

# Inside & Out

## The Role of the Non-Poor

### in Targeting Resources to the Poor

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#### **Abstract**

The role that communities play in fostering or hindering the targeting of foreign aid to the poor has been the subject of growing attention. While the role of the target group itself and the role of elites in the targeting process is increasingly discussed in the literature, there is little work to date that considers how the rest of the community can influence whether the most vulnerable community members actually receive the aid to which they are entitled. In this paper, I study the role of non-poor, non-elite community members in influencing how elites choose to allocate resources to the poor. Using a lab-in-the-field experiment, I look at public distributions of money within small groups of people from the same rural villages in eastern Democratic Republic of the Congo (DRC). Each group comprises one members of the local elite, two poor villagers and two non-poor, non-elite villagers. I vary whether elites receive instructions to target the poor or not, whether the non-poor have veto power or not, and whether the allocation process is actively monitored by a third-party or not. I find that the non-poor have a significant influence on the allocation, not by simply keeping the elites in check through peer-pressure, but by actively bargaining with the elites over whom should receive what. In fact, when resources are targeted to the poor, and in particular when they have adversarial relationships with the elites, they can effectively advocate in favor of allocating a bigger share of the windfall to the poor.

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Targeting the poor is widely considered the most efficient and ethical means of allocation for humanitarian and development aid assistance when resources are scarce (NRC, 2013, FAO, 2006). Yet empirical evidence suggests that targeted aid programs often benefit elites and other non-poor groups in recipient communities, while their impact on poor, vulnerable populations can be mixed at best (Strauss-Kahn, 2018f). Ensuring that a windfall reaches those for whom it is intended and bypasses those for whom it is not is especially challenging since aid agencies rarely have complete control over the allocation process (Maxwell & Burns, 2008). In low-income or fragile countries, aid agencies frequently face time, capacity, and information constraints that compel them to rely formally or informally on recipient communities themselves to assist with targeting (Galasso & Ravallion, 2005). Numerous aid programs now engage in community-based targeting—in which communities themselves select beneficiaries—in the belief that community leaders or members at large are better positioned to identify qualified recipients and distribute goods. In other cases, aid agencies identify beneficiaries through objective or data-driven methods but nevertheless still rely on communities or local leaders for assistance with finalizing beneficiary lists or distributing goods (Alatas et al., 2013). Overall, the outcomes of targeted aid programs can vary significantly in terms of whom in recipient communities ends up benefitting from the aid windfall.

The role that communities play in fostering or hindering the targeting process has been the subject of growing attention. While community involvement in targeting can increase the legitimacy of and satisfaction with aid programs (Winters, 2014, Alatas et al., 2012), it also raises concerns about undesirable consequences. It is now widely appreciated that community involvement in aid distributions can increase the scope for capture of the aid windfall by community members who are not intended beneficiaries (Jaspars & Shoham, 1999, Young & Maxwell, 2009). There is also evidence that

targeting can result in heightened conflict and jealousy within recipient communities, undermining social cohesion (de Sardan, 2014, de Sardan et al., 2014). Recent work emphasizes that group dynamics that are established long before the arrival of an aid program and continue to matter long after an aid program has ended can be particularly important in understanding specific patterns of capture (Paler et al., 2018). There are reasons to think that community involvement in the targeting process can be more efficient in ensuring the respect of targeting instructions than monitoring or sanctions by the aid agency (Strauss-Kahn, 2018a). Yet, without knowing exactly what it is about community involvement that makes a difference about who receives the aid windfall, aid programs can hardly be improved effectively.

Various groups within recipient communities are involved in the targeting process. They differ in their roles and in their capacities to influence the aid allocation. Elites, since they are individuals with formal political authority in the village, are often in a position to directly influence how aid is allocated and to capture a share of the aid for themselves (Platteau, 2004, Alatas et al., 2013). By contrast, target groups—those community members that are supposed to receive the aid—are mostly vulnerable groups within the community (such as the poor, widows, internally displaced persons, or conflict victims) with little influence in the community in the first place. Interestingly, there always exists yet another part of the community that is both likely to have the capacity to influence how aid is allocated, yet usually left out of aid programs: those community members who do not belong to the target group but also are not part of the elite. They can be, for example, non-poor community members in a program targeted at the poor; former militants in a program targeted at civilians; men in a program targeted at women; members of an ethnic majority in a program targeted at an ethnic minority; or long-time members of a community in a program targeted at migrants or internally-displaced persons. Like elites, they are

not entitled to receive any of the aid; unlike elites they do not have a formal role in the targeting process; but by contrast with the target group they are not particularly weak or vulnerable and can be influential members of the community. Whereas the role of the target group and elites is increasingly discussed in the literature, there is little work to date that considers how all these other community members—who are both inside the community and have long-standing relationships with both the target group and the elites, yet are formally speaking left out of the aid program— can influence whether the target group actually receives the aid to which it is entitled.

In this paper, I study the role of non-targeted, non-elites group members in influencing how elites choose to publicly allocate resources to a target group in rural villages in eastern Democratic Republic of the Congo (DRC). In a lab-in-the-field experiment, I measure behaviors and attitudes of small groups of five villagers—among which there is one member of the elite, two poor and two non-poor villagers—when I ask them to allocate money among themselves in a dictator-game like setting. I randomize whether non-poor group members have veto power over the elites’ decision or not. Using a factorial design, I also randomize whether the distributor is provided with instructions to target the poor or not, and whether the enumerator actively monitors the decision-making process or not. As a result, I can identify the causal effect of an exogenous variation in non-poor group members influence on resource allocation separately from the effect of targeting instructions and monitoring by a third-party.

The Kivus region in eastern DRC, in which I have gathered the empirics for this study, is currently one of the world’s most complex and long-standing humanitarian crisis. This allows me to leverage both a relevant context and rich data about real-life power structures, social roles, and relationships between participants. Since the villages in the sample are rather small, study participants are likely to interact with

one another on a regular basis, such that I can use interesting variation in how much they know, like or trust each other as control measures. In practice, each participant in the study was first administered a baseline survey. The baseline survey measured their economic and social situation within the village, as well as included questions on their perceptions of social norms in the village. Additional survey data was then gathered before, during and after each public distribution. Since rural villages in eastern DRC are used to the presence of humanitarian organizations, asking villagers to allocate resources among themselves while providing them with targeting criteria was not considered as a surprising nor an artificial exercise by the participants. In the end, in addition to the behavior of each participant during public distributions, I can also measure their attitudes toward other group members, their understanding and perceptions about the outcome of the public distribution, as well as changes in their perception of social norms post-treatment.

Overall, the results of the experiment strongly suggest the existence of complex bargaining dynamics among group members during public distributions. More specifically, I find that giving non-poor group members veto power *in distributions targeted to the poor* increases both the share allocated to the non-poor *and* the share allocated to the poor. While the first part of this finding is generally consistent with the literature on bargaining, the second is surprising and of substantial interest. This increase in the share of the windfall that poor group members receive in targeted distributions with non-poor veto players is strong, robust and consistent across specifications. In addition, the increase is even significantly larger in effect size when elites and non-poor group members are at odds with each other, or when poor group members are close friends with non-poor group members. By comparison, and contrary to expectations, poor group members are for example not necessarily better-off when they are close friends with the elites. In-depth quantitative and qualitative analysis of group

discussions during public allocations confirms that in some contexts non-poor group members actively attempt to influence elites into giving out more of the windfall not only to themselves but also to poor group members.

In unpacking the nature of the community dynamics at play in aid targeting, this paper contributes in at least two ways to a better understanding of what makes for effective distribution of foreign aid, and possibly for effective social transfers to the poor in general. First, the results of this study specifically highlight the role in making targeting effective of some members of recipient communities who are otherwise often purely and simply excluded from aid programs and whose influence on the resource allocation process is rarely taken into account—in this case non-poor, non-elites group members—. Since targeting a transfer to one specific group necessarily leads to excluding another group who is going to be both “inside and out” of the targeted program, this warrants further work on the role of this excluded group and a better understanding of the contexts in which it will either foster or hinder the effectiveness of a social transfer.

