

OUTSIDE IN
TARGETING AID WITHIN COMMUNITIES

Camille Strauss-Kahn

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Abstract

Outside In - Targeting Aid Within Communities

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In this volume, I present a collection of three articles that are representative of my research on the targeting of humanitarian & development aid. These papers focus on highlighting the role of non-targeted, non-elite community members in fostering or hindering the process of aid distribution to vulnerable community members.

In the first paper, “[Allocating Resources To The Poor: The Effects of Targeting Instructions, Community Involvement and Monitoring](#)”, I use a lab-in-the-field experiment to examine resource allocation at the micro-level. More specifically, I study how small groups within rural communities in eastern Democratic Republic of the Congo — each composed of elites, poor, and non-poor village members — decide to share money among themselves. In a dictator-game like setting, I vary whether the groups are provided with instructions to target the poor, whether the decision-making process is private or public, and whether it is monitored by a third-party or not. I find that (1) by themselves, instructions to target the poor seem to actually benefit both the poor and the non-poor, but that (2) the effectiveness of targeting instructions in reaching poor group members is largely moderated by the presence of community members during the decision-making process, while (3) by contrast, monitoring does not contribute much to the effective allocation of resources to the poor.

In the second paper, [“Inside & Out: The Role of the Non-Poor in Targeting Resources to the Poor”](#), I use a similar experimental set-up to study further the nature of the community dynamics that affect the allocation of resources to the poor. More specifically, I look at the role of non-poor, non-elite community members in influencing how elites choose to allocate resources to the poor. I find evidence that (1) community effects have to do with bargaining dynamics more than peer-pressure; (2) non-elite, non-poor members of the community have an significant role in fostering the allocation of resources to the poor, and that (3) their influence on resource allocation depends crucially on existing alliances or rivalries between various group members.

Finally, in the third paper, [“Is Bigger Always Better? How Targeting Bigger Aid Windfalls Affects Capture and Social Cohesion”](#), co-authored with Laura Paler & Kohran Koçak, I model the provision of targeting instructions as enforcing a bargaining environment in which three groups - the target group, the elites, and the excluded group - compete over the aid windfall. I predict that success in aid targeting depends primarily the size of the windfall, the relative influence and the historical relationships between these three groups. Poor, vulnerable groups are more efficiently targeted in environments in which the elites and the excluded group are rivals, as they will then both prefer for the windfall to be allocated to the target group rather than for it to be captured by one another. I provide support for these predictions using a regression discontinuity design and original survey data from an aid program implemented in Aceh, Indonesia.

With these three articles, I aim at providing a substantive theoretical and empirical contribution to the growing literature on aid targeting effectiveness by bringing light to the role in the targeting process of a part of recipient communities that is otherwise largely overlooked, namely all those community members that are both in the community, yet left out of targeted aid programs.

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List of Acronyms

AfDB African Development Bank

BRA Badan Reintegrasi-Damai Aceh

CBT Community-Based Targeting

CDD Community-Driven Development

DFID Department for International Development (UK)

DRC Democratic Republic of the Congo

EC European Commission

ECHO European Commission Humanitarian Aid Office

EMOP Emergency Operation

EU European Union

FAO Food and Agricultural Organization (UN)

FIC Feinstein Institute Center

GAM Gerakan Aceh Merdeka (a.k.a. Free Aceh Movement)

LIST OF ACRONYMS

GDP Gross Domestic Product

GoS Government of Sudan

IDP Internally Displaced Person

KDP Kecamatan Development Program

MSF Medecins Sans Frontieres

NFI Non-Food Item

NGO Non-Governmental Organization

OCHA Office for the Coordination of Humanitarian Affairs (UN)

ODA Official Development Assistance

OECD Organization for Economic Cooperation and Development

UN United Nations

UNDP United Nations Development Program

UNHCR United Nations High Commissioner for Refugees

UNICEF United Nations Children and Education Fund

US United States

WB World Bank

WFP World Food Programme

Acknowledgments

Should I readily confess what many of you already suspect? I have in fact finished writing up my dissertation several years ago, and since then I have only been working on this acknowledgments section. It turns out that this has been one of the most important, overwhelming, yet difficult part for me to write.

It is really important obviously since it is the first thing that many people are going to look at. And probably in fact the only thing that most people will ever read from this volume.

It is also overwhelming as it prompts me to think back and consider all the love and support I have received from my loved ones over the past ~~5 years~~ ~~6 years~~ 7 years. It has been a long time indeed, and a lot has happened, and I am infinitely grateful to all of you who have been there for me throughout the hardships of writing and of life.

Lastly, it is difficult because I have tried hard to think of what would be appropriate to say, and in the end I couldn't quite figure it out. But I guess most of you already know that I am not appropriate most of the time anyway, so it doesn't matter that much. You also know that I am rarely concise, so I won't try to be.

My first thoughts and my first thanks go to my small tribe of close friends and family for their unconditional love and support. You know how important you are to me. Yet, I'll tell you again anyway.

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Preface

Targeting is simply the task of ensuring that assistance reaches people who need it, when and where they need it, in an appropriate form, in appropriate quantities, and through effective modalities. Conversely, it also aims to ensure that aid does not flow to people who do not need it.

BARRETT, *Food Aid After Fifty Years* (2006)

All foreign aid to developing countries targets a particular recipient. At the country-level, donors select which government receives official development assistance (ODA) or not. For example, in 2015 the United States chooses to promote its foreign policy interest by funding Nigeria over Mali in its fight against Boko Haram (US Department of State, 2015). In the same time, the World Food Programme (WFP) has decided to direct its food assistance within Nigeria to highly food insecure young children in the North-Eastern part of the country (Brown, 2016). At the sub-national level, aid agencies and NGOs target regions within countries, communities within regions, households or even individuals within communities. Foreign aid cannot – and is not intended to – reach everyone: a selection must occur as to who receives it.

Over the past 25 years, donors have increasingly chosen to target local aid distributions to specific groups that are considered most in need or *vulnerable* within recipi-

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ent communities. Targeting the poorest, most vulnerable individuals or households is now the norm for humanitarian and development aid and the international standard when selecting beneficiaries of foreign aid. According to conservative calculations, the World Bank's commitments for such community-level development projects targeted at the vulnerable has gone up from \$325 million in 1996, to \$2 billion in 2003, to \$7 billion in 2008, to nearly \$15 billion in 2014.¹

While there is general consensus that targeting those most in need is necessary to allocate scarce resources, empirical evidence from evaluations of targeted aid projects suggests that the outcomes of these programs are mixed at best. They vary significantly both in terms of whom ends up benefitting from the aid windfall, and how this distribution affects social cohesion in the recipient community. Results range from cases where aid reaches the designated beneficiaries effectively and exclusively to cases where it is entirely diverted by some other group. Likewise, there are communities that are satisfied with the outcome of the targeting program, and others where the process of distributing to some and not others generates tensions and competition up to the point of creating violent conflict (Young & Maxwell, 2009, de Sardan et al., 2014, de Sardan, 2014).

It is not surprising *per se* that giving goods to some and not others within the same community has the potential to yield adverse outcomes that are either conflicting with the objectives of aid targeting in terms of efficiency (by diverting aid away from the target group) or with the objectives of humanitarian and development programs in terms of protection (by creating tensions in the recipient community between those who get something and those who do not). Yet, as with foreign aid programs in

¹These estimations have been calculated using World Bank project data reported by AidData.org. I have included only spending earmarked as development projects and exclusively in sectors that can be identified as community-level assistance. Notably, I have excluded budget-support as well as public construction projects.

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general, targeted aid projects were launched long before there was compelling theory, or compelling evidence, that proved they could work. The concern that aid be utilized by those who most need it is a legitimate one. But it is yet to be established how aid agencies can design programs to ensure this.

In my research, I have studied this variation in the outcomes of targeted humanitarian and development assistance projects in which aid is distributed at the local level in the form of divisible goods such as food, cash or objects. In articles not presented in this volume, I show that while targeting practices are myriad (depending on the context in which they are implemented and the choices that were made in the design of the aid program), targeting outcomes have patterns. Yet, I also show that these patterns in the levels and types of aid diversion yielded by targeted distributions and in their broader social, political and economic consequences do not depend neither on the design nor on the context of intervention. In the collection of three papers presented in this volume, I argue to the contrary that the observed variation is primarily explained by within-community dynamics, and more specifically by the competition between various groups. In doing so, I offer a much needed theory of targeting in which my main original contention is that it is specifically the role of those who are *not* targeted to receive assistance that is paramount in explaining targeting outcomes.

Targeting is “a double-edged sword” (Barrett, 2006). By selecting some and not others as intended beneficiaries, targeting a windfall necessarily creates an outgroup. The role of this outgroup in fostering or hindering the efficiency of targeted distributions has been largely ignored. Yet, it is a part of the recipient community that is both inside (the community) and out (of the aid program) and typically has influence on resource allocation. By definition the outgroup is the non-targeted: it is the rich to the poor, the strong and powerful to the weak and vulnerable. In practice,

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outgroups of aid targeting have many faces: traditional leaders, armed groups, host communities, those not or less affected by conflict, droughts, floods,... And among them, some are involved in aid distribution processes (typically local elites) while the rest is not, henceforth referred to as *the excluded group*. I contend that depending on their historical relationships, their respective influence and the stakes of the distribution, competition between these elites and the excluded can result in aid capture by one or the other if one is significantly stronger than the other, or by both if they get along well enough to collude, or in the actual distribution of the aid windfall to the target group if they are both strong but don't get along with one another.

Literature Review

My research builds on a substantive theoretical and operational literature on aid effectiveness and targeting. In the early 2000, Burnside & Dollar (2000) have presented evidence that at the macro-level foreign aid has little benefits to recipient populations of developing countries that had poor policies. This vastly influential paper has durably instilled doubt among economists, political scientists and practitioners on the effectiveness of foreign aid. The potential of aid interventions for adverse effects has since been the object of growing attention (Easterly et al., 2004). At the country-level, studies have shown that like for other valuable resources there is a potential for capture of foreign aid (Collier, 2006, Morrison, 2012), that foreign aid can reinforce bad governance and the lack of accountability of authorities toward their population (Brautigam & Knack, 2004, Moss et al., 2006, Wright, 2009), that it can induce rent-seeking behaviors and corruption (Svensson, 2000a, Djankov et al., 2008, Bueno de Mesquita & Smith, 2009, Grossman, 1992), that aid shocks can induce violence (Nielsen et al., 2011), and that assistance can also create a form of dependency

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among the recipients (Brautigam, 2000, Knack, 2001).

Practitioners have long assumed that it was the design of a targeted intervention that mattered most in terms of fostering good outcomes (Sharp, 2001). Throughout the 1990s, aid workers have experimented with varying some elements of interventions' design looking for best practices and "the right way" to do aid (Taylor & Seaman, 2004).² This belief in the existence of an ideal, "one-size-fits-all" targeting design is still very much alive today (Slater et al., 2009). Many models of aid provision focus their conceptual understanding of aid dynamics on the principal-agent relationship between a donor - the aid agency - and a recipient - the community, envisioned as a unitary actor (Paul, 2006a). And as a result, experts often tend to recommend relying on monitoring to solve targeting errors, which are considered as nothing more than residual outcomes (Killick, 2004).

However, there is growing evidence since the 2000s that aid interventions targeted at the local level are in fact fraught with all the same problems than foreign aid in general, and that adverse outcomes in targeted aid programs are a pervasive problem. Various regional or program-level studies, in particular by the Feinstein Institute Center (FIC) in recent years, provide suggestive evidence that aid project evaluations tend to under-report targeting errors, and that adverse outcomes in aid programs may actually stem from complex social dynamics within recipient communities.³ A new trend has thus started to develop among practitioners and eventually in the literature (Jaspars & Maxwell, 2008).

²Most notably, many have argued that conditional aid would work better than non-conditional aid, that women recipient were less likely to capture aid windfalls for themselves, that transparency would increase the accountability of the community, that community-based targeting would be empowering. However, there is still limited to no compelling evidence that any of these specific schemes yields better outcomes. See for example Mansuri & Rao (2003).

³See primarily the series on Targeting in Complex Emergencies in Columbia by Frize (2008), in Ethiopia by Jaspars & Maxwell (2008), in Darfur by Maxwell & Burns (2008), and in South Sudan Young & Maxwell (2009). See bibliography for additional work by FIC.

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In their efforts to improve targeted aid interventions, experts have started to take into account characteristics of recipient communities rather than focusing solely on the role of the aid agency and the nature of the intervention (Alatas et al., 2012a, Banerjee et al., 2015). This new trend in targeting literature and practices has arguably developed in two stages. First, the aid community has started to increasingly think about how to properly define the target group. More specifically, aid agencies have started considering the difference between objective and subjective notions of poverty, and taking into account their economic, social, and political correlates. This has led to the use of more refined targeting criteria with a historical shift from using the concept of poverty to using the concept of vulnerability as a basis for entitlement to assistance (Jaspars & Shoham, 1999a, Pritchett et al., 2000, Narbeth, 2001).⁴

More recently, scholars and practitioners have also started to take more into account the role of another important part of recipient communities, the elites (Platteau, 2004a, Fritzen, 2007, Dutta, 2009, Alatas et al., 2013a, Platteau et al., 2014). My work, and in particular the three papers presented in this volume, follows in the path such studies while building on my own experience in the field as a humanitarian worker and as a researcher. In addition to the target group and the elites, I bring light to the role of yet another part of recipient communities, namely all the other members of the community at large. These are the people who live with or around the elite and the target group, yet are both inside the community and formally out of the aid program. I believe that one needs to understand their role and how they

⁴Semantically, the difference between these two notions in lay language may seem thin. But it has several important practical consequences. The idea behind the notion of vulnerability—as opposed to poverty—is that what puts people at risk of not being able to take care of themselves, and hence what justifies external assistance, are not just economic factors. It can also be political, social, or physiological factors (Morduch, 1994). In practice, the shift from poverty to vulnerability has led for example to a broadening of targeting criteria to include not just the economic poor but also other segments of the population such as young children, displaced persons, women head-of-household, etc.

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interact with aid programs and their recipients in order to fully explain the outcomes of a targeted program within a given community.

Contribution

The three standalone papers on aid targeting presented in this volume are part of a broader research agenda. Overall, my research on the topic of targeting can be broken down into three parts. First, I have provided the first macro-level large-N empirical analysis of adverse outcomes in targeted aid programs and I have offered a new typology to conceptualize them. Second, I have provided the first empirical analysis of the causal effects of targeting instructions on resource allocation. Third, I have built on these insights to offer a new theory of aid targeting that I have tested empirically, and from which I have eventually derived policy implications. The collection of empirical pieces presented hereafter correspond to the second and to the beginning of the third part of this research agenda.

In “[Allocating Resources to the Poor](#)”, henceforth Paper 1, I look at the causal effect of instructions to target the poor, community involvement and monitoring on resource allocation at the micro-level. Using behavioral experiments within rural communities in eastern Democratic Republic of the Congo (DRC), I examine how small groups of villagers composed of elites, poor, and non-poor village members decide to share money among themselves under various treatment conditions. I show (1) that the effectiveness of targeting instructions is largely moderated by community-level dynamics, but that (2) by contrast monitoring does not contribute much to the effective targeting of the poor.

In “[Inside & Out](#)”, henceforth Paper 2, using a similar design, I look further into the community dynamics that affect the allocation of resources to the poor. I show

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that (1) these community effects have to do with bargaining dynamics rather than peer-pressure; (2) there is suggestive evidence that the non-elite, non-poor members of the community have an important role in fostering the allocation of resources to the poor, and that (3) the influence of these non-elite, non-poor community members on resource allocation depends crucially on existing alliances or rivalries between various group members.

Finally, in “[Is Bigger Always Better?](#)”, henceforth Paper 3, I use formal modeling to offer a theory of targeting. More specifically, along with my two co-authors, Laura Paler and Koran Koçak, I argue that targeting instructions enforce a bargaining environment in which three groups - the target group, the elites, and the excluded group - compete over the aid windfall. I predict that targeting can result in capture by the elites, capture by the excluded group or effective distribution to the poor depending on the size of the windfall, the relative influence and the historical relationships between these three groups. The main intuition behind this new theory of targeting is that poor, vulnerable groups are more efficiently targeted in environments in which the elites and the excluded group are rivals, insofar as they will then both prefer for the windfall to be allocated to the target group rather than for it to be captured by one another. Using a regression discontinuity, I test these predictions in the context of an aid program targeted to conflict victims in villages in Aceh, Indonesia.

When appropriate, I also mention in this volume my other research papers on aid targeting for which drafts are available upon request. Together, all these pieces are a substantive theoretical and empirical contribution to the growing literature on aid targeting effectiveness. For reference, I will now briefly describe the contents of the papers that are not included in this volume.

While there is a large number of impact evaluations for targeted aid programs available individually, there is no systematic comparison of the social, political and

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economic outcomes of these programs, let alone of their adverse outcomes.⁵ This is in part due to the long-held belief among aid practitioners that adverse targeting outcomes, also called targeting “errors”, are merely residuals. In [“Missing The Target”](#), I look at targeting errors at the macro-level. Using unique cross-country data on targeted aid projects in humanitarian and development programs around the world since 1990 that I have collected and coded, I show that (1) diversion of aid and conflict are actually pervasive outcomes of targeted aid programs; (2) there is large variation in the magnitude of these outcomes across aid programs, and that they vary independently from one another; and (3) the variation in these outcomes is not explained by macro-level data (such as country- or region-specific variables or context-related variables) nor by program-level data (such as design choices for the intervention). In [“Just An Error?”](#), I further analyze patterns of missingness in the same dataset and discuss incentives for aid agencies to under-report adverse outcomes in targeted aid programs.

In [“Hidden Figures”](#), I build further on my conclusions that inclusion errors and exclusion errors are neither independent nor homogeneous categories. More specifically, I offer a new, more refined typology for the study of the adverse outcomes of aid targeting that helps differentiate between phenomena that tend otherwise to be lumped together, such as the capture and the sharing of aid for example. I use this typology in a comparative case study of food aid distributions in South Sudan and in Darfur between 2000 and 2015 in which I disaggregate aid programs outcomes as well as conflict dynamics and population movements both spatially and temporally. I show that: (1) despite the fact that these two cases are most similar at the aggregate

⁵The most notable attempts at providing a general overview of aid program outcomes are Barrett (2006) on food aid and (Peppiatt et al., 2001) on cash transfers. However, both studies are limited in their geographic range, their time span and the type of intervention they consider. And within their own scope, neither is comprehensive either.

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level, there is more variation within these aid programs than across them; (1) there are patterns in the outcomes of targeted distributions in terms of capture, sharing, taxation, theft, exclusion, and conflict; and (3) that there are reasons to think that community-level dynamics might account for these dynamics.

In “[I’d Rather Lose Than Let You Win](#)”, I generalize the model presented in Paper 3 and offer an extension that examines the role of aid agencies and program design in fostering good outcomes for targeted aid programs. More specifically, I look at (1) how aid agencies can optimize the choice of targeting instructions under time and information constraints, and (2) how the theoretical implications of aid targeting vary with the primary objectives of aid organizations and the context of intervention. Finally, in “[Bringing the Outside In](#)”, I use all the evidence I have accumulated to derive policy implications as well as practical tools and frameworks that can be used to better take into account the role of excluded groups in the design of aid programs.⁶

Scope and External Validity

The scope of the discussion in this volume is limited to the targeting of foreign assistance distributed in the form of divisible goods to local, mostly rural communities. Each of these three elements - foreign aid, divisible goods and communities - have implications for the external validity of the results that are presented.

