

Electoral Competition, Rent Seeking and Opportunism*

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Abstract

Electoral competition may provide countervailing incentives to elected officials: competitive elections may mitigate private rent seeking, but may simultaneously promote opportunistic vote buying. We examine these mechanisms by studying a decentralized public program that selected households eligible for social assistance benefits intended for the poor. While the overall amount of resource misallocation is only weakly related to electoral competition, the *composition* of the misallocation varies: as electoral competition increases, officials' private rent seeking (i.e., allocating benefits to non-poor family members) decreases relative to vote buying (i.e., allocating benefits to non-poor, non-family members). Apart from being related to the official, other household characteristics associated with receiving benefits are unaffected by electoral competition, suggesting that favoritism works mainly through family ties.

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1 Introduction

Elections are often utilized as a mechanism by which constituents can hold policymakers accountable. Electoral competition, however, may provide countervailing incentives to elected officials: competitive elections may mitigate private rent seeking, but may also promote opportunistic vote buying. While both channels have been addressed in the literature, empirical research has tended to analyze these incentives in isolation and, to our knowledge, has not addressed the inherent trade-off between these activities.

This paper examines how policymakers adjust their private rent seeking and vote buying activities in response to electoral competition. The analysis utilizes survey data that contain information about a decentralized program in a developing country that selected households eligible for social assistance benefits intended for the poor. The data contain detailed household-level information about program beneficiaries and non-beneficiaries, allowing us to identify the types of households that were inappropriately included and excluded from the program. These data are then linked to census and election data, which are used to examine the trade-offs incumbents face.

Over the last several decades, many countries have decentralized government programs based on the belief that local officials are more accountable to constituents and have better information regarding the needs and preferences of the local population.¹ Some have argued, however, that in developing countries decentralized policies are particularly prone to capture by local elites,² a theory that has been supported by a growing literature that documents evidence consistent with such capture.³ A key question, therefore, is how local elections influence local government policy decisions in low income settings.

Within the theoretical political economy literature, research such as Banks and Sundaram (1993) has examined conditions under which incumbents' rent extraction is mitigated because of reelection motives and benefits of retaining power. Empirically, Ferraz and Finan (2011) use an objective measure of corruption to test the theory that electoral incentives discipline politicians' rent seeking behavior. The authors exploit randomized audits of local Brazilian governments to construct direct measures of appropriation and corruption. They find a strong causal impact between mayors' reelection incentives and the extent to which these mayors engage in corrupt activities.

On the other hand, another strand of the political economy literature, such as the probabilistic voting models of Lindbeck and Weibull (1987) and Dixit and Londregan (1996), examines political parties' incentives to target (or redistribute) benefits to swing voters

¹See Oates (1999), and references therein, for a review of the "fiscal federalism" literature, which examines the relative costs and benefits of decentralizing certain types of government functions. Empirically, Alderman (2002) provides evidence that local officials and villagers possess local information that is helpful for identifying households that are poor.

²See, for instance, Bardhan and Mookherjee (2005) and Drèze and Sen (1989).

³See, among others, Reinikka and Svensson (2004), Galasso and Ravallion (2005), Olken (2007) and Araujo et al. (2008).

to increase vote shares in an election. Empirically, Schady (2000) and Camacho and Conover (2011) provide evidence consistent with the theory that politicians target public resources to potential swing voters ahead of elections in Peru and Colombia, respectively.⁴

These two strands of literatures suggest that while electoral competition may reduce rent extraction, it may at the same time foster opportunistic “vote buying behavior,” such that the net impact on overall resources misallocation remains a priori ambiguous. Our analysis merges these literatures by looking at how electoral competition jointly affects rent extraction and vote buying behaviors. To do so, we examine the way in which politicians distribute benefits across constituents as the degree of electoral competition changes.

The paper draws from survey data from rural villages in a developing country that contain information about an anti-poverty targeting program. The program establishes procedures for selecting poor households that qualify for a poverty identification card, which entitles cardholders to social assistance benefits. These procedures are implemented within each village and call for an important role of the local officials, such as the village Chief, with the goal of taking advantage of local knowledge of which households are poor. The program was designed so that the number of identified beneficiaries approximately equals the overall poverty rates computed by the central government, in an effort to respect capacity constraints inherent to social assistance programs.

Within each village, a committee of local implementers, appointed by the Chief, interviews households in the village using a short questionnaire. This questionnaire, a Proxy Means Test (PMT), gathers information about household livelihood and contains an objective decision rule regarding whether or not a household should be selected as a beneficiary. Village authorities, however, have some discretion to adjust the list of beneficiaries at the margin upon consultation with the villagers, in order to utilize local knowledge of household circumstances.⁵

As may be expected, these procedures lead to the selection of households that reflect more villagers’ perceptions of who are poor, rather than poverty status based upon a consumption indicator, and may lead toward higher satisfaction among beneficiaries (Alatas et al., 2010). In fact, overall targeting accuracy of the program was relatively good, compared to similar programs in other countries. At the same time, however, allowing for discretion of beneficiary selection may open the door for the political leader of the village, the Chief, to influence the selection process and strategically include or exclude households. We study this possibility by looking, *ex post*, at the characteristics of households that were selected.

⁴Other papers, such as Drazen and Eslava (2010) and Kneebone and McKenzie (2001), provide evidence that the composition of government spending is modified directly ahead of elections.

⁵A companion paper examines the selection and performance of the appointed local implementers, and provides evidence that the skill level of these implementers matters for determining targeting accuracy.

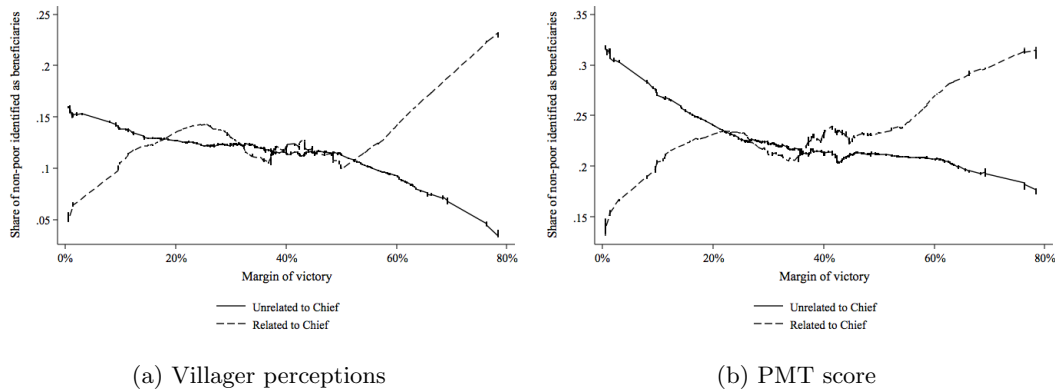


Figure 1: Share of non-poor households selected as beneficiaries, by incumbent margin of victory

Our data include several metrics of household livelihood that include per capita daily consumption, villagers’ poverty perceptions and the objective PMT score, as well as information regarding households’ family ties to the Chief. We draw upon this information to assess the extent to which non-poor relatives of the Chief were included in the list of beneficiaries (a measure we interpret as rent-seeking), and the extent to which non-poor, unrelated households were included (a measure we interpret as opportunistic vote buying behavior).

With the targeting information in hand, we then link these data to information regarding the degree of electoral competition in local elections. Periodically, a local Council that represents several villages is elected via proportional voting. Across the localities in our data, there is effectively two party competition for these Council seats, and the party that wins majority control of the Council has the ability to appoint the Chiefs in all villages under the Council’s jurisdiction. Within the country, it is widely understood that the appointed Chief is typically a strong party representative in the village. Anecdotal evidence suggests that some Chiefs act as an “agent” for the “principal” incumbent party within the village.

As we show below, electoral competition does not seem to influence overall exclusion of poor households and inclusion of non-poor households. In fact, if anything, the share of non-poor beneficiaries is increasing in electoral competition, which is consistent with the evidence presented by Camacho and Conover (2011) for Colombia. An interesting pattern emerges, however, between electoral competition and the *type* of non-poor households identified as beneficiaries. Figure 1 shows the share and type of non-poor households that were identified as beneficiaries, by the incumbent party’s margin of victory in the previous Council election.⁶ Figure 1a identifies households as non-poor using villagers’ subjective

⁶Margin of victory is computed as [vote share of winning party] – [vote share of runner-up party]. Thus a margin of victory of 10 percent corresponds to the winner receiving 10 percentage points more

livelihood perceptions, whereas Figure 1b utilizes the targeting program’s objective PMT score to classify livelihood status. The dashed (solid) lines depict the probability that a non-poor household related (unrelated) to the Chief was selected as a beneficiary. Figure 1a indicates that in villages where the previous Council election was highly competitive (i.e., a margin of victory close to 0), about 5 percent of non-poor households *related to the Chief* were identified as beneficiaries, whereas about 16 percent of non-poor households *unrelated to the Chief* were included as beneficiaries. As the incumbent’s margin of victory grows to the point where all seats on the Council were allocated to the incumbent’s party, however, around 25 percent of non-poor households related to the Chief were included as beneficiaries, while the proportion of unrelated, non-poor households that were included falls below 5 percent.

