, again only the PDP party in Nigeria appears more likely to garner support if voters are targeted more frequently. Together, these two results suggest that, despite the impact it had on turnout in 6 out of the 17 elections, this higher political mobilization did not necessarily translate into support for a particular political party. In other words, from Figure 6 it is clear that, despite the prevalence of offers, the statistical association with political support is unclear.

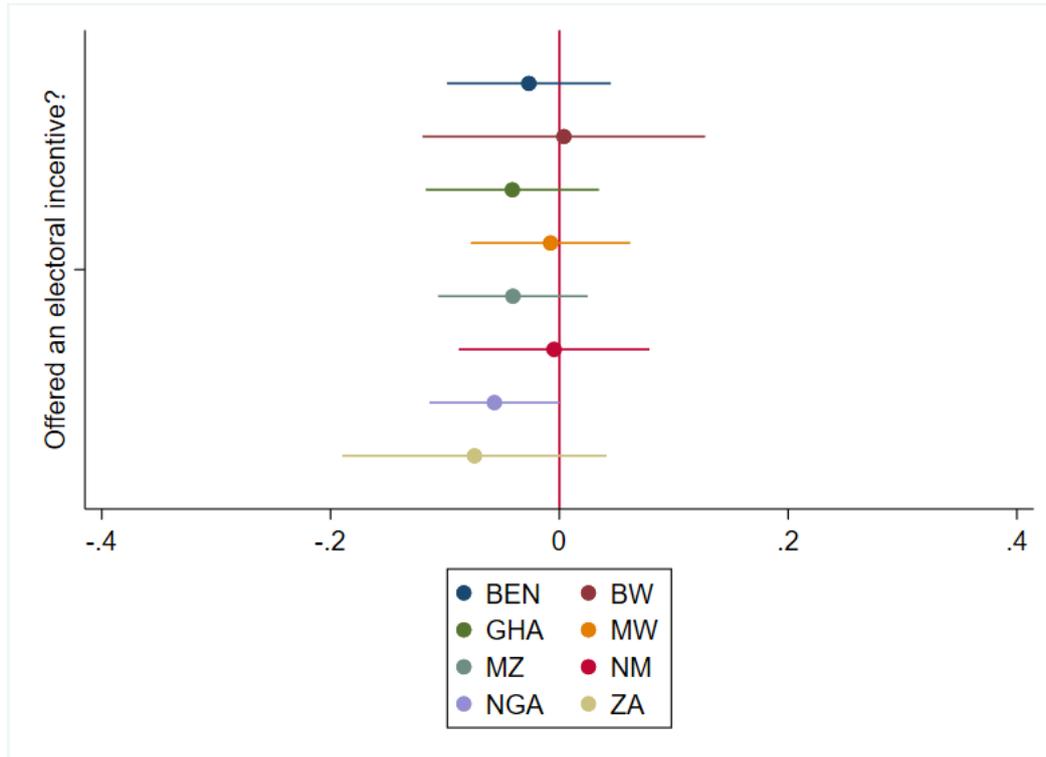
As in the case of turnout, one possible explanation for this lack of association is that handouts are delivered by many parties, such that support for any of them is blunted by the offers by other parties to other voters (or to the same ones). In this scenario, being offered a handout does impact electoral preferences but it does so in many directions, such that on average it appears to be a zero effect. To address this possibility, we focus on the likelihood of declaring any political preference at all. The idea is that if handouts impact political support, on average those targeted with a handout should be more likely to declare a political affiliation as opposed to answering 'no one'.²⁰

As shown in Figure 7, for the sample of countries with <2 years since the last election, those who were offered an electoral incentive were not more likely to declare any type of political support. Again, consistent with the results from Figure 6, only Nigeria has a lower likelihood of *not* having

²⁰ We exclude 'refuse' as an 'undeclared' affiliation category, as the individual could have an affiliation but decline to share it.

a declared preference in the presence of an electoral handout. This further corroborates our finding that, on average, electoral incentives have a small to null effect on outcomes, especially vote choice. Moreover, there is very little heterogeneity in this result across countries with very different settings and elections.

Figure 7: Handout offers and no political support for a party



Note: 95 per cent confidence intervals. Estimates of Equation (2) using OLS. All estimates include district-fixed effects and standard errors clustered at the district level.