⁶In addition to these research papers, I also reference “[Varying An Invariant](#)”, a think piece that offers a brief description of targeting practices and aims at constituting aid targeting as a proper object of study for political science. In this piece, I show that despite the many possible choices in theory for the design of targeted aid programs, time, cost and information constraints are such that there is limited variation in the ways in which these programs are implemented in practice. I also argue that the few choices that are left effectively carry such agenda-setting power that the process of targeting, and who controls it, has become a highly political matter.

Foreign Aid

I have restricted my research on targeting to the targeting of *foreign aid* that is intended to be directly distributed and used by beneficiaries (as opposed to budget supports or institution building, etc). There are many other types of situations in which economic transfers are targeted to households or individuals: governments target social policies toward some specific segments of their population, firms target bonuses to their most productive workers, etc.⁷ But precisely because it is aid, and because it is foreign, one can expect that, compared to government transfers and productivity pay, aid targeting is going to display singular dynamics both in terms of ownership of the windfall by the recipient and accountability between the recipient and the donor.

One specificity of foreign aid compared to other transfers is that the donor is not an agent to whom the recipient would act as a principal as is the case for an elected government. Actually, the donor and the recipient do not have any form of contractual relationship that binds them together outside of the allocation of the aid windfall. As a result, there is no accountability mechanism that exists naturally, neither from the donor to the recipient, nor the other way around. Another important characteristic of foreign aid provision is that it is a situation in which a windfall is gifted rather than earned by its recipient. Sociology studies have shown such situations raise complex considerations about the legitimacy of the transfer both on the part of the donor and on the part of the recipient. This is especially true when there is no possibility, as is the case with foreign aid, for the recipient to give something back to the donor in exchange, let alone something of similar value.⁸ The various aspects and effects of

⁷Parts of the theoretical contribution in this volume can apply more broadly to various contexts of targeted policy interventions. I will discuss in more details such concerns about the external validity of my research as appropriate.

⁸See first and foremost Mauss & Cunnison (1954) on the forms and functions of gifts in archaic

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these lacks in ownership and in accountability inherent to foreign aid provision have been discussed at length in the literature on aid effectiveness,⁹ and I will refer to these discussions in the rest of this volume as appropriate.

Divisible Goods

The focus of my research is also restricted, within foreign aid, to the distribution of *divisible goods* at the local level. There are three main types of such aid provision to communities: food aid, cash transfers, and objects, also called non-food items. Non-food items (NFIs) can typically be tents, blankets, pots and pans, kitchen utensils, soap and other hygiene items, jugs, clothes, etc. All three types of aid are highly targeted.¹⁰ While restrictive, this still covers the vast majority of aid projects at the local level. Targeted cash transfers within communities now represent more than \$7 billion in the budget of the World Bank. As for food aid, out of nearly six million metric tons of food aid provided worldwide in 2000 by WFP, about 83% was for targeted distributions.

Because the goods distributed are divisible, they are *rival* in the economic sense. And since they are targeted, they are also excludable by definition. As opposed to health services, water and sanitation infrastructures, or education which are types of foreign aid provision to communities that mostly function either as public or club goods, targeted distributions are essentially *private goods* that can be coveted, competed for, and even stolen by those who have not received them. As with many

societies.

⁹About accountability problems in aid distribution, see Brautigam (2000), Paul (2006a). About ownership problems in aid distributions, see citetmaxwell2008c, maxwell2011.

¹⁰See (Strauss-Kahn, 2018h) for a discussion on the choice of aid type in the design of a targeted program, and (Strauss-Kahn, 2018g) for more details on the breakdown of aid projects by type of transfers. While aid can also be provided at the local level in the form of water and sanitation infrastructures and education or health services, these are public or club goods that are not typically targeted and hence not relevant for this study.

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easily extractible and valuable resources, the main question about targeted goods then becomes to determine how much will actually be diverted from their intended beneficiaries, and in which ways aid targeting and aid diversion will affect the social, political and economic relationships between aid recipients and non-recipients within a given community.

Community Distributions

The third scope restriction of my research is that I focus exclusively on distributions within communities, although I have a rather loose understanding of what constitutes a community. As noted by Maxwell & Burns (2008), the definition of a community in an aid context is often problematic, and this is an idea that I will develop throughout the volume. Aid distribution systems are often based on an idealistic notion of community and little attention is paid to existing social and cultural realities.

By community, I mean an ecosystem or a set of people who live together within a given geographical area, compete over the same basic resources, and interact with each other on a regular basis. Because I use a geographical criteria rather than a political one, communities are not necessarily composed of populations that have strong historical ties with one another. It encompasses for example displaced populations that arrive at a given site from multiple locations. The competition criteria serves to define a minimum level of co-dependency between community members. For example, it generally excludes most aid projects in urban settings, but it comprehends in a given area both displaced populations and their host communities. Finally, the interaction criteria serves to include non-traditional relationships between populations that live within a given area and share resources, such as for example the relationship of members of armed groups to the villages in which their families live.

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Communities are thus defined both in terms of primary groups (indigenous institutions that are ascriptive, including kinship ties, traditional political institutions, authority structures, and territorial networks, ...) and in terms of secondary groups that include social and economic organizations (Chazan, 1992). In a way, it opens the possibility of a “moral economy” without presupposing it and combines it with self-preserving individualistic tendencies that could include elite capture of goods and services (Scott, 1976, Sardan, 1999).

A lot of aid diversion and corruption can also happen before aid reaches recipient communities.¹¹ Transparency International (2014) even dedicates an entire section of their report on corruption to institutional and agency-level risks. While these phenomena are definitely important in getting the fuller picture about aid diversion, I am not going to address them in this volume. I focus on what happens to the aid that actually reaches local communities. Given the loose definition of communities that I have just given, this still includes to some extent for example cases of trucks that are stopped by roadblocks on their way to recipient communities, but such occurrence will not be central to the discussion.

Methods

The topic of aid targeting lends itself to particular methodological pitfalls. Each of them is discussed in more details as they are encountered in the rest of this volume. In this introduction, I provide an overview of the major challenges posed by the study of targeting and discuss the ways I have found to resolve them. In general, I have tried to approach my object of study in as many different ways as I could think of.

¹¹For example, in February 2007, WFP received an anonymous tip that lower level local employees in key positions of implementing agencies were diverting food deliveries, building supplies for personal gain, in their programs in India. Similar examples are available in several other countries (Transparency International, 2014).

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I have used various levels-of-analysis, various empirical and theoretical methods and various qualitative and quantitative data collection technique, including large-N cross-country data analysis, lab-in-the-field experiments, regression discontinuities, case studies, game theory, data mining, surveys, interviews, participatory observation,... I strongly believe that it is the combination of all these approaches that ensures the consistency and validity of the results that I present.

Causality

The study of targeting raises many of the same challenges that are encountered by aid effectiveness studies, and in general by social science studies that aim at determining the effect of a phenomenon for which there is no observable counterfactuals. How do I determine the causal effect of targeting when I cannot know how the allocation of an aid windfall would differ if it was not targeted? How can I compare it to what the allocation of a non-aid windfall would have been? How can I estimate what the situation would have been if there had not been any aid distributed at all? While there is no general answer to such questions, I have attempted to the best of my ability to answer them in various ways.

In Paper 1 & 2, I look at the causal effect of targeting instructions on the allocation of a non-aid windfall at the micro-level using an field experiment, and I discuss the limitations of such experimental designs as abstract constructs used to represent complex realities. In Paper 3, I consider the limitations of quasi-experimental designs in terms of external validity, and I briefly discuss the difference between assumptions and hypotheses in theory-building when using a game-theory model. In other work, I discuss the importance of identification strategies when considering correlations in macro-level quantitative observational data Strauss-Kahn (2018g).

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Data

Another methodological difficulty arises from the fact that aid targeting is a relatively new topic of study for political science: there is no existing, consistent, reliable data on the outcomes of targeted aid programs. For the purpose of my research, I have collected and / or compiled a lot of new data at various levels-of-analysis. In doing so, I have had to give a lot of thought to the proper variables that I could and / or needed to consider and control for. In Paper 1 & 2, I discuss the attention given to building culturally sensitive survey questionnaires as well as some additional considerations about ethical data collection in the field. In Paper 3, I discuss concerns with the lack of variance in survey questions that had not been tested ahead of time. In other work, I discuss in more details the difficulties of inference-making on data from secondary sources as well as the challenges of implementing large-scale data mining in a comprehensive, systematic manner so as to get a sample with no selection bias Strauss-Kahn (2018g,c).

Measurement

Finally, the study of aid targeting also shares some of the methodological problems of studies related to corruption and / or violence: the topic is hard to study because outcomes tend to be both inconsistently reported and under-reported. In Paper 1 & 2, I discuss some of the challenges of measuring behaviors and attitudes at the micro-level that respondents might have an incentive to lie about, as well as the importance of multiple measurements in surveys for questions on which respondents are susceptible of answering strategically. In Paper 3, I discuss the difficulties of measuring theoretical variables with empirical indicators that are imperfect proxies as well as the proper use of suggestive anecdotal evidence gathered during field work.

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In other work, I discuss issues of censoring and missingness (Strauss-Kahn, 2018f), as well as issues of reliability with qualitative measurements and recall data (Strauss-Kahn, 2018c).

Overall, I have used all the methods at my disposal to study the multi-faceted phenomenon that is aid targeting. Each of them individually presents challenges and can only uncover a little part of the story. Yet taken together and used properly, I believe that these various pieces of the puzzle effectively help getting at the bigger picture. What remains are difficulties specific to research topics that are both of academic interest and have important policy implications. How can I make useful recommendations and develop practical tools from theoretical inferences? How do I take into account the constraints in time and information that decision-makers are actually facing? These are issues that are largely discussed in Strauss-Kahn (2018b).

Allocating Resources to the Poor

The Effects of Targeting Instructions, Community Involvement and Monitoring

Camille Strauss-Kahn*

Abstract

Efficiently allocating resources to the poor is a major concern for all sorts of social transfers, and in particular in the distribution of foreign aid. In this paper, I estimate the causal effect of three highly prevalent factors in the process of distributing foreign aid at the local level — targeting the poor, community involvement and monitoring. More specifically, I use dictator-game-like distributions among small groups of people from rural communities in eastern Democratic Republic of the Congo to determine whether the decision to allocate resources to poor group members, non-poor group members and elites is affected by the provision of instructions to target the poor, the presence of community members during decision-making and the monitoring of the decision by a third-party. I find that what primarily increases the share of the windfall that poor group members receive is the presence of other community members during the decision-making process. By contrast, I find little to no evidence that monitoring increases the allocation of resources to the poor at all. As for instructions to target the poor, by themselves they are in fact less effective than community involvement in ensuring that the poor benefits from the windfall. Their effect is also complex: they actually benefit both the poor and the non-poor and seem to be essentially understood as a signal that the windfall is not intended to benefit elites.

*Ph.D. candidate, Columbia University (Email: cs2899@columbia.edu). The author would like to thank Macartan Humphreys, Chris Blattman, Grant Gordon, and the participants of the advising group at Columbia University as well as all the group from WZB. The author would also like to thank Annie, Doudou, Koko, Aimable, Yves, and all her team of enumerators in DRC.

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Allocating resources to the poor in the most efficient manner is a major concern for all sorts of social transfers, and it is most of all paramount in the distribution of foreign aid. Targeting the poorest, most vulnerable individuals has become the norm for the distribution of humanitarian and development resources at the local level. Rather than distributing money, food, and non-food items to entire villages, non-governmental organizations usually identify a restricted pool of beneficiaries within recipient communities and attempt to deliver assistance only to them. More than 85 percent of the aid intended for individuals now takes the form of targeted distributions of such divisible goods (Wahlberg, 2008, Barrett, 2006). Yet, as with foreign aid programs in general, targeted aid projects were launched long before there was compelling theory, or compelling evidence, that proved they could work (Maxwell & Burns, 2008).

The results of targeted distributions are often not what aid agencies originally expected nor intended (Strauss-Kahn, 2018g). The outcomes of targeted aid programs vary significantly in terms of whom in recipient communities ends up benefiting from the aid windfall as well as in terms of whether beneficiaries and non-beneficiaries are satisfied with the aid program or not (Jaspars & Shoham, 1999a). Results range from situations where aid reaches the designated beneficiaries effectively and exclusively to cases where it is entirely diverted by one or several other groups which are arguably not the most vulnerable, such as elites, well-off population groups or even armed groups (Paler et al., 2018). Likewise, there are communities that are satisfied with the outcome of targeted distributions, and others where the process of distributing to some and not others generates dissatisfaction, tensions and competition up to the point of creating violent conflict between those who have received part of the aid windfall and those who have not (Young & Maxwell, 2009, de Sardan et al., 2014, de Sardan, 2014). Getting a better understanding of what makes for successful

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targeting is thus an essential step in the perspective of increasing aid efficiency.

Aid agencies usually face high cost, time and information constraints in the field, such that there is little variation in the way targeting is implemented at the local level.¹² Most commonly, an NGO will come to a village and inform community members that an aid windfall is intended for the most vulnerable among them. It will then rely to some extent on local leaders to make the actual allocation, and will monitor the distribution of goods to the best of its ability before leaving the village. In other words, targeting aid is a complex process which can not be simply reduced to the provision of targeting instructions. Notwithstanding the context of intervention, there are three elements that are characteristic of targeted distributions: the provision of some instructions to target poor community members, the involvement of the recipient community in the process albeit in various ways and to various degrees, and some level monitoring by the aid agency.¹³ While it stands to reason that community involvement and monitoring are two important factors that could possibly moderate the effect of targeting criteria, aid programs can hardly improve effectively without better knowledge of the relative impact of each of these elements on windfall allocation.

Aid agencies usually assume that the effectiveness of their targeting depends either

¹²In theory, the design of a targeted aid program can vary in terms of which group is targeted within the recipient community and how targeted community members are identified. While the general principle is always to reach those community members who are most in need, the actual definition of targeting criteria and indicators can be either based on status - for example displaced populations, women, the elderly - or on needs - such as the poor, conflict or disaster-affected populations. The design of a targeted aid program can also vary in terms of targeting levels and methods (geographical or household targeting, administrative or community-based targeting, etc). In other work, I discuss in more details the constraints that prompt aid agencies in practice to pool on a limited number of designs depending on the context of intervention (Strauss-Kahn, 2018h).

¹³In this paper, for the sake of simplicity, I use the term “poor” as a generic characteristic of target groups. In general, the consensus among aid organizations since the 1990s is that aid targeting should aim at reaching the most “vulnerable” community members. In humanitarian jargon, the concept of vulnerability is meant to encompass a complex definition of needs that focuses not only on the economic dimension of poverty but also on social and political factors. In other work, I discuss in more details the possible nuances in the definition of targeting instructions (Strauss-Kahn, 2018c).

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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). But the mere suggestion that targeting does something at all can seem surprising. The period of time for which an aid organization actually interacts with the recipient community of an aid program is rather short.¹⁴ Why should the fact that a relative stranger gives instructions about how to allocate resources matter at all to a community that has long-standing relations, practices, and traditions? In the absence of a counterfactual, there are actually no ways to know for sure that targeting really makes community leaders allocate resources differently than they would have otherwise. There is even evidence to the contrary: elites seem to actually do whatever they want in any case. Even when WFP aid workers attempt to bypass local power structures and organize direct distributions to households, local leaders are frequently reported to organize and oversee a ‘re-distribution’ of the food within the community after the ‘official’ distribution is finished and the aid organization has left (Duffield, 1996, Narbeth, 2001).

In this paper, I study the causal effect of targeting instructions, community involvement and monitoring on resource allocation. Building on large literature in social psychology about the determinants of allocation decisions in small groups of people, I have identified three theoretical pathways through which the aforementioned factors could affect the allocation of an aid windfall: distributor effects, group effects, and third-party effects. To test for these effects, I use a lab-in-the-field experiment in rural villages in eastern Democratic Republic of the Congo (DRC) in which I combine three treatments with a factorial design. More specifically, I measure behaviors and atti-

¹⁴The length of time between the first contact with a recipient community and the end-line distribution of goods can vary from approximately 3 weeks to 2 years depending on whether the aid program is implemented in an emergency or a development context. During this time, aid workers will only have sporadic contacts with the recipient community.

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tudes 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 the form of 27 tokens in a dictator-game like setting. In these distributions, I randomize whether the distributor is provided with instructions to target the poor or not, whether decision-making is done in the presence of other group members or not, and whether the enumerator actively monitors the decision-making process or not.

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. First, because 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. Second, since these villages 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.

Overall, I find limited support for an effect of targeting instructions on their own (distributor effects) and strong support for an effect of the presence of group members during decision-making (group effects) in the way targeting is affecting resource allocation. It is the presence of other group members during decision-making that has the biggest impact on resource allocation overall. More specifically, I find that both targeting instructions and public decision-making diminish the share of the windfall that the elites keep for themselves. But while by themselves instructions to target the poor actually benefit both poor and non-poor group members to the detriment of elites, it is when combined together that targeting instructions and community

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involvement are effective in targeting the poor: the share of the windfall allocated to the poorer recipients is biggest in targeted, public distributions. By contrast, I find little to no effect of monitoring in general (third-party effects), and I find that in this experiment monitoring only affects private decision-making. While all these treatment effects are significant and robust across specifications, I also find that baseline allocation decisions, especially in public distributions, tend to be egalitarian. In other words, I find that what targeting instructions, monitoring and public distributions affect is whom in the group receives one or two extra tokens, after the distributor has seemingly given 5 of the 27 tokens to each of the five group members, including themselves.

The main contribution of this paper is to show that group effects are most important in explaining effective allocation of resources to the poor. By contrast, the results presented in this paper suggest that monitoring by a third-party is a surprisingly inefficient way to target the poor. Thus, this paper suggests that properly harnessing the community dynamics at play in public distributions may be a more promising way to improve the efficiency of targeted aid programs than investing in the capacity of an aid agency to enforce its targeting criteria through monitoring and sanctions mechanisms. Consistent with observations in aid targeting contexts, this paper also highlights the existence of strong sharing norms in recipient communities and the traditional role of elites in conflict management that can both foster and hinder the effective reaching of the poor. While these findings warrant further work in unpacking the precise nature of the group effects observed in public settings, they contribute to a better understanding of what makes the targeting of foreign aid effective or not, and of some of the dynamics of social transfers in general.

1 Theory

Aid targeting is a public and monitored process in which an aid agency provides local elites with a windfall and specific instructions to distribute it to those most poor in the recipient community. While it is largely assumed that targeted distributions yield resource allocations that are more beneficial to the poor than non-targeted distributions, there is no clear predictions about what part of the process of targeting brings about this result (Maxwell & Burns, 2008).¹⁵ Building on a large literature in social psychology about allocation decision in small groups, I have determined that the ways in which targeting can affect windfall allocation can typically be classified in one of three broad categories of explanation: 1/ distributor effects, 2/ group effects, and 3/ third-party effects.

The nature and degree of involvement of traditional elites in defining targeting criteria and identifying beneficiaries can vary. Yet elites seem to invariably end up acting as a distributor once an aid program is implemented in their village, whether the aid agency in charge actually wants it or not.¹⁶ By *distributor effects*, I mean all the ways in which the local elites' own preferences—which can include both their self-interest and considerations about norms of justice— affect how they decide to allocate resources.

¹⁵What allocation is “more beneficial” to the poor is a complex matter, insofar as the effects of targeting are multi-dimensional. They include not only economic and distributional aspects, but also social and political ones. As I have argued in other work, in some contexts, allocations to the poor that are smaller in size are still more beneficial, for example in terms of diminishing the level of conflict in the community (Paler et al., 2018). In this paper, however, I only look at allocations in terms of amount received.