Second, the results of this study also suggest that the involvement of the community at large in the targeting process influences elites’ behaviors as a result of complex bargaining dynamics rather than simply through peer-pressure. One way to understand these results is that it is possible that the rest of the community can in some contexts serve as a counter-balance to the power of the elites and effectively limit their capacity to capture aid windfall. This in turn suggests that a promising way to improve the effectiveness of aid programs in the future could come from not only higher but better involvement of recipient communities in targeted programs. In particular, a more thorough analysis of existing social relationships and potential coalitions within recipient communities could contribute to balancing elites’ propensity for aid capture, and ultimately to a better targeting of the poor.

# 1 Theory

Targeting resources invariably creates an outgroup: within any group, by selecting some to benefit from a windfall, one necessarily excludes others (Barrett, 2006). In this section, I first briefly build on some general empirical observations about the distribution of foreign aid at the local level to highlight the relevance of this notion of targeting outgroup in the context of aid targeting. I then consider a vast literature on group dynamics, including research in social psychology and about non-cooperative bargaining, and discuss existing arguments that shed light on the influence that such group that is both inside (the community) and out (of the aid program) —typically influent in the recipient community, yet in theory not an intended beneficiary of the windfall— could have on resource allocation. For the sake of clarity, I present these arguments in terms of whether they fall into one of two families of explanations, namely peer-pressure and bargaining dynamics.

While aid capture has long been a major concern for foreign aid program, aid agencies usually assume that the effectiveness of their targeted aid programs depend either on the fairness and acceptability of the targeting criteria, the degree of transparency of the process within the recipient community, or the quality of their monitoring (Transparency International, 2014). Recent work points to the possibility that the provision of instructions to target the poor and vulnerable may actually not be effective on its own, but rather that targeting can be fostered either through community involvement or through monitoring (Strauss-Kahn, 2018a).<sup>1</sup> Involving recipient communities in the targeting process through community-driven programs may seem much more feasible and effective than monitoring in many contexts. Yet

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<sup>1</sup>In work most similar to this paper, I compare resource allocations by elites in private and public settings and I find significant differences in their allocations to the poor. In this paper, I build on these results and try to unpack the dynamics at play during public allocations specifically.

there is also ample evidence that community dynamics can in fact either foster or hinder the targeting of the poor in aid distributions (Maxwell & Burns, 2008, Jaspars & Maxwell, 2008, Young & Maxwell, 2009).

Aid agencies face time, cost and operational constraints that highly limit their actual influence in the allocation process at the local level. Most commonly, an implementing NGO will come to a village and inform the members of the community that an aid windfall is intended for the most vulnerable among them. It will then rely to some extent on some influential community, non-targeted members to make the actual allocation, and will monitor the distribution of goods to the best of its ability before leaving the village. In practice, traditional elites seem to invariably end up acting as a distributor when an aid program is implemented in their village, whether the aid agency in charge actually wants it or not. And most of the time, aid agencies have little to no abilities to effectively monitor these distributions, let alone credibly sanction contraveners whenever targeting instructions are not respected. In the end, aid distributions are arguably very similar to a dictator-game in which a distributor—the elites—have to allocate resources to a receiver—the target group. Hence in a way, since the target group comprises almost by definition weaker, more vulnerable community members, aid targeting puts elites *de facto* in a position to capture a share of the aid windfall for themselves.

Existing work on the role of recipient communities in targeting usually focuses either on target groups or on elites as the only two relevant actors to consider: for instance recent studies of aid capture distinguish between the poor and non-poor (Galasso & Ravallion, 2005, Bardhan & Mookherjee, 2006) or between elites and non-elites (Alatas et al., 2013).<sup>2</sup> Yet, there is numerous anecdotal evidence that non-

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<sup>2</sup>Alatas et al. (2013) distinguish between formal and informal elites but do not theorize how the role of informal elites differs from the role of traditional elites nor the interaction between these actors.



beneficiaries other than elites can either help a lot in securing favorable outcomes to aid programs (Maxwell & Burns, 2008, Harragin & Chol, 1998) or can intervene in the aid allocation process to try to expropriate a share of the resources for themselves (de Sardan, 2014). By pooling non-beneficiaries other than elites with other groups in the community, these studies overlook the possibility that elites, target groups and the rest of the community can have different objectives and that this can have important implications for distributional outcomes.

Research in social psychology has long shown that distributors can be significantly influenced in their decision-making by other group members (Messick & Sentis, 1983, Robert & Carnevale, 1997, Bornstein & Yaniv, 1998). One could think that in aid targeting contexts, elites are unlikely to be influenced by anything other than their self-interest since they are from the start in a two-fold position of power: first they are individuals with high influence in the community because of their social role, and second they have decision power on the allocation of resources because of their position as a distributor. But insofar as their decision-making is to some extent transparent and public, it stands to reason that elites could also have to take various other elements into consideration: they have long-standing relationships with other community members and they will have to stand by their decisions long after the aid agency has left. The question is thus not so much whether but rather whom in the community has the most capacity to influence the elites' decisions and in what ways.

The capacity of group members to affect group decision is generally thought of as a function of their influence or bargaining power (Caplow, 1956, Gamson, 1961a). There are reasons to think that target groups have little to no influence in their community *ex ante*, and henceforth that they have little influence in the aid allocation process as well. They are targeted in the first place precisely because they are often both the most in need and the most at risk of being marginalized from resource al-

location without special consideration (NRC, 2013, OCHA, 2014, de Sardan et al., 2015). While it is in theory possible that targeting helps empower recipients to hold their elites accountable (Winters, 2014), in practice it is often unlikely that targeting fundamentally alters enduring power asymmetries within communities Galasso & Ravallion (2005), Bardhan & Mookherjee (2006), Dreze & Sen (1989). By contrast, the rest of the community is likely comprised by at least some influential individuals, and it is credible that these influential community members may have an impact on allocations' outcomes.

In a public distribution, there are fundamentally two ways to think about how group members may influence the distributor's decision, namely through peer-pressure or through bargaining dynamics. Peer-pressure includes any explanation according to which it is the mere presence of some given individuals during the decision-making process that influences the distributor into behaving differently than he would have otherwise. In the context of aid targeting, peer-pressure could for example foster targeting the poor if community members' have more prosocial preferences than the elites (a.k.a. a direct peer-pressure effect of community involvement), or if targeting instructions prompted the community to expect more prosocial behavior from their elites, for example out of considerations about fairness (aka. in which case peer-pressure would be an indirect effect of community involvement insofar as it interacts with targeting instructions) (Messick & Sentis, 1983, Kahneman et al., 1986, Brockner et al., 2001, Van Dijk et al., 2004).<sup>3</sup> It could also be that there is some peer-pressure at play, but that the effect of peer-pressure is not favorable to the poor. For example,

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<sup>3</sup>There is in fact some anecdotal evidence that several competing conceptions of fairness are at play in aid recipient communities (Young & Maxwell, 2009). And while recipient communities are not generally poor-oriented societies, practitioners typically assume that specific segments of their polities —like the poor themselves or women— hold more prosocial preferences than their elites (Heinz et al., 2012). In such cases, co-opting recipient communities in the targeting process through awareness campaigns and community-based targeting in general can help a lot in securing favorable outcomes to aid programs (Maxwell & Burns, 2008, Harragin & Chol, 1998).

in line with research on the importance of friendships and rivalries in resource allocation (Rusinowska, 2002, Laengle & Loyola, 2012, 2015), the presence of community members could pressure elites into favoring their friends rather than their enemies in distributions. If so, the provision of targeting instructions would have little to no effect on elites' decisions while friendship with the elites would be a strong explaining factor for receiving a share of the windfall.