Source: authors' construction.

The result also does not vary if we focus on the intensity of offers as opposed to the dichotomous measure employed here (Figure A5). We should remember, though, that these estimates are based on the *typical* respondent and do not preclude specific instances of *quid-pro-quo* exchanges succeeding, as often documented in the media.

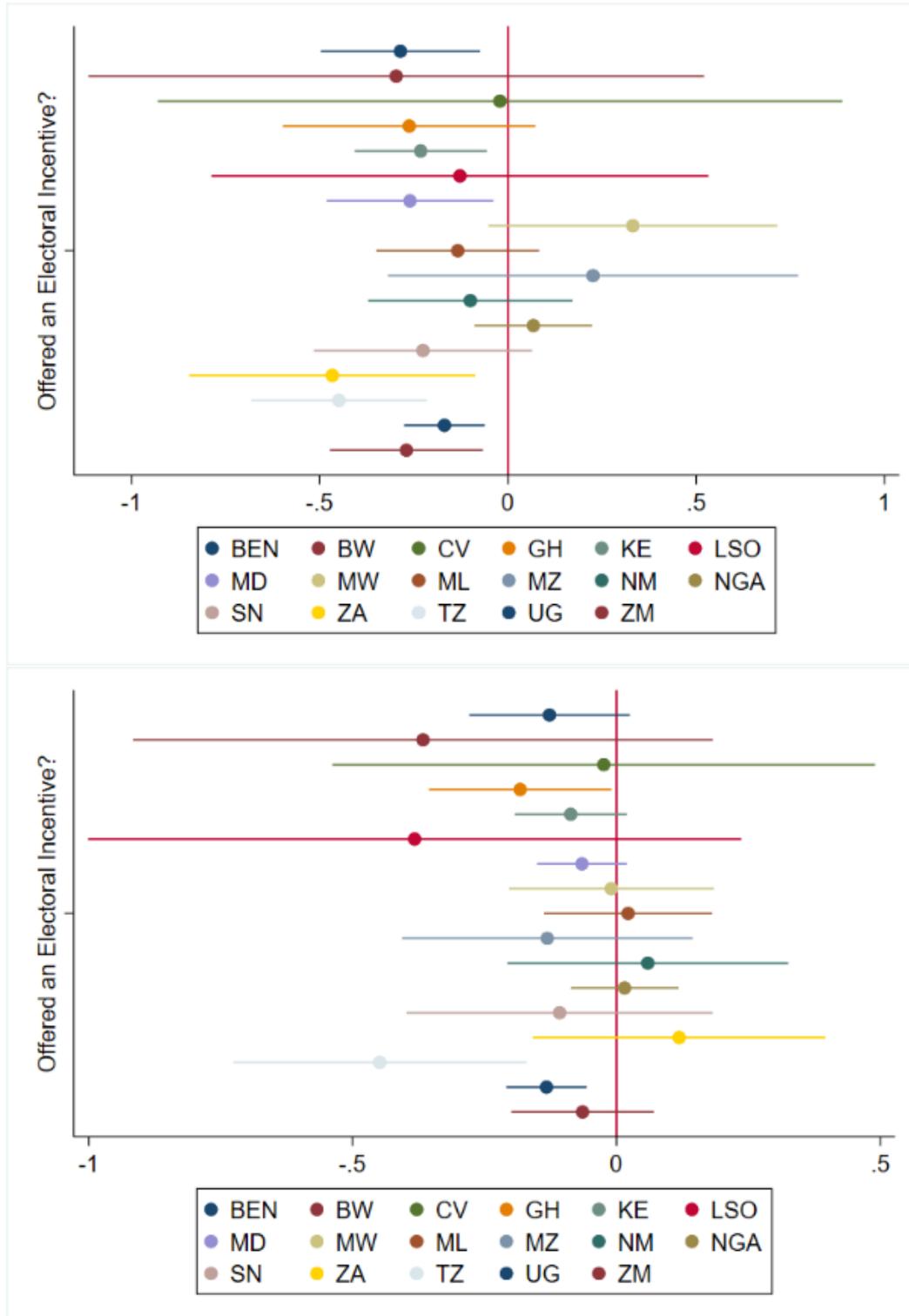
5.1 Robustness and alternative explanations

One concern is whether some bias in our measure of electoral handouts is leading to the null result. Therefore, as a robustness check, we instead use a more indirect measure to gauge potential vote-buying. Namely, we examine whether those who think politicians frequently distribute gifts during campaigns (as opposed to being offered one directly, which is our current measure) are also more likely to turn out to vote. As shown in Figure A6, this shows an even smaller and imprecise relationship with turnout across the 17 elections initially studied, suggesting that the null result is unlikely to be driven by variation in the wording of the questions used.