¹⁶Arguably, if ones takes out of the equation the aid agency and the rest of the community for a second, aid distribution at the local level is very similar to a dictator-game in which a distributor, the elites, have to allocate resources to a receiver, the target group. By construction, this receiver is a weak group with no bargaining power, insofar as it was designated to receive the windfall precisely because it is poor, vulnerable and otherwise marginalized from resource allocations. As with dictator games in general, the preferences of the distributor will matter most in explaining the final allocation.

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Research in social psychology has long shown that, in dictator games, the predictions that distributors follow purely their self-interest are not verified. Allocation decisions are actually a mix of economic self-interest - which prompts a distributor to keep as much of the windfall as she can for herself - and some form of altruistic considerations (Blanco et al., 2011), which typically have a lot to do with conceptions of fairness (Messick & Sentis, 1983, Brockner et al., 2001, Van Dijk et al., 2004). Similarly, the provision of instructions to target the poor to elites in an aid targeting context could potentially affect one of two things: the relative importance that elites give to altruistic considerations over egoistic ones, and the nature of these altruistic considerations. The literature shows that different norms of justice - such as procedural justice or distributive justice for example - can be applied to a single situation and typically lead to different allocations within a group (Lind & Tyler, 1988, Tyler, 1994), and that framing effects can affect the type of norms applied to a given situation (Larrick & Blount, 1997, Blount & Larrick, 2000). One could expect that targeting instructions act as a framing mechanism on the elites and affect the norms of justice the elites apply to the aid allocation or their perception of the intended recipient (Kravitz & Gunto, 1992). In other words, it could be that targeting instructions convince the elites that giving to the poor is the appropriate thing to do when it is not their opinion in the first place.

There is little actual evidence of what traditional elites consider a fair allocation of resources to begin with, let alone evidence that targeting instructions could change their mind on the subject. The evaluation literature provides some support for the idea that different conceptions of fairness are at play in aid targeting and that they are often conflicting with one another (Maxwell & Burns, 2008, Young & Maxwell, 2009). On the one hand, aid agencies partake in the ideals of distributive justice that are widely spread in western societies and believe that the fairest allocation of

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resources is to distribute to the poor. On the other hand, recipient communities are often reported to see targeting as “not fair” and ideals of procedural justice are typically opposed at least by some to attempts at targeting the poor: “what is fair is to give to everyone”. But it is less clear whether such “universal” norms of justice are in fact universally shared within recipient communities, and in particular how strongly traditional elites partake in them.¹⁷

Distributor effects could also be strictly speaking elite effects (Brass & Burkhardt, 1993). Elites’ behavior and their sensitivity to targeting instructions or lack thereof could be the consequence of their political role and responsibilities within the community in general rather than the result of their position as a distributor within the aid allocation system. But it is not clear whether one should expect elites’ preferences to be prosocial or not. On the one hand, as political representatives and public servants, elites are typically expected to care more about the public good and to be more accountable to the poor than other community members. Yet since in most aid-related contexts elites are not elected representatives, it could also be on the contrary that traditional leaders cater more to their own interests or to the interests of other segments of their polity than the poor. As a matter of fact, some aid agencies do seem to believe so, since they sometimes try to bypass local elites by picking someone else in the community as a distributor such as with the creation of food relief committees by the World Food Programme. Yet there is little evidence to date that aid targeting affects resource allocation differently depending on how the distributor is picked within the recipient community.

¹⁷The idea that recipient communities are generally not poor-oriented societies is often an assumption on the part of aid agencies more than an assessment. This assumption is often very strong among aid workers and it is implicitly self-supported (“if these societies were taking care of their poor, there would be no need for targeting”). It serves both as a justification for targeting itself and as a “proof” that that it works (“target groups are vulnerable precisely because outside of the context of aid targeting they tend to get less than others from resources allocation at the village level”).

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On the other hand, distributors have been shown to be significantly influenced in their decision-making by other group members (Messick & Sentis, 1983, Robert & Carnevale, 1997, Bornstein & Yaniv, 1998). In the context of aid targeting, influential community members could very well influence elites' decisions through peer-pressure or bargaining, especially since the elites will have to stand by these decisions toward the rest of the community even after the aid agency has left. The role that communities at large play in the targeting process has been subject to growing attention. Numerous aid programs now directly involve communities in the design of targeting aid programs in the belief that members of the community are better positioned than government or aid agencies to identify qualified recipients and get goods to them efficiently. While it is widely appreciated that involving communities in identification and distribution raises the prospect that local leaders will influence the allocation process in a way that benefits them or their supporters (Alatas et al., 2013a), there is also evidence that involving the community at large can also be accountability and legitimacy enhancing (Winters, 2012, Alatas et al., 2012a). It is possible that the public nature of aid programs could also serve to keep elites in check and counter-balance their natural tendency to over-favor their self-interest in resource allocation. I call such explanations in which it is the rest of the community that influences the elites' decisions *groups effects*. Group effects, whether in the form of peer-pressure or bargaining dynamics, are the results of community involvement, insofar as they can only be at play when decision-making is public to some extent.

In some regards, it may be more credible for group effects to affect allocations rather than for distributor effects, especially in one-off distributions like most humanitarian emergencies. Distributor effects simply hinge on the fact that instructions given by a third-party are sufficient to convince elites to change their long-held preferences over resource allocations. Yet, while long-run, repeated framing effects

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can influence preference formation (Tversky & Kahneman, 1985, Brewer & Kramer, 1986, Slovic, 1995), there is little compelling evidence that isolated framing effects can have strong effects on preferences, let alone long-lasting ones (Levin et al., 1998, Druckman, 2004). On the other hand, groups effects are consistent with the fact that community members have long-standing relationships with one another. Recent work on dictator games has also shown that altruistic preferences actually tend to disappear in private, anonymous distributions which further suggests that it may be the fact that the targeting process is public or monitored that actually fosters its efficiency (Winking & Mizer, 2013).

The third family of explanations that could account for differences in allocation between non-targeted and targeted windfalls are *third-party effects*. One other major way in which aid targeting is different from other situations in which resources are allocated within a community is that there is third-party overseeing the process, namely the aid agency. Third-party effects regroup all the mechanisms that hinge on the presence of the aid organization during distributions, which range from Hawthorne effects (it is simply the presence of an external observer that matters)¹⁸ to effects that have more to do with the capacity of the aid agency to enforce sanctions or rewards.¹⁹ It is a long-held belief in the aid community that monitoring plays a big role in making aid targeting effective, and in particular that it can help supersede adverse community dynamics. Yet, this has not been substantiated with empirical evidence to date, and there are also good reasons to think that monitoring and sanctions may not be strong incentives to respect targeting criteria in the context of aid programs.

In practice, most aid agencies have little to no effective monitoring capacities neither during nor after a program. Simple monitoring of distributions is already very

¹⁸Wallace (2015) also calls it the “Heisenberg’s lesson of aid intervention”.

¹⁹The community and /or distributor could either expect sanctions if they don’t comply or expect benefits from repeated interactions with the aid organization if they comply.

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complicated. Most aid agencies often only check that the individuals that come to the distribution correspond to lists of beneficiaries that have been established. However that doesn't guarantee that the beneficiaries' lists were drafted correctly in the first place, nor does it prevent the re-allocation of resources once the NGO has left. Similarly, the idea that aid agencies as a principal can effectively enforce sanctions and rewards toward the recipient community as their agent is not very credible.²⁰ Once funds have been received from donors and work with the community has begun, it is very unlikely that an aid agency will decide not to proceed with the planned distributions, a reality of which recipient communities are probably aware.²¹ Sanctions to individual contraveners are also unlikely. First, the imposition of sanctions supposes effective complaint mechanisms. While such mechanisms usually formally exist, they are not much used to report targeting errors or incidents. Second, it supposes that individual sanctions can actually be enforced. But aid workers are not likely to come back to the recipient community and get back distributed goods from individuals that were not supposed to receive them. Overall, there are reasons to think that monitoring has much less effect on resource allocation than is usually expected.

Finally, there is yet another way in which the provision of targeting instructions could affect the allocation of aid resources, namely through *learning effects*. There

²⁰The fact that the decision-making process is monitored by a third-party can either make the distributor care about norms of fairness or make the community compelled to enforce them. Several principal-agent frameworks would adequately describe this situation, in particular three-tiered models with a principal (the aid agency), a supervisor (the elites) and an agent (the rest of the community) (Pendergast, 2001). Since aid workers usually do not have not long-standing, sustained relationship with recipient communities, the capacity of the aid agency, as a principal, to successfully enforce a contract over the respect of targeting criteria with the community hinges on its monitoring capacities and its credible commitment to enforce sanctions if targeting instructions are not respected.

²¹In very rare cases, NGOs have threatened "misbehaving" recipient communities with stopping the aid program and pulling back when targeting criteria were not respected. This was the case for example with an OXFAM program in eastern DRC in 2013 that was fraught with corruption and capture. But, in the end, as in most cases, the incident was often "resolved" somewhat peacefully by loosening targeting criteria to include a larger pool of beneficiaries.

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is some anecdotal evidence of a long-time framing effect of aid targeting instructions at the community-level. The increased number of complex emergencies since the 1990s has resulted in multiple, repeated humanitarian and development interventions targeting the same geographical areas and the same populations. Aid workers now report that recipient communities that have become “used to” receiving assistance often display learned behaviors such as being less resistant to the imposition of targeting criteria than “first-timers” and even facilitating the process of targeting by having lists ready, or even having “the vulnerable” present themselves upfront for identification.²² According to observers, it is not so much that prolonged exposure to targeted aid transfers contributes to reshape distributive preferences either on the part of the recipient community at large or of its elites, nor that these communities have abandoned their traditional conceptions of fairness (Maxwell & Burns, 2008). It is rather that they strategically adjust their behaviors or display attitudes that are believed to yield more benefits (Cason & Mui, 1998, Van Dijk & Vermunt, 2000).

2 Empirics

Now let us explore empirically what it is, if anything, about providing targeting instructions, involving the recipient community, or monitoring by a third party that actually changes how resources are allocated. To distinguish between various possible explanations for the effect of targeting on resource allocation, I have gathered data in eastern Democratic Republic of the Congo about how carefully selected groups of villagers choose to allocate money among themselves in various situations. Each group in the sample is composed of five people from the same village: one is a member of the

²²Arguably, the fact that “being a vulnerable” (rather than “being vulnerable”) has become an identity marker in places that receive assistance regularly suggests a strategic use of humanitarian lingo and targeting categories.

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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). In each group, one person is in charge of allocating a sum of money among themselves and the four other group members. 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 distributor is alone when making a decision or whether other group members are present (Treatment 2: *private or public 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.

The distributor effects, group effects and third-party effects described in the previous section can be identified with specific combinations of the three treatments. Distributor effects are observable as the effect of targeting instructions in private, non-monitored allocations. Community or group effects can only be at play in public allocation, whether monitored or not. Conversely, third-party effects can be observed when comparing monitored allocations, whether public or private, to non-monitored allocations. The design also allows for the exploration of the various interactions between the three treatments.

2.1 Context

In order to leverage a relevant context and rich data about real-life power structures, social roles, and relationships between participants, I have gathered the empirics for this study in the regions of North-Kivu and South-Kivu in eastern Democratic

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Republic of the Congo (DRC).²³ Since 2004, armed conflict, general insecurity and the lack of economic development in the Kivus region have contributed to create one of the world's most complex and long-standing humanitarian crisis. With one out of ten persons forcefully displaced at least once in the past decade and in need of assistance, it has become one of the largest humanitarian mission currently in operation.

In 2017, out of the 208 relief organizations operating in the entire country, 162 organizations were regularly assisting internally displaced persons (IDPs) in eastern DRC by distributing food, relief items (such as tents, cooking utensils, hygiene kits, ...) or cash transfers (OCHA, 2017). While most of these aid projects target specifically IDPs in their sites of displacement, similar programs that seek to assist vulnerable individuals more generally both among displaced and host populations are also quite common. Overall, whether they happen to have directly benefited from assistance or not, rural villages in eastern DRC are used to the presence of humanitarian organizations.²⁴

Despite the increasing appeal of Goma and Bukavu, the two major local urban centers, the population of North- and South-Kivu is still largely rural. Most of these rural communities are agro-pastoralist, with small trade being the second most common activity and source of income. Small rural villages such as those sampled for this study usually comprises from 50 to 150 households. With an average of 5 members per household, such that one could more or less argue that everybody knows everybody in these communities.

As in many humanitarian and development contexts, the definition of poverty

²³While the same effects of targeting and its moderators could theoretically be observed in the lab, among participants that do not know each other, I believe I am much more likely to recover effects of interest in this context, especially since I don't have to prime group members about their respective roles.

²⁴In that regard, I would argue that in the study sample, while all villagers were by no means "used" to assistance, asking them to allocate resources among themselves while providing them with targeting criteria was not a surprising nor weird exercise for them either.

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in rural eastern DRC is complicated, insofar as sources of vulnerability are not just economic but also social, ethnic and political. While arguably everybody is poor with regard to international standards, there are still observable differences between the poorest and richest individuals in each village. For example, in all of the villages in the sample, the poorest decile of respondents reported earnings less than \$4/month while the richest respondents declared earning more than \$150/month. In most villages, the population can be broken down into wealth categories using indicators defined by the community itself: in informal discussions, the groups often self-identify as ‘better-off’, ‘regular’ or ‘very poor’ (Save The Children, 2002).²⁵ Indicators of wealth differ from one village to the next, but overall the two most important criterion of wealth are access to land and therefore the size of the plots cultivated, as well as the number of small livestock a household owns. Typically, “better-off” households own between 1 to 3 hectares of cultivated land and have a few chickens, goats, sheep, or cows. Poorer people, on the other hand, work in fields owned by others for around 500 Congolese Francs (CF) per day — about .5\$US— and own no livestock if one chicken.

In those villages, 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. As such, the village chief is not particularly accountable to poorer segments of the village population. Traditionally, the primary role of the chief is in fact to allocate land and to settle disputes in the village. In fact, according to 87% of respondents in my baseline survey, the village chief is expected to treat everyone in the community equally, and only 4% of respondents consider that taking

²⁵See Household Economic Assessment methodology. According to some focus groups, it would seem that ‘rich’ households no longer exist in most of the villages in the sample due to their migration to urban centres during the periods of conflict. For that reason, I prefer henceforth to talk about the poor and the non-poor in each village.

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care of the poor is one of the roles of the chief. On the other hand, the chief is expected to benefit to some extent from his position and status. For 32% of respondents in the sample, when the chief makes a decision he actually mostly takes into account his own interest rather than the interest of the entire community (54%). In practice, for example, in occasions where resources are shared among villagers, such as food during a banquet, the chief is usually both the primary decision-maker about allocation as well as the first- and best-served.

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, these elites are involved in the process of distributing the aid windfall in aid-recipient villages. Even though they are not usually intended beneficiaries of aid programs, NGOs usually rely on these elites for various aspects of the beneficiary identification and / or aid distribution process. The economic status of elites can vary, although in practice they are mostly in the better-off or the regular category and rarely among the poor. However, their social status is distinctly higher than non-elites village members. As the village chief, while they do not receive official compensation for their work, they are often expected to somehow profit from their position.

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 external validity 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.

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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.²⁶ The non-elite respondents were a gender-balanced, representative sample of the rest of the village.²⁷

Using this baseline survey, I have constructed a poverty index and sorted the 32 non-elite respondents into the 16 poorest (hereafter the ‘poor’ half of the sample) and the 16 richest (the ‘non-poor’).²⁸ All 40 respondents are then randomly dispatched into 8 groups of 5 persons that each included 2 poor, 2 non-poor and 1 elite. Each group is assigned a “room” in which distributions occur.²⁹

For each distribution, one of the five group members is designated by the enumerator as the distributor. The distributor is asked to allocate 13.500CF (13.5\$US) among all group members. The allocation is done by putting 27 tokens, each representing (and somewhat resembling) a bill of 500CF into five cardboard ballot boxes on top of which there is a picture of one of the group members. 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.³⁰

²⁶The 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.

²⁷The representative sample of the rest of the village specifically excluded the chief and members of the elite, but it could include their family members. In a different paper, I leverage the fact that the sample is balanced to provide a gendered analysis of the result of the experiment.

²⁸The poverty index is described in more details in the next section.

²⁹Distribution surveys were administered in specific, private spaces. Often times, actual school rooms were used since part of the surveying was done over the holidays. Other times it was churches or other local public buildings depending on availabilities and permission. Occasionally, separate room-like spaces were set-up ad hoc with poles and pagne cloth.

³⁰In practice, one round to be rewarded is randomly selected among the rounds played, and each participants earnings are scaled and added to a fixed compensation of 500FC. On average, participants have received 2500CF, which is the equivalent of two to three days of work in the fields. See Field

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Each group is randomly assigned one of the $2 \times 2 \times 2$ possible treatment conditions. First, the distributor is either alone in the room and presented with pictures of the group members (including herself) on ballot boxes or the distributor is in the room with the other group members and presented with the same ballot boxes. Second, the distributor 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.”). 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.³¹

Treatment assignment is decided at the group level. While the targeting and monitoring treatments are factorial, I have decided not to randomize the public/private treatment due to considerations about the power of the design. In practice, in a given group, each member has first separately received the same monitoring and targeting treatments as a distributor in private. Then, a public allocation is organized under the same treatment condition in which the elite is *always* distributor and with all other group members present in the room. Each individual in each group thus participates in BOTH a public and a private distribution.³² From a methodological

Manual for more details on the payment scheme.

³¹Monitoring treatments are often considered in the literature as some of the most difficult manipulation to credibly enforce in such settings. With this manipulation, I have attempted to make both the no-monitoring and the monitoring treatment as strong as possible. As a further manipulation check, in addition to debriefing questions on perceptions about monitoring, the first round of measures with a given population was systematically done without either the monitoring nor the no-monitoring treatment. This offers the possibility to assess whether each treatment individually as well as the difference between treatment is strong enough that one could expect it influences actual behaviors.

³²I have also decided not to randomize the order of the public / private distributions under the

standpoint, assumptions such as sequential ignorability are likely to be satisfied.

Finally, to further increase the size of the sample, after each group has gone through a round of private and public allocations, the 40 respondents are shuffled into new, different groups. The permutation of respondents is such that two people who have been together in a group in one round can not be in the same group again in subsequent rounds.³³ In the end, two or three such rounds of private and public distributions were played in each village.³⁴ The final dataset comprises a total of 224 public distributions and 1120 private distributions.³⁵

2.3 Estimation Strategy

I look at the causal effect of targeting instructions (X) on resource allocation (Y) while controlling for two moderating factors, namely group-related effects ($M1$) and third-party related effects ($M2$) (Baron & Kenny, 1986). In equation form, the general causal model including a treatment indicator for targeting (X with a specific level represented by $x \in \{0, 1\}$), two mediators ($M_1 \in \{private, Public\}$, $M_2 \in \{non - monitored, Monitored\}$) and a dichotomous outcome (Y), where X may affect Y directly and/or X may affect any of the $M_j, j \in \{1, 2\}$, which then affect Y , is:

$$Y = i + \alpha X + \beta_1 M_1 + \beta_2 M_2 + \gamma_1 X \cdot M_1 + \gamma_2 X \cdot M_2 + \gamma_3 M_1 \cdot M_2 + \delta X \cdot M_1 \cdot M_2 + \epsilon$$

assumption that doing the private distribution first is not likely to influence the public distribution, while the other way around is probable. This exclusion assumption is not uncommon in social psychology studies, in which it is generally assumed that individuals reveal their actual preferences in private distributions.

³³This was intended to avoid collusion. Further steps were taken in order to satisfy exclusion restrictions and limit the risk of spill-overs of the treatments.

³⁴While, the design of the experiment was originally intended to permit up to five different rounds of distribution measures in each village, time constraints ended up limiting the surveying activities.

³⁵Due 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.