Group members can also influence the distributor's decision through bargaining and coalition formation Komorita & Chertkoff (1973), Komorita & Moore (1976), Komorita & Kravitz (1979), Komorita & Lapworth (1982), Komorita & Miller (1986). A large literature shows that bargaining outcomes in groups are the result of the formation of a minimum winning coalition that depends on players initial resources and power (Gamson, 1961b, 1964). Different theories, each with a specific set of assumptions about norms and motives, explain the emergence of various types of coalitions depending on context (Kahan & Rapoport, 2014, Bausch, 2017). One particularly relevant argument in the case of aid targeting would be minimal group theory (Tajfel & Turner, 1986, Kelley et al., 1966). Typically, targeting instructions could serve as a focal point around which the community coordinates its expectations about the allocation of the aid windfall. Social psychology studies have shown that, when resources are allocated in groups, even arbitrary and virtually meaningless distinctions can lead to coalition formation around such "minimal groups" (Tajfel, 1982). The effect of targeting would then be to enforce a specific bargaining environment that would typically be different from the bargaining environment that would govern non-targeted allocations.<sup>4</sup>

Overall, peer-pressure and bargaining explanations differ empirically in several

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<sup>4</sup>Typically, rather than forming a minimum winning coalition with some community members that they like individually, the elite would have to treat the targeted and the non-targeted as groups - in this empirical case the poor and the non-poor - with whom it has to bargain as unitary actors.

ways. First, bargaining dynamics involve active participation of group members, typically through discussions, while peer-pressure does not. Second, while bargaining effects will vary with bargaining power, peer-pressure effects will not. With peer-pressure, it is merely the existence of a common knowledge environment that influences elites' behavior: since you are looking at me, even if you don't say anything, I know that you know what decision I am making. In other words, as soon as an influential group member is present, the distributor should start behaving in a certain way. Bargaining dynamics suggest on the other hand a more active involvement of some community members in the process, for example through discussions.<sup>5</sup> As a result, traditional bargaining theory would predict that the more bargaining power some group members have, the more these group members are able to extract resources for themselves. On the other hand, peer-pressure rather suggests the existence of a ratchet effect: it either exists or not but it does not vary with bargaining power.

## 2 Empirics

I have gathered data in eastern Democratic Republic of the Congo about how carefully selected groups of villagers choose to publicly allocate money among themselves in various situations. In previous work closely related to this paper, I have used a similar experimental design to study whether the provision of targeting instructions, the presence of other group members during the decision-making process and the monitoring by a third-party influence the allocation of resources to the poor (Strauss-Kahn, 2018a). I have found that public allocations—in which other group members are present—differ vastly from private ones. In this paper, I focus specifically on

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<sup>5</sup>In a sense, while one can expect the effect of peer-pressure to be binary—either influential group members are present and there is peer-pressure, or they are not—the effect of bargaining dynamics can be thought of as more gradual. The more effort an influential group member puts in bargaining, the more effect it may have.

these public distributions to further explore the ways in which the presence of other group members affect a distributor’s decision.

While the original experimental design as well as the specificities of the context of DRC are discussed in more details in Strauss-Kahn (2018a), I briefly describe their main features here. Each group in the sample is composed of five people from the same village: one is a member of the local elites, two are sampled from the poorer half of the village, and the other two are sampled from the other half of the village (the richer half). The elite is in charge of allocating a sum of money among all five group members while the four other group members are present. Using a  $2 \times 2 \times 2$  factorial design, I vary three things about these allocations: whether the distributor is asked to target poor group members or not (Treatment 1: *targeted or non-targeted distributions*), whether the two non-poor group members are veto power over the elite’s decision or not (Treatment 2: *veto or non-veto distributions*), and whether the enumerator is obviously and actively monitoring the distributor’s decision or not (Treatment 3: *monitored or non-monitored distributions*). I then compare the share of the windfall that is allocated to the two poorer group members, to the elite and to the two non-poor group members in each of the eight treatment conditions.

Comparing veto and non-veto distributions, I can distinguish between peer-pressure and bargaining explanations. According to traditional veto theory (Tsebelis, 1995), the veto treatment exogenously varies the bargaining power of the non-poor group members. While bargaining dynamics should be affected by the introduction of veto players, the allocation outcome would not change if only peer-pressure is at play (Guth & Huck, 1997, Rodriguez-Lara, 2016). With this design, I can further sparse into the effect of community involvement and whether it is affected by targeting instructions by looking at the interaction of the veto and the targeted treatment. Finally, I can control for peer-pressure effects induced by a third-party rather than by community

members by looking at the effect of the monitoring treatment.

## 2.1 Context

The empirics for this study have been gathered in the regions of North-Kivu and South-Kivu in eastern Democratic Republic of the Congo (DRC).<sup>6</sup> Since 2004, the Kivus region have become one of the world’s most complex and long-standing humanitarian crisis as well as one of the largest humanitarian mission currently in operation. As a result, whether they happen to have directly benefited from assistance or not, rural villages in DRC have become used to the presence of humanitarian NGOs.

Despite a growing trend toward urbanization, the population is largely rural. Villages are relatively small — for example, those sampled for this study usually comprises from 50 to 150 households. With an average of 5 members per household, one could that everybody knows more or less argue everybody in these communities. Despite the fact that everybody is arguably poor with regard to international standards, there are also observable differences between poorer and richer households within each village. For example, in all of the villages in the sample, the poorest respondents reported earnings less than \$4/month while a few of the richest respondents declared earning more than \$150/month. Political power is largely concentrated in the hands of the village chief, who is most of the time male (90%) and whose authority is derived from custom rather than election.<sup>7</sup> The chief is assisted in his public duties by various elites. These elites are called the “eyes of the chief”, and they can act as representatives of the chief in various instances. In rural eastern DRC as in many other humanitarian contexts, in aid-recipient villages these elites are usually involved in the process of distributing the aid windfall, even though they are not intended

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<sup>6</sup>See Strauss-Kahn (2018a) for an in-depth discussion of the benefits of using a relevant context and rich data about real-life power structures, social roles, and relationships between participants.

<sup>7</sup>See Strauss-Kahn (2018a) for an extensive discussion of the characteristics of the sample.

beneficiaries of said aid programs.

## 2.2 Methodology

A total of 400 respondents from 10 rural villages in five districts of the Kivus were surveyed for this study. Out of consideration for the external validity of the study and possible heterogeneous treatment effects, the villages were block sampled by district, such that in each district one village that had received aid from an NGO in the past five years and one that had not would be picked.

In each village, I have administered a baseline survey to 8 elites and 32 non-elite members of the village. The members of the elite were randomly sampled from a comprehensive list provided by the village chief.<sup>8</sup> The non-elite respondents were a gender-balanced, representative sample of the rest of the village.<sup>9</sup> The baseline survey includes questions about the economic and social situation of each respondent as well as questions about social norms in the village.

Using this baseline survey, I have sorted the 32 non-elite respondents using a poverty index into the 16 poorest (hereafter the ‘poor’ half of the sample) and the 16 richest (the ‘non-poor’). All 40 respondents are then randomly dispatched into 8 groups of 5 persons that each included 2 poor, 2 non-poor and 1 elite.<sup>10</sup> In each group, the elite member has to allocate 13.500CF (13.5\$US) among all group members while the other four group members are present. The allocation is done by putting 27 tokens, each representing (and somewhat resembling) a bill of 500CF into five cardboard

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<sup>8</sup>The chief of the village was asked to provide in advance a list of “the eyes of the chief”, local elites that could act as his representatives in the distributions. The list included all village members that had official formal duties related to the governing of the village. In short, they were all more or less village council members.

<sup>9</sup>The representative sample of the rest of the village specifically excluded the chief and members of the elite, but it could include their family members.

<sup>10</sup>See the Experimental Protocole as well as Strauss-Kahn (2018a) for a more detailed discussion of the experimental setup.

ballot boxes on top of which there is a picture of one of the group members.<sup>11</sup>

Each group is randomly assigned one of the  $2 \times 2 \times 2$  possible treatment conditions. First, the elite member is either instructed to target poor group members (“Distribute this windfall as you want among the members of your group. It is intended for the poorest people in your group.”) or receives no specific targeting instructions (“Distribute this windfall as you want among the members of your group.”). Second, the two non-poor group members are either informed that they will have veto power over the elites’ final decision (“You have been randomly selected as a veto player. This means that I will ask you whether you agree with the distributor’s decision or not before he puts the tokens in the boxes. If you don’t agree, I will take back all the tokens and no one will receive anything.”) or not. Finally, the enumerator either goes out of the room and informs the distributor that he will not know anything about his decision or stays in the room, actively observes the allocation process, takes notes, asks the distributor to explicitly state how many tokens are put in each box, and informs the distributor that all these observations will impact the payment received at the end of the day. All group members know and hear the treatment conditions.<sup>12</sup>

Treatment assignment is factorial and decided at the group level. From a methodological standpoint, the sequential ignorability assumption are likely to be satisfied. In order to increase sample size, once a round of distribution has been done, the 40 respondents are shuffled into new, different groups. Two people who have been together in a group in one round can not be in the same group again in subsequent rounds. In the end, 2 or 3 such rounds of distributions were played in each village. The final dataset comprises a total of 224 public distributions of which 112 are tar-

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<sup>11</sup>Participants are incentivized to take these distributions seriously since they are informed that the compensation that they stand to receive at the end of the day for their participation in the surveying activities will in fact reflect the results of one of the distribution rounds that they have participated in during the day. See Strauss-Kahn (2018a) for more details on the payment scheme.