A different possibility behind the null findings is that the effectiveness of handout offers may be higher for local elections than the national ones considered so far. To examine this possibility, we look at the effect of handout offers on the respondent's perception of their own local leaders vis-à-vis that of their national ones. If handouts play a role at the local level, we would expect the relationship with local leader's performance to be positive. As shown in Figure 8, if anything, there

is a negative correlation between being offered an electoral handout and an individual's evaluation of the performance of national or local leaders, suggesting that, if anything, handouts are associated with less support. This might explain why handouts seem not to be associated with higher political participation or support for a political party.

Figure 8: Handout offers and evaluation of national (top) and local (bottom) leaders: all elections



Note: 95 per cent confidence intervals. Estimates of Equation (2) using OLS. All estimates include district-fixed effects and standard errors clustered at the district level.

Source: authors' construction.

Finally, in terms of other possible explanations for our results, it should be noted that other country and district features that could plausibly impact the effectiveness of electoral handouts are already captured by the district-fixed effects included in all specifications. These features include type of electoral system, country (and district) level of economic development, overall level of democratic quality, and institutional development. In particular, it is possible that certain countries have more (or less) developed party ‘machines’ than others, thus making vote-buying efforts more (or less) likely. However, because all comparisons are within-country, and within-district, these features would be held constant across all of our specifications.

6 Discussion and avenues for future research

Our findings show that the impact of election handouts on turnout and political preferences is small, to the point that it is statistically undetectable in most of the elections studied. These results are not likely to be driven by country- or district-fixed characteristics; similarly, they are unlikely to be driven by ill-targeting by political parties or the cross-pressure from multiple incentives to different groups leading to average effects equal to zero. However, it is possible that individuals receive multiple offers such that they may simply follow their own preferences. For instance, among those offered a handout, 60 per cent were offered handouts ‘a few times’ or more, as opposed to ‘once or twice’, consistent with this possibility.

In the light of these findings, we hope to encourage the vote-buying literature in three directions: first, to look beyond the targeting strategies of politicians and explicitly address their effectiveness in the research design stage and analysis. Lacking a complete treatment of the *quid-pro-quo* transaction—from handout offers to ballot-box behaviour—it is difficult to attribute vote-choices (or mobilization) to handouts and not to a myriad of other possibilities. While our evidence suggests that these types of transactions are generally incomplete in the elections we studied, this may not be the case in other regions of the world or during other periods.

We also hope that our results prompt more discussion as to why—if not to ‘buy’ votes—handouts are useful, if at all. Why do politicians keep engaging in this practice? Are handouts simply an ‘entry fee’ to politics? Or are politicians simply following social norms and expectations peculiar to the contexts in which they belong? One possibility is that politicians face a prisoner’s dilemma-type interaction, where cash handouts are an ineffective but inevitable feature of political competition (Chauchard 2016). Another is that politicians distribute handouts to enhance their credibility and perceived quality (Keefer and Vlaicu 2007; Kramon 2016; Muñoz 2014). Qualitative evidence also suggests that most handouts are not really a *quid pro quo* of gifts for votes but simply a form of compensation—a ‘participation fee’—for voters’ time attending a rally or meeting. While some of these possibilities have been already explored in the literature, our findings provide further motivation to move in this direction.

Finally, given the overall small aggregate impact of these activities in the elections we examined, we should ask ourselves whether the extreme attention to these alleged vote-buying activities is warranted in the face of more distortionary election-related behaviour such as patronage, violence, and intimidation.