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where α is the direct effect of the targeting instructions on the decision of the distributor; β_1 is the direct effect of the group dynamics; β_2 is the direct effect of the third-party; γ_1 is the effect of the interaction of targeting instructions and group dynamics ; γ_2 is the effect of the interaction of targeting instructions with third-party; γ_3 is the effect of the interaction of group dynamics and third-party; δ is the effect of the interaction of targeting instructions, group dynamics and third-party.

In terms of potential outcomes, I can identify the effects of targeting instructions by comparing the outcomes of targeted and non-targeted distributions. To look at the effect of community involvement, I compare the outcomes of public and private distributions. Finally, for the effect of monitoring, I compare the outcomes of monitored and non-monitored distributions.

	<i>PRIVATE</i>		<i>PUBLIC</i>	
	<i>NON-MONITORED</i>	<i>MONITORED</i>	<i>NON-MONITORED</i>	<i>MONITORED</i>
<i>TARGETED</i>	Y(1,p,m)	Y(1,p,M)	Y(1,P,m)	Y(1,P,M)
<i>NON-TARGETED</i>	Y(0,p,m)	Y(0,p,M)	Y(0,P,m)	Y(0,P,M)

Table 1.1: Table of Potential Outcomes

$$\begin{aligned}
 \alpha &= E\{Y(1, p, m) - Y(0, p, m)\} \\
 \beta_1 &= E\{Y(0, P, m) - Y(0, p, m)\} \\
 \beta_2 &= E\{Y(0, p, M) - Y(0, p, m)\} \\
 \gamma_1 &= E\{Y(0, P, M) - Y(0, p, M)\} - E\{Y(0, P, m) - Y(0, p, m)\} \\
 \gamma_2 &= E\{Y(1, p, M) - Y(1, p, m)\} - E\{Y(0, p, M) - Y(0, p, m)\} \\
 \gamma_3 &= E\{Y(1, P, m) - Y(1, p, m)\} - E\{Y(0, P, m) - Y(0, p, m)\} \\
 \delta &= [E\{Y(1, P, M) - Y(0, P, M)\} + E\{Y(1, p, m) - Y(0, p, m)\}] \\
 &\quad - [E\{Y(1, p, M) - Y(0, p, M)\} + E\{Y(1, P, m) - Y(0, P, m)\}]
 \end{aligned}$$

3 Data

Each respondent was administered a baseline survey as well as additional surveys before, during and after each private and public allocation. The survey instruments include both behavioral and attitudinal measures.³⁶ Additional qualitative, village-level data was also gathered during focus groups.³⁷

3.1 Outcome measures

The main outcome measured both in private and public distributions is the number of tokens —among the 27 that are distributed— that each group member receives in a given allocation.³⁸ Enumerators also record additional information during public distributions, including which group members participates in the discussion, in what ways, and whether that seems to influence the elites' (who is always the distributor in public allocations) decision.

³⁶For more details about the data collection process see Field Manual, for the wording of the questionnaires see Appendix A.1.

³⁷In between their participation in the private and public distribution survey in a given round, while other group members were taking the private distribution survey, respondents were also encouraged to participate in various focus groups. The point of the focus groups was both to limit direct 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. See Field Manual for more information of the focus groups.

³⁸Social 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 1344 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.

3.2 Poverty measures

I use the baseline survey to distinguish richer from poorer respondents, and hence to block randomize groups for distribution surveys. Getting reliable measures of poverty in the baseline survey was thus paramount. To that effect, a lot of attention has been given to developing several appropriate, concurring measures of economic and social status.³⁹

In the baseline survey, there are several questions related to assets, revenues, housing and other objective measures of the economic poverty of the respondent. The baseline survey also includes questions about social relations and connections in order to provide objective measures of social vulnerability.⁴⁰ These measures were combined into a poverty index,⁴¹ and the 32 respondents from the general population were sorted into two groups accordingly. The high correlation across the various measures of economic poverty and social vulnerability should give reasonable confidence that the poorer and richer half of the sample were correctly identified ($\rho = .65$). Since the 50% cut-off is arbitrary, there are no theoretical reasons to expect much difference between the richest member of poorer half of the population and the poorest member of richer half. As a control variable in all estimations, I use a measure of within-group inequality along the poverty index.

Economic poverty and social vulnerability are also assessed subjectively both by the respondents themselves and by the enumerators in charge of administering the

³⁹See Dille & Boudreau (2001), Galasso & Ravallion (2005a) for extended discussions on how conceptions of poverty are subjective and Harragin & Chol (1998), Ravallion (2009) on how poverty is not just economical.

⁴⁰See Appendix A.1 for the questionnaires.

⁴¹The index was developed based on the results of a pilot, and in consultation with the enumerators team. In the index, I use the measures of objective social and economical poverty that were both the most meaningful and that had the largest variation in the pilot sample. These included: earning more than 50\$/month (-), or less than 10\$ (+), owning a cell phone (-), being unable to send children to school because of money (+), owning more than one bedframe or one mattress (-), not owning any blankets (+), owning more than one field (-), owning cows (-), etc.

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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.

The correlation between being one of the two poorer group members in the experiment and the poverty index is high ($\rho = .86$), which is expected since the index was used to block randomize the composition of the groups. The fact that there is less correlation between the experiment's poor indicator and other measures of economic poverty is not a source of major concerns since it is most likely the reflection of one of three things.⁴² First, poverty or *vulnerability* is multi-dimensional and cannot be just measured with economic indicators. This has been often emphasized by aid organizations themselves, and it has prompted the shift since the 1990s from using economic poverty as a targeting criteria to using the more generic notion of vulnerability that encompasses social and political sources of poverty (Dilley & Boudreau, 2001). It is also one of the reasons why I have chosen to use a multi-dimensional index in this study, the other main reason being that it is harder to "cheat" with a multi-dimensional index. An alternative explanation for the poor "poor" correlation could be indeed that villagers that participated in the study are used to being surveyed by aid agencies. This might be create an incentive not to answer truthfully to questions that are obviously attempting to determine wealth. This explanation is supported by the fact that the enumerator's subjective evaluation of the respondent's economic situation is more accurate than the respondents own responses.

⁴²The correlation with income is .46, and the correlations with subjective measures of economic poverty either by the respondent himself (.17) or by the enumerator (.25) are even smaller.

3.3 Other measures

Attitudes toward group members

In each round, respondents are surveyed on their attitudes toward their group members. Survey questions about within-group relationships include estimations of the level of knowledge, friendship and trust respondents have for each group member.

Pro-poor preferences

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. I include pro-poor proclivity as a control in all estimations.

Since the same questions are also included in all post-distribution surveys, it is possible to assess changes in justice norm perceptions pre- and post-treatment within individual respondents.

Expectations

Both the baseline survey and the post-distribution surveys include questions about the role of the chief in the village to assess respondents’ expectations about elites’ behaviors. After public distributions, respondents are also debriefed on their perceptions of the allocation process and its outcome.

4 Results

In this section, I present the results of the experiment. I start with a few general remarks about compliance, treatment checks and some characteristics of the sample population. Then I look at the main results of interest. For all additional findings, please refer to Appendix A.2-4.

4.1 Preliminary remarks

Compliance and Treatment Checks

There are no compliance issues in this experiment. In general, the intended main treatment (target the poor or not) seems to have been correctly understood by participants. In post-distribution surveys, participants were asked to recall whether the distribution in their previous group was intended to benefit to all group members or only to the poor members of the group. They have reported the correct treatment 95% of the time, and among elites - who were the distributors in the public sessions - only 1.8% have reported the wrong treatment.

Interestingly, respondents have perceived that the elites decision in their last public distribution complied with the treatment 70% of the time when the allocation was not targeted and and 78% of the time when the allocation was intended to benefit the poor. The elites report a compliance with targeting instructions of 73% and 83% respectively. The two measures are highly correlated ($\rho = .82$) which suggests both that targeting instructions were understood in the same way by the elites and other group members and that there are some instance of intentional non-compliance with the targeting instructions.

Identifying the Poor

An important part of the experiment relies both on the capacity of the survey team to sort respondents properly into two categories - the poor and the non-poor - so that the composition of each group can be controlled for, and on the capacity of group members to correctly identify the poorer members of their group. Participants were asked to rank subjectively each of their four group members in comparison to themselves, both in terms of economic wealth and social status. When asked whether each group member is poorer, the same or richer than themselves, non-poor group members mis-categorize poor group members (i.e. consider a poor group member to be richer than themselves) only 1.5% of the time. Conversely, poor group members mis-categorize non-poor group members (i.e. consider a non-poor group member to be poorer than themselves) only 3% of the time. It thus seems that participants did not have any problems in identifying the poorer members within their group in the same way as the experiment intended.

Interestingly, participants were also asked to rank subjectively each of their four group members in comparison to the rest of the village. But the variance in the village ranking is much smaller than the variance of the personal ranking: a lot of respondents refused the idea that there were differences within the village and gave all group members systematically the same ranks both on economic and social measures. In comparison, they had no resistance and no difficulty evaluating the difference in economic poverty and social status locally between group members and themselves. This possibly points to another explanation for the low correlation between subjective measures of poverty and our poverty index. There is widespread conception within aid recipient communities that community members should not be ranked according to their poverty, especially by a stranger, and that within villages objective differences

should be minimized: in other words “everybody is poor”.

4.2 Main Findings

In Table 1.2, I present the causal effect of targeting instructions, public setting and monitoring on the share of the windfall that the poor receives. I present the same results on the share of the windfall that the non-poor and the elites receive in Tables 1.3 and 1.4 respectively. The data presented in these tables correspond to public allocations ($n = 221$) and private allocations ($n = 221$) in which the elite was the distributor.⁴³

By construction, the share of the windfall that does not go to poor group members or to the member of the elites in the group goes to the non-poor group members. In other words, in terms of interpretation of the findings, when I find both an increase in the share that the poor receives and a decrease in the share that the elite keeps to themselves, it suggests that there is a transfer from the elites toward the poor. When I find only for the former, it rather suggests that there is transfer from the non-target group (the non-poor) toward the poor. Finally, when I find support for the latter but not the former, it could be that broader “sharing” mechanisms are at play, in which elites are prompted to share the windfall with the community at large, but not with the poor in particular.

The first, striking observation to take away from these tables is that in all specifications the value of the constant is around 5 or 6—a number that represents the numbers of tokens received on average by players in non-targeted, non-monitored, private distributions—and the effect size of each treatment is between 0 and 1 in absolute value. In other words, the story I am telling here about those group alloca-

⁴³Since the monitoring treatment only starts in round 2, models (4) to (7) include only 284 rather than 442 observations.

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tions is really about how the elites decide to allocate the two extra tokens, after they have seemingly given 5 of the 27 tokens to each of the five group members, including themselves. That said, there is a much higher variability in private allocations (in which it was not rare to observe distributor allocate to themselves more than half up to the entire windfall while other group members would receive none) than in public ones (in which the number of tokens received by any group members at any point ranges from 3 to 8). What targeting instructions and monitoring affect is precisely whom in the group receives one or two extra tokens. This finding is consistent with something that came out of the discussions in focus groups: most villagers displayed a strong norm when first talking about their village to the facilitators, and possibly to any observer from outside the village, to first and foremost describe the village as homogeneous. Yet eventually, as conversations became more specific, large differences in socio-economic situations and status among villagers appeared to be well-identified by everyone. It may also speak to the fact that a large majority of villagers recognized the importance for the chief to treat everyone somewhat equally to avoid creating conflict within the village.

Effect of Targeting Instructions

Targeting instructions have a significant, yet small positive effect on the share of the windfall that the poor receives.⁴⁴ From a baseline of 5.39 tokens (FC2700 / \$2.70), an additional 0.13 token (FC60 / \$.07) of the 27 tokens allocated (\$13.5) goes to the poor when the windfall is targeted compared to when it is not, which represents a little over 2.4% increase.⁴⁵ There is a similar increase in the share that the non-poor receives.

⁴⁴Here I consider the effect of targeting instructions on their own, hence I look at specification (1) in tables 1.2, 1.3 and 1.4.

⁴⁵There are 27 tokens allocated in each distribution, each representing FC500 / \$0.5. I present the result tables with the increase in number of tokens for readability, but in the text, I will present the findings in terms of their corresponding monetary value.

Table 1.2: Effects of Targeting Instructions, Public & Monitoring

	On the Share of the Poor						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Targeted	0.13** (0.05)	0.13** (0.05)	0.02 (0.07)	0.09 (0.06)	0.17* (0.09)	0.09 (0.06)	0.05 (0.12)
Public		0.38*** (0.05)	0.28*** (0.07)			0.39*** (0.06)	0.36*** (0.12)
Monitored				0.09 (0.06)	0.17* (0.09)	0.09 (0.06)	0.24** (0.12)
Targeted \times Public			0.21** (0.10)				0.24 (0.17)
Targeted \times Monitored					-0.16 (0.13)		-0.14 (0.17)
Public \times Monitored							-0.15 (0.17)
Targeted \times Public \times Monitored							-0.05 (0.24)
Constant	5.39*** (0.19)	5.20*** (0.18)	5.25*** (0.18)	5.35*** (0.22)	5.33*** (0.22)	5.15*** (0.20)	5.14*** (0.21)
Observations	442	442	442	284	284	284	284

With controls. Significance levels: * 10%, ** 5%, *** 1%
(Distributions by the elite only)

Table 1.3: Effects of Targeting Instructions, Public and Monitoring

	On the Share of the Non-Poor						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Targeted	0.11** (0.05)	0.11** (0.05)	0.16** (0.07)	0.06 (0.06)	-0.02 (0.08)	0.06 (0.06)	-0.02 (0.12)
Public		0.19*** (0.05)	0.25*** (0.07)			0.14** (0.06)	0.13 (0.12)
Monitored				0.04 (0.06)	-0.03 (0.08)	0.04 (0.06)	-0.10 (0.12)
Targeted \times Public			-0.10 (0.10)				0.00 (0.17)
Targeted \times Monitored					0.15 (0.12)		0.26 (0.17)
Public \times Monitored							0.13 (0.17)
Targeted \times Public \times Monitored							-0.20 (0.23)
Constant	5.34*** (0.18)	5.24*** (0.18)	5.22*** (0.18)	5.27*** (0.20)	5.29*** (0.20)	5.20*** (0.20)	5.23*** (0.21)
Observations	442	442	442	284	284	284	284

With controls. Significance levels: * 10%, ** 5%, *** 1%
(Distributions by the elite only)

Table 1.4: Effects of Targeting Instructions, Public & Monitoring

	On the Share of the Elite						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Targeted	-0.48*** (0.12)	-0.48*** (0.10)	-0.38*** (0.14)	-0.32** (0.14)	-0.34* (0.19)	-0.32*** (0.12)	-0.08 (0.24)
Public		-1.16*** (0.10)	-1.05*** (0.14)			-1.06*** (0.12)	-0.94*** (0.23)
Monitored				-0.26* (0.13)	-0.28 (0.19)	-0.26** (0.12)	-0.28 (0.24)
Targeted × Public			-0.21 (0.20)				-0.51 (0.33)
Targeted × Monitored					0.04 (0.27)		-0.24 (0.34)
Public × Monitored							0.00 (0.33)
Targeted × Public × Monitored							0.57 (0.47)
Constant	5.53*** (0.39)	6.12*** (0.35)	6.07*** (0.35)	5.74*** (0.46)	5.74*** (0.46)	6.27*** (0.41)	6.23*** (0.42)
Observations	442	442	442	284	284	284	284

With controls. Significance levels: * 10%, ** 5%, *** 1%
(Distributions by the elite only)

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Accordingly, the share of the elite decreases by four times this amount in targeted allocations compared to non-targeted allocations. From a slightly higher baseline of 5.53 tokens (\$2.75), targeting instructions cause a decrease of 0.48 tokens (\$.24), which represents a decrease of almost 9%. This suggests that targeting instructions prompt a transfer from the elite to both the poor and the non-poor.

Effect of Public Setting

The effect of the public treatment on the amount received by the poor is significant and almost three times as large as the effect of targeting instructions. In public settings, each poor receives on average \$.20 (0.38 tokens) more than it would have in a private distribution. The effect of the public treatment is also positive and significant on what the non-poor gets. The size of the direct effect of the public treatment is about twice as big as the size of the direct effect of targeting instructions with an increase of \$.10 (0.19 tokens) of the amount received by each non-poor group member.

In model (3) of Table 1.2, both the direct effect of the public treatment and the interaction of public and targeting treatments are positive and significant for the share of the poor. Targeting instructions, when provided in public settings, further increase the share received by each poor group members by \$.10 (0.21 tokens). On the other hand, looking at the share of the non-poor in Table 1.3, only the direct effect of public is positive and significant and it is about half the size of the effect on the share of the poor in model (2) —no interaction— and comparable in size in model (3) —with interaction. This is consistent with a transfer from the elite to the rest of the group between private and public distributions, and from the non-poor to the poor between public non-targeted and public targeted distributions.

The direct effect of targeting instructions drop in significance and in effect size

when accounting for its interaction with public settings —models (3) and (7). This suggests that it is actually *in public settings* that targeting instructions have an effect on the share of the windfall given to the poor, and that this is what drives the positive effect of targeting instructions overall.⁴⁶

As expected, the effect of the public treatment on the share of the windfall that the elite keeps for themselves reflects all these findings. In public settings, the elite decides to keep \$.60 (1.16 tokens) less than what they would have in private, whether in a targeted or non-targeted allocation. This represents a decrease of almost 20% of their share. (See Appendix A.3 for further analysis of the conditional effects of the public treatment).

Effect of Monitoring

According to Table 1.2, in most specifications the effect of monitoring on the amount received by the poor is either slightly significant or insignificant. There is no effect of monitoring at all on the share that the non-poor receives. However, in models (5) and (7), in which the targeting treatment and the monitoring treatment are interacted, monitoring has a significant positive direct effect of increasing by \$.08 or 0.17 tokens (resp. \$.12 or 0.24 tokens) what the poor receives. Combined with the previous findings about conditional effect of targeting instructions in public, this suggests that monitoring has an effect on the share of the windfall received by the poor when the allocation is made in private, but only then.

In Appendix A.4, I show that changes in the identity of the distributor have no effect on these main results. When I compare private distributions in which non-poor group members or poor group members are distributors to private distributions by

⁴⁶This is consistent the findings presented in Appendix A.3: in Table 1, I find that targeting instructions have a positive, significant effect on the share received by the poor in public settings only.

the elite in the same groups, the share received by the poor, the non-poor and the elite respectively are not significantly impacted by targeting instructions or monitoring. If anything, targeting instructions and monitoring seem to only have an effect on the allocation when the elites is the distributor. If anything, poor and non-poor respondents tend to display in private more predatory behaviors than their elites. In fact, the few instances in which a distributor allocated the entire windfall to themselves in private were for poor or non-poor distributors.

4.3 Heterogeneous treatment effects

In the following section, I present additional findings about the effects of the various treatments in villages that had received aid from an aid organization in the previous five years and villages that did not. I also present an analysis of the evolution of the effects of the various treatments over time by rounds.

Aid and No Aid

In Table 1.5, I show the direct causal effects of each of the three main treatments on what the poor, the non-poor and the elite receive in private and public distributions in which the elite is the distributor in the five villages in the sample that had received assistance from an NGO recently and in the five villages in the sample that had not.

I find that targeting instructions have a slightly significant, positive direct effect of + \$.08 (0.16 tokens) on the amount received by the poor only in villages that received aid. Conversely, only in villages that received aid does the provision of targeting instructions cause the elite to keep \$.27 (0.53 tokens) less of the windfall than they would have if the allocation was not targeted to the poor.