This pattern suggests that as electoral competition increases, the Chief trades off rent seeking (i.e., allocating benefits to his or her non-poor relatives) for allocating resources to potential swing voters (i.e., non-poor households that are unrelated to the Chief), while keeping the overall level of resource misallocation constant. To be sure, the pattern in Figure 1 is only suggestive; in particular, the analysis does not account for household-level characteristics that might have caused a “borderline non-poor” household to be accidentally included as a beneficiary. Moreover, it does not account for possible omitted variables that could drive the observed pattern.

To examine whether the pattern that emerges in Figure 1 is robust, we utilize a number of econometric specifications to control for other covariates and the potential endogeneity of electoral competition. We employ four separate specifications, including instrumental variables, (village-specific) fixed-effects and fixed-effects-instrumental variables techniques. We also leverage two metrics for classifying a household as poor or non-poor, and also test whether other household characteristics associated with being selected as a beneficiary, in addition to familial ties to the Chief, are affected by the degree of electoral competition.

Our paper is related to several recent studies that analyze the impact of electoral competition at the sub-national level on reform incentives and program manipulation.⁷ Bardhan and Mookherjee (2010) study the impact of electoral competition on land reforms in West Bengal. They find strong evidence of opportunistic behavior by leftist local governments who, in spite their supposed ideology, enact land reforms only if pressured by tight electoral results.⁸ Camacho and Conover (2011) document manipulation of Colom-

votes than the runner-up. The plots in Figure 1 were generated using the `lowess` command in Stata.

⁷Another related paper, Niehaus et al. (2011), examines the problem of designing a PMT when the PMT implementers have incentives to manipulate beneficiary selection, and the monitoring of PMT implementers is imperfect. The authors test their theoretical predictions using data from India.

⁸Bardhan and Mookherjee imbed both an electoral (i.e., “Downsian”) motive, a moral hazard (e.g., “rent seeking”) motive and an ideological motive within their theoretical model. In their empirical analysis, the authors test whether political parties act in a manner consistent with ideology (i.e., adhere to the stated platform) or whether their actions are more consistent with a Downsian (or “quasi-Downsian”) framework (i.e., parties are primarily motivated by reelection and rents).

bia’s targeting system (SISBEN) by local authorities, and find that the overall number of households identified as beneficiaries is significantly higher in municipalities where mayoral elections are more competitive. Unlike Bardhan and Mookherjee (2010) and Camacho and Conover (2011), however, we are able to construct a direct measure of rent seeking and political opportunism by leveraging household-level data. This allows us to jointly study rent seeking and opportunistic incentives under a unified framework. Moreover, we go beyond the municipal level of analysis studied in these papers, and document household-level characteristics that reinforce or mitigate the capture effect under electoral competition.

The paper is organized as follows. Section 2 presents a simple theoretical model linking electoral competition to both rent seeking and opportunistic vote buying behavior. Section 3 describes program context and data. Section 4 presents our econometric strategy. Section 5 discusses the results, and Section 6 concludes.

2 Theoretical framework

The purpose of discussing a simple theoretical framework is twofold: first, this framework structures our empirical analysis; and second, the framework illustrates the institutional setting that we study.⁹ We consider a two period model ($t = 1, 2$), and three types of players: political parties (of which there are two), local village Chiefs and voters. For simplicity, we assume there is no discounting; period 2 is simply included so that voters have an incentive to vote rationally.

The two political parties share control of a Council that governs the constituency. The parties’ share of Council power is based upon a proportional voting rule, in that the number of seats held is equal to the share of votes received from the electorate. At the beginning of the game, one party (the “incumbent”) is exogenously endowed with majority control of the Council. The other party will be referred to as the “opposition,” but we will primarily focus our attention on the incumbent. The incumbent’s utility is strictly increasing in its vote share: the incumbent receives payoff $P(S)$ from receiving a vote share $S \in [0, 1]$. We assume that, over the feasible values of vote shares, $P'(\cdot) > 0$ and $P''(\cdot) < 0$: greater vote share implies more seats on the Council, more “spoils from office” and more opportunities to exploit power for private gain. The marginal benefit from seats, however, is diminishing: an additional Council seat when all other seats are controlled holds relatively little added value.

The constituency is comprised of an infinite number of villages of measure 1. To be sure, in reality a Council governs around nine villages, but this assumption is made for technical ease. Each village is governed by a Chief that is appointed by the party that

⁹Some features of the theoretical model also appear in the frameworks of Ferraz and Finan (2011) and Besley (2006).

controls the Council. For each party, the identities of its Chiefs are fixed within the villages: if the incumbent party receives sufficient votes to maintain majority control of the Council at $t = 2$, then the same Chief will hold power in village v at $t = 1, 2$. This modeling approach corresponds to anecdotal evidence that the Chief is typically the strong party representative in the village.

Each period, the Chief of village v has an “excess” amount of resources from a program that can be allocated to village constituents. Within the context of the targeting program, we view these excess benefits as additional resources that can be allocated to non-poor constituents, in addition to the intended poor constituent beneficiaries.¹⁰ To distribute these excess resources, the Chief of village v can take one of two actions: either allocate the excess benefits to his or her relatives ($g_{vt} = 0$), or allocate these benefits to potential swing voters ($g_{vt} = 1$).

Chiefs are one of two types j : a *rent seeking* type r , or a *party loyalist* type l . The Chief’s type is privately known to the Chief. The *ex ante* probability that a Chief (either incumbent or opposition) is type $j = l$ is π . Rent seeking Chiefs receive a payoff of U from allocating the excess benefits to relatives, whereas loyalist Chiefs derive utility from allocating the excess benefits to swing voters. The incumbent party, however, can offer the Chief of village v a costly transfer payment of $T_v \geq 0$ in exchange for the Chief allocating resources to swing voters. These transfers can be interpreted as favors such as promises to share in the Council’s “spoils of office” that give the Chief some utility. In equilibrium, to provide incentives to rent seeking Chiefs to influence swing voters to vote for the incumbent, the party will have to offer the same utility to the Chief as allocating excess benefits to relatives, such that $T_v = U$.

Within each village, there is a measure 1 of voters, of which λ are potential (non-poor) swing voters. These voters care only about their expected utility from being allocated the excess benefits. In each period, these voters receive a payoff of $u(g_{vt})$, where $\bar{u} = u(1) > u(0) = 0$.¹¹ The remaining $(1 - \lambda)$ voters in the village, on the other hand, are not influenced by (g_{v1}, g_{v2}) ; these voters are thought of as partisans. A fraction $s + \epsilon$ of these voters turn out to vote for the incumbent, where s is a publicly observed signal of how many of the partisans will vote for the incumbent in the village (e.g., the signal could be voters’ historical preferences for the party, observed from previous electoral outcomes) and $\epsilon \sim F$ is a noise term that is only observed after the elections occur. Consequently, if swing voters in a fraction \hat{x} of the villages vote for the incumbent party, then the

¹⁰As we discuss below, the targeting program does a relatively good job at selecting poor households, suggesting that the Chief and other villagers internalize the benefits the program will bestow upon the impoverished. In practice, however, additional (technically non-eligible) beneficiaries are also selected by the local implementers. Given that program monitoring is only weakly enforced, the Chief and incumbent party have the ability to allocate some additional “excess” benefits to the non-poor.

¹¹We abstract from the fact that the number of non-poor swing voters λ may exceed the budget of excess benefits. This can be rationalized by assuming that the Chief randomly allocates benefits across the swing voters if $g_{vt} = 1$. Thus, $u(1)$ would incorporate a voter’s expected utility from receiving benefits, conditional on $g_{vt} = 1$.

incumbent party's expected payoff from the election is $\int P(s + \hat{x}\lambda + \epsilon)dF(\epsilon)$.

The timing of the game is as follows: in period 1, all Chiefs privately observe their types. The incumbent party can then privately offer a non-observable contingent transfer payment to some or all of the incumbent Chiefs to provide incentives to these Chiefs to select $g_{v1} = 1$. After observing this private offer, each Chief then selects $g_{v1} \in \{0, 1\}$. Voters observe the policy, vote for the incumbent or opposition party and period 1 payoffs are realized. In period 2, the winning party's Chiefs make the allocation decision $g_{v2} \in \{0, 1\}$, and then voters' period 2 payoffs are then realized.

We solve for the Bayesian Nash Equilibrium via backward induction. In period 2, each Chief will select the policy that maximizes his or her utility, so that $g_{v2}(r) = 0$ and $g_{v2}(l) = 1$. Therefore in period 1, swing voters will vote for the party whose Chief is most likely to be a party loyalist type. Voters can condition their beliefs about the incumbent Chief's type, j_v , by observing the Chief's policy decision g_{v1} . Swing voters will thus vote for the incumbent if and only if $E[j_v = l | g_{v1}] \bar{u} \geq \pi \bar{u}$.

In the first period, party loyalist types will always want to select $g_{v1} = 1$, since this decision maximizes their utility. On the other hand, rent seeking type Chiefs can attempt to hide their type from voters and select $g_{v1} = 1$ to pool with the party loyalist types. Given that there are a continuum of villages, voting in any one village does not influence the overall Council election outcome, and thus an individual Chief does not internalize the influence that voting in his village has on the incumbent party's reelection prospects.¹² The incumbent party, however, can provide incentives to the Chief of village v to select $g_{v1} = 1$ by offering this Chief a transfer $T_v = U$ in exchange for this action. Since these transfers are costly, the incumbent party will balance the marginal cost and benefit of influencing the Chiefs. Let $x \in [0, 1]$ denote the measure of villages for which the incumbent party offers the deal of $T_v = U$ to the Chief. If the voters expect that the incumbent offers the incentive to measure x of the Chiefs, then voters in village v will have the expectation that:¹³

$$prob[j_v = l] = \begin{cases} 0 & \text{if } g_{v1} = 0 \\ \frac{\pi}{\pi + (1 - \pi)x} & \text{if } g_{v1} = 1 \end{cases} .$$

¹²An alternative model would allow the Chief to internalize some of the benefits that are accrued to his or her party from winning the election. Results of such a model would introduce the same trade-off we discuss below.