<sup>12</sup>See Strauss-Kahn (2018a) for a more in-depth discussion of the targeting and monitoring treatments.



geted and 112 are not; 112 have veto players and 112 do not; and 152 are monitored and 72 are non-monitored.<sup>13</sup>

## 2.3 Estimation Strategy

In this paper, I look at the causal effect of having veto players ( $X$ ) on resource allocation ( $Y$ ) while controlling for a moderating factor, namely targeting instructions ( $M_1$ ), and controlling for monitoring ( $C$ ) (Baron & Kenny, 1986). In equation form, the general causal model is very simple and includes a treatment indicator for veto players ( $X$  with a specific level represented by  $x \in \{0, 1\}$ ), a moderator ( $M_1 \in \{\text{non-targeted}, \text{Targeted}\}$ ), a dichotomous outcome ( $Y$ ) and a control ( $C \in \{\text{non-monitored}, \text{Monitored}\}$ ), where  $X$  may affect  $Y$  directly and/or  $X$  may affect  $M_1$ , which then affects  $Y$ , is:<sup>14</sup>

$$Y = i + \alpha X + \beta_1 M_1 + \beta_2 C + \gamma_1 X \cdot M_1 + \epsilon$$

where  $\alpha$  is the direct effect of having veto players on the decision of the distributor ;  $\beta_1$  is the direct effect of targeting instructions ;  $\beta_2$  is the direct effect of monitoring ; and  $\gamma_1$  is the effect of the interaction of targeting instructions and veto power.

In terms of potential outcomes:

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<sup>13</sup>Due to concerns about the monitoring treatment and to ensure a clear first round of data, the monitoring treatment only started at round 2. In other round, at round 1, the only treatment assigned was for the distribution to be targeted or not targeted.

<sup>14</sup>There are no theoretical reasons to expect the interaction of monitoring with neither the veto nor the targeted treatment to be significant, so I don't include the interaction terms in the general causal model. See (Strauss-Kahn, 2018a).

	<i>NON-VETO</i>		<i>VETO</i>	
	<i>NON-</i>	<i>MONITORED</i>	<i>NON-</i>	<i>MONITORED</i>
<i>TARGETED</i>	<i>MONITORED</i>	Y(0,T,m)	<i>MONITORED</i>	Y(1,T,m)
		Y(0,T,M)		Y(1,T,M)
<i>NON-</i>		Y(0,t,m)		Y(1,t,m)
<i>TARGETED</i>		Y(0,t,M)		Y(1,t,M)

Table 1: Table of Potential Outcomes

$$\begin{aligned}
\alpha &= E\{Y(1, t, m) - Y(0, t, m)\} \\
\beta_1 &= E\{Y(0, T, m) - Y(0, t, m)\} \\
\beta_2 &= E\{Y(0, t, M) - Y(0, t, m)\} \\
\gamma_1 &= E\{Y(1, T, m) - Y(1, T, m)\} + E\{Y(1, T, M) - Y(1, T, M)\} \\
&\quad - E\{Y(0, T, m) - Y(0, t, m)\} - E\{Y(0, T, M) - Y(0, t, M)\}
\end{aligned}$$

### 3 Data

Each respondent is first administered a baseline survey. Then, respondents are administered additional surveys before, during and after each allocation. The survey instruments include both behavioral and attitudinal measures. During the public allocations, enumerators have also collected data about the nature, intensity and content of discussions within each group. Finally, additional qualitative, village-level data about is also gathered during focus groups in which all participants have participated together. In between two rounds, respondents were encouraged to participate in various focus groups. The point of the focus groups was both to limit direct, strategic interactions between group members before the public distribution and to gather more qualitative information about several aspects of the village culture that could

be relevant in explaining further the results of the surveys.<sup>15</sup>

### 3.1 Outcome measures

#### Allocations

The main outcome measured in the public distributions is the number of tokens — among the 27 that are distributed— that each group member receives in a given allocation.<sup>16</sup>

#### Group discussions

Enumerators have also recorded information about who participated in discussions during public distributions.<sup>17</sup> For each public distribution, the enumerators indicate for each group member both whether they have actively participated in the discussion and how much. Enumerators also indicate who participated most in their opinion.

Participation measures also include measures of the content of the discussion. For each individual group member that participated in the discussion, enumerators record whether they mostly argued in their own favor or in favor of other group members. Finally, enumerators also record their opinion about whom among individual group members seemed to influence the elites' decision.<sup>18</sup>

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<sup>15</sup>See Field Manual for more information about the data collection process the questionnaires, and the focus groups.

<sup>16</sup>Social psychology experiments have shown that the divisibility of the windfall affects the way it is allocated in group distributions, and specifically which coalitions are formed (van Beest et al., 2004). I have voluntarily chosen a number of tokens that is not divisible by five in order to force respondents to choose to favor at least one group member in the allocation. In the 224 distributions observed, there are 5 instances in which respondents refused to allocate all the tokens and preferred the equal distribution of 5 tokens to each group members while giving back the 2 extra tokens to the enumerator.

<sup>17</sup>All enumerators were extensively trained to record such information during the recruitment process. And although the process of evaluating the content of a discussion can seem subjective, the pre-tests done on mock discussions show extraordinary consistency in coding across enumerators.

<sup>18</sup>The survey records the language in which the discussion occurred. Most discussions were either in Kiswahili or in Kinyarwanda, both languages in which all enumerators were trained.

## **3.2 Other measures**

### **Attitudes toward group members**

In each round, respondents were also surveyed on their attitudes toward their group members. About each other group member, respondents were asked whether they liked them and whether they considered them a friend. Survey questions about within-group relationships also included several more specific estimations of the level of knowledge, friendship and trust each respondent has for each other group member. The level of knowledge was evaluated with questions about specific informations, such as asking whether the respondent knew the name, the number of children or the location of the house in the village of each other group member. Friendship and trust were assessed using several subjective attitudinal measures as well as questions about friendly and trusting behaviors that the respondent may or may not have regarding other group members, such as whether they would be willing to share a meal, lend them their work tools or let them take care of their kids.

### **Poverty measures**

I distinguish richer from poorer respondents and block randomize groups for distribution surveys based on the baseline surveys. A lot of attention has been given to developing several appropriate, concurring measures of economic and social status. The baseline survey includes questions related to assets, revenues, housing and other objective measures of the economic poverty as well as questions about social relations and connections in order to provide objective measures of social vulnerability for each respondent. These measures have served to create a poverty index specific to each village that was subsequently used to sort the 32 respondents from the general pop-

ulation into two groups. Economic poverty and social vulnerability are also assessed subjectively both by the respondents themselves and by the enumerators in charge of administering the surveys. For these subjective measures, the questions ask whether a respondent’s situation is worse, the same or better than the rest of the village on a given dimension. The same type of questions were used to test the capacity of respondents to assess correctly who were the poorer member(s) in their group.<sup>19</sup>

## Control Measures

All the regressions presented in this article include traditional control measures such as the gender and age of participants. I also include a measure of the pro-poor proclivity of the distributor as a control in all estimations.<sup>20</sup> Heterogeneity in group composition in terms of economic situation is also controlled for using a measure of within-group inequality. When appropriate, I also use survey questions about respondent’s perceptions of the role of the village chief as controls for their expectations about elites’ behaviors as well as questions about their level of involvement in village activities as controls for their baseline level of participation in group activities and group discussions. Finally, after public distributions, respondents are debriefed on their perceptions of the allocation process and its outcome, which allows to assess compliance.

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<sup>19</sup>The high correlation across the various measures of economic poverty and social vulnerability used in this experiment ( $\rho = .65$ ) gives reasonable confidence that the poorer and richer half of the sample were correctly identified both by the PI and the participants. See Strauss-Kahn (2018a) for a more detailed discussion of the poverty measures and the poverty index used in this experiment.

<sup>20</sup>In the baseline survey, respondents also answer questions about their conceptions of fairness in order to assess their preferences for distributive justice (“Which is more just: (1) giving to all the same; (2) giving more to the poor; etc.”). I use the answer that it is “More just to give more to the poor” to this fairness question as an indicator of pro-poor proclivity. Since the same questions are also included in all post-distribution surveys, it is also possible to assess changes in justice norm perceptions pre- and post-treatment within individual respondents.