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Appendix

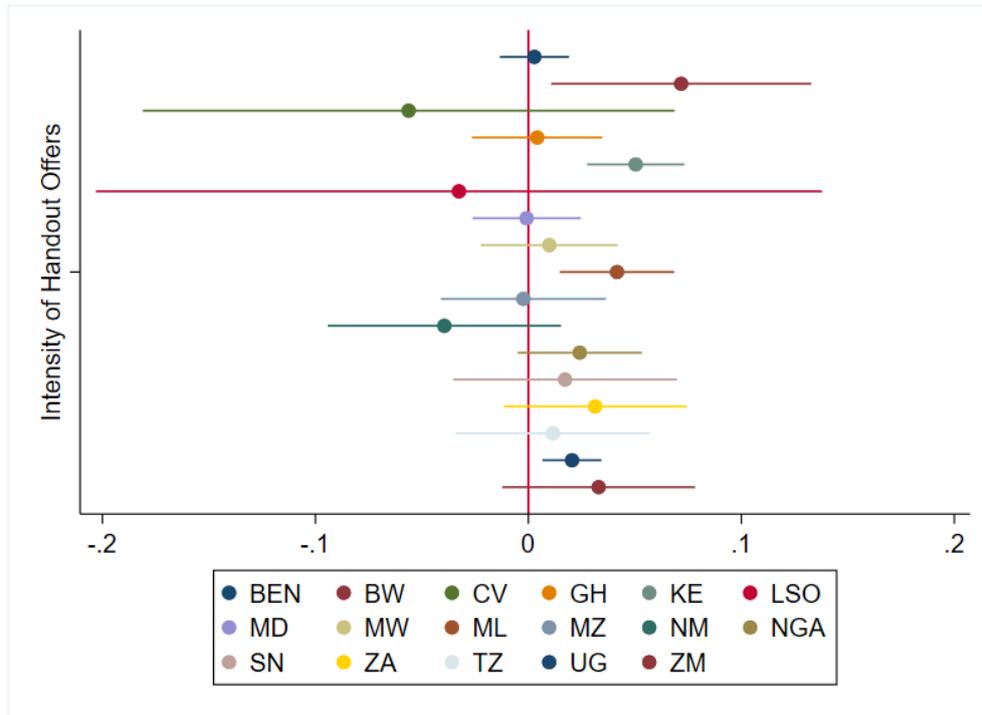
Table A1: Effect of self-reported party identification on handout offers

	(1)	(2)	(3)	(4)
DV: Offered electoral handouts in the last election?				
Benin		Botswana	Ghana	Malawi
Kerekou	-0.016 (0.053)	BDP 0.008 (0.008)	NDC 0.050 (0.034)	DPP 0.011 (0.021)
Houngbedji	0.065 (0.057)	BNF 0.000 (0.008)	NPP 0.030 (0.023)	MCP 0.011 (0.032)
Soglo	-0.128** (0.051)	BCP 0.002 (0.011)	No party 0.092*** (0.020)	UDF -0.017 (0.023)
No party	-0.001 (0.038)	No party 0.010 (0.008)		No party -0.006 (0.015)
	(5)	(6)	(7)	(8)
DV: Offered electoral handouts in the last election?				
Mozambique		Namibia	Nigeria	South Africa
FRELIMO	0.018 (0.023)	SWAPO -0.002 (0.017)	PDP 0.052* (0.027)	ANC 0.019 (0.016)
RENAMO	0.013 (0.056)	DTA -0.002 (0.036)	AD 0.002 (0.062)	DA 0.019 (0.030)
No party	0.041* (0.023)	COD 0.017 (0.043)	ANPP 0.037 (0.035)	IFP 0.015 (0.064)
		No party 0.007 (0.026)	No party 0.069*** (0.022)	No party 0.016 (0.018)

Note: The dependent variable in all specifications is whether an individual was offered an electoral gift in the previous election. Each row per country represents a separate regression. Robust standard errors clustered at the district level in parentheses. All specifications include district-fixed effects and controls for age, gender, belonging to the majority ethnic groups, whether individual received formal schooling, employment status, objective poverty index, and subjective poverty index. *** p<0.01, ** p<0.05, * p<0.1

Source: authors' construction.