¹³Note that

$$\begin{aligned} prob[j_v = l | g_{v1} = 1] &= \frac{prob[j_v = l \wedge g_{v1} = 1]}{prob[g_{v1} = 1]} \\ &= \frac{prob[j_v = l] \cdot prob[g_{v1} = 1 | j_v = l]}{prob[g_{v1} = 1]} \\ &= \frac{\pi}{\pi + (1 - \pi)x} . \end{aligned}$$

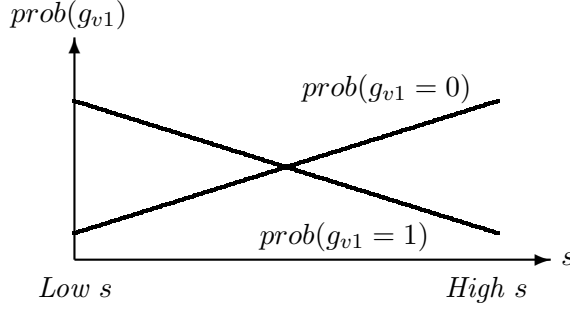


Figure 2: Benefits extracted by Chiefs ($g_{v1} = 0$) and allocated to swing voters ($g_{v1} = 1$)

Observe that, for any $x \in [0, 1]$, swing voters will vote for the incumbent party if $g_{v1} = 1$ because $\text{prob}[j_{v1} = l \mid g_{v1} = 1] \geq \pi$. Therefore the incumbent party's problem is

$$\max_{x \in [0,1]} \int P\left(s + \lambda[\pi + (1 - \pi)x] + \epsilon\right) dF(\epsilon) - xU$$

where again x denotes the share of villages for which the incumbent party incentivizes the Chief to select $g_{v1} = 1$, and $\pi + (1 - \pi)x$ denotes the measure of villages for which $g_{v1} = 1$. Assuming an interior solution, it follows that

$$\lambda(1 - \pi) \int P'\left(s + \lambda[\pi + (1 - \pi)x^*] + \epsilon\right) dF(\epsilon) = U,$$

where the left hand side of the equation denotes the party's marginal benefit from increasing the vote share by making the transfer offer to x^* share of village Chiefs, while the right hand side is the marginal cost of making this offer. Then from the Implicit Function Theorem, it follows that

$$\begin{aligned} \frac{\partial x^*}{\partial s} &= -\frac{\lambda(1 - \pi) \int P''(\cdot) dF}{[\lambda(1 - \pi)]^2 \int P''(\cdot) dF} \\ &= -\frac{1}{\lambda(1 - \pi)} < 0. \end{aligned}$$

This result states that as the incumbent party observes a higher signal s regarding the number of votes that it will receive in the upcoming election, fewer benefits will be targeted to swing voters (and more benefits will be targeted to the Chiefs' relatives). This follows from the fact that, throughout the constituency, the overall share of swing voters allocated benefits in the first period, $\pi + (1 - \pi)x^*$, is decreasing in the expected share of votes the incumbent will receive from the partisan types. Figure 2 depicts this relationship graphically: as the incumbent party expects a higher vote share s , then more Chiefs extract the private benefits ($g_{v1} = 0$) because the incumbent party offers fewer benefits to its agents, because the swing voters' support is less valuable.

3 Program context and data

This section reviews the the data we utilize to perform our empirical analysis, and the setting in which these data were collected. We begin by describing the targeting program in detail, and then proceed to elaborate on the government structure. We then describe our dataset and review the livelihood measures that we construct to partition our sample into poor and non-poor households.

3.1 The targeting program

The targeting program that we study aims at identifying impoverished households that reside in rural villages within the country we consider. Households that are deemed eligible for assistance are allocated an identification card. Across the country, households that received the card (“beneficiaries”) have access to free health care at participating medical facilities, which amounts to free health insurance. Recently, these identification cards have increasingly been used by the development community to provide other assistance, such as food distribution, and for targeting land distribution intended for the poor.

Implementation of the targeting procedures is decentralized and call for an important role of local officials, with the goal of leveraging local knowledge of households’ livelihood conditions. In each village, a committee of around seven villagers (the “local implementers”) is tasked with identifying the households that are poor in the village. In theory, the committee of local implementers is selected in a two-stage process: first, the local Chief proposes a list of candidate implementers, and second, villagers vote for their preferred implementers during a village meeting.¹⁴ In practice, however, voting occurs in only around 60 percent of villages, and when voting does occur, the total number of candidates exceeds the total number of implementers by only one or two in the average village. Therefore, the Chief wields considerable influence over the selection of the local implementers.

In each village, the local implementers determine which households are eligible for assistance in three steps: in a first step, the local implementers interview households in the community by administering a short questionnaire (a Proxy Means Test, or PMT) that was developed centrally by the targeting program. As with similar programs around the world, the PMT questions are intended to gather information such as the quality of the household’s dwelling and asset ownership. For each question, the household is assigned a point value based on the household’s response, and households with a total point score below a certain range are deemed as an eligible beneficiary.

¹⁴In the village meeting, villagers can propose additional candidates. However, this occurred in fewer than 15 percent of villages, according to the Chief.

In a second step, local implementers can use their local knowledge of a household's circumstances (e.g., the household recently suffered a crop failure, or the household head is a widow) to adjust the livelihood category indicated by the PMT. Finally, in a third step, the implementers are supposed to display the draft list of beneficiaries in public, and the implementers and Chief are supposed to organize a village-level meeting to receive feedback regarding the list of beneficiaries. According to villagers, however, this third step in the process is often not implemented: only 21 percent of households reported that a draft list of beneficiaries was publicly displayed (36 percent did not know), and only around 29 percent of households reported that a village-level meeting occurred to present and discuss the list of beneficiaries (37 percent did not know).¹⁵

The decentralized nature of the targeting procedures raises the possibility that local leaders may capture the process and manipulate which households are selected as beneficiaries. Capture can happen through several channels: local implementers are allocated some freedom to subjectively modify the list of beneficiaries; the Chief is involved in overseeing and guiding implementation; and while the national program staff monitor the overall percentages of households that are categorized as poor (by comparing the shares of households selected as beneficiaries with other poverty rates implied by other surveys), there is little-to-no external monitoring of village-level program implementation and whether households selected as beneficiaries are actually poor. Anecdotal evidence and informal accounts of some villagers suggest that nepotism and, in particular, non-poor relatives of the Chief are sometimes included as program beneficiaries.

3.2 Electoral competition and government structure

In the country under study, there are a number of political parties that compete for public office. Two parties, however, have emerged as serious contenders for power in the states where the data were collected. We refer to these parties as the Left party (LP) and Right party (RP). The LP is the stronger party; at the time of the data collection, LP controlled the national and several sub-national government bodies. The LP is a socialist-leaning, populist party. The opposition RP, on the other hand, promotes a capitalist, liberal platform and maintains different views on foreign policy.

The elected government body that is most decentralized is the local Council. This Council contains around seven members (depending upon the size of the constituency) and governs around nine villages. The Council members are elected according to a closed list proportional voting system, implying that constituents vote for their preferred party and

¹⁵As mentioned in the Introduction, targeting procedures may lead to the selection of households that *villagers perceive as poor*, as opposed to households that are *technically below a (consumption- or income-based) poverty line*. In fact, the targeting program's objective was to create a PMT that proxies for villagers' poverty perceptions, instead of benchmarking the PMT to a consumption-based poverty metric. This is noteworthy, because villagers' perceptions of who is poor can differ significantly from households that fall below an objective poverty line, as discussed in Alatas et al. (2010).

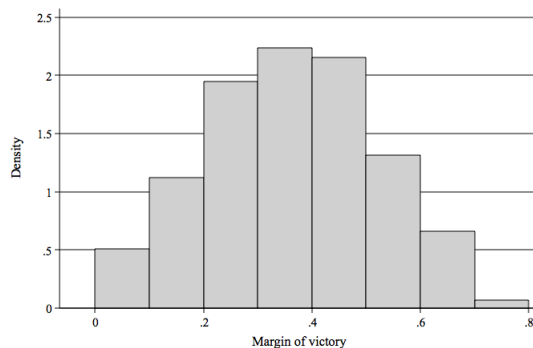


Figure 3: Distribution of margin of victory

seats on the Council are allocated to parties based upon the overall proportion of votes received in the election.

In the most recent Council election prior to the collection of our data (held in 2007), LP and RP were allocated over 93 percent of all Council seats in our randomly selected sample of villages (described below). The LP received around 60 percent of Council votes in the median village, whereas the RP received around 25 percent of votes (with the remaining votes split between several less popular parties). There is substantial heterogeneity, however, in the degree of electoral competition across Council elections, and in fact, the RP received the majority of votes in some jurisdictions. Figure 3 displays the distribution of the margin of victory for the winning party in this election in our sample of jurisdictions.¹⁶ The degree of electoral competition varies considerably: in some jurisdictions, the first-place party won by a margin of less than 1 percent, whereas in other jurisdictions, the margin of victory for LP exceeded 60 percent of votes. We utilize margin of victory as our measure of electoral competition, as opposed to simply the vote share, because the percentage of votes received by the “fringe” of smaller parties varies across Council jurisdictions. This measure allows us to compare the level of competition between the two main parties.