## 4 Results

In this section, I present the main results for this experiment. For reference, see Strauss-Kahn (2018a) for results on the effect of targeting instructions, community involvement and monitoring on allocation outcomes.

An in-depth discussion of compliance, treatment checks and some pre-treatment descriptive characteristics of the sample population is offered in Strauss-Kahn (2018a). In short, there are no compliance issues in this experiment and the intended main treatment seems to have been correctly understood by participants (error  $\leq 1.8\%$ ). Participants also seem not to have had any problems in identifying the poorer members within their group (error  $\leq 1.5\%$ ). More generally, I have found that more than the provision of targeting instructions and the monitoring by a third-party, it is the fact that other group members are present during the decision-making process that has the most effect on allocation to poor group members. This paper subsequently focuses more specifically on what happens during such public distributions. Finally, another important preliminary observation is that in this experiment all group members tend to receive relatively similar shares of the windfall.<sup>21</sup> The value of the constant in the results presented below strongly suggests that most of the time 5 of the 27 tokens are allocated to each of the five group members and that the distributor’s decision is really mostly about how to allocate the two “extra” tokens.

### 4.1 Main Results

In this experiment, treatment effects are rather small in size, but they are significant, consistent and robust across specifications. The small effect size is not of much

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<sup>21</sup>This observation holds for public distributions specifically. There is more variance in private distributions. See Strauss-Kahn (2018a) for more details.

concern: the stakes in the distributions, while sufficient to motivate the participant to take the simulation seriously, were relatively small.<sup>22</sup> In comparison, one can expect aid targeting to yield bigger effects in size and cause larger, more substantive variations in the shares received by recipients.

### **Effect of Veto**

In Table 2, I present the results for the veto treatment by comparing in public distributions the allocations where the rich had no veto power to the allocations where the rich had veto power.

I find that the veto treatment has a strong significant effect on the share of the windfall allocated to the non-poor that is consistent with traditional bargaining theory. In line with existing theory about veto players, when considering model (2) without interactions, the direct effect of the veto treatment is an increase in what each non-poor group member receives by a positive, significant amount of 0.24 tokens, which represents \$.12 and an increase of almost 5% from their baseline (public distributions with no veto players). In other words, as theory predicts, the more bargaining power a receiver has in a dictator game, the more this player can extract from the distributor. Conversely, the share of the elite decreases significantly by twice as much (\$.24).

When including the interaction of the veto treatment and the targeted treatment in model (3), the share of the elite still increases significantly by 0.38 tokens as a direct effect of the veto treatment. There is a similar, significant decrease in the average share received by the poor (-.15) with the veto treatment. More surprisingly and interestingly, the interaction of targeting instructions and the veto treatment has a

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<sup>22</sup>As mentioned, in each round the participants were allocating the equivalent of the salary for 7 days of work in the fields between five people and that is about what they expected to receive as compensation at the end of the day.

Table 2: Main Effects of the Veto Treatment

	Share of the Poor			Share of the Non-Poor			Share of the Elite		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Targeted	0.26*** (0.06)	0.26*** (0.06)	0.10 (0.09)	0.03 (0.06)	0.03 (0.06)	0.16* (0.09)	-0.59*** (0.11)	-0.59*** (0.11)	-0.49*** (0.16)
Veto		0.01 (0.06)	-0.15* (0.09)		0.24*** (0.06)	0.38*** (0.09)		-0.48*** (0.11)	-0.38** (0.16)
Targeted x Veto			0.32*** (0.12)			-0.27** (0.12)			-0.19 (0.22)
Monitored	0.06 (0.07)	0.06 (0.07)	0.07 (0.07)	0.04 (0.07)	0.04 (0.07)	0.04 (0.07)	-0.20 (0.12)	-0.21* (0.12)	-0.21* (0.12)
Constant	5.63*** (0.21)	5.62*** (0.22)	5.74*** (0.22)	5.30*** (0.22)	5.13*** (0.22)	5.03*** (0.22)	5.23*** (0.40)	5.57*** (0.39)	5.50*** (0.40)
Observations	224	224	224	224	224	224	224	224	224

With controls. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%  
(Public distributions only)



significant, large, positive effect on the amount received by the poor, which increases by \$.16 or more than 5.5% compared to their baseline. The share of the non-poor decreases significantly by a similar amount (\$.13).

In other words, when the distribution is targeted to the poor, an increase in the bargaining power of the non-poor increases not only their own share of the windfall but *also* the share that the poor receives to the detriment of the share that is captured by the elite.

In Table 3, I look more closely at the direct effects of the veto treatment on the average share received by poor group members conditional on the allocation being targeted to the poor or not. I find that the effect of having non-poor veto players is positive and significant when distributions are targeted to the poor. The share received by the poor increases by 0.17 tokens compared to targeted distributions with no veto players. Conversely, in distributions that are not targeted to the poor, the share received by the poor decreases by 0.16 tokens when there are non-poor veto players compared to when there are no veto players. This suggests that the presence of non-poor veto players plays in favor of poor recipients only in targeted distributions. More specifically, in non-targeted distributions, the existence of veto players seems to prompt a transfer from the elite and the poor in favor of the non-poor. In targeted distributions, the existence of veto players prompts a transfer from the elites to the non-poor and an additional transfer from the elites to the poor.

Interestingly, I also find that monitoring has a negative, significant effect (0.21 tokens) on the share that the elites retain for themselves overall. In fact, this effect comes from non-targeted, public distributions, in which monitoring has a positive significant effect on the share that each poor group member receives (\$.10) and a corresponding negative effect on the share that the elites retain (\$.23). On the other hand, monitoring has no significant effect on the allocation when public distributions

are targeted to the poor. These findings possibly point to the role of monitoring as a substitute at best to the provision of targeting instructions.

### **Who Participates in the Public Discussions?**

Now let us look more closely into what happens during the public discussions. In this section, I analyze data about the participation of each group members to the discussion during public allocations. Enumerators have gathered data not only about who participates in these discussions and how much, but also about the nature of each players intervention, namely whether they make arguments about receiving more money themselves or in favor of giving more money to another group member.

In all the tables presented in this section, I control for attendance to village meetings. It is during these public meetings that the village chief makes most decisions regarding the village, including decisions about the allocation of various resources to village members. According to the baseline survey, neither poor nor non-poor village members are used to attending village meetings regularly: 83% of poor village members and 85% of non-poor village members never or rarely attend village meetings. On the other hand, 64% of elites report that they often or always attend such meetings. And during meetings they attend, 74% of elites have the perception that they participate in discussions more than other village members while poor villagers conversely have the perception that they tend to participate less than others (44%) or the same (48%). To the same question, most non-poor village members similarly respond that they participate as much as others (53% ) or less (31%).

In table 4, I present the propensity of poor, non-poor and elite group members to actively participate in group discussions depending on whether the distribution was targeted and whether the non-poor could veto the elites' decision. The first observation that can be made is that on average, with a baseline of about .47, the

Table 3: Main Effects of Veto Conditional on Targeted

	If Not Targeted			If Targeted		
	Poor	Non-Poor	Elite	Poor	Non-Poor	Elite
Veto	-0.16* (0.09)	0.40*** (0.09)	-0.40** (0.16)	0.17* (0.09)	0.11 (0.09)	-0.56*** (0.16)
Monitored	0.19** (0.09)	0.05 (0.09)	-0.46*** (0.17)	-0.05 (0.10)	0.05 (0.10)	0.02 (0.17)
Constant	5.94*** (0.30)	4.96*** (0.30)	5.27*** (0.55)	5.59*** (0.31)	5.27*** (0.33)	5.30*** (0.56)
Observations	112	112	112	112	112	112

With controls. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%  
(Public distributions only)

Table 4: Participation in Public Discussions

	Poor			Non-Poor			Elite		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Targeted	-0.05 (0.05)	-0.05 (0.05)	-0.11 (0.07)	-0.00 (0.05)	-0.00 (0.05)	-0.12* (0.06)	-0.03 (0.07)	-0.03 (0.07)	-0.01 (0.10)
Veto		0.04 (0.05)	-0.02 (0.07)		-0.13*** (0.05)	-0.25*** (0.06)		0.08 (0.07)	0.10 (0.10)
Targeted x Veto			0.12 (0.10)			0.24*** (0.09)			-0.04 (0.14)
Monitored	-0.04 (0.05)	-0.04 (0.05)	-0.04 (0.05)	-0.00 (0.05)	-0.00 (0.05)	0.00 (0.05)	0.03 (0.07)	0.03 (0.07)	0.03 (0.07)
Constant	0.50*** (0.18)	0.47** (0.18)	0.51*** (0.19)	0.04 (0.17)	0.12 (0.17)	0.20 (0.17)	0.50* (0.25)	0.44* (0.26)	0.43 (0.26)
Observations	204	204	204	204	204	204	204	204	204

With controls. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%  
(Public distributions only)

poor tend to participate a lot in discussions in general, as much as the elites and about four times more than the non-poor (.12). Looking at the data more closely, at least one poor group members participated in 78% of discussions, and both of them participated in over 26% of discussions. In 62% of cases, poor group members were actually the members of the group who participated the most in the discussions according to the enumerators. In the meantime, non-poor group members participated in discussions only 49% of the time and in only 8,5% of cases did both of them participate. Similarly, even though the elite was actually in charge of making the allocation, they effectively participated in the discussion only 46% of the time.