By contrast, the effect of the public treatment is positive, large and significant in

Table 1.5: Comparing the Effects of Targeting in Villages that received Aid or not

	Poor		Rich		Elite	
	No Aid	Aid	No Aid	Aid	No Aid	Aid
Targeted	0.01 (0.08)	0.16* (0.09)	0.06 (0.09)	0.10 (0.08)	-0.17 (0.19)	-0.53*** (0.16)
Public	0.26*** (0.08)	0.52*** (0.08)	0.26*** (0.09)	0.02 (0.07)	-1.04*** (0.17)	-1.08*** (0.16)
Monitored	0.05 (0.08)	0.09 (0.09)	0.16* (0.09)	0.01 (0.08)	-0.40** (0.18)	-0.21 (0.17)
Constant	5.15*** (0.29)	4.97*** (0.32)	5.21*** (0.33)	5.22*** (0.28)	6.23*** (0.64)	6.66*** (0.61)
Observations	140	144	140	144	140	144

With controls. Significance levels: * 10%, ** 5%, *** 1%
(Distributions by the elite only)

all contexts. Yet the size of the effect (0.52 tokens or \$.26) on the share received by the poor is twice as big in villages that received aid than in villages that did not (0.26 or \$.13). In the meantime, the public treatment also has a significant, positive and large effect on the amount received by the non-poor, but only in villages that did not receive assistance recently. Overall, this suggest that, in villages that are not used to receiving aid, the part of the windfall that the elites distribute to the rest of the community is split between the non-poor and the poor, whereas it goes mostly to the poor in villages that are used to receiving assistance by NGOs.

Finally, the results in Table 1.5 suggest that monitoring only has an effect in villages that are not used to receiving assistance and that this effect mostly benefits to the non-poor, who receive \$.08 more on average when allocations are monitored in non-aid villages.

These differences between aid and no aid villages are also supported by the survey data. In terms of influence of the group in public distributions, in villages that are used to receiving assistance, in 25% of the cases, respondents considered that group

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discussions did not have much influence on the elite's decision and in 36% of the cases, they report that the group had a lot of influence whereas in non aid villages, respondents report that group discussions had not much influence in 21% of the cases and that they has a lot of influence in 42% of the cases. And the perception of influence is bigger during non-targeted allocations than during targeted allocations in non aid villages whereas it doesn't vary across treatment conditions in aid villages. On the other hand, only 11% of poor respondents think they have influence in non-aid contexts (and 22% of the non-poor), while in aid villages, the rates for the poor and the non-poor are similar and are around 20% as previously reported. Finally, non-poor group members speak in favor of others significantly more often in aid contexts (40%) than in non-aid contexts (30%).

Rounds

In Table 1.6, I present the direct causal effect of the three main treatments on what the non-poor, the poor and the elite receives by round.

The analysis by rounds suggests that the effect of targeting instructions diminishes over time. While the causal effect of targeting instructions on the share of the poor (resp. the elite) is positive and significant (resp. negative and significant) in the first round, both the size and the significance of the effect disappear in subsequent rounds.

By contrast, the direct causal effect of the public treatment on what the elite gives away and what the poor gets are significant and consistent over rounds. But the positive, significant effect of the public treatment on the share of the non-poor observed in the first round (.29), while still significant in the second round is much smaller (.16) and is smaller and insignificant in the last round (.12). This suggests that the effect of the public treatment on the share that the non-poor gets disappears over time, while the effect of the public treatment on the share that the poor receives

Table 1.6: Comparing the Effects of Targeting in Successive Rounds

	Poor			Rich			Elite		
	R1	R2	R3	R1	R2	R3	R1	R2	R3
Targeted	0.21** (0.10)	0.11 (0.08)	0.02 (0.09)	0.16 (0.10)	0.03 (0.08)	0.10 (0.08)	-0.74*** (0.19)	-0.31* (0.17)	-0.29* (0.17)
Public	0.37*** (0.10)	0.38*** (0.08)	0.41*** (0.09)	0.29*** (0.10)	0.16* (0.08)	0.12 (0.08)	-1.35*** (0.18)	-1.07*** (0.16)	-1.03*** (0.17)
Monitored		0.08 (0.08)	0.10 (0.10)		0.12 (0.09)	0.03 (0.09)		-0.40** (0.17)	-0.23 (0.18)
Constant	5.04*** (0.35)	4.97*** (0.29)	5.27*** (0.29)	5.36*** (0.38)	5.35*** (0.31)	5.14*** (0.26)	6.23*** (0.68)	6.39*** (0.60)	6.08*** (0.54)
Observations	158	158	126	158	158	126	158	158	126

With controls. Significance levels: * 10%, ** 5%, *** 1%
(Distributions by the elite only)

can be sustained over time.

Finally, there are only two rounds to compare with the monitoring treatment, but the fact that it has a negative, significant effect on the share retained by the elite in the second round and a smaller, negative, non significant effect in the third round suggests that, as for targeting instructions and contrary to public settings, the effects of monitoring may also be temporary.

5 Discussion

In this section, I draw some conclusions from the results of the experiment about the existence of distributor effects, community effects and third-party effects in targeting. I also discuss more broadly the external validity of these findings, and how they shed light on our understanding of the targeting of foreign aid resources or of other windfalls in general.

5.1 General remarks

Generally speaking, the first characteristic of the findings in this study is that the effects of each treatment when they exist are rather small in size. But they are also significant, consistent and robust across specifications. The small effect size is not of much concern: the stakes in the private and public distributions, while sufficient to motivate the participant to take the simulation seriously, were relatively small.⁴⁷ 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.

⁴⁷As 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.

5.2 Importance of Community Dynamics

Overall, this study strongly supports the idea that group effects, or more generally community dynamics, play an important role in making targeting the poor effective. There is also some support for the idea that the provision of targeting instructions prompts broader sharing dynamics at the distributor-level. The one consistent effect of targeting instructions is to induce less capture by the elite. However, the part of the windfall that is not retained by elites does not necessarily go to the poor. There is consistent evidence in this study that the non-poor can also receive more as a result of targeting. Since it benefits both poor and non-poor group members, the nature of the effect of targeting instructions is consistent with sharing dynamics rather than with targeting the poor strictly speaking.

In general, there is strong support for community effects in targeting that are consistent over time and robust across specifications, as well as much larger in size than the effect of targeting instructions or monitoring. There is also evidence in support of a positive interaction of community effects (made possible by the public treatment) and distributor effects (induced by targeting instructions) on targeting the poor. In the experiment, the instance in which targeting instructions actually contribute to targeting the poor is during public distributions. Combined together, these findings could suggest that transparency about targeting criteria, and the involvement of the community at large in the process of targeting can help limit elite capture. Conditional on elite capture being limited, the role of targeting instructions is to limit capture by the non-targeted. Getting a better understanding of the community dynamics at play in aid targeting seems a promising way to further improve targeted aid programs and their outcomes. In further work based on the same experiment, I investigate whether the community dynamics at play are more consistent

with peer-pressure or with bargaining dynamics (Strauss-Kahn, 2018e).

Overall, the results of this experiment strongly point to transparency and a better involvement of the recipient community at large in the targeting process as the most efficient ways to improve the outcomes of aid programs. Conversely, spending more money and time trying to increase monitoring capacities or to define more precise targeting instructions may not be as promising leverages as one could have thought.

5.3 The Role of Outsiders

Aid agencies often think that better monitoring and evaluation systems would be enough to ensure good targeting outcomes (Transparency International, 2014). In this experiment, however, there is little consistent evidence that monitoring can have a sustainable, positive effect in targeting the poor. It could be that the monitoring treatment in this experiment is not strong enough. Monitoring treatments are particularly difficult to implement credibly and successfully. However, since monitoring seems to have an effect in some specific contexts in the study, even though relatively small in sizes, it may well be that the findings reflect the absence of a strong effect of monitoring in targeting in general.

Monitoring has a limited effect in private distributions, yet the nature of this effect seemingly is consistent with sharing dynamics rather than with targeting the poor. And I have consistently found little to no support for the effects of monitoring by a third-party in public distributions. These findings are consistent with several recent papers which suggest that altruistic preferences actually tend to disappear in dictator games when the anonymity of the process is better enforced, that is when the distributor is not pressured into being altruistic neither by the knowledge that he is being observed by a third-party nor by the judgment of the receiver in front of him (Franzen

& Pointner, 2012, Winking & Mizer, 2013). In other words, monitoring could actually work in private settings as a substitute for peer-pressure by the community. The results also suggests that third-party effects when they exist may be superseded by long-standing community dynamics. This is especially credible in cases where the interactions of the third-party with the community are limited, which is arguably the case in most humanitarian contexts. Finally, the effect of monitoring also seems to diminish over time and with previous exposure to aid programs. This idea that the effect of monitoring, when it exists, might be temporary is consistent with observations in the field that recipient communities eventually learn about the inefficiency of complaint mechanisms and that sanctions eventually loose their credibility.

In line with existing work on the influence of in-group versus out-group enforcers (Goette et al., 2006, Bernhard et al., 2006), one way to interpret both the findings about monitoring and the findings about community involvement in this paper would be that while outsiders can affect the allocation process between a distributor and a receiver, outsiders that are also part of the group (in this case non-poor, non-elite group members) are more effective than external observers. In other words, while this observation warrants additional work on this specific point, it could be that monitoring and sanctions by an in-group are more effective in aid targeting than monitoring and sanctions by an outsider.

5.4 Sharing and Conflict Management

Sharing dynamics—in which aid windfalls are shared within the recipient community at large rather than allocated to the target group only—are often observed in aid targeting contexts. Yet this possible result of targeted aid programs arguably defeats the purpose of concentrating resources on those most in need. It is also an outcome

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that is often difficult to interpret. It can either be seen as a positive outcome — a correction mechanism when targeting criteria or indicators are ill-defined⁴⁸ or as an adverse outcome —the collusion of the elites with the non-poor to the detriment of the poor. When looking at outcome perceptions by participants in this study, the results however point to neither of these explanations. Combined with qualitative information from the baseline surveys, I suggest that this study rather points to the possibility that sharing, by effectively limiting between-group differences in a public setting, could be a conflict management mechanism.

In this experiment, there is no evidence that targeting criteria were misunderstood nor that targeting indicators were inappropriate: group members had no difficulty identifying poorer members of the group compared to themselves. In cases where targeting instructions were not respected, survey evidence suggests that it is not that these instructions were not well understood by the distributors or by the community at large, but rather that they were voluntarily ignored. Yet, there is no evidence of attempts at specifically marginalizing or excluding the poor from the allocations either. This all points to the dilution of the windfall among a larger group of beneficiaries than the target group rather than to a possible collusion of the elites and the non-poor against the poor.

Participants in the experiment seem to agree on not aspiring to stronger targeting of the poor, and they declare being satisfied with outcomes in which targeting criteria were not respected. Overall, 90% of respondents report that the outcome of their last public distribution did correspond to their personal preference. And when respondents - both non-poor and poor - had the perception that the allocation out-

⁴⁸While it could also have pointed to the fact that the study's understanding and participants' understanding of "the poor" may not aligned, this interpretation is largely disproven by the survey data that shows that participants identified correctly the relative economic situation of their group members.

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come benefitted mostly to the elite or the non-poor, they were still satisfied with the outcome more than 80% of the time (70% for the poor).⁴⁹

The task of targeting the poor was considered fair and acceptable by most. Yet, survey results also point to the fact that the community in general was resistant to making differences among village members. As mentioned, in a lot of cases participants purely and simply gave the same economic and social ranking to all group members in comparison with the rest of the village. This is consistent with reports in the aid literature that recipient communities often try to argue with aid workers that “everybody is poor”.

According to ethnographic and sociological studies, public behaviors of “indistinction” can act as a mechanism for conflict prevention.⁵⁰ The weak effect size of targeting instructions could thus actually result from unexpected, counteracting community dynamics. Indistinction as a means of conflict management is also an explanation consistent with multiple reports about targeted aid interventions that recipient communities were reluctant to targeting specific groups (Maxwell & Burns, 2008). Finally, it is also consistent with the fact that the baseline shares received by each group members in public distributions were quite similar. In private distributions, there was much more variance, private distributions by elites were also much closer to equal sharing than the rest.⁵¹

Equal sharing is actually an outcome that happens rather often in community-

⁴⁹In non-targeted public allocations, 7% of respondents report that the allocation benefitted mostly to the elite (while no respondents report that the allocation benefitted mostly to the non-poor). On the other hand, in targeted public allocations, 6% of respondents report that the allocation benefitted mostly to the non-poor and powerful (while only 1% of respondents report that it benefitted mostly to the elite).

⁵⁰On the role of distinction in the organization of human societies, see also Bourdieu (1984) on the use of distinction by elites see Daloz (2009).

⁵¹In public distributions, every member of the group received between 2 and 8 of the 27 tokens to be allocated with a mean between 5 and 6, and a standard deviation by type of recipient inferior to 1. On the other hand, in distributions, allocations spanned from 0 to 27 tokens, except for the elite (0 to 14) and standard deviations were at least twice as big.

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based targeted aid programs, even though it is explicitly discouraged since it dilutes the effect of the windfall in a large pool of recipient. Similarly, although this experiment was specifically designed to avoid the possibility of equal distribution, there were a few instances in which distributors allocated 5 tokens to each group members and refused to distribute the two additional tokens and would rather give them back to the enumerator. This propensity to equal sharing speaks to the existence of strong collective norms of equality in the allocation of outside windfalls in these villages, despite large actual inequalities between groups.

For aid organizations, such observations that sharing may be a result that is sometimes strongly preferred by the recipient community to effective targeting may be of importance. It emphasizes the fact that the objectives of targeted aid programs (economic redistribution to the poor) and the objectives of humanitarian and development assistance at large (do no harm) can sometimes be conflicting. Targeting has indeed effects both on economic redistribution and social cohesion. If there are contexts in which the compliance with targeting instructions work against the preservation of peaceful relationships within the community, there is a choice to make in terms of which sets of objectives should subsume to the other.

5.5 Pro-poor Preferences and the Role of the Elites

In this experiment, there is little evidence of distributor effects that could play in favor of targeting the poor. Targeting instructions do not seem to affect distributors' preferences into being more prosocial and there appears to be no groups in the community that have significantly more pro-social preferences than the elites such that they would allocate bigger shares of the windfall to the poor if they were to be in the position of the distributor.

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Overall, there is not as much heterogeneity between groups as one could have expected within the sampled communities in terms of prosocial preferences, attitudes and behaviors. 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: the poor do not have more prosocial preferences than the rest of the general population.⁵² And overall, the communities in the sample do not have strong prosocial norms to begin with: prioritizing the poor in allocations is not the preference, nor the expectation in usual, non-targeted distributions. Despite having more prosocial preferences than their elites, both poor and non-poor group members do not behave accordingly when they are the distributor in private allocations: they do not allocate greater shares of the windfall to the poor than the elite would do. On the contrary, one thing that varies with changes in the identity of the distributor in private allocations is the range and variance in one's allocation to oneself. For both poor and non-poor distributors, there are instances in which the distributors take all 27 tokens for themselves, which never happens with elite distributors. By contrast, despite having less pro-poor preferences than the general population, elites actually behave in a more prosocial manner than the rest of the population in private distribution.

Not only are baseline preferences across groups within the community not consistent with usual theories about distributor effects, observed changes in those preferences are not in the expected direction either. If anything, the targeting treatment seem to further decrease participants' prosocial proclivities. And while there are

⁵²While counter-intuitive and against their self-interest, this aversion can be explained by the existence of a stigma in being assisted and speaks to the multi-dimensionality of vulnerability. Poor community members may be rendered less economically vulnerable by recognizing their status as poor, but they could become in the same time more socially vulnerable. This is also consistent with the observation that the poor refused at least as much as the rich if not more to rank other people in the village in terms of their wealth.

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changes in conceptions of fairness and pro-poor preferences within individual respondents pre- and post-treatment, there is little evidence that these changes are due to the treatment since they happen in both ways. After the first round, the proportion of non-elite respondents that favor giving more to the poor has decreased from 42% to 36% (pooling both those who received the targeting treatment and those who did not), while 54% of respondents now prefer giving the same to everyone. But among elites, it is the contrary: the proportion in favor of giving to the poor has increased from 26% to 44% after the first round of distributions. Among those who have changed their preferences after receiving the targeting treatment, 46% changed in favor of more pro-poor preferences (56% among elites, 40% among the poor, 46% among the rich), but 54% changed for less pro-poor preferences. After receiving the non-targeted treatment, 30% of those who changed their preferences were more pro-poor than in the baseline (41% among elite, 30% among the poor, 25% among the rich), and 70% were less pro-poor than before.

Overall, these results suggest that changing the identity of the distributor might not be an effective way to target the poor. In the same time, while elites seem to ultimately behave in a more prosocial way than the rest of the community, this behavior is not consistent with their own preferences. I suggest that this observed disconnect between preferences and behaviors can reflect the fact that in this experiment, and in aid targeting more generally, elites actually have two roles to play. Each of these roles come with a different set of constraints that are hard to reconcile. As community leaders, they have duties toward the community as a whole: this points toward sharing windfalls among all community members. As an agent the aid organization intervening in the community, they are temporarily in the position to capture the windfall according to their self interest, while in the meantime being prompted to target the poor.

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While it may come as a surprise to some aid workers, in many traditional societies, it often is not the role of chiefs to redistribute to the poor. For example, only 4% of respondents in the sample actually think that taking care of the poor is one of the roles of the chief. Contrarily to representatives in western societies, most of the time traditional leaders are not elected and they often retain their chieftom for life. As a result, there is arguably no natural accountability mechanism that makes elites cater to poor populations in order to gain votes, which is a common explanation of preferences for redistribution. On the contrary, according both to the elites and the general population of the communities in our sample, the primary role of a chief is to avoid conflict within the village. And this finding most likely applies to many other aid recipient communities (Strauss-Kahn, 2018c). In other words, this could explain that chiefs —or their representatives— are expected to allocate resources to all community members indiscriminately whenever possible.⁵³

Several anthropological studies have suggested that capture can also be a way for a chief to express and to legitimate power (Boehm & Boehm, 2009). In other words, a chief that doesn't take what he can where it can may be considered not strong enough to be chief. If capture is a display of strength, then changing the distributor to non-elites could actually increase the need to display and legitimate said power and lead to more capture by the distributor rather than less. In the experiment, non-elite distributors clearly had less self-control during private allocations, especially during the first round. Several of them, poor and rich alike, did not hesitate to capture the entire 27 tokens at their disposal. Conversely, targeted instructions have an effect on

⁵³According to the baseline survey, 87% of respondents think the chief should treat everyone in the village equally. Among the elites themselves, the perception that the role of the chief is to treat everyone equally rises to 96%. This contrasts with the fact that 42% of the same populations think that it would be fairer to give more to the poor in a distribution. In a sense, there seems to be somewhat of a mismatch between villagers' expectations about their elites and their own aspirations: villagers seem to have more pro-poor preferences than their institutions entail.

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elites as distributors in the sense of diminishing capture, but not on any other type of distributors.

Overall, these findings about the behaviors of alternates to elites as distributors may be of importance for aid organizations. Aid agencies sometimes attempts to bypass local power structures by changing the distributor from traditional elites to women, new elites, or other “gatekeepers”. The expectation is that these alternate distributors are less likely to capture the aid windfall. While this might be the case in the short term, several findings of the experiment suggest that it may not be the case in the long-term. First because these alternate distributors do not have more distributive preferences than the elites, and they don’t have either preferences for sharing that would stem from their duties toward the community at large, so there are no reasons why they would be guided by anything other than their self-interest. This is already reflected in this experiment in extreme capture behaviors in many non-monitored private distributions by non-elites.

5.6 Learning Effects

Finally, the findings of the experiment support the existence of learning effects in targeting both over time and by exposure to the targeting process. For an aid agency, these learning effects can be either fostering or hindering the implementation of a successful targeted aid program. On the one hand, while targeting instructions and monitoring do seem cause actual targeting of the poor, their capacity to do so also seem to diminish over time. Only the public effect of the targeting process seem to be stable over time. On the other hand, in places that are used to receiving assistance and, hence, to aid targeting, there seems to be more actual targeting of the poor and less sharing of the assistance with non-targeted beneficiaries.