Party control of the Council is important not only for setting jurisdiction-level policies, but also because the Council appoints all local Chiefs under its jurisdiction. As mentioned above, it is generally understood that the party with majority control of the Council typically appoints an influential party representative as the Chief in each village. The Chief has then the opportunity to marshal support for his or her party.

¹⁶Margin of victory is defined as the vote share of the first-place party, minus the vote share of the second-place party. This variable, or a transformation thereof (e.g., $1 - \text{margin of victory}$), has been used in the literature to measure electoral competition: see, for instance, Camacho and Conover (2011) and Besley and Burgess (2002).

3.3 Data description

Our analysis draws from a survey that was fielded within several months of the decentralized implementation of the targeting procedures, but before the program’s identification cards were allocated to the households. Data were collected in 299 randomly selected villages in four states within the country, where the probability of village selection was proportional to the number of households residing in the village.

Within each village, data were collected from three different populations: households, local implementers and the Chief. Ten households were randomly selected for interview in each village.¹⁷ Household selection was stratified such that five of the sample households had been selected as beneficiaries, and five of the sample households had not been selected as beneficiaries. Stratification was conducted using the official list of beneficiaries that was created by the local implementers. In total, 2,943 household were interviewed, of which 1,467 (1,476) were (not) beneficiaries.¹⁸ The Chief in each village and local implementers that administered the targeting procedures were also interviewed.¹⁹

Overall, three different questionnaires were designed to collect various types of information. The household questionnaire was by far the most detailed of the three survey instruments. Much of this questionnaire was derived from the government’s socio-economic survey household questionnaire, including the consumption modules. The questionnaire also includes a replica of the targeting program’s PMT, and questions regarding whether or not the local village implementers and Chief followed the procedures indicated by the program.

In the spirit of Alatas et al. (2010), the household questionnaire also included two modules that gathered information from each respondent household regarding the other sample households that were interviewed in the village. The first module elicited respondents’ perceptions regarding the livelihood status of the other sample households in the village. The respondent was shown a card with the name of the household head written on it and asked,

Households in your village can be classified into one of four livelihood levels:
very poor, poor, adequate or rich. In your opinion, is (*name of household*

¹⁷In villages with 250 or more households, the village was partitioned into smaller segments. One village segment was then selected to be the area of interview, again with the probability proportional to the number of households residing in the segment. In one large village, two sets of ten households were selected for interview.

¹⁸In each village, four replacement households (two beneficiaries, and two non-beneficiaries) were selected, in the event that a household was unavailable for interview. In 74 percent of these cases where a household member was not available, the reason was because household members were at work or away from the village. Sampling weights are adjusted to reflect the actual number of beneficiaries and non-beneficiaries in each village, and are also adjusted for non-response.

¹⁹In one (three) [one] village, data were not collected for the local implementers (Chief) [both implementers and Chief] because respondents were unavailable. Data were collected from over 80 percent of local implementers. In 93 percent of villages where a leader was interviewed, the actual Chief was interviewed. If the Chief was unavailable, then the Deputy Chief or other official local leader was interviewed.

head)’s household very poor, poor, adequate or rich?

This module of the questionnaire was extensively piloted and strategically placed towards the beginning of the questionnaire, before any mention of the targeting program, to mitigate response bias.

The second module elicited information regarding whether the other sample households in the village are related to either the Chief, or any of the local implementers. Respondents were asked about other sample households in order to mitigate the possibility of a household misreporting its own relationship status. Each respondent household in the sample was asked if each of the other sample households was related to either the Chief, or any of the local implementers. The answers provided by the respondent households in the village were aggregated to identify the relationship statuses of each sample household in the village.²⁰ Below, this variable is utilized to gauge the extent to which non-poor relatives of the Chief (and local implementers) are identified as beneficiaries (a measure we will link to rent-seeking), and the extent to which non-poor, unrelated households are identified as beneficiaries (a measure we will link to opportunistic, vote buying behavior).

The local implementer and Chief questionnaires were designed to elicit information regarding characteristics of these individuals. These survey instruments gathered information regarding the individual’s demographic background and years of completed schooling, and also included a short nonverbal intelligence test (a shortened version of the Raven’s Progressive Matrices). The Chief questionnaire also included questions regarding general characteristics about the village.

3.4 Livelihood measures and summary statistics

The subsequent analysis requires that we classify each sample household as either poor or non-poor, in order to determine whether a household was inappropriately included or excluded as a beneficiary. To this end, two distinct methods are utilized to partition the households. One of these methods utilizes the targeting program’s formal PMT, which was re-administered by the professional enumerators that collected the data utilized in this paper. This benchmark is objective in nature and follows the targeting program’s devised method for classifying households as poor.

The targeting program’s intended “poverty benchmark,” however, was villagers’ perceptions of whether or not a household is poor (see Footnote 15). While the program’s

²⁰Respondents were informed that familial relations included siblings, parents and children; grandparents and grandchildren; and aunts/uncles, nieces/nephews and cousins. A household-level indicator variable for being related to the Chief was created; this variable equals 1 if either (i) a member of the household is the Chief; (ii) the household respondent identified itself as relative to the Chief; or (iii) two or more other respondents in the village identified the household as related to the Chief (and 0 otherwise). An analogous variable was created with regards to the local implementers. This definition of familial relationships is similar to the elite connectedness variable in Alatas et al. (2010).

PMT was designed to proxy for such perceptions, some local implementers modified the livelihood category indicated by the PMT. Therefore, we also classify households as poor or non-poor using villagers' poverty perceptions. To do so, we draw from the poverty perceptions module of the household survey and identify a sample household as poor if the household was classified as poor by half or more of the other respondent households in the village that classified the sample household.²¹

As may be expected, there are differences between the perceptions-based livelihood categorization and the PMT. According to the targeting program's PMT, 17.9 percent of the households are eligible beneficiaries (i.e., 17.9 percent of the households received a PMT score below the targeting program's poor threshold), whereas villagers perceive 36.6 percent of the households as poor. The large difference in the overall poverty rate is driven by the poverty threshold: only 15.9 percent of all "PMT poor" households are perceived as not-poor, and only 4.8 percent of all households perceived as poor are classified as "PMT non-poor." This indicates that households identified as poor by the PMT are, in general, also perceived as poor by villagers; villagers, however, perceive that significantly more households are poor, relative to the PMT. Given these differences and that in practice, local implementers were allowed to use local knowledge about poverty perceptions to include additional households, we take the poverty perceptions as the main "poverty benchmark" when classifying households as poor or non-poor. As a robustness exercise, we then estimate our econometric models by partitioning households using the PMT livelihood categorization.

Table 1 presents summary statistics for beneficiary (31.1 percent of the population) and non-beneficiary households (68.9 percent of the population). The first set of variables are explicit livelihood measures, such as the household's PMT score. The second set of variables are additional household characteristics that implementers may have (legitimately) considered when determining whether to identify the household as a beneficiary. Finally, the third set of variables capture the degree of social connectedness of the household.

While some types of variables, such as the livelihood measures, differ for the two groups, other variables, such as those listed under the social connectedness heading, are quite similar. The heads of non-beneficiary households are more educated (3.7 years of schooling versus 2.7 years), but educational attainments remain overall fairly low for both groups. There are more female-headed households among beneficiaries, while ethnic minorities have equal presence in both groups. Non-beneficiary households are also more likely to have suffered from crop failure in the previous year, which could be a reflection of the fact that landlessness is more prevalent among beneficiaries.

Overall, households related to the Chief (and related to a local implementer) are evenly

²¹If half of the villager respondents perceived a sample household as poor and the other half as non-poor, then the sample household was classified as poor. If fewer than three respondent households reported a livelihood category for a sample household, then the perceptions variable was coded as "missing."

Table 1: Means of household-level variables

	Beneficiary	Non-beneficiary	Total
Livelihood measures			
Normalized PMT score (0=lowest, 1=highest)	0.40	0.67	0.58
Villagers perceive as non-poor (%)	0.24	0.82	0.63
Per capita daily consumption, USD	1.72	2.40	2.19
Household characteristics			
Number of household members	4.55	4.88	4.78
Years of schooling: Head	2.66	3.69	3.37
Head is female (%)	0.38	0.26	0.29
Ethnic minority (%)	0.05	0.05	0.05
Suffered crop failure: Loss of USD 25+ (%)	0.32	0.46	0.41
Owens 1+ motorcycle (%)	0.21	0.57	0.46
Owens 1+ plough (%)	0.21	0.39	0.34
Owens 1+ tractor (%)	0.02	0.12	0.09
Owens 1+ tv (%)	0.32	0.68	0.57
Social connectedness			
Related to Chief (or is Chief) (%)	0.18	0.18	0.18
Related to implementer (or is implementer) (%)	0.38	0.39	0.39
Number of social organizations	0.11	0.21	0.18
Percent of sample households			
	0.31	0.69	1.00

Notes: Column 1 (Column 2) reports sample means for the 1,467 (1,476) sample households that were selected (not selected) as beneficiaries by the program. Column 3 reports sample means for the entire sample of 2,943 households. Household variables cover all households, except the villager poverty perceptions variable, which is defined for 2,715 households. The normalized PMT score divides the number of points the household received on the PMT test (as implemented during the data collection) by the total number of possible points. The number of social organizations variable considers membership in an artisan, farmer's, religious, savings or women's association. All means are sample weighted.

distributed between beneficiary and non beneficiary households. Interestingly, however, a majority of households related to the Chief (69 percent) are deemed as non-poor according to the villagers’ poverty perceptions (the proportion raises to 86 percent for the PMT). We interpret these benefits allocated to non-poor relatives of the Chief as our measure of rent extraction. Similarly, we interpret the benefits allocated to unrelated, non-poor households as a measure of vote buying behavior. To be sure, some related households may be swing voters. Nevertheless, given that familial ties are quite strong in the country, it remains an unlikely event. More importantly, non-poor, non-relative voters may also contain partisan voters (in addition to swing voters). The vote buying indicator will thus be measured with error, generating a bias of the parameter estimate towards zero.