Interestingly, when taking into account both the direct effect and the interaction of the treatments in model (3), the results in table 4 show that both the poor and the non-poor tend to participate significantly less in discussions when the windfall is either targeted to the poor (a decrease of -.45 and -.32 percentage points respectively) *or* when the non-poor have veto power (-.37 and -.53 respectively).<sup>23</sup> But on the other hand, when the windfall is both targeted to the poor *and* the non-poor have veto power, then the discussions are most animated with an increase of .57 and .69 percentage point in the propensity of at least one of the two poor or non-poor group members participating in the discussion respectively.

Insofar as enumerators have also recorded whether the participants were discussing their respective wealth and incomes or not, this experiment also provides some information as to the contents of the discussions. The arguments made by participants were much more often money-related during targeted distributions (93% of the time) than during non-targeted allocations (76%). By contrast, there is no observable dif-

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<sup>23</sup>The fact that the effect size seems to be bigger for the treatment that favors them (the targeted treatment for the poor and the veto treatment for the non-poor respectively) could be interpreted as an indication that these treatments effectively “empower” the group that they relate to, insofar as they would then have less need to participate in discussions.

ference in the contents of discussions between distributions with veto players and distributions without veto players. Now to get a better sense of what it is specifically that the non-poor talk more about in targeted, vetoed distributions, let us look more closely at the type of arguments that poor, non-poor and elite group members make respectively when they participate in discussions. Table 5 shows the propensity of group members to argue for themselves while table 6 presents the propensity of group members to argue in favor of others.

Interestingly, I find that poor group members tend to speak mostly in favor of others rather than themselves, including during targeted distributions (in which typically they should feel entitled to speak for themselves). Overall, baseline results suggest when they intervene in public discussions, poor group members almost never argue in favor of receiving more money for themselves (.0) but rather argue in favor of others (.47), while by contrast both elites and non-poor group members tend to make their case selfishly more often than not. More specifically, the elite tend to argue only in their own advantage (.24) when they choose to intervene and never in favor of others (.0), while on the other hand the non-poor can intervene both in their own interest (.32) and in favor of others (.16).

While table 5 suggests that the various treatments have no effect on the propensity of group members to advocate selfishly for a share of the windfall, table 6 shows on the other hand that the propensity of non-poor group members to advocate for others increases significantly by almost 20 percentage points when the windfall is targeted to the poor.<sup>24</sup> More precisely, this effect comes from the interaction of the veto and the targeted treatment. Looking at model (3), it is during discussions for a windfall that

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<sup>24</sup>Even though there is no way to tell from the data that was collected which other group member benefits from the advocacy when a group member is reported to “speak in favor of another”, there are good reasons to think, in this instance, that non-poor group members tend to advocate for poor group members.

Table 5: Argue in Favor of Oneself

	Poor			Non-Poor			Elite		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Targeted	0.05 (0.04)	0.05 (0.04)	0.01 (0.06)	0.02 (0.05)	0.02 (0.05)	0.04 (0.07)	-0.08 (0.06)	-0.08 (0.06)	-0.13 (0.09)
Veto		0.00 (0.04)	-0.03 (0.06)		0.05 (0.05)	0.07 (0.06)		0.03 (0.06)	-0.01 (0.09)
Targeted x Veto			0.08 (0.09)			-0.05 (0.09)			0.09 (0.13)
Monitored	0.01 (0.04)	0.01 (0.05)	0.01 (0.05)	-0.00 (0.05)	-0.00 (0.05)	-0.00 (0.05)	-0.02 (0.07)	-0.02 (0.07)	-0.02 (0.07)
Constant	-0.01 (0.15)	-0.01 (0.15)	0.01 (0.16)	0.37** (0.15)	0.32** (0.16)	0.31* (0.16)	0.27 (0.21)	0.24 (0.22)	0.27 (0.23)
Observations	150	150	150	150	150	150	151	151	151

With controls. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%

(Public distributions only)

Table 6: Argue in Favor of Another Group Member

	Poor			Non-Poor			Elite		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Targeted	-0.09*	-0.09**	-0.06	0.09**	0.09**	0.03	-0.02	-0.02	-0.04
	(0.05)	(0.05)	(0.07)	(0.04)	(0.04)	(0.06)	(0.07)	(0.06)	(0.09)
Veto		0.14***	0.18***		0.02	-0.05		-0.16**	-0.19**
		(0.05)	(0.07)		(0.04)	(0.06)		(0.06)	(0.09)
Targeted x Veto			-0.07			0.13*			0.05
			(0.09)			(0.08)			(0.13)
Monitored	0.02	0.02	0.02	0.02	0.02	0.02	-0.02	-0.02	-0.02
	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(0.07)	(0.07)	(0.07)
Constant	0.58***	0.47***	0.45***	0.17	0.16	0.20	-0.18	-0.07	-0.05
	(0.16)	(0.16)	(0.17)	(0.14)	(0.14)	(0.15)	(0.23)	(0.23)	(0.23)
Observations	203	203	203	203	203	203	203	203	203

With controls. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%

(Public distributions only)



is *both* targeted and when they have veto power that non-poor group members tend to advocate in favor of others the most, with a significant increase of 26 percentage points.

Altogether, these findings paint the following picture: it is not the poor themselves that voice their entitlement when resources are targeted to them. It is rather the non-poor that speak up in favor of the poor when the windfall is targeted to the poor, and especially when they have some power over the elites' decision. This is also consistent with the findings from table 2 that the poor tends to receive a bigger share of the windfall in targeted distributions with veto players.

## Friends and foes

In this section, I use the rich data collected for each villager about how much each person knew, liked, and trusted each other to look at various configurations at the group level in terms of whom is friends (resp. enemies) with whom. I also use data from the baseline survey about participants' social networks in the village to control for sociability at the respondent's level.

The villages in our sample are relatively small villages, and the baseline survey confirms that everyone knows everyone quite well. Most respondents are even able to correctly answer questions that were quite specific about other villagers and group members such as their name, the age of their children, etc.<sup>25</sup> While there is no

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<sup>25</sup>By looking more specifically at who seeks help from whom (and who offers help to whom) in case of trouble, the baseline survey paints an interesting picture of relationship networks and social safety nets within the sampled villages. Overall, neither neighbors nor religion are strong sociability networks in those villages. That said, the elite's sociability seem to differ from the sociability of the poor and the non-poor. For example, 100% of the elite expect that they'll always be offered help by someone in case of trouble while some proportion of the poor (8%) and of the non-poor (6%) declare that no one in the community would offer them assistance. While the poor and the rich mostly seek and receive help from their family (78%), the elites' safety nets depends more on their friends (85%) than their family (53%). While the rich and the poor do not expect assistance from the chief (20%) or from elites (7%), the elites rely on those networks much more (58% and 26% respectively). Finally, while the rest of the village declare they would never seek nor expect to receive assistance

variation in whether people declare that they “like” each other, there is substantive variation in whether participants consider other group members as friends or not.<sup>26</sup> Interestingly, friendship is not necessarily reciprocal: overall, when a group member considers another group member a friend, the reverse is also true only less than 70% of the time. Within each group, I look at whether the poor, the non-poor and the elite are mutual friends or mutual foes.