6 Conclusion

In aid targeting, there are three characteristic elements that always go together: the targeting instructions, the community involvement and the monitoring. Since there is always a little bit of all three albeit in various ways, it is difficult to distinguish what does what in the targeting process. This paper aims at shedding light on the causal effect of targeting instructions and these two moderators. In doing so, I aim at contributing to a better understanding of the variation in the outcomes of targeted aid programs and to foster their improvement in three ways.

First, the results of this paper contribute to debunk some traditional assumptions in the aid world, by calling into question the effectiveness of monitoring by a third-party. Second, this paper points to the existence of strong sharing norms in recipient communities that can run contra the implicit redistributive norms of aid targeting as well as to possible underlying tensions for elites between their role as a distributor and their role as community leaders. As such, rather than focusing on the capacity of the aid agency as a principal to enforce a contract with the recipient community as a whole, these observations call overall for a better understanding of community dynamics and the ways in which they can either foster or hinder the effectiveness of targeting. Finally, the results suggest that rather than targeting instructions by themselves, it is mostly the public aspect of the targeting process that consistently contribute to the effective targeting of the poor over time.

In subsequent work, using the same setting, I further unpack the community dynamics at play in public distributions in order to gain a finer understanding of the bargaining dynamics and peer-pressure effects that actually prompt effective targeting of the poor (Strauss-Kahn, 2018e). While this paper already highlights the potential role of non-poor non-elites community members in fostering successful targeting,

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additional work will explore the nature of their influence on elites' decision-making as well as offer a more detailed theoretical framework that models the interactions between these various actors within recipient communities (Paler et al., 2018).

Inside & Out

The Role of the Non-Poor in Targeting Resources to the Poor

Camille Strauss-Kahn*

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.

*Ph.D. candidate, Columbia University (Email: cs2899@columbia.edu). The author would like to thank Macartan Humphreys, Chris Blattman, Grant Gordon, and the participants of the advising group at Columbia University as well as all the group from WZB. The author would also like to thank Annie, Doudou, Koko, Aimable, Yves, and all her team of enumerators in DRC.

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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, 2018g). 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, 2005a). 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., 2013a). 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., 2012a), 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, 1999a, Young & Maxwell, 2009). There is also

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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, 2004a, Alatas et al., 2013a). 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

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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

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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

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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 affluent 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).⁵⁴ Involving recipient communities in the targeting process through community-driven programs may seem much more feasible and effective than monitoring in many contexts. Yet

⁵⁴In 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.

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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, 2005a, Bardhan & Mookherjee, 2006a) or between elites and non-elites (Alatas et al., 2013a).⁵⁵ Yet, there is numerous anecdotal evidence that

⁵⁵Alatas et al. (2013a) 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.

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non-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-

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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 (2005a), Bardhan & Mookherjee (2006a), 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).⁵⁶ 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.

⁵⁶There 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).

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For example, 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.⁵⁷

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

⁵⁷Typically, 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.⁵⁸ 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

⁵⁸In 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.

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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).⁵⁹ 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.⁶⁰ 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

⁵⁹See 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.

⁶⁰See 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.⁶¹ The non-elite respondents were a gender-balanced, representative sample of the rest of the village.⁶² 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.⁶³ 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

⁶¹The 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.

⁶²The representative sample of the rest of the village specifically excluded the chief and members of the elite, but it could include their family members.

⁶³See the Experimental Protocole as well as Strauss-Kahn (2018a) for a more detailed discussion of the experimental setup.

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ballot boxes on top of which there is a picture of one of the group members.⁶⁴

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.⁶⁵

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-

⁶⁴Participants 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.

⁶⁵See 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.⁶⁶

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 ($M_1 \in \{\text{non-targeted, Targeted}\}$), a dichotomous outcome (Y) and a control ($C \in \{\text{non-monitored, Monitored}\}$), where X may affect Y directly and/or X may affect M_1 , which then affects Y , is:⁶⁷

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

where α is the direct effect of having veto players on the decision of the distributor ; β_1 is the direct effect of targeting instructions ; β_2 is the direct effect of monitoring ; and γ_1 is the effect of the interaction of targeting instructions and veto power.

In terms of potential outcomes:

⁶⁶Due 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.

⁶⁷There 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-MONITORED</i>	<i>MONITORED</i>	<i>NON-MONITORED</i>	<i>MONITORED</i>
<i>TARGETED</i>	$Y(0,T,m)$	$Y(0,T,M)$	$Y(1,T,m)$	$Y(1,T,M)$
<i>NON-TARGETED</i>	$Y(0,t,m)$	$Y(0,t,M)$	$Y(1,t,m)$	$Y(1,t,M)$

Table 2.1: 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.⁶⁸

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.⁶⁹

Group discussions

Enumerators have also recorded information about who participated in discussions during public distributions.⁷⁰ 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.⁷¹

⁶⁸See Field Manual for more information about the data collection process the questionnaires, and the focus groups.

⁶⁹Social 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.

⁷⁰All 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.

⁷¹The 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-

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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.⁷²

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.⁷³ 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.

⁷²The 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.

⁷³In 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. For all additional findings, please refer to Appendix A.2-4.

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.⁷⁴ 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.

⁷⁴This observation holds for public distributions specifically. There is more variance in private distributions. See Strauss-Kahn (2018a) for more details.

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 concern: the stakes in the distributions, while sufficient to motivate the participant to take the simulation seriously, were relatively small.⁷⁵ 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 2.2, 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

⁷⁵As 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 2.2: 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)

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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 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 2.3, 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

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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%).

Table 2.3: 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 2.4: 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)

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In table 2.4, 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 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 2.4 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).⁷⁶ 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

⁷⁶The 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.

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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 difference 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 2.5 shows the propensity of group members to argue for themselves while table 2.6 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 2.5 suggests that the various treatments have no effect on the propensity of group members to advocate selfishly for a share of the windfall, table 2.6 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

Table 2.5: 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 2.6: 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)

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targeted to the poor.⁷⁷ 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 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 2.2 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.⁷⁸ While there is no

⁷⁷Even 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.

⁷⁸By 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

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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.⁷⁹ 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 2.7, 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

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

⁷⁹Arguably, 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 2.7: 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)

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 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 2.8, 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.⁸⁰

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

⁸⁰I 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 2.8: 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)

more broadly on our understanding of aid targeting.

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

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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.⁸¹ And if anything, receiving targeting instructions seem to actually make group members less prone to pro-poor attitudes.⁸²

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.

⁸¹As 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.

⁸²This 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.

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 veto power over the elites' decision.⁸³ 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

⁸³For 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.

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 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.⁸⁴ 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

⁸⁴In 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.

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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 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.⁸⁵ 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.

⁸⁵See additional results as well as Lavergne & Strobel (2004) and Engelmann & Strobel (2006) on maximin and inequality in group allocations.

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 a group that is not targeted. In this study, it is the non-poor in distributions were 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 suggest 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.

Is Bigger Always Better?

How Targeting Bigger Aid Windfalls Affects Capture and Social Cohesion

Laura Paler* Camille Strauss-Kahn* & Korhan Koçak*

Abstract

A central challenge in development involves ensuring that humanitarian and development aid reaches those in greatest need. Aid agencies typically try to achieve this by targeting aid to vulnerable individuals or groups. Despite the prevalence of targeting, we know little about its effects on distributional outcomes and social cohesion in communities where some are intended to benefit and others are excluded. We investigate this by formalizing targeting as a bargaining game with coalition formation involving three players—the target group, the elite, and an excluded group. We find that whether more aid reaches the target group depends on competition between elites and the excluded group. We provide support for predictions using a regression discontinuity design and original survey data from an aid program implemented in Aceh, Indonesia. This paper demonstrates the importance of understanding the role of community dynamics in shaping the economic and social outcomes of targeted aid programs.

*Assistant Professor, University of Pittsburgh.

*Ph.D. candidate, Columbia University

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One of the central challenges in development involves ensuring that humanitarian and development aid—whether provided by international or domestic, governmental or non-governmental actors—reaches those in greatest need. In order to achieve this, most aid agencies rely on some form of targeting. Targeting is the process of setting criteria for who should receive aid, identifying eligible beneficiaries, and delivering resources to them. Vast amounts of assistance are channeled through targeted aid programs to individuals, households, or groups. More than 85 percent of the aid intended for individuals now takes the form of targeted distributions of divisible goods like money and food (Wahlberg, 2008, Barrett, 2006). For instance, the World Bank has supported approximately 400 cash transfer projects targeting the poor in 94 countries valued at almost \$30 billion (Wong, 2012). In recent years, the World Food Program has targeted 54 percent of 4.4 million metric tons of food aid to vulnerable populations (World Food Programme, 2011)

Despite the prevalence of aid targeting, its consequences for the economic and social outcomes at the heart of concerns about aid effectiveness has received relatively little attention in the literature.⁸⁶ The main goal of this paper is to examine what happens after aid reaches a recipient community and, especially, when targeting aid will succeed in delivering more benefits to those for whom it is intended when some individuals are eligible to receive assistance and others are not.

In doing so, we argue that understanding the consequences of targeting aid depends on examining dynamics within the communities in which intended beneficiaries live. Communities play a role in almost all targeted aid programs because successful

⁸⁶For one review of the aid literature, see (Wright & Winters, 2010). For exceptions to the lack of literature on individual-level aid targeting, see Winters, 2014, Jablonski, 2014, Alatas et al., 2012b. Much of the literature on aid targeting has employed cross-national research to explain how aid is targeted across countries or localities. Micro-level research on aid has tended to focus on the effectiveness of specific interventions but do not examine the effects of targeting *per se* (see, for example, Beath et al., 2013, Fearon et al., 2009).

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targeting is challenging for aid agencies, especially for those operating in low-income or fragile countries.⁸⁷ In some cases, aid agencies opt for community-based targeting—in which community members or leaders select beneficiaries—in the belief that it is more sensitive to local knowledge and context (Coady et al., 2004). Even in settings where aid agencies identify beneficiaries through more objective data-driven methods, they nonetheless often face time, resource, and information constraints that lead them to turn to communities for assistance at different stages of the targeting process (Alatas et al., 2013b, Jablonski, 2014).⁸⁸

While community involvement in targeting can result in greater satisfaction and other benefits (Winters, 2014, Alatas et al., 2012b), it can also have unwelcome consequences such as elite capture, non-beneficiary capture, and heightened social divisions. One Oxfam program that aimed to help drought victims in three East African countries helps to illustrate the variation. As Jaspars & Shoham (1999b) detail, in Tanzania, the program successfully targeted the most drought-affected households while maintaining a high level of community satisfaction. In Kenya, communities were also pleased with the program but extensive mis-targeting occurred. Finally, in South Sudan, there was both extensive elite capture and communal fighting over the aid, resulting in local tensions that endured long after the program ended.

Existing studies on targeting within communities have limited ability to explain such variation in effective targeting, capture, and social tensions. For one, they often study either elite capture (Bardhan & Mookherjee, 2006b, Alatas et al., 2013b) *or* non-beneficiary capture (Galasso & Ravallion, 2005b) but rarely study both together. In doing so, they overlook the fact that elites and non-beneficiaries can be independent

⁸⁷A targeted aid program is typically considered successful when the number of eligible households that did not receive benefits (exclusion error) and ineligible households that did receive benefits (inclusion error) is small (Coady et al., 2004).

⁸⁸For a review of different approaches to targeting, see Coady et al. (2004).

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actors who have their own strategic interests and who might each seek to appropriate a share of the aid windfall. Second, existing studies on aid targeting often focus on either its economic or social consequences but rarely consider how these relate at the local level. For instance, research on targeting within communities in non-conflict settings has primarily focused on economic outcomes, with little attention to its affects on social cohesion within those communities (Bardhan & Mookherjee, 2006b, Galasso & Ravallion, 2005b, Alatas et al., 2013b). Alternatively, there is a growing literature concerned with the effects of targeting aid at vulnerable populations on rebel or government-initiated violence (Wood & Sullivan, 2015, Zurcher, 2017). Yet, these studies have generally not yet addressed the question of when target populations are more likely to benefit or how conflict outcomes vary by local context.

In our model, the elites offer a division of the aid to the target group and to non-beneficiaries (hereafter the excluded group), which in turn decide whether to accept the offer or contest it. If contestation occurs, groups may form coalitions. Equilibrium strategies depend on three parameters: the amount of aid (which determines the stakes of the game); the relative influence of the groups (which determines bargaining power); and the quality of group relations (which determine the costs of contestation).

The model shows that, when windfall size is small, the benefits of contestation to the excluded group do not exceed the costs, resulting in elite capture. As windfall size increases, however, the excluded group becomes more likely to contest but will only do so under certain conditions, namely when it is both influential (meaning it has more bargaining power) *and* has bad relations with other groups (reflecting lower costs to contestation). It is in precisely those communities with a high threat of excluded group contestation (hereafter ‘high threat’ communities) that elites offer the target group more in order to buy their support and prevent excluded group contestation. In this way, our model shows how successful targeting depends *not* on

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the bargaining power of the target group but rather on competition between two more powerful players in the community. It also underscores the sobering fact that it is hard to improve targeting without also increasing mis-targeting: bargaining among the excluded group and elites results in greater allocations not only to the target group but to the excluded group as well.

An additional implication of the model is that better aid targeting can come at the expense of social cohesion. While bigger aid windfalls result in better targeting in high threat communities, they also increase the likelihood of contestation everywhere. Since we model the costs of contestation as the deterioration of group relations, this means that increasing distributions to the target group might invariably result in worsened social outcomes.⁸⁹ We note, however, that actual contestation is not necessary to drive the predicted distributive outcomes; the *threat* of contestation is sufficient. Nevertheless, it is important to investigate the effects of targeting bigger windfalls on social cohesion since aid agencies—which typically operate under a ‘do no harm’ principle—hope that their programs to improve economic well-being will not do so at the expense of social welfare.

The model developed here is relatively general and could be tested in a wide variety of targeted aid programs in both conflict and non-conflict settings. We provide a test of the predictions in the context of one post-conflict community-driven development (CDD) project implemented in the Indonesian province of Aceh. The BRA-KDP program studied here aimed to promote both economic welfare and social cohesion following 30 years of separatist conflict between the Free Aceh Movement (*Gerakan Aceh Merdeka*, or GAM) and the central government of Indonesia. Two features of BRA-KDP make it well-suited to an empirical test of the theory. First, BRA-

⁸⁹Modeling contestation as worsened relations accords with anecdotal reports of heightened social divisions. For instance, de Sardan (2014) notes with respect to a program in Niger: “Cash transfers are not the devil...They are sharpening conflicts that are already there.”

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KDP targeted civilian conflict victims, which enables us to examine how community dynamics among victims, an excluded group of former GAM combatants, and village elites shaped distributive outcomes and social relations. Second, BRA-KDP used an arbitrary cutoff in village population to determine windfall size, which allows us to use a regression discontinuity design to gain causal leverage over a key parameter in the model. We draw on original survey data from 504 civilians, former combatants and village heads to estimate how windfall size and the threat of excluded group contestation interact in driving distributive and social outcomes in 75 BRA-KDP villages.

Consistent with the main predictions of the model, we find that bigger aid windfalls resulted in the target group receiving a greater share in communities with a high threat of *excluded* group contestation. Conversely, targeting more aid resulted in a smaller share going to the target group in lower threat communities. We also show that bigger aid windfalls resulted in the excluded group getting more, and elites less, in high threat relative to lower threat communities. While our findings on social cohesion are more suggestive, our results indicate that bigger windfalls reduced acceptance of former GAM combatants overall but improved conflict resolution in high threat villages with bigger windfalls. This pattern is consistent with a story in which distributive outcomes in high threat villages are due to the greater *threat* of excluded group contestation rather than outright contestation, and that avoiding contestation might have actually yielded social benefits.

This paper makes several contributions to research on aid effectiveness in conflict and non-conflict settings. First, it sheds important light on the conditions under which aid targeting is more likely to be effective, emphasizing the importance of windfall size and the presence of an excluded group that is willing and able to challenge elite authority. Second, by distinguishing between three groups in a community, it

helps to clarify when elites or non-beneficiaries are more likely to appropriate aid, which is essential to obtaining a clear picture of the nature and extent of capture. Third, it clarifies when effective targeting might come at the cost of social cohesion, with important implications for the design of targeted aid programs. And, finally, by considering how windfall size interacts with community characteristics, it adds nuance to a large literature on the ‘aid curse’ by showing how bigger windfalls can be helpful or harmful depending on local conditions. We return to these contributions in the conclusion.

1 Theory

We begin by developing a simple formal model to shed light on how community dynamics shape distributional outcomes from a targeted aid program. We make four assumptions that we build into the model: (1) communities can in fact influence distributional outcomes; (2) there is a target group that is vulnerable; (3) elites have some authority over distributions and can also try to capture aid for themselves; and (4) there are other community members who are ineligible to receive benefits but who can also try to capture a share of the aid. Recognizing that targeted aid programs create *three* players—the target group, the elites, and the excluded group—that can influence distributional outcomes is the main innovation of our approach. Before turning to the details of the model we explain these assumptions and characterize the players.

First, we assume that communities can influence the distributional outcomes of targeted aid programs. In some cases, aid agencies opt for community-based targeting approaches, knowingly relinquishing some control in exchange for a process that is more sensitive to local context and information (Coady et al., 2004). In other cases,

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aid agencies face logistical constraints that lead them to rely (at least to some extent) on community assistance, for instance by confirming lists of beneficiaries or managing distributions. Even when aid agencies seek to control the targeting process, the same constraints can limit their monitoring and enforcement abilities, which again creates scope for community dynamics to influence targeting outcomes.⁹⁰ While aid agencies take steps to mitigate capture and mis-targeting, they are difficult to eliminate. We thus follow on Galasso & Ravallion (2005b) in assuming that the aid agency has imperfect control over aid targeting, which shifts our focus to understanding the importance of community dynamics.

Our second assumption is that there exists a target group that is supposed to receive the most benefits but that is weak. We note that aid agencies often aim to deliver assistance to the most vulnerable elements within a community, such as the poor, widows, internally displaced persons, or conflict victims (NRC, 2013, OCHA, 2014, de Sardan et al., 2015). Vulnerable groups are targeted precisely because they are often the most in need and the most at risk of being marginalized from resource allocation without special consideration. While targeting can help to empower recipients to hold agencies and elites accountable (Winters, 2014), we follow on existing research that suggests it is unlikely that targeting can be so empowering as to erase existing power asymmetries within the community (Galasso & Ravallion, 2005b, Bardhan & Mookherjee, 2006b, Dreze & Sen, 1989). Indeed, what is unique about targeting—and what differentiates it from other distributive contexts—is that it makes a weak group a relevant player despite its lack of formal bargaining strength. We reflect the weakness of the target group by modeling it as a player that has relatively low levels of influence within the community.

⁹⁰The most common way to enforce targeting criteria is to punish violations by making future distributions of aid conditional on previous performance, but there are also significant challenges to conditionality (Paul, 2006b).