The second row of the table indicates that 18 percent of non-beneficiary households are perceived as poor (which is similar to other similar programs internationally), whereas 24 percent of beneficiary households are perceived as non-poor. In particular, the analysis shall focus on these non-poor beneficiaries.

Table 1 also reports sample means for estimated per capita household consumption. This livelihood metric follows the methodology that the national government utilizes to construct consumption estimates that are used to analyze socio-economic trends. This measure is essentially constructed by summing daily household consumption across a variety of categories and dividing by the number of household members. The natural log of this measure is included in our analysis to control for the fact that local implementers may have selected households, in part, using information about consumption.

In some econometric specifications, we also make use of village and Council characteristics listed in Table 2. Data on Chief, local implementers and some village-level characteristics are derived from our survey data, whereas the other characteristics are derived from census data. Margin of victory is generated using publicly available voting data.

4 Econometric specification and identification

4.1 Baseline specification

We begin our econometric analysis by relating the beneficiary status of a household to a set of household, village and Council jurisdiction variables. We explicitly allow for a relationship between recipient status and the margin of electoral victory in the Council’s jurisdiction, which can be affected by the extent to which a household is connected to the Chief. Our baseline estimation equation is given by:

$$\begin{aligned}
 B_{ivc} = & X_{ivc}\beta_1 + X_{vc}\beta_2 + X_c\beta_3 \\
 & + \gamma_1 margin_c + \left(\delta_0 + \delta_1 margin_c + \nu_{vc} + \nu_c \right) R_{ivc} + \lambda_{vc} + \lambda_c + \epsilon_{ivc},
 \end{aligned} \tag{1}$$

Table 2: Means of village- and Council jurisdiction-level variables

	Mean	N
Village characteristics		
# households in village	197.60	299
# households in village / # households in jurisdiction	0.12	299
% of households: Own \leq 0.5 ha of land	0.41	299
% of households: Own \geq 2.0 ha of land	0.21	299
Distance to state administrative center (km)	31.40	299
Chief characteristics		
Age	56.83	295
Percentile score on the cognitive test	0.52	295
Years of completed schooling	5.29	295
Number of social organizations	0.69	295
Average local implementer characteristics		
Average age	42.66	297
Average percentile score on the cognitive test	0.60	297
Average years of completed schooling	6.80	297
Average number of social organizations	0.49	297
Council jurisdiction characteristics		
Incumbent party's margin of victory	0.41	231
% of households: Own 1+ mobile phone	0.27	231
% of households: Own 1+ tv	0.53	231
% of adults: Completed primary school	0.31	231
% of adults: Literate in the local language	0.65	231

Notes: Local implementer characteristics are averaged over the implementers that responded to the implementer survey. All means are sample weighted. Variables are derived from the survey data and the most recent census. The number of social organizations variables were constructed as discussed in the notes for Table 1. The sample size for the Council jurisdiction-level variables is smaller than for the village characteristics because in some Council jurisdictions, multiple villages were randomly selected; these variables are defined for all 299 villages in the sample.

where B_{ivc} equals 1 if household i in village v of Council c was selected as a beneficiary (and 0 otherwise); X_{ivc} , X_{vc} and X_c are vectors of household, village, and Council jurisdiction control variables (as listed in Tables 1 and 2); $margin_c$ is the margin of victory of the 2007 elections for the winning political party in Council c ; and R_{ivc} equals 1 if household i is related to the Chief (and 0 otherwise). Observe that we use a linear probability model because we are particularly interested in interpreting interaction effects, and this is difficult to do in a non-linear estimation setting (see, for instance Ai and Norton (2003)).²²

The model allows for various types of error terms. First, it allows for village- and Council-specific effects in the overall error structure, $(\lambda_{vc}, \lambda_c)$. Second, it allows for village- and

²²When we estimate the model using a Probit specification, however, most estimates remain qualitatively unaffected (results are available upon request).

Council-specific effects in the association between relationship to Chief and beneficiary status, (ν_{vc}, ν_c) . And third, it allows for a household-specific idiosyncratic error term, ϵ_{ivc} . In our base-case estimation, we assume that these terms are, conditional on the other variables in the model, uncorrelated with the margin of victory. Below, we discuss possible deviations from this assumption, and how we modify the estimation accordingly. Specifying the model in this way allows us to recover several parameters of particular interest, namely:

(i) *the additional probability δ_0 that relatives of the Chief are selected as beneficiaries when the margin of victory is equal to zero.* For non-poor constituents, it should be expected that $\delta_0 < 0$: when the electoral environment is very competitive, relatives of the Chief should be less likely to be selected as a beneficiary;

(ii) *the impact δ_1 of the margin of victory on the additional probability that relatives of the Chief are selected as beneficiaries.* For non-poor constituents, our theory predicts that $\delta_1 > 0$: as the incumbent’s margin of victory increases, relatives of the Chief should be more likely to be selected as a beneficiary, relative to other non-poor households. Moreover, $\delta_0 + \delta_1 > 0$: when the incumbent expects to retain all Council seats, then the Chief’s relatives should be strictly more likely to be selected; and

(iii) *the impact γ_1 of the margin of victory on the probability that a household unrelated to the Chief is selected as beneficiary.* For non-poor constituents, our theory predicts that $\gamma_1 < 0$: swing voters are less likely to be selected as a beneficiary when the incumbent party is confident of reelection.

Observe that these predicted signs for $(\delta_0, \delta_1, \gamma_1)$ should primarily occur for non-poor households. The total number of beneficiaries selected is a “soft” constraint, and the fact that the targeting program under study performs relatively well in including poor households as beneficiaries suggests that local implementers and the Chief internalized the social value of the program for the poorest households in the village. Moreover, opportunistic vote buying behavior and inappropriate allocation of benefits should primarily occur for non-poor households, that is, households that are “not supposed” to receive benefits. Therefore, we shall investigate the signs of these parameters separately for poor and non-poor households.

4.2 Augmented specifications

The baseline specification (1) assumes that the error terms are uncorrelated with the margin of victory. This assumption may be violated, however, which would lead to inconsistent parameter estimates. We address this problem of identification using instrumental variables and fixed-effects estimators.

4.2.1 Instrumental variables

The first approach accounts for a potential correlation between $(\lambda_{vc}, \lambda_c)$ and $margin_c$, which could come about in the presence of unobserved household preferences in the Council's jurisdiction that are correlated with both the electoral outcome in the local election, and the extent to which program benefits are valued by constituents. In particular, the party that wins the Council election, and the resulting margin of victory, could be related to constituents' preferences over the *local policies* that these parties support. As previously discussed, the two strong parties that compete in the local elections have different platforms: the dominant party (Left party) holds a socialist/populist platform, whereas the weaker opposition party (Right party) promotes a liberal/capitalist platform.

Given the parties' traditional platforms, we could expect that in areas with strong support for LP's platform, which are the majority of areas, there would be a desire for greater local government intervention to extend benefits to more households, i.e. a positive correlation between $(\lambda_{vc}, \lambda_c)$ and B_{ivc} . Given the nature of electoral competition, this would also imply a positive correlation between $(\lambda_{vc}, \lambda_c)$ and $margin_c$, which would bias γ_1 upwards (i.e., towards zero) since the non-poor should be more likely to receive benefits; the bias imparted to δ_1 remains less clear, as we would not expect there to be any "additional" probability of receiving benefits associated with being related to the Chief. Similarly, if the the dominant party in a particular locality is particularly good at delivering benefits to swing voters, we would also expect a positive correlation between $(\lambda_{vc}, \lambda_c)$ and B_{ivc} . Note that in both cases, the bias imparted under these conditions is away from the direction predicted by our theory, suggesting that in the basic model we are likely to underestimate the impacts of electoral competition on benefits.