In Table 7, I find that for the poor being friends with the non-poor has a significant, positive effect on the share that they receive, while being friends with the elite does not have much effect. More specifically, when distributions are not targeted to the poor, the poor benefit from being friends with the non-poor: the share of the windfall that they receive significantly increases by .52 compared to when they are not friends. Looking at the share that the non-poor receives and that the elite keeps, it is clear that the transfer goes from the elite to the poor and not from the non-poor to the poor. In other words, the effect of friendship between the non-poor and the poor is that it compels the elite to give more to the poor *in addition* to what they already give to the non-poor. Separately, the direct effect of targeting instructions is still positive and significant (.19): in other words, it has a positive effect on the share that the poor receive even when the poor and the non-poor are not friends. On the other hand, the interaction of targeting instructions and friendship seem to completely cancel out the benefits of friendship, in terms of the extra share of the windfall that the poor would have received / that the elite would have given out.

Interestingly, the fact that the non-poor have veto power only positively and significantly affects the share that the poor receives when the windfall is targeted to the poor (.22), in which case it has an even bigger effect size if the poor and the

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from NGOs (.4%), the elites are much more likely to use such means (8%).

<sup>26</sup> Arguably, the fact that everyone systematically declares that they “like” other villagers suggest the existence of a strong social norm that promotes a form of unity in the village.

Table 7: Main Effects of Mutual Friendship on Resource Allocation

	Elite x Poor			Elite x Non-Poor			Non-Poor x Poor		
	Poor	Non-Poor	Elite	Poor	Non-Poor	Elite	Poor	Non-Poor	Elite
Targeted	0.19 (0.12)	0.11 (0.12)	-0.56*** (0.21)	0.27** (0.11)	0.16 (0.11)	-0.81*** (0.20)	0.19** (0.09)	0.14 (0.10)	-0.64*** (0.17)
Veto	-0.12 (0.11)	0.30*** (0.11)	-0.25 (0.20)	-0.06 (0.11)	0.30*** (0.11)	-0.39** (0.20)	-0.05 (0.09)	0.31*** (0.10)	-0.44** (0.17)
Targeted × Veto	0.18 (0.16)	-0.12 (0.16)	-0.25 (0.28)	0.15 (0.15)	-0.21 (0.16)	0.01 (0.28)	0.22* (0.13)	-0.23* (0.14)	-0.06 (0.25)
Elite x Poor:									
Friends	0.04 (0.14)	0.06 (0.15)	-0.16 (0.26)						
Friends × Targeted	-0.20 (0.19)	0.10 (0.20)	0.16 (0.35)						
Friends × Veto	-0.08 (0.19)	0.04 (0.19)	-0.04 (0.35)						
Friends × Targeted × Veto	0.31 (0.27)	-0.21 (0.28)	-0.06 (0.49)						
Elite x Non-Poor:									
Friends				0.23 (0.14)	0.05 (0.14)	-0.51** (0.25)			
Friends × Targeted				-0.45** (0.19)	-0.02 (0.20)	0.91** (0.35)			
Friends × Veto				-0.25 (0.18)	0.01 (0.19)	0.40 (0.34)			
Friends × Targeted × Veto				0.34 (0.26)	0.14 (0.27)	-0.86* (0.48)			
Non-Poor x Poor:									
Friends							0.52*** (0.17)	-0.00 (0.18)	-1.00*** (0.32)
Friends × Targeted							-0.58** (0.23)	0.06 (0.24)	1.00** (0.42)
Friends × Veto							-0.66*** (0.22)	0.12 (0.23)	1.08*** (0.40)
Friends × Targeted × Veto							0.64** (0.30)	-0.01 (0.32)	-1.25** (0.56)
Constant	5.77*** (0.24)	5.07*** (0.24)	5.32*** (0.43)	5.75*** (0.24)	4.99*** (0.25)	5.49*** (0.44)	5.67*** (0.22)	5.11*** (0.23)	5.46*** (0.41)
Observations	221	221	221	218	218	218	220	220	220

With controls. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%  
(Public distributions only)

non-poor are friends (.64). But if the windfall is not targeted, even if the poor and the non-poor are friends, the poor will still not benefit from the non-poor having veto power (-.66).

In Table 8, I find that the poor seem to benefit in general from situations where some subgroups are enemies with one another, insofar as their baseline share is systematically higher than in the various configurations of friendship. More specifically, the poor benefits most from situations where the elite and the non-poor are enemies. In situations where the elite and the non-poor are mutual enemies, the poor receive a significantly bigger share of the windfall when the distribution is either targeted to them (.60) or when the non-poor have veto power over the elites' decision (.52). In other words, in situations in which the elites and the non-poor are enemies, the windfall doesn't have to be specifically targeted to the poor for the poor to benefit from the fact that the non-poor have veto power.<sup>27</sup>

## 5 Discussion

In this section, I discuss the conclusions that can be drawn from these results about the nature of the involvement of the non-poor in public distributions and the ways in which they can contribute to effective targeting the poor or not. I also offer a discussion of the external validity of these findings and of how they can shed light more broadly on our understanding of aid targeting.

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<sup>27</sup>I have run additional analysis for various other specifications of friendships and enmities between subgroups. I find that no other specifications yield significant, robust results, which in turn suggests that the story is really about coalition formation and subgroups being allies or not with one another.

Table 8: Main Effects of Mutual Enmities on Resource Allocation

	Elite vs Poor			Elite vs Non-Poor			Non-Poor vs Poor		
	Poor	Non-Poor	Elite	Poor	Non-Poor	Elite	Poor	Non-Poor	Elite
Targeted	-0.03 (0.11)	0.25** (0.11)	-0.39* (0.20)	-0.16 (0.12)	0.27** (0.12)	-0.21 (0.22)	-0.02 (0.12)	0.19 (0.13)	-0.25 (0.23)
Veto	-0.15 (0.11)	0.29*** (0.11)	-0.23 (0.19)	-0.30*** (0.12)	0.37*** (0.12)	-0.12 (0.21)	-0.20* (0.12)	0.29** (0.12)	-0.07 (0.22)
Targeted × Veto	0.42*** (0.16)	-0.15 (0.16)	-0.64** (0.28)	0.52*** (0.16)	-0.16 (0.17)	-0.74** (0.30)	0.37** (0.17)	-0.13 (0.17)	-0.59* (0.31)
Elite x Poor:									
Foes	-0.05 (0.13)	-0.01 (0.13)	0.17 (0.24)						
Foes × Targeted	0.35* (0.19)	-0.25 (0.19)	-0.27 (0.34)						
Foes × Veto	-0.01 (0.19)	0.15 (0.19)	-0.22 (0.35)						
Foes × Targeted × Veto	-0.30 (0.26)	-0.15 (0.27)	0.89* (0.47)						
Elite x Non-Poor:									
Foes				-0.26** (0.13)	0.03 (0.13)	0.39* (0.24)			
Foes × Targeted				0.60*** (0.18)	-0.27 (0.19)	-0.61* (0.34)			
Foes × Veto				0.35* (0.18)	-0.15 (0.19)	-0.29 (0.34)			
Foes × Targeted × Veto				-0.58** (0.26)	0.06 (0.26)	0.90* (0.47)			
Non-Poor x Poor:									
Foes							-0.13 (0.13)	-0.05 (0.13)	0.46** (0.23)
Targeted × Foes							0.20 (0.18)	-0.04 (0.19)	-0.43 (0.33)
Foes × Veto							0.10 (0.18)	0.07 (0.19)	-0.39 (0.33)
Foes × Targeted × Veto							-0.10 (0.26)	-0.17 (0.26)	0.57 (0.46)
Constant	5.80*** (0.23)	5.06*** (0.24)	5.32*** (0.42)	5.81*** (0.23)	5.10*** (0.23)	5.23*** (0.41)	5.85*** (0.23)	5.09*** (0.24)	5.11*** (0.42)
Observations	222	222	222	221	221	221	222	222	222

With controls. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%  
(Public distributions only)

## 5.1 Peer-Pressure, Bargaining & Coalition Formation

In this experiment, all indications point to the idea that bargaining dynamics are at play during public distributions and not simply peer-pressure. First and foremost, there are multiple consistent observations that group members did actively discuss with one another during the distributions. Second, there is a significant and large difference between distributions with veto players and without veto players. The existence of veto players should only affect allocation outcomes if bargaining dynamics are at play. Finally, the absence of strong, significant, or consistent effect of monitoring suggests that there is no peer-pressure from the enumerator either.