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Our third assumption is that elites, as individuals with formal political authority in the village, are often in a position to influence how aid is allocated and to capture a share of the aid for themselves. When aid agencies involve communities in targeting, they typically turn first to community leaders to assist with identifying beneficiaries or delivering assistance. While this can help to ensure that targeting incorporates local knowledge, it also invariably creates scope for elite capture (Platteau, 2004b, Angeles & Neanidis, 2009, Alatas et al., 2013b, Bardhan & Mookherjee, 2006b). Dreze & Sen (1989, 107) summarize concerns about elite capture in targeted aid programs:

The leaders of a village community undoubtedly have a lot of information relevant for appropriate selection. But in addition to the informational issue, there is also the question as to whether community leaders have strong enough motivation—or incentives—to give adequately preferential treatment to vulnerable groups. Much will undoubtedly depend on the nature and functioning of political institutions at the local level, and in particular on the power that the poor and the deprived have in the rural community. Where the poor are also powerless—as is frequently the case—the reliance on local institutions to allocate relief is problematic, and can end up being at best indiscriminate and at worst blatantly iniquitous, as numerous observers have noted in diverse countries.

One important piece of the puzzle of explaining when elites distribute to the target group—and our fourth assumption—is that there exists yet another group in the community that can also influence how aid is allocated: the excluded group. Critically, targeting *by definition* creates beneficiaries and non-beneficiaries, or individuals who live in the community but who do not meet the eligibility criteria and therefore should not receive benefits (Duffield, 1996). Who comprises the excluded group depends on the nature of the program, but could be the non-poor in programs targeted at the poor; men in programs targeted at women; members of an ethnic majority in programs targeted at an ethnic minority; host community members in a program targeted at migrants or refugees; rebel groups in programs targeted at vulnerable

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populations; or (as in our empirical case) ex-combatants in a program targeted at civilians.

Unlike elites, the excluded group does not have a formal role in the targeting process. There is, however, evidence that non-beneficiaries also often intervene to try to expropriate a share of the resources for themselves (de Sardan, 2014, Kilic et al., 2013). For instance, in one cash transfer program in Niger, non-beneficiaries contested a targeted aid program designed to assist widows, the disabled, migrants, and women from vulnerable households (de Sardan et al., 2015). In Bangladesh, Galasso & Ravallion (2005b) find that non-beneficiaries in a community-based targeting program were more likely to try to capture aid intended for the poor in villages with high income inequality (implying that the non-beneficiaries were relatively powerful). Wood & Sullivan (2015) show that, in conflict settings, rebel groups often aim to appropriate aid targeted at vulnerable civilian populations. Importantly, while the problem of non-beneficiary capture is well recognized, much of the literature to date—especially that on non-conflict settings—has overlooked the strategic role of the excluded group independent of both the elites and the target group. The main contribution of our approach is thus to model the excluded group as a third player that is also relatively influential and that has the option to contest an aid allocation proposed by the elites.

All in all, the numerous accounts cited above suggest that targeting aid windfalls can induce competition over resources by different groups within a community, namely a target group, elites, and an excluded group of non-beneficiaries. We note that one additional factor—the *size* of the aid windfall—plays a crucial role in the competition by determining the stakes of the game. In our model, bigger windfalls make contestation more attractive to the excluded group, but whether it acts to appropriate that bigger windfall also depends on its pre-existing influence within the community and on the quality of its relations with other groups. It is the interaction

of bigger windfalls and these aspects of local context that make excluded group contestation more likely, which in turn drives elites to make the target group a better offer.

1.1 Model

We model aid distribution as a bargaining game between elites L , excluded group X , and target group T with both bargaining breakdown and coalition formation. The timing of the game is as follows. Given the size $S > 0$ of the windfall, the strategic interaction begins when the elite L proposes a take-it-or-leave-it division of the aid windfall among the three players $\alpha = (\alpha_L, \alpha_X, \alpha_T)$.⁹¹ The excluded group X observes α and decides whether to accept the elite's offer or not. If X accepts, the game ends and the windfall is divided according to α . If X rejects, we say there is contestation.⁹² Contest winners share the aid among themselves while the losers get nothing.

If the excluded group chooses to contest the elite's proposal, they can try to sway the target group to their side by making them an offer $\hat{\alpha}$. T observes the offers from both L and X and decides which powerful group to form a coalition with; depending on the offers, probabilities of winning, and costs of contestation defined below. If it sides with L , with probability $1 - p_X$ they win and the outcome is $(1 - \alpha_T, 0, \alpha_T)$, and with probability p_X the excluded group wins and gets the whole windfall, $(0, 1, 0)$. Similarly, if T sides with X , they win with probability p_{XT} and the outcome is $(0, 1 - \hat{\alpha}_T, \hat{\alpha}_T)$, and with probability $1 - p_{XT}$ the outcome is $(1, 0, 0)$.⁹³ Either way,

⁹¹We assume that the size of the aid windfall is exogenous to characteristics of the communities, as in Galasso & Ravallion (2005b) and our empirical context.

⁹²Conceptually, contestation could take different forms depending on the context, ranging from predation or extortion in conflict-settings to major disagreement in community meetings in non-conflict settings.

⁹³We assume that the probability of winning a contest is weakly greater for a coalition than the sum of the probabilities of each of its constituents, $p_{ij} \geq p_i + p_j$ for all $i, j \in \{L, X, T\}$. We deliberately do not assume a functional form to keep the analysis as general as possible, however in a real world

the game ends after T 's choice of coalition and payoffs are realized.

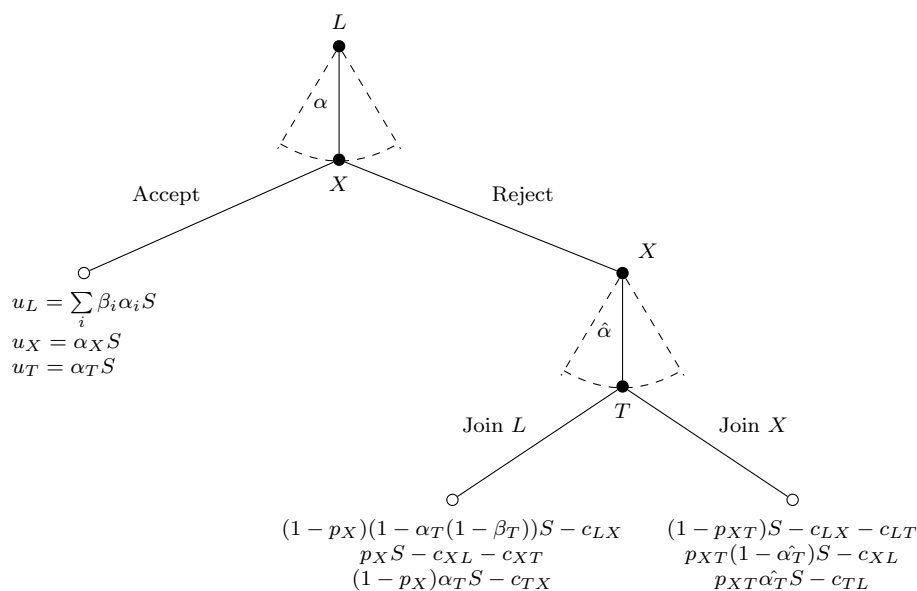


Figure 1: Extensive form of the game.

Each player derives utility from the amount of the windfall they receive, but they incur costs from contestation. While these costs and benefits capture X and T 's utility functions completely, we assume that the elites care not only about short-run benefits from the windfall but also their relative power in the long-run. The elites' utility function therefore also includes weights that they attach to the bargaining share received by other groups when contestation is avoided. See Figure 1 for an extensive form of the game.

In writing utilities, we focus on two aspects of community relations that are intuitively important to understanding community dynamics but also conceptually distinct. The first aspect is the quality of relations between the groups. Better relations bring economic and social benefits, such as trade, information-sharing, intermarriage, and social insurance. It is often argued that the better relations are, the more any

setting we expect p 's to depend on factors such as group size, wealth, or access to means of coercion.

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one actor has to lose by taking an action that might do long-lasting harm to those relations and disrupting access to such valuable benefits (Polachek, 1980, Baker et al., 2002). We follow on this logic to assume that, *ceteris paribus*, better relations make contestation *less* likely. We capture the costs of contestation as a loss of above-mentioned benefits, supposing that each group i pays a cost $c_{ij} > 0$ for all groups j they face off against during contestation. Thus, groups that have good relations with the rest of the community will face higher costs of contestation.

A second feature of community interactions pertains to the influence of different groups in the community, particularly whether groups are weak or strong. By influence, we refer to attributes including but not limited to group size or access to resources that improve a group's abilities to influence outcomes. To understand how variation in the influence of groups affects bargaining outcomes, we write the elite's reduced form continuation payoff as follows: $u_L(\alpha) = \sum_i \beta_i \alpha_i S$ where β_i refers to the weight L assigns to the share of group i (Galasso & Ravallion, 2005b). We fix the weight the elites assign to their own share to one, $\beta_L = 1$. We assume that elites care more about their own share of the windfall than others', $\beta_i < 1$ for $i \in \{X, T\}$, and so would keep the whole windfall for themselves in the absence of a credible threat of contestation.

These weights allow us to capture two distinct and diametrically opposed incentives for the elite. On one hand, when pressed, the elite can behave generously and opt to share the windfall with others in the community, for instance because their legitimacy depends in part on keeping others happy or because they want to be seen as complying with aid agency requirements. We refer to these as *reputation* considerations. On the other hand, the elite fear giving resources to other influential groups that might one day use these resources to challenge their political control. We refer

to these as *rivalry* considerations.⁹⁴ Thus, we assume that weights assigned to the shares are lower for more influential groups. With respect to the excluded group, the rivalry considerations dominate the reputation considerations (since the group is influential and not supposed to receive aid anyways), and we have $\beta_X \leq 0$. For the target group, reputation considerations dominate rivalry considerations (since the group is weak and is supposed to receive aid), resulting in $\beta_T \geq 0$.

Our solution concept is Subgame Perfect Nash Equilibrium.⁹⁵ There are three types of equilibrium outcomes. First, when the windfall is small, contestation never occurs in equilibrium because the costs of contestation for X exceed the potential benefits.⁹⁶ In such cases, elites capture the entire windfall. Second, when the windfall is large and the costs of contestation for the excluded group are very low, there is always contestation in equilibrium.⁹⁷ Formally, there is a threshold $c^*(\beta_X, S)$ which we define in Appendix A such that when $c_{XL} + c_{LX} + c_{XT} \leq c^*(\beta_X, S)$, there is no possibility to find a negotiated solution. The intuition behind unavoidable contestation is straightforward: when the excluded group is very influential *and* has bad relations with other groups, the elite's concerns about empowering them overcome their incentives to maintain good relations. In this case, the elites set $\alpha_X = 0$ and $\alpha_T = \frac{p_{XT} - p_X}{1 - p_X} + \frac{c_{TX} + c_{XT} - c_{TL}}{(1 - p_X)S}$, the excluded group rejects, and the target group sides

⁹⁴An interesting extension of this model would be to look at a repeated version of this game where aid received in previous periods change the influence of groups in later periods. While a complete analysis of a repeated bargaining game is beyond the scope of this paper, the reduced form payoff function of the elite captures this intuition.

⁹⁵To avoid multiplicity of equilibria and open set problems, we assume that each player when indifferent accepts the most recent offer. Similarly, we assume that when a group is indifferent between offering zero and a positive amount to another group, they offer zero.

⁹⁶See Appendix A for the formal statement of this condition.

⁹⁷This is consistent with work on the possibility of disagreement under complete information. For instance, Laengle & Loyola (2015) show that bargaining breaks down in equilibrium when one player derives negative externalities from the share received by another player. We show that introducing a third player (the target group) reduces the range of bargaining breakdown. When the excluded group and the elite are rivals, each might not want to let the other capture aid but both can agree to distribute more to the target group, which presents a threat to neither.

with the elite.⁹⁸

Finally, aside from these two more extreme outcomes, there is a third equilibrium outcome in which S is large enough for contestation to be feasible but relationships are not bad enough for contestation to be inevitable. We now focus on this intermediate situation and look at how different parameters affect the target groups share. To avoid contestation the elite must make sure X is at least as well off accepting the offer as rejecting. When contestation is feasible but avoidable, there are two possible cases, one in which L either offers a larger share to X and ignores T (which we refer to as an \mathcal{A} ppropriation case and denote $\alpha^{\mathcal{A}}$) and one in which L gives a smaller share to X and a large enough share to T to make sure they would never side with X in case of contestation (which we refer to as an \mathcal{I} nclusion case and denote $\alpha^{\mathcal{I}}$).

Whether elites offer $\alpha^{\mathcal{A}}$ or $\alpha^{\mathcal{I}}$ depends on the excluded group's influence, which is inversely related to β_X (the weight the elite attaches to X 's share). Specifically, there is a threshold $\beta_X^* = \frac{\beta_T - p_X}{1 - p_X}$ such that when the excluded group's influence is relatively high ($\beta_X < \beta_X^*$), the elite offers $\alpha^{\mathcal{I}}$, and otherwise offers $\alpha^{\mathcal{A}}$. This is because, when β_X is low (excluded group influence is high), the elite's incentives to withhold the windfall from a very influential X become stronger; so much so that they are willing to take a smaller share themselves.

The intermediate equilibrium outcome is summarized in the following proposition:

Proposition 1.

(\mathcal{A}) When $\beta_X \geq \beta_X^*$, L offers $\alpha_X^{\mathcal{A}} = p_{XT} - \frac{c_{XL}}{S} + \frac{\max\{c_{TX} - c_{TL}, 0\}}{S}$ and $\alpha_T^{\mathcal{A}} = 0$, X and T accept, windfall is divided accordingly.

(\mathcal{I}) When $\beta_X < \beta_X^*$, L offers $\alpha_X^{\mathcal{I}} = p_X - \frac{c_{XL} + c_{XT}}{S}$ and $\alpha_T^{\mathcal{I}} = \frac{p_{XT} - p_X}{1 - p_X} + \frac{c_{TX} + c_{XT} - c_{TL}}{(1 - p_X)S}$,

⁹⁸ For sake of convenience, we assume that the target group's influence is low enough so that the expected payoff for the elite to buy T 's support is always greater than letting them side with X ; $\beta_T > \frac{c_{TX} + c_{XT} - c_{TL} - c_{LT}}{(p_{XT} - p_X)S + c_{TX} + c_{XT} - c_{TL}}$.

X and T accept, windfall is divided accordingly.

Proof. In Appendix A. □

1.2 Predictions

Our central interest is understanding when aid targeting is more effective, meaning that the target group receives a bigger share of the aid to which it is intended, despite its lack of influence.⁹⁹ Putting together the three equilibrium outcomes described above, we make predictions on how a change in windfall size affects the share received by T , conditional on excluded group influence and relations. Figure 2 shows our main comparative statics for α_T .

	Low influence	High influence
Good relations	$\frac{\partial \alpha_T}{\partial S} = 0$	$\frac{\partial \alpha_T}{\partial S} < 0$
Bad relations	$\frac{\partial \alpha_T}{\partial S} = 0$	$\frac{\partial \alpha_T}{\partial S} > 0$

Figure 2: **Main prediction on allocations to the target group.** Change in the shares of the target group as windfall size increases for different parameter regions. The bottom-right quadrant denotes high threat communities where the excluded group is both strong and has bad relations with other groups. The remaining three cells characterize lower threat communities.

Our main prediction is that what the target group receives differs in ‘high threat’ communities—where the excluded group is both influential ($\beta_X < \beta_X^*$) *and* has bad relations with other groups—and in ‘lower threat’ communities, where the excluded group is not influential ($\beta_X > \beta_X^*$) and/or has good relations with the other two

⁹⁹We note that our predictions focus on shares—and consequently on the distributive outcomes of aid—rather than simply claiming that different groups get bigger amounts as windfall size increases.

groups ($c_{XL} + c_{LX} + c_{XT} \geq c^*(\beta_X, S)$ and $c_{XT} + c_{TX} > c_{TL}$).¹⁰⁰ This yields the following hypothesis:

Hypothesis 1. *As the amount of aid increases, the equilibrium share of the target group increases in ‘high threat’ communities and (weakly) decreases in ‘lower threat’ communities.*

To understand this prediction, it is first important to recall that bigger windfalls increase the material benefits of contestation for the excluded group relative to the costs, making contestation more likely in general. But whether the excluded group actually contests also depends on whether it is both influential (which exacerbates the elites’ rivalry concerns) and has bad relations with other groups (meaning low costs to contestation). All in all, because bigger windfalls in high threat communities make excluded group contestation more likely, elites have a greater incentive to offer the target group a bigger share of the aid to form a coalition to forestall excluded group contestation. The bottom right cell in Figure 2 shows how it is the *interaction* of these three parameters that drives our main prediction for high threat communities. In lower threat communities (the remaining three cells of the figure), elites lack such incentives and the share received by the target group is (weakly) decreasing in those contexts.¹⁰¹

The model also suggests that as aid windfalls become larger, there will be more excluded group capture—and less elite capture—in high threat communities.¹⁰² Where

¹⁰⁰When $\alpha_T > 0$, whether T ’s share is increasing or decreasing in windfall size ($\frac{\partial \alpha_T}{\partial S}$) depends on the sign of $\frac{c_{TX} + c_{XT} - c_{TL}}{(1-p_X)S}$, which can be rewritten as $c_{XT} + c_{TX} > c_{TL}$, namely whether the relations of the target group with the excluded group are better than its relations with the elite.

¹⁰¹When the equilibrium outcome is Appropriation, T ’s share is always zero, regardless of the size of the windfall (left column of Figure 2). When the equilibrium outcome is Inclusion, and T ’s relations with L are better than their relations with X ($c_{XT} + c_{TX} \leq c_{TL}$), the surplus L must offer T to keep them from forming a coalition with X shrinks in relative terms (upper right quadrant).

¹⁰²We focus on the predictions for high threat communities in order to understand the trade-off between effective targeting and capture. For the full set of predictions for excluded group and elite capture, see Appendix A.

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the elites want to avoid contestation in equilibrium, bigger windfalls mean that they must now offer the excluded group a bigger share. Specifically, in an *Inclusion* equilibrium, the elites use their first-mover advantage to extract $c_{XL} + c_{XT}$, the costs that the excluded group would have to endure if there were contestation. As S increases, the excluded group's gains from contestation increase but their costs stay the same, and so does the amount L can extract and keep for themselves. Hence, the share that L needs to offer X to avoid contestation grows in windfall size.¹⁰³

***Hypothesis 2.** As windfall size increases, the equilibrium share of the excluded group increases—while the elite's equilibrium share decreases—in high threat communities.*

It is important to note that the main predictions of the model are driven by a greater *threat* of contestation in communities where the excluded group is both strong and has bad relations; actual bargaining breakdown is not necessary for our predictions to hold. Nevertheless, by expanding the set of parameter values that result in contestation, the model predicts that bigger windfalls make contestation—and hence a deterioration in community relations—more likely in general. Critically, this means that while bigger windfalls might be necessary to obtain better targeting in high threat communities, bigger windfalls could bring a general loss in social cohesion. Given that aid agencies often hope their programs will also enhance—or at least not undermine—social cohesion, contestation is an unwelcome outcome that merits investigation.

***Hypothesis 3.** As windfall size increases, contestation (a deterioration in community relations) becomes more likely in both high and lower threat communities.*

¹⁰³We also show in Appendix A that if contestation occurs due to bigger windfalls, the excluded group also gets a bigger share of the windfall in expectation.

1.3 Discussion of the Model

Showing that competition between two stronger players can have distributive benefits for a weak player is counter-intuitive from the perspective of canonical bargaining models, which predict that bargaining situations with both weak and strong players will result in the latter getting almost all of the benefits (Rubinstein, 1982, Baron & Ferejohn, 1989). Our approach introduces insights from other models of non-cooperative bargaining with coalition formation to demonstrate how allowing a weak player to form a coalition with a stronger player can alter these bargaining dynamics.¹⁰⁴ Our approach also differs from canonical models of group rent-seeking contests, which show that the beneficial effects of bigger windfalls dissipate due to competition among multiple powerful groups (Svensson, 2000b). While we have a similar interest in the effect of windfall size, our approach differs in its focus on bargaining rather than rent-seeking and in our central concern for the consequences of aid windfalls for a *weak* group.