Nevertheless, we address this potential bias by building on Bardhan and Mookherjee (2010), who examine local elections in India and base their instrumental variables approach on the fact that voters have inherent biases for certain political parties, based for instance on historical traditions, ideology and policy positions that are unrelated to local policy (i.e., national-level foreign policy). Specifically, when analyzing the impact of local elections, Bardhan and Mookherjee use as instruments outcomes of national elections and previous local elections to account for these underlying, unobserved factors. These instruments correlate with national-level policies and the innate preferences of voters, but should be orthogonal to the particular local policies that will affect the upcoming local election. In full similarity, we use two instruments for our primary measure of electoral competition:

(i) *The lagged margin of victory in the 2002 Council elections.* The 2007 Council election cycle that we utilize to construct the margin of victory $margin_c$ represents the first election that followed major decentralization efforts to empower Councils to determine local policies. Prior to this election, the local Councils had very limited power, essentially

no budget and no ability to officially allocate benefits or assistance to constituents. As such, prior Council elections were more symbolic, as the Council was not relevant for local policy. The lagged margin of victory in the Council elections is therefore likely to reflect the heterogeneity of constituents' preferences for national-level parties and policies, as opposed to local policies (which were not yet well established) or outcomes of the local party machine.

(ii) *The margin of victory in the 2008 national elections.* The most recent national elections were also conducted via a closed list, proportional election system. We compute the average vote shares for each party, where the average is taken over the district where Council c is located. Since preferences for political parties systematically vary geographically, district-averaged margin of victory should capture the dispersion of regional voters' preferences for political parties' national-level policies.

Based on these two variables, we define four variables that we use to instrument for the margin of victory $margin_c$ and its interaction $margin_c \cdot R_{ivc}$.²³

- $margin_c^{C-1}$: The lagged margin of victory that occurred in the previous Council election.
- $margin_c^A$: The margin of victory in the most recent national election, averaged at the district-level.
- $R_{ivc} \cdot margin_c^{C-1}$: The interaction between $margin_c^{C-1}$ and R_{ivc} .
- $R_{ivc} \cdot margin_c^A$: The interaction between $margin_c^A$ and R_{ivc} .

Table 3 reports estimates from an OLS regression of the incumbent party's most recent margin of victory in the Council election on the instruments discussed above and Council controls. Estimates indicate that the instruments are significant in predicting the degree of electoral competition in the most recent Council election.

4.2.2 Village-level fixed-effects

The second approach we use to account for potential omitted variables biases is fixed-effects. We estimate a specification of equation (1) that includes a full set of village dummy variables, which will capture all village- and Council-level variables that affect the *level* of benefits distributed. We therefore estimate the model

$$B_{ivc} = X_{ivc}\beta_1 + \left(\delta_0 + \delta_1 margin_c + \nu_{vc} + \nu_c\right)R_{ivc} + \tau_{vc} + \epsilon_{ivc} \quad (2)$$

²³See ?, which explains why $[margin_c^{C-1}, margin_c^A] \cdot R_{ivc}$ are valid instruments for $margin_c \cdot R_{ivc}$ (conditional on $[margin_c^{C-1}, margin_c^A]$ being valid instruments for $margin_c$).

Table 3: Margin of victory regressions

	[1]	[2]	[3]
$margin_c^{C-1}$	0.401*** (0.071)		0.357*** (0.069)
$margin_c^A$		0.609*** (0.123)	0.506*** (0.111)
F -statistic	31.982	24.709	22.889
R^2	0.386	0.347	0.444
N	231	231	231

Notes: Each column reports results for the OLS specification that regresses $margin_c$ on the instruments listed on the table and Council jurisdiction-level controls X_c listed in Table 2 and state fixed-effects. Robust standard errors are reported in parenthesis. The star levels ***, ** and * correspond to statistical significance at 1%, 5% and 10%, respectively.

where τ_{vc} are the village fixed-effects.²⁴ The validity of this approach rests on the assumption that (ν_{vc}, ν_c) are mean-zero errors that are uncorrelated with $margin_c$ (conditional on the other variables in the model). While this assumption seems plausible to us, it is possible that it could be violated. A violation of this assumption could occur, for instance, if more benefits are distributed to the Chief’s relations and fewer benefits to swing voters, while concurrently the Chief “delivers” more votes to swing the election to the incumbent party. Given that each village contains only about 12 percent of constituents that vote in the Council election, this seems somewhat unlikely.

Note that in this specification, we cannot identify the direct effect of village- or Council-level variables. We cannot therefore estimate the effect γ_1 of $margin_c$ on swing voters’ likelihood of receiving benefits, but only the additional probability that Chiefs’ relatives are awarded benefits, compared to non-relatives.

4.2.3 Fixed-effects instrumental variables

The third approach combines these two previous methods and estimates a fixed-effects instrumental variables model. While this increases the set of assumptions required for the approach to be valid, it allows us to assess the robustness of our results to controlling for the alternative sources of bias. In this specification, we use $R_{ivc} \cdot [margin_c^{C-1}, margin_c^A]$ to instrument for $R_{ivc} \cdot margin_c$.

²⁴Results remain very similar if Council fixed-effects are used, and village-level controls are retained in the model.

5 Empirical results

We begin by assessing whether the overall prevalence of mistargeting of poor and non-poor voters varies systematically with the degree of electoral competition. To investigate this possibility, we partition our sample into poor and non-poor household subsamples, and then estimate the following modified version of (1)

$$B_{ivc} = X_{ivc}\beta_1 + X_{vc}\beta_2 + X_c\beta_3 + \gamma margin_c + \delta R_{ivc} + \lambda_{vc} + \lambda_c + \epsilon_{ivc} \quad (3)$$

separately for these two subsamples. Equation (3) is identical to our baseline equation (1), except the random parameter for R_{ivc} has been replaced with a fixed parameter δ . In this specification, the parameter γ captures how the degree of electoral competition influences the overall probability that a household was selected as a beneficiary (regardless of whether or not the household is related to the Chief).

Results for this model are reported in Table 4. Columns 1-3 (Columns 4-6) report parameter estimates for the poor (non-poor) subsample of households, where households were partitioned using villagers' perceptions of which households are poor.²⁵ Columns 1 and 4 report estimates of (3) using the OLS estimator, while Columns 2, 3, 5 and 6 report estimates for the instrumental variables model in which $margin_c$ is instrumented for by $(margin_c^{C-1}, margin_c^A)$.

In our data, the degree of electoral competition varies considerably across the states within our sample, but varies to a lesser extent within states. Consequently, we estimate the instrumental variables specification both omitting state fixed-effects (Columns 2 and 5) and including them (Columns 3 and 6). The specification that omits the state fixed-effects utilizes the full variability of the electoral outcomes across all Councils, but may also omit other state-specific characteristics that influence targeting. When the state fixed-effects are included, the estimates utilize only the within-state variation of competition to identify the parameters of interest, which remains however considerably smaller.

Columns 1-3 in Table 4 suggest that the incumbent's margin of victory does not influence the probability that a poor household is selected as a beneficiary; the γ coefficient is insignificant across all three specifications. On the other hand, Columns 4-6 provide mixed evidence regarding the relationship between electoral competition and the share of non-poor households (inappropriately) included as beneficiaries. Columns 4 and 5 indicate that the share of non-poor beneficiaries is decreasing in the incumbent's margin of victory, which is consistent with the findings of Camacho and Conover (2011). Nevertheless, when state fixed-effects are incorporated into the instrumental variables model, the parameter estimate γ remains negative but becomes insignificant. Observe that the

²⁵Qualitative results are the same if households are partitioned according to the PMT. The coefficient for $margin_c$, however, is negative and insignificant in Column 5.

Table 4: Overall probability of being selected as a beneficiary

	Poor household sample			Non-poor household sample		
	[1]	[2]	[3]	[4]	[5]	[6]
	Base	IV (1)	IV (2)	Base	IV (1)	IV (2)
R_{ivc}	0.043 (0.045)	0.044 (0.044)	0.047 (0.045)	0.009 (0.018)	0.009 (0.017)	0.010 (0.017)
$margin_c$	0.027 (0.105)	0.098 (0.191)	0.264 (0.220)	-0.108** (0.055)	-0.173* (0.095)	-0.062 (0.120)
X_{ivc}	Yes	Yes	Yes	Yes	Yes	Yes
(X_{vc}, X_c)	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	No	No	Yes	No	No	Yes
R^2	0.153	0.153	0.170	0.152	0.151	0.159
N	1,277	1,277	1,277	1,391	1,391	1,391

Notes: Each column reports parameter estimates of a linear probability model in which the dependent variable equals 1 if the household was selected as a beneficiary and 0 otherwise. Columns 1-3 (Columns 4-6) report parameter estimates for the subsample of households that are perceived as poor (non-poor) by other households in the village. Column titles correspond to the respective econometric specification that was used to estimate (3). Household-level controls, X_{ivc} , are listed in Table 1. Village- and Council jurisdiction-level controls, (X_{vc}, X_c) , are listed in Table 2. Standard errors are clustered by village and reported in parenthesis. Observations are sample weighted. The star levels ***, ** and * correspond to statistical significance at 1%, 5% and 10%, respectively.

parameter estimates for δ suggest that relatives of the Chief have the same likelihood of being selected as a beneficiary, when compared to poor non-relatives.

Next, we investigate whether the incumbent party’s margin of victory influences the *composition* of the types of poor and non-poor households selected as beneficiaries, in a manner consistent with rent seeking and opportunistic vote buying. Table 5 reports the key parameter estimates ($\gamma_1, \delta_0, \delta_1$) for each of the specifications discussed in Section 4, separately for both the poor and non-poor household subsamples (again, partitioned according to respondents’ perceptions of households’ poverty status). The instrumental variables specification is again estimated both without (IV (1)) and with (IV (2)) state fixed-effects.