The abundant qualitative and quantitative evidence about the degree and nature of participation of various group members in discussions further suggests that the bargaining dynamics at play are complex. More specifically, some specific patterns of friendships and enmities across subpopulations within groups have strong, significant effects on allocation outcomes while others do not. This points to the possibility that in public settings aid targeting prompts coalition bargaining. Interestingly, the poor seem to benefit more from conflict configurations—in which some sub-groups within the recipient community are enemies with one another—than from friendships and collusions.

Another element that supports the hypothesis that peer-pressure plays little role in this experiment is the fact that participants' norms of justice don't seem to affect allocation decisions either. The story here does not seem to be either about strong prosocial norms of justice among non-elite group members nor about expectations that the elites will respect targeting instructions. In fact, according to the baseline survey, communities in the sample do not expect their elites to favor the poor in general. On the contrary, all community members, including the poor themselves,

seem to agree on the fact that it is not the role of the elites to do so. In that sense, even if there is an effect of peer-pressure, it shouldn't play in favor of the poor. Similarly, non-elites are not more prosocial than elites in their preferences. On the contrary, it is in fact the elites that have more pro-poor preferences than the rest of the community.<sup>28</sup> And if anything, receiving targeting instructions seem to actually make group members less prone to pro-poor attitudes.<sup>29</sup>

Overall, all these observations suggest that it is not sufficient to make the targeting process transparent to ensure that elites will be held accountable of their decisions by their population. Since community involvement doesn't operate simply by peer-pressure, not only the degree but also the nature of the involvement of the community at large in the targeting process is going to be essential to ensuring that aid resources effectively reach the target group. In particular, since the outcome of an aid program may depend crucially on the type of coalition that emerges, aid agencies may significantly improve the effectiveness of their interventions by taking more closely into account the existing relationships between the elites, the target group and the rest of the community.

## 5.2 Empowerment of the Poor

One limitation of this study is that the veto treatment is not tested on the poor. Ideally, one would have wanted to know the effect of poor group members having

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<sup>28</sup>As discussed in Strauss-Kahn (2018a), there is not as much heterogeneity in the communities in the sample in terms of prosocial preferences, attitudes and behaviors as one could have expected. According to the baseline survey, 42% of non-elites and only 26% of elites originally have pro-poor preferences. And similarly, only 41% of the poor in our sample have pro-poor preferences.

<sup>29</sup>This is also consistent with observations from a previous study that targeting instructions by themselves seem to benefit both the poor and the non-poor (Strauss-Kahn, 2018a). In other words, the provision of targeting instructions seem to prompt elite into altruistic dynamics in general, by capturing a lesser share of the windfall for themselves, but not to targeting the poor in particular. It is something else about the interactions within the group during public discussions that yields benefits for the poor or not.

veto power over the elites' decision.<sup>30</sup> As a result, the message that comes out of this study about whether targeting can actually contribute to empower the poor is somewhat ambiguous.

According to the baseline survey, the poor participate little in village meetings and discussions, and in any case they participate less than elites or non-poor village members. In that respect, one remarkable aspect of the experiment presented in this study is that poor group members participated a lot in the public discussions, even after controlling for their propensity to participate in village discussions in general. In that sense, one could argue that the process of targeting seems to provide the target group with a "voice" that they would otherwise not have within their community. However, it doesn't follow that they use this opportunity to participate in decision-making in a way that effectively benefits them.

According to all actors, including poor group members themselves, while poor group members participate actively in group discussions their actual influence on the allocation outcome is limited. Only 10% of poor participants report that they think they have had influence on the distributor's decision in their group, and this perception is shared by the enumerator watching over the group in only 5% of cases. By comparison, 20% of non-poor respondents report that they personally influenced the elite's decision. And according to enumerators' estimations, in 77% of the cases it is the non-poor that participated most in the discussions and in 73% it is also them that had the most influence on the elite both in targeted and non-targeted settings. Interestingly, self-perceptions about participation are not correlated with the enumerators' observations. When asked about their own participation in their

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<sup>30</sup>For power considerations considering sample size, I had to choose whether to look at non-poor veto players or poor veto players. I have eventually chosen to look at non-poor veto partly out of conviction that the poor have little power in the community in general and so that giving them veto power would not yield any significant difference in allocations.



group's discussions, 38% of the poor say that they participated very little in the discussions (resp. only 28% of the non-poor) while 35% of the non-poor report that they participated a lot (resp. only 28% of the poor).

Two other striking observations about the behavior of poor group members in this experiment is that the poor seem to participate less in discussions during targeted distributions (49%) than during non-targeted distributions (55%), and that when they participate in discussions in general they seem to advocate in favor of other group members much more than for themselves.<sup>31</sup> This finding can seem counter-intuitive at first and somewhat hard to interpret. Yet, something similar can be observed regarding the non-poor: they participate more in discussions when they have veto power than when they do not. Sociological studies have long established that real power does not need to be expressed or enforced (Weber, 1965). In this experiment, it could be the case that when group members have actual influence, their legitimacy is self-evident and they actually do not have to talk as much to defend their self-interest. In a sense, this would also be consistent with the fact that elites—who are in a position of power—are speaking less than all other group members.

### 5.3 The Role of the Non-Poor

Overall, what this study reveals is that it is non-elite group members that participate in the discussions actively and efficiently. Survey results further suggest that in doing so, they try to secure a share of the windfall for themselves, but that in some contexts they can also advocate in favor of poor group members. In both cases, they seem to have influence in their bargaining with the elite and to affect the allocation outcome.

This suggests that aid agencies should pay more attention to the role of community

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<sup>31</sup>In the same time, elites suggest that they are more susceptible to be influenced by group members during non-targeted distributions. Elites report that other group members have had a lot of influence on their decision in 54% non-targeted allocations but only in 40% of targeted ones.

members who are usually formally excluded from targeted aid programs, insofar as they can turn out to be powerful, influential groups in the recipient community long before and long after the aid distribution has occurred.

There is no existing theoretical explanation in bargaining theory for the increase in the share of the windfall received by the poor—who have little to no bargaining power—when the non-poor have veto power. Yet the effect is large, robust and consistent across specifications. The non-poor seem to advocate in favor of the poor even though they don't have particularly prosocial preferences. It could be that this interaction effect of targeting instructions and the bargaining environment has something to do with minimal group theory. Targeting instructions, by selecting some and excluding others as intended recipients of the windfall, would contribute to crystallizing a specific bargaining environment in which the elite, the poor and the rich are unitary actors. There is limited evidence in support of this argument in the fact that targeting instructions and public treatment have a negative, significant effect on the within-group variance of the share allocated to the rich and to the poor.<sup>32</sup> This is also consistent with an interpretation of the results about friends and foes in terms of collusions, rivalries and coalition-making. In contexts where they are foes of the elite, or to a lesser extent when they are friends with the poor, the non-poor contribute to counter-balance the power of the elite and prevent them from capturing the aid windfall for themselves.

## 6 Conclusion

This paper sheds light on the importance of an actor in targeting that is usually largely overlooked. Targeting is a double-edged sword (Duffield, 1996): there is always

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<sup>32</sup>See additional results as well as Lavergne & Strobel (2004) and Engelmann & Strobel (2006) on maximin and inequality in group allocations.

a group that is not targeted. In this study, it is the non-poor in distributions where the poor is targeted. This group plays an important role in fostering or hindering the effective targeting of the poor. . In doing so, I hope to contribute to better understanding of community-level variation in the outcomes of targeted aid programs and to foster their improvement.

The results suggest that public dynamics in targeting are bargaining more than peer-pressure. In particular, in some contexts, the non-poor can actively participate in discussions to help the poor and this seems to have a decisive effect. This is particularly true when poor and non-poor are allies or when the non-poor and the elites are at odds. This suggests a logic of village politics that is akin to balance of power: the non-poor can balance the power of the elites and limit their ability to capture aid windfalls.

On the other hand, the results of this paper also contribute to debunk a lot of traditional assumptions and long-held ideas in the aid world. In particular, I find little to no support for the role of monitoring in this study. As a conclusion, I intend in further work, to explore the possibility of offering a theory of targeting based on these findings.

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