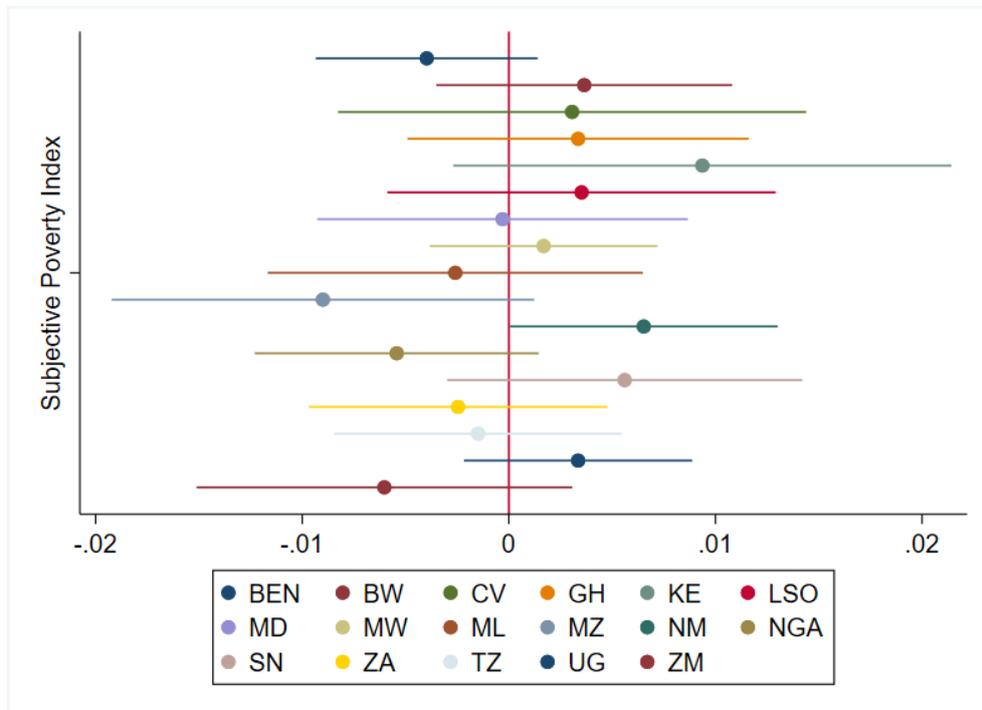
Figure A1: Effect of intensity of handout offers on turnout



Note: 95 per cent confidence intervals. Estimates of equation (1) using OLS. All estimates include district-fixed effects and standard errors clustered at the district level.

Source: authors' construction.

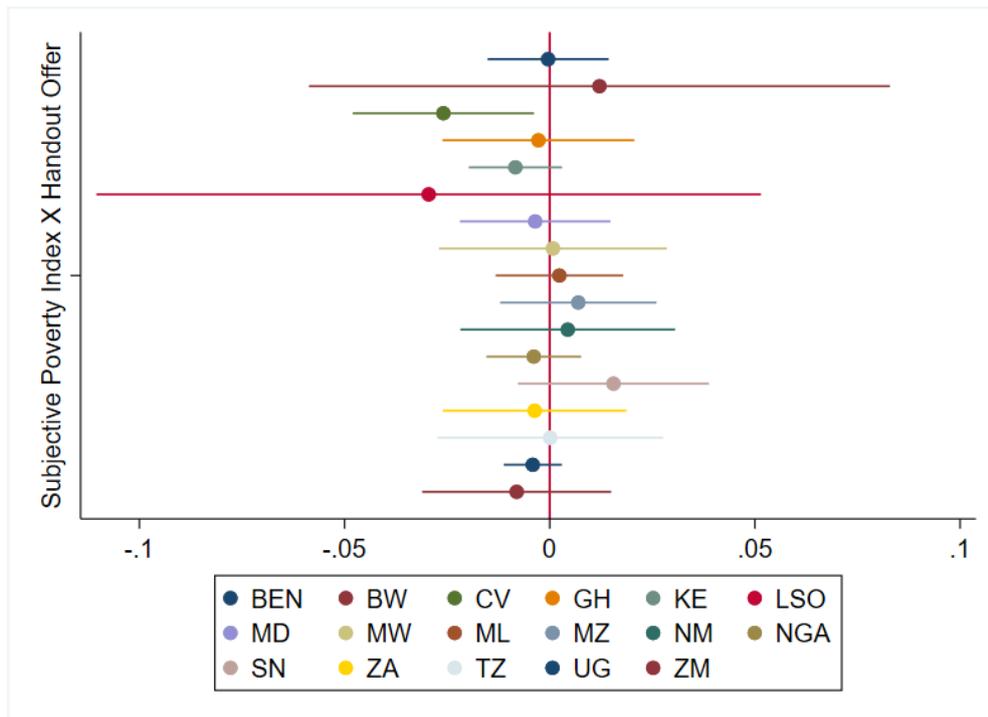
Figure A2: Effect of self-reported poverty index on turnout



Note: 95 per cent confidence intervals. Estimates of Equation (1) using OLS. All estimates include district-fixed effects and standard errors clustered at the district level.