Overall, results are consistent with the theoretical predictions. The reported estimates provide strong support to strategic rent seeking behavior for the non-poor subsample (Columns 6-10), whereas family ties to the Chief does not seem to affect beneficiaries’ selection among the poor, which confirms the hypothesis that the “soft” budget constraint allows for covering most poor households. Results also have strong economic meaning. The negative coefficients for R_{ivc} in the regressions for non-poor households indicate that, with a zero margin of victory, non-poor households with family ties to the Chief are around 10 to 20 percentage points *less* likely to be selected as beneficiaries, relative

Table 5: Testing the rent seeking and vote buying mechanisms

	Poor household sample				Non-poor household sample					
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
	Base	IV (1)	IV (2)	FE	FE-IV	Base	IV (1)	IV (2)	FE	FE-IV
R_{inc}	-0.133 (0.128)	0.011 (0.208)	0.065 (0.222)	-0.144 (0.170)	-0.032 (0.164)	-0.082* (0.045)	-0.183** (0.085)	-0.178** (0.088)	-0.104* (0.057)	-0.275* (0.148)
$R_{inc} \cdot margin_c$	0.447 (0.298)	0.081 (0.515)	-0.046 (0.562)	0.378 (0.423)	0.167 (0.398)	0.228** (0.105)	0.482** (0.205)	0.473** (0.212)	0.301** (0.135)	0.765** (0.379)
$margin_c$	-0.036 (0.115)	0.071 (0.222)	0.258 (0.248)			-0.151** (0.061)	-0.274** (0.107)	-0.166 (0.129)		
X_{inc}	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
(X_{vc}, X_c)	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No	No
State FEs	No	No	Yes	No	No	No	No	Yes	No	No
Village FEs	No	No	No	Yes	Yes	No	No	No	Yes	Yes
$\text{prob}(\delta_0 + \delta_1 = 0)$	0.089	0.768	0.956	0.385	0.576	0.028	0.015	0.020	0.022	0.039
R^2	0.156	0.154	0.170	0.399	0.101	0.154	0.150	0.159	0.313	0.228
N	1,277	1,277	1,277	1,297	1,283	1,391	1,391	1,391	1,418	1,408

Notes: Each column reports parameter estimates of a linear probability model in which the dependent variable equals 1 if the household was selected as a beneficiary and 0 otherwise. Columns 1-5 (Columns 6-10) report parameter estimates for the subsample of households that are perceived as poor (non-poor) by other households in the village. Column titles correspond to the respective econometric specifications discussed in Section 4. Household-level controls, X_{inc} , are listed in Table 1. Village and Council controls, (X_{vc}, X_c) , are listed in Table 2. In Columns 1-5 and 6-10, standard errors are clustered by village and observations are sample weighted. The star levels ***, ** and * correspond to statistical significance at 1%, 5% and 10%, respectively.

to other non-poor households. The positive coefficients associated with the interaction $R_{ivc} \cdot margin_c$, and the fact that $\delta_0 + \delta_1 > 0$, also indicate that as the incumbent party’s margin of victory increases, the Chief’s non-poor family members become relatively more likely to be identified as beneficiaries. In fact, when the margin of victory is large (close to one), the situation almost reverses: non-poor households with family ties to the Chief are 15 to 30 percentage points *more* likely to be selected as beneficiaries, relative to other non-poor households.

On the other hand, results provide somewhat mixed support to opportunistic vote buying behavior: the negative coefficient on $margin_c$ suggests that a 10 percentage point increase in the winning party’s margin of victory is associated with about a 1.5 to 2.5 percentage point decline in the probability that a non-poor, unrelated household is selected as a beneficiary. When including the state fixed-effects in Column 8, however, the γ_1 parameter becomes insignificant, suggesting that the data may not have enough power to capture the within-state variability in the effect of $margin_c$ on the probability that non-poor, unrelated households are selected as beneficiaries.²⁶

5.1 Robustness

We now turn to investigate the robustness of the results presented in Table 5. We begin by delving deeper into the possibility that the statistical relationships observed in Table 5 are spurious in nature. Up until this point, we have imposed the assumption that $margin_c$ only affects the parameter of one household-level variable: R_{ivc} . Under the theoretical framework presented in Section 2, this assumption may seem natural: the incumbent’s margin of victory shouldn’t necessarily influence the probability that, say, female-headed households, households that suffered a crop crisis or television-owning households are included as beneficiaries. If the results in Table 5 are indeed spurious, and the hypothesized political economy mechanisms are not active, then it may be the case that $margin_c$ also influences the marginal effects of other household-level variables included in X_{ivc} .

To test this possibility, we augment the baseline model (1) and estimate

$$B_{ivc} = X_{ivc} \overbrace{\left(\eta_0 + \eta_1 margin_c + \zeta_{vc} + \zeta_c \right)}^{\text{Replaces } \beta_1} + X_{vc} \beta_2 + X_c \beta_3 + \gamma_1 margin_c + \left(\delta_0 + \delta_1 margin_c + \nu_{vc} + \nu_c \right) R_{ivc} + \lambda_{vc} + \lambda_c + \epsilon_{ivc},$$

where the elements $\{\eta_0, \eta_1, \zeta_{vc}, \zeta_c\}$ are vectors of dimension $\dim(X'_{ivc})$ and have an analogous interpretation as $(\delta_0, \delta_1, \nu_{vc}, \nu_c)$. The key question is whether $\eta_1 \neq 0$, i.e. whether, for some household-level controls, $margin_c$ is influential in determining the variable’s

²⁶If state fixed-effects are incorporated into the baseline specification in Column 6, the parameters $(\delta_0, \delta_1, \gamma_1)$ are all statistically significant at the 10 percent or 5 percent level.

parameter value. In addition to estimating this augmented baseline specification, we also augment the other specifications that were estimated in the results reported in in Table 5.²⁷

Estimates for (δ_1, η_1) are reported in Table 6. Observe that Table 6 reports estimates from the model that incorporates all interaction terms *jointly*. Columns 1-5 display estimates for the poor household subsample, and Columns 6-10 for the non-poor subsample. For non-poor households, the main qualitative conclusions regarding rent-seeking still hold across specifications: after including all of the interaction terms, the interaction of the $margin_c$ and R_{ivc} remains significant under all specifications. In contrast, *all other terms are insignificant*: only three of the 55 parameters are significant, and only at the 10 percent level. This undermines the hypothesis that we capture a spurious relation, and suggests that, for non-poor households, $margin_c$ primarily affects the household-level parameters through familial ties with the Chief, R_{ivc} . Observe, in particular, that the interaction of the margin of victory with households' relations with the local implementers is not significant. This confirms the role of the village Chief as an influential and visible party representative in the village.

For poor households, the only interaction term that is significant across multiple (four) specifications is the one concerning ethnic minorities: a 10 percentage point increase in margin of victory for the incumbent implies that poor, ethnic minority households are about 10 to 20 percentage points less likely to be included as a beneficiary. This suggests that a narrow margin of victory may benefit against discrimination of ethnic minorities.

Next, we consider if results are sensitive to the way in which the poor and non-poor households subsamples are divided. Tables 4-6 utilized data that partitioned households using an inherently *subjective* welfare metric, villager perceptions. Conceptually, such perceptions may vary systematically across villages. To check for robustness, we partition households into poor and non-poor subsamples using the *objective* PMT point score computed by the professional enumerators that executed the data collection. A household is classified as poor (or eligible) if the household's PMT score is below the targeting program's point threshold.

Table 7 presents these estimates. In Columns 1-5 we show results for technically eligible households, and in Columns 6-10 we show results for technically non-eligible households. While the estimates, for non-eligible households, have the correct sign, their significance drops. In particular, the interaction between margin of victory and households' relation with the Chief loses significance under the IV estimations, and the estimates for vote buying (γ_1) are only significant in the baseline specification (Column 6). In full similarity with the main analysis, estimates for eligible households are all insignificant.

The drop in significance does not necessarily undermine our findings, but rather suggests

²⁷For the instrumental variables specifications, we instrument for $margin_c \cdot [1, R_{ivc}, X_{ivc}]$ using the vectors $margin_c^{C-1} \cdot [1, R_{ivc}, X_{ivc}]$ and $margin_c^A \cdot [1, R_{ivc}, X_{ivc}]$.