Source: authors' construction.

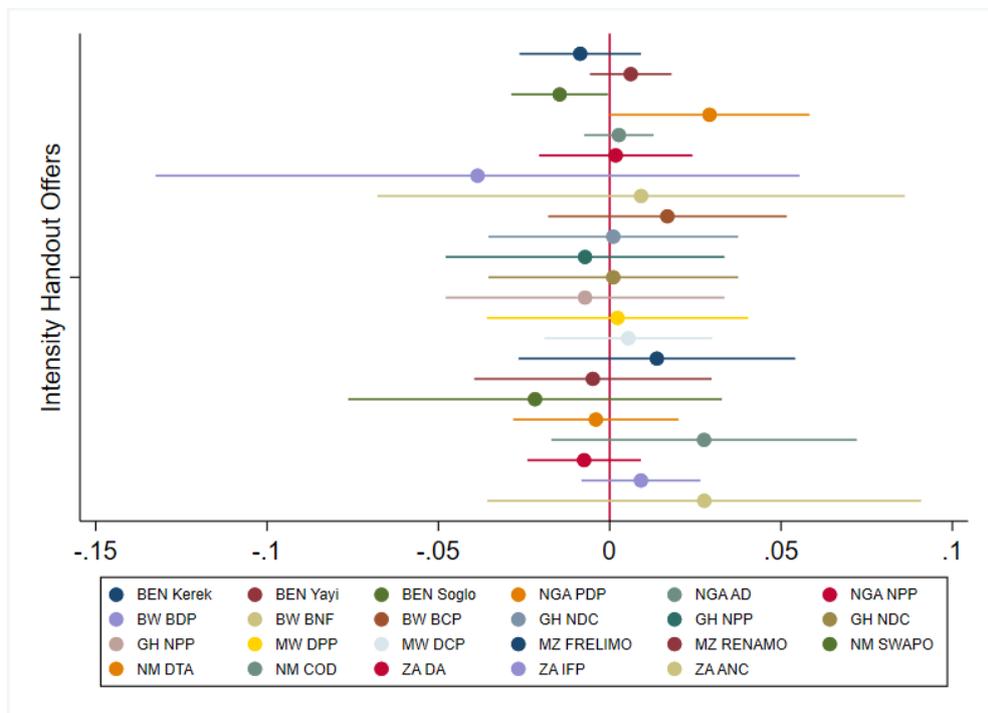
Figure A3: Effect of self-reported poverty index interacted with handout offers on turnout



Note: 95 per cent confidence intervals. Estimates of Equation (1) using OLS. All estimates include district-fixed effects and standard errors clustered at the district level.

Source: authors' construction.

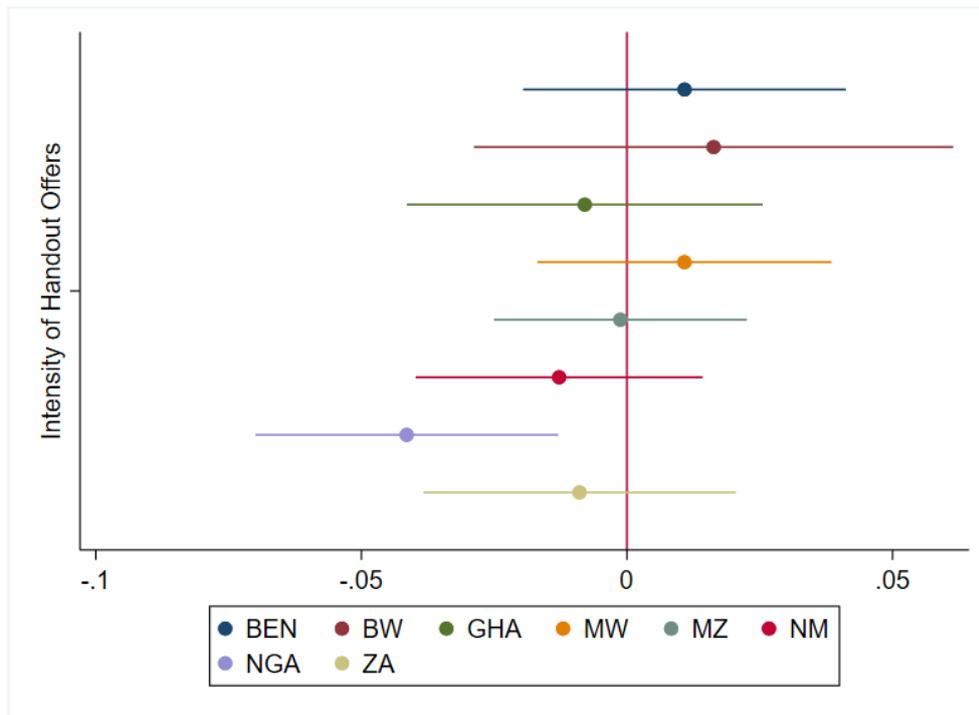
Figure A4: Effect of intensity of handout offers on support for a political party



Note: 95 per cent confidence intervals. Estimates of Equation (2) using OLS. All estimates include district-fixed effects and standard errors clustered at the district level.

Source: authors' construction.

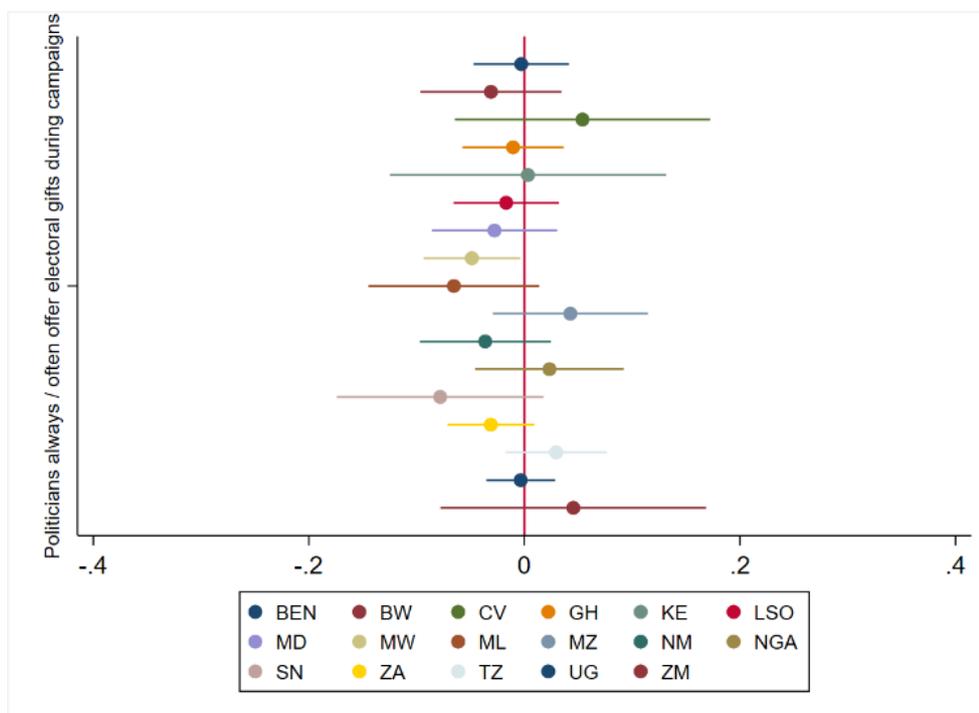
Figure A5: Effect of intensity of handout offers on having a political preference



Note: 95 per cent confidence intervals. Estimates of Equation (2) using OLS. All estimates include district-fixed effects and standard errors clustered at the district level.

Source: authors' construction.

Figure A6: Effect of politicians' gifts on having a political preference



Note: 95 per cent confidence intervals. Estimates of Equation (2) using OLS. All estimates include district-fixed effects and standard errors clustered at the district level.

Source: authors' construction.