Table 6: Interaction between $margin_c$ and other household-level controls

	Poor households				Non-poor households					
	[1] Base	[2] IV (1)	[3] IV (2)	[4] FE	[5] FE-IV	[6] Base	[7] IV (1)	[8] IV (2)	[9] FE	[10] FE-IV
$R_{ivc} \cdot margin_c$	0.414 (0.286)	0.044 (0.501)	-0.119 (0.537)	0.298 (0.418)	0.069 (0.412)	0.220* (0.124)	0.627** (0.247)	0.626** (0.253)	0.276* (0.160)	0.805** (0.394)
# household members $_{ivc} \cdot margin_c$	-0.073* (0.044)	-0.043 (0.097)	0.000 (0.097)	-0.054 (0.055)	-0.020 (0.091)	-0.036 (0.025)	-0.001 (0.038)	-0.004 (0.039)	-0.054* (0.029)	-0.016 (0.067)
Years of schooling $_{ivc} \cdot margin_c$	0.000 (0.036)	-0.093 (0.080)	-0.105 (0.078)	-0.006 (0.044)	-0.021 (0.056)	-0.009 (0.014)	-0.008 (0.022)	-0.011 (0.022)	-0.007 (0.018)	-0.011 (0.039)
Head is female $_{ivc} \cdot margin_c$	0.128 (0.192)	-0.532 (0.437)	-0.589 (0.412)	-0.159 (0.226)	-0.136 (0.345)	-0.179 (0.115)	-0.187 (0.199)	-0.208 (0.197)	-0.243 (0.157)	-0.562* (0.340)
Ethnic minority $_{ivc} \cdot margin_c$	-1.079*** (0.370)	-2.154*** (0.980)	-1.817** (0.898)	-1.173* (0.657)	-0.441 (1.474)	0.314 (0.204)	0.186 (0.310)	0.197 (0.318)	0.171 (0.353)	0.632 (0.682)
Suffered crop failure $_{ivc} \cdot margin_c$	0.007 (0.227)	0.180 (0.478)	0.144 (0.457)	0.288 (0.276)	0.332 (0.368)	0.109 (0.103)	-0.033 (0.188)	-0.060 (0.190)	0.043 (0.143)	-0.231 (0.325)
Owens 1+ motorcycle $_{ivc} \cdot margin_c$	0.450 (0.298)	0.261 (0.657)	0.403 (0.664)	0.349 (0.405)	0.279 (0.466)	0.034 (0.093)	0.066 (0.199)	0.077 (0.200)	0.058 (0.117)	0.039 (0.290)
Owens 1+ plough $_{ivc} \cdot margin_c$	0.010 (0.232)	0.231 (0.396)	0.032 (0.408)	-0.115 (0.300)	-0.048 (0.368)	-0.117 (0.112)	-0.271 (0.184)	-0.310* (0.180)	-0.110 (0.188)	-0.449 (0.331)
Owens 1+ tractor $_{ivc} \cdot margin_c$	1.092* (0.645)	0.830 (1.022)	0.683 (0.993)	0.385 (0.727)	0.105 (0.735)	0.092 (0.105)	0.063 (0.186)	0.076 (0.189)	0.185 (0.197)	0.136 (0.434)
Owens 1+ tv $_{ivc} \cdot margin_c$	-0.181 (0.234)	0.418 (0.519)	0.264 (0.527)	-0.173 (0.274)	0.470 (0.329)	0.036 (0.094)	0.129 (0.171)	0.119 (0.169)	0.009 (0.114)	0.108 (0.305)
Related to implementer $_{ivc} \cdot margin_c$	0.070 (0.213)	0.895* (0.528)	0.795 (0.508)	0.158 (0.262)	0.350 (0.371)	-0.015 (0.111)	-0.070 (0.186)	-0.075 (0.188)	-0.073 (0.154)	0.076 (0.323)
# social organizations $_{ivc} \cdot margin_c$	0.048 (0.283)	-0.453 (0.677)	-0.516 (0.681)	0.394 (0.369)	0.291 (0.533)	-0.105 (0.091)	-0.139 (0.170)	-0.109 (0.172)	-0.068 (0.116)	-0.367 (0.291)
X_{ivc}	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
(X_{vc}, X_c)	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No	No
State FEs	No	No	Yes	No	No	No	No	Yes	No	No
Village FEs	No	No	No	Yes	Yes	No	No	No	Yes	Yes
R^2	0.168	0.119	0.138	0.405	0.092	0.159	0.149	0.156	0.318	0.225
N	1,277	1,277	1,277	1,297	1,283	1,391	1,391	1,391	1,418	1,408

Notes: See the notes for Table 5.

that *where the eligibility threshold is set* in splitting the sample is of high importance. The PMT only identified 17.9 percent of households as poor, against 36.6 percent for the perception measure. Recall that 31.1 percent of the population in our sample villages is a beneficiary. Thus, the PMT splits the sample at a threshold around which most households are beneficiaries, and includes in the sample of non-eligible households many households that, independently from their relation with the Chief, are deemed poor by everybody in the village and have been fairly included among the beneficiaries. Such an inclusion obviously affects the ability to capture rent seeking and vote buying effects.

To investigate this issue, we recalibrated the PMT beneficiary eligibility cutoff to reflect the overall percentage of households that were identified as poor by local implementers (31.1 percent). The household sample was then partitioned such that households receiving a PMT score below the recalibrated cutoff were classified as eligible. Parameter estimates for these specifications are reported in Table 8. For the non-poor subsample, the baseline, fixed-effects and fixed-effects instrumental variables specifications (Columns 6, 9 and 10) provide strong support for our theoretical predictions. In the two instrumental variables specifications (Columns 7 and 8), however, the key parameter estimates generally have signs that correspond to the theoretical predictions, but are statistically insignificant, thus providing mixed evidence in support of the countervailing incentives.

6 Conclusion

This paper tests the degree to which policymakers' private rent seeking and vote buying behaviors respond to electoral competition. The analysis employs survey data that contain information regarding a decentralized program in a developing country that selected households eligible for social assistance benefits intended for the poor. We find that the overall level of resource misallocation is invariant or, if anything, increasing in the degree of political competition. The composition of *who benefits* from misallocation, however, depends upon the level of competition: as electoral competition increases, local officials trade off extracting private benefits for allocating resources to potential swing voters. Non-poor relatives of the local Chief are about 10 to 20 percentage points *less* likely to be allocated benefits, relative to other non-poor households, when the previous election outcome was competitive. When the incumbent party won the previous election with a large margin of victory, however, the situation reverses: non-poor relatives of the local Chief are about 15 to 30 percentage points *more* likely to be allocated benefits. Some evidence, though weaker, also suggests that opportunistic vote buying behavior increases in response to competition: non-poor voters that are unrelated to the Chief are about 15 to 20 percentage points more likely to be identified as beneficiaries if expected electoral competition is high. We also find that other household characteristics associated with receiving benefits are unaffected by electoral competition, suggesting that favoritism works

mainly through family ties.

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Table 7: Identifying poor and non-poor households using the PMT score

	Poor household sample					Non-poor household sample				
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
	Base	IV (1)	IV (2)	FE	FE-IV	Base	IV (1)	IV (2)	FE	FE-IV
R_{iivc}	-0.032 (0.139)	-0.203 (0.288)	-0.220 (0.278)	-0.215 (0.219)	-0.149 (0.320)	-0.079 (0.052)	-0.065 (0.092)	-0.055 (0.097)	-0.109* (0.065)	-0.176 (0.114)
$R_{iivc} \cdot margin_c$	0.181 (0.302)	0.607 (0.681)	0.622 (0.646)	0.491 (0.488)	0.321 (0.743)	0.245** (0.124)	0.210 (0.233)	0.189 (0.246)	0.345** (0.161)	0.514* (0.293)
$margin_c$	0.118 (0.151)	-0.239 (0.292)	-0.185 (0.280)			-0.128** (0.065)	-0.121 (0.116)	0.062 (0.144)		
X_{iivc}	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
(X_{ec}, X_c)	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No	No
State FEs	No	No	Yes	No	No	No	No	Yes	No	No
Village FEs	No	No	No	Yes	Yes	No	No	No	Yes	Yes
$\text{prob}(\delta_0 + \delta_1 = 0)$	0.401	0.311	0.283	0.338	0.691	0.035	0.319	0.380	0.024	0.066
R^2	0.200	0.189	0.218	0.583	0.141	0.303	0.303	0.310	0.403	0.387
N	660	660	660	675	605	2,008	2,008	2,008	2,040	2,039

Notes: See the notes for Table 5. Columns 1-5 (Columns 6-10) of this table report parameter estimates for the subsample of households that received a PMT score indicating that the household is poor (non-poor).

Table 8: Estimates after recalibrating the PMT eligibility cutoff

	Poor household sample					Non-poor household sample				
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
	Base	IV (1)	IV (2)	FE	FE-IV	Base	IV (1)	IV (2)	FE	FE-IV
R_{ivc}	0.033 (0.108)	-0.036 (0.244)	0.002 (0.241)	-0.015 (0.142)	-0.008 (0.207)	-0.110* (0.057)	-0.078 (0.093)	-0.074 (0.094)	-0.168** (0.072)	-0.234* (0.123)
$R_{ivc} \cdot margin_c$	-0.019 (0.245)	0.157 (0.595)	0.076 (0.587)	0.117 (0.334)	0.055 (0.510)	0.329** (0.137)	0.250 (0.237)	0.232 (0.243)	0.445** (0.178)	0.630** (0.311)
$margin_c$	0.031 (0.122)	0.039 (0.265)	0.082 (0.276)			-0.132** (0.062)	-0.161 (0.109)	0.023 (0.129)		
X_{ivc}	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
(X_{ivc}, X_c)	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No	No
State FEs	No	No	Yes	No	No	No	No	Yes	No	No
Village FEs	No	No	No	Yes	Yes	No	No	No	Yes	Yes
$\text{prob}(\delta_0 + \delta_1 = 0)$	0.929	0.735	0.825	0.624	0.882	0.012	0.244	0.296	0.016	0.040
R^2	0.194	0.194	0.215	0.478	0.159	0.256	0.256	0.266	0.377	0.343
N	1,117	1,117	1,117	1,141	1,121	1,551	1,551	1,551	1,574	1,570

Notes: See the notes for Table 5. Columns 1-5 (Columns 6-10) of this table report parameter estimates for the subsample of households that received a PMT score indicating that the household is poor (non-poor), after adjusting the PMT point cutoffs.