One potential concern with the model might stem from our decision to allow the target group to influence distributive outcomes through forming a coalition with one of the stronger players. In other words, if the target group is weak, can it overcome the collective action dilemma and act as a group? Critically, one way to think about targeting is that it helps to overcome the collective action dilemma by designating a group that did not exist as such previously. This is consistent with the notion that targeting can have an empowering effect (Winters, 2014). A related concern might be that, by allowing T to join a coalition, we are in fact imbuing a weak group with outsized power. We view the possibility of coalition formation as consistent with a large

¹⁰⁴Our model is closest in setup to Dal Bó & Powell (2009), who show that government can co-opt an opposition by offering a share of a resource windfall. While the distributive outcomes in their model rely on information asymmetries, however, we show that it is possible to get similar outcomes under perfect information (see more below).

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literature that suggests that weak groups can in fact exercise influence—for instance by having power in numbers (DeNardo, 1985), by being pivotal in their support for one party over another (Smith & De Mesquita, 2012), or by influencing outcomes by opting *not* to join a coalition (Maschler, 1963)—but rarely do so through direct challenges to elites.¹⁰⁵

Another possible question pertains to our assumption that all actors have full information on windfall size. Practically-speaking, it is common in targeted aid programs for donors to publicize the aid amount, which makes incomplete information over windfall size (or targeting criteria) less of a concern (World Food Programme, 2005, United Nations Childrens Fund (UNICEF), 2005). More importantly, a key contribution of our model is to show how community dynamics impact effective targeting and capture *even in situations of complete information*. While we could get similar predictions from a model using information asymmetries, a main advantage of our approach is that we show that the dynamics described do not depend on uncertainty or information advantages among players and as such that they would not be solved simply by increasing transparency in the targeting process.

Finally, we emphasize that the model is relatively general in that it could be tested in any targeted aid program in which aid agencies have imperfect control, elites play some formal or informal role in distributing aid, and the target group is vulnerable. These are scope conditions that are met in many different types of aid programs—including community-driven development, conditional cash transfer, and humanitarian aid programs—in both conflict and non-conflict settings. In what follows, we provide empirical support for the model’s predictions based on evidence

¹⁰⁵In Appendix A we study a more general version where T can make a counter-offer or contest both powerful groups at once. We show that in our setting the general version of the model is functionally the same as the simplified version presented here and yields the same results.

from one case. In Section 5 we return to a discussion of the relevance of our approach to targeted aid programs more broadly.

2 The Aceh Context

We test our predictions in the context of an aid program implemented in Aceh, Indonesia. For nearly 30 years, GAM waged a separatist struggle in Aceh against the central government. While the conflict evolved in several stages, civilians frequently suffered from violence committed by GAM forces, the Indonesian military, or both. The conflict resulted in approximately 30,000 deaths as well as widespread instances of murder, torture, rape, internal displacement, and property destruction.

The 2005 peace agreement contained provisions to reintegrate GAM combatants and to provide assistance to civilian conflict victims. The Aceh Peace Reintegration Agency (*Badan Reintegrasi-Damai Aceh*, or BRA) was established to manage this process and partnered with the World Bank-supported Kecamatan Development Program (KDP) to reach conflict-affected communities. The resultant BRA-KDP program aimed to disburse aid windfalls ranging in size from 60 to 170 million rupiah (about USD \$6,000-\$17,000) to more than 1,700 villages. The program also sought to target those funds to civilian conflict victims, which had the effect of creating an excluded group of former combatants as elaborated below.

In order to identify civilian conflict victims, BRA-KDP opted for a community-based targeting approach. Each village organized a series of meetings to select the criteria for identifying conflict-affected households. Civilian conflict victims were targeted precisely because they were viewed as among the most vulnerable members of the community. As one conflict victim stated: “Conflict victims have less education and are a minority in this village. We don’t have leverage in the community. If we

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rely on the community to determine who qualifies for assistance, we won't get the benefits we deserve" (Morel et al., 2009, 19). Following the determination of eligible beneficiaries, villagers developed proposals that were then voted on at community meeting. Communities had discretion over how to allocate funds but were instructed to prioritize proposals submitted by the most conflict-affected.

Elites also played a distributional role in BRA-KDP, despite BRA-KDP efforts to minimize the possibility of elite capture by using external facilitators to implement the program within villages. Nevertheless, anecdotal evidence suggests that village elites still managed to influence the decision-making process. As one villager stated with respect to BRA-KDP community meetings: "Meetings are normally attended only by village authorities. Hamlet heads, religious figures, community leaders and village government officials attend." And, according to another: "It is always a group of people who are close to the village authorities that monopolize the benefits" (Morel et al., 2009, 27).

Moreover, by targeting civilian conflict victims, BRA-KDP invariably created an excluded group consisting of former GAM combatants. While ex-combatants were not supposed to benefit directly from the program, in many villages they felt entitled to receive some of the aid. In the words of one former commander: "Everyone should understand that returning GAM are heroes. We should receive money. There are 1,000 combatants here...and there's potential for them to conduct criminal acts if BRA-KDP doesn't target them. GAM are conflict-affected people as well and therefore we should also get money" (Morel et al., 2009, 28).

BRA-KDP personnel documented numerous instances in which GAM took—or threatened to take—actions to try to appropriate a share of the funds and that could be construed as contestation. These actions included extortion, theft of funds, protest, threats and demands, and, in rare instances, physical intimidation (Morel et al., 2009,

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27-33). These methods are consistent with how, during the conflict, GAM often demanded that villages pay ‘taxes’ to finance its operations (Aspinall, 2009). As stated by one villager: “There is a rumor here that GAM have requested 20 percent of the [BRA-KDP] project funds. I think the money should go to them first, not the community. Because once they have received something, the process will go more smoothly” (Morel et al., 2009, 30). BRA-KDP reports suggest that such actions by GAM generated tensions and community resentment (Morel et al., 2009).

While these dynamics were well-documented, they still call for a more systematic explanation as to why targeting was more effective in some BRA-KDP communities than others. Crucial for our analysis, the conflict produced substantial and enduring village-level variation in both GAM influence and relations, which allows us to examine how the effects of bigger aid windfalls vary depending on local conditions.

Indeed, villages varied in the extent to which they supported GAM during the conflict, with implications for the quality of relations post-conflict. For much of the conflict, GAM enjoyed relatively high levels of community support in the eastern part of Aceh due to a shared ethno-nationalist ideology. In other parts of Aceh, however, support for GAM was more variable and many villages—especially those with significant non-Acehnese populations—supported Indonesian military forces (or neither side). As GAM moved into such areas in the later stages of conflict, it often used coercion, violence, and intimidation to control local communities, damaging local support (Schulze, 2004). Importantly, there is also evidence that community sympathy or antipathy for GAM endured following the conflict, shaping relations and reintegration prospects (Tajima, 2018).¹⁰⁶

Similarly, GAM’s influence varied at the village-level both during and after the

¹⁰⁶Our fixed effects regressions, discussed below, allow us to investigate the effects of village-level variation in support for GAM within districts.

conflict and did so independently of its popular support. GAM primarily fought a guerrilla war, which necessitated the creation of local strongholds and bases of operations and had the effect of enhancing its influence over village affairs. GAM often established strongholds in or near villages where it had support (Schulze, 2004, Aspinall, 2009), although even then its influence within the community varied depending on factors such as the strength of other forms of local authority (Morel et al., 2009). GAM also established strongholds in areas where it lacked community support but that were of strategic importance, relying on coercion and intimidation to ensure popular cooperation (Schulze, 2004). Given that most GAM fought near their home villages (Aspinall, 2009), the influence over village affairs that GAM established during the conflict often extended into the post-conflict period (Morel et al., 2009). In the next section, we explain how we use our data to capture such village-level variation in both GAM strength and the quality of its relations with others in the community, which in turn determine whether GAM posed a high or low threat of contestation to targeted aid in the post-conflict period.

3 Empirical Strategy

3.1 The data

Our main data come from original household surveys of a random sample of 504 civilians, former GAM combatants, and village heads from 75 villages that participated in BRA-KDP. The surveys were implemented in 2008, approximately 12 months after BRA-KDP ended, and were conducted face-to-face by trained enumerators from a professional survey firm. Sampling followed a multi-stage cluster sampling approach in which villages were first sampled within strata and then civilians and ex-combatants

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were randomly sampled within villages (see Appendix B.2 for details on the sampling strategy). Question-wording for all survey questions used in the analysis can be found in Appendix B.3.

Coding threat of contestation. We use data from the village head survey to code villages as having a high or lower threat of excluded group contestation. The survey included questions about the strength and nature of relations between ex-combatants and other community members from 2001 to 2005, which was the final—and most violent—stage of the conflict. Following on the discussion in Section 2, we proxy for GAM influence using a question about whether a village was a GAM stronghold (‘basis GAM’) during that period. In doing so, we draw on the qualitative evidence that ex-combatants remained more influential in communities where they also had a stronger presence during the conflict (Morel et al., 2009). We proxy for the nature of community relations with a survey question that inquired into whether the majority of villagers actually supported GAM during this period or did not (meaning that they supported the Indonesian military or neither side). We consider relations between GAM and the community to be better in villages where GAM had at least majority support (implying high costs to contestation) and worse in places where the village supported the Indonesian military or neither side (implying lower costs to contestation).

We combine these two measures to create a binary indicator where ‘high threat’ villages (those in which GAM is influential and has worse relations) are coded 1 and ‘lower threat’ villages (those in which GAM has little influence and/or good relations) are coded as 0. Our coding is summarized in Table 3.1. We use this binary coding in the main analysis because it provides the most direct test of the main model predictions. In Appendix B.9 we show that the empirical results follow the predictions when we disaggregate this measure into its component parts.

		Village was a GAM stronghold (2001-2005)	
		No	Yes
Majority of village supported GAM during the conflict (2001-2005)	Yes	j=13 i=90 Lower threat=0	j=17 i=129 Lower threat=0
	No	j=23 i=135 Lower threat=0	j=22 i=150 High threat=1

The table shows the over-lapping measures of GAM influence and relations taken at the village-level, where j refers to the number of villages in the sample and i to the number of individuals. Villages in which GAM is both influential and has bad community relations are considered to have a high threat of contestation, all other villages have a lower threat of contestation.

Table 3.1: Measure of village-level threat of excluded group contestation

Controls. Importantly, while we have exogenous variation on windfall size (described next), the threat of excluded group contestation is not exogenous. There could in fact be numerous factors that predict both excluded group threat and our outcomes of interest. To address concerns about omitted variable bias, we employ a rich set of pre-treatment controls using data from the 2000 PODES survey—an extensive survey conducted regularly in every Indonesian village. Our controls include measures of village poverty; terrain and proximity to a forest; remoteness from services, markets, and population centers; government capacity; security; and the presence of criminal networks. Descriptive statistics for all PODES variables used in the analysis can be found in Appendices B.5.

The PODES data also allows us to conduct a rough analysis of the factors that predict excluded group contestation threat. Appendix B.6 presents a regression of our binary measure of threat on the control variables. We find a positive association between threat and village proximity to a forest (consistent with the notion that GAM often used forests as bases for fighting) as well as between threat and duration of village head time in office (which could proxy for elite strength). These correlations

help to confirm the validity of our threat measure. While we do not present regressions displaying controls in the main text, these results are available in Appendix B.11.

3.2 Exogenous variation in windfall size

One benefit of our empirical context is that we have exogenous variation in windfall size, which gives us causal leverage over a key parameter in the model that determines the stakes of the game. This is also an advantage over existing observational research on aid windfalls, which give rise to concerns that windfall size is endogenous to unobservable community characteristics.

The World Bank initially selected 67 sub-districts to participate in BRA-KDP, with all villages in those sub-districts guaranteed some amount of aid (Barron et al., 2009b). BRA-KDP used two measures to determine aid amounts at the village-level. First, it used a continuous measure of *sub-district* conflict intensity and employed arbitrary cutoffs to categorize sub-districts as low, medium, or high conflict-affected. Second, it used a continuous measure of village population and imposed exogenous cutoffs to classify villages as small (0-299 people), medium-sized (300-699 people), or large (700 or more people). BRA-KDP then crossed these measures to create nine strata, with each strata receiving a different amount of aid.

While the BRA-KDP assignment process in fact created multiple thresholds, the analysis in this paper focuses on the one for which we have a sufficiently large sample near the threshold and which passes the McCrary (2008) density test (discussed below).¹⁰⁷ Specifically, we focus our analysis on the cutoff between small and medium-sized villages in high conflict-affected sub-districts. All villages with 0-299 people

¹⁰⁷The fact that the villages included in our analysis are not a representative sample of those that participated in BRA-KDP does not affect the internal validity of our results given our empirical strategy. In Appendix B.4 we provide a more detailed description of the assignment process and explanation as to why we do not estimate effects at other thresholds.

received an aid windfall in the amount of 120 million rupiah (about \$12,000) while all villages with 300-599 people received an aid windfall of 150 million rupiah (about \$15,000)—an increase of 30 million rupiah (about \$3,000) at the cutoff of 300 persons. This is equivalent to an increase in 100,000 rupiah (\$10) per capita, or 560,000 rupiah (\$56) per household. The top part of Figure 3 shows the distribution of our 75 sampled villages around the population variables (centered at 300 persons) while the bottom shows the distribution of villages by whether they are high threat or lower threat.

3.3 Estimation

The fact that windfall size was determined by an arbitrary cutoff in a continuous measure of village population makes analysis suitable to a regression discontinuity approach (Imbens & Lemieux, 2008). Our main empirical goal is to estimate the effect of an increase in windfall size on aid allocations in high threat and lower threat villages. To do this we estimate weighted least squares regressions of the following form:

$$Y_{ij} = \alpha + \tau Z_j + \delta V_j + \gamma Z_j \times V_j + f(Z_j, V_j, \tilde{P}_j) + \omega_m X'_{jm} + \epsilon_{ij}$$

where Y_{ij} refers to the outcome for individual i in village j .¹⁰⁸ Z_j is a binary indicator for treatment assignment that equals one for villages that received a larger windfall (are above the threshold) and zero otherwise.¹⁰⁹ V_j is the binary indicator which equals one for high threat villages and zero for lower threat villages and \tilde{P}_j is the continuous measure of population centered at 300 ($\tilde{P}_j=0$ at the threshold). Standard errors are clustered at the village level and all analysis employs survey weights to

¹⁰⁸While the main outcomes in the theoretical model are group shares, our empirical analysis employs individual-level proxies, as described below.

¹⁰⁹This is a ‘sharp’ RD in that by all World Bank accounts the cutoff completely determined assignment.

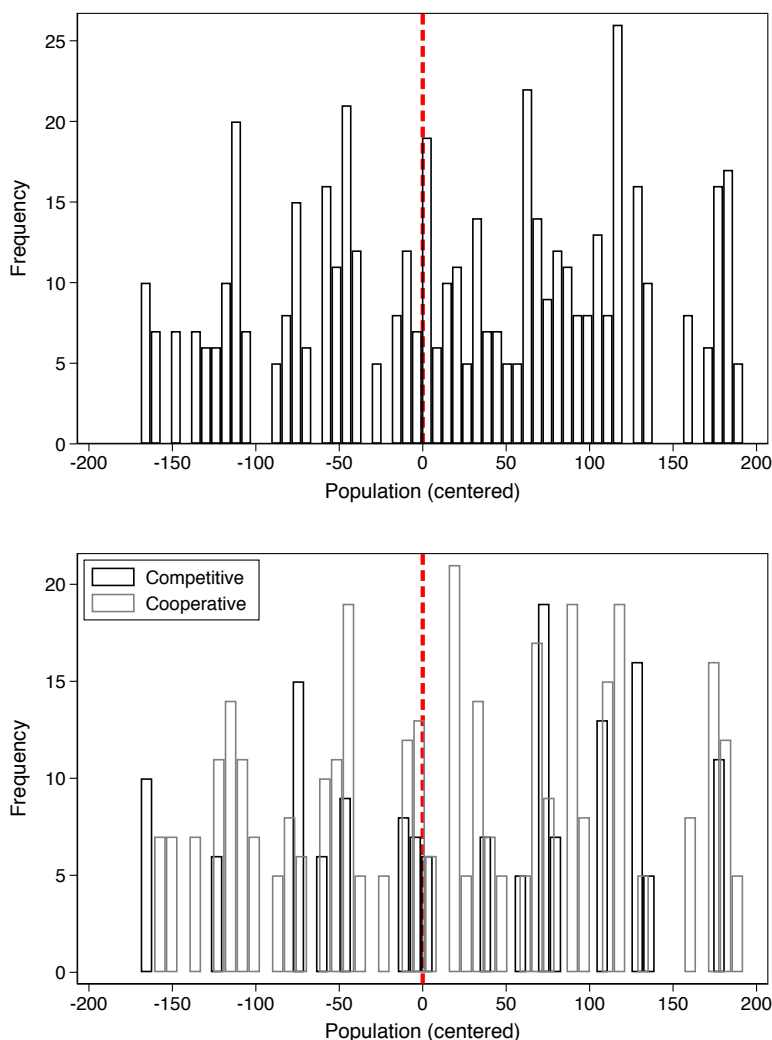


Figure 3: Distribution of individual-level observations around the population threshold centered at zero. Top panel shows the full sample; bottom panel shows the distribution in high threat and lower threat villages.

account for sampling probabilities.

The term $f(Z_j, V_j, \tilde{P}_j)$ refers to variables included in the regression to fit models flexibly on either side of the threshold. Specifically, we fit linear and quadratic models separately on either side of the threshold.¹¹⁰ The coefficient γ identifies the effect of

¹¹⁰For our linear spline, $f(Z_j, V_j, \tilde{P}_j) = \beta_1 \tilde{P}_j + \beta_2 Z_j \tilde{P}_j + \beta_3 V_j \tilde{P}_j + \beta_4 Z_j V_j \tilde{P}_j$. Our quadratic spline includes the additional terms: $\beta_5 \tilde{P}_j^2 + \beta_6 Z_j \tilde{P}_j^2 + \beta_7 X_j \tilde{P}_j^2 + \beta_8 Z_j V_j \tilde{P}_j^2$.

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a bigger windfall in high threat relative to lower threat villages while τ captures the effect of targeting a bigger windfall in lower threat communities.¹¹¹ We also include in our regressions X'_{jm} , the vector of m village-level controls obtained from the PODES 2000 data.

One central concern with regression discontinuity designs is the choice of bandwidth. All main analyses presented in this paper employ a bandwidth of ± 150 , which restricts our analysis to 63 villages. In Appendix B.7 we check the robustness of all results to alternative bandwidths of ± 100 and ± 200 . We also check robustness to nonparametric local linear regression using an optimal data-driven bandwidth (Calonico et al., 2014).

The key identifying assumption of an RDD is the continuity of potential outcomes at the threshold (Imbens & Lemieux, 2008). Following the literature, we check this assumption by testing for discontinuities in our m pre-treatment village-level controls and our measures of excluded group threat at the threshold. The results, presented in Appendix B.7 support the continuity assumption. This assumption would also be violated if villages had sorted themselves on either side of the threshold, for instance if they had been able to manipulate strategically their population scores. To check this, we implement a McCrary density test and find no evidence of sorting (see Appendix B.7).

¹¹¹We are interested in estimating effects at the cutoff point where $\tilde{P}_j = 0$. The terms in $f(\cdot)$ that are used to flexibly fit the regression drop out at this point and thus are not included in the calculation of marginal effects.

4 Results

4.1 Distributive outcomes

Our main goal is to understand when the target group, as a vulnerable group, gets a greater share of the benefits to which it is entitled. Descriptive statistics from the household survey, reported in Appendix B.5, show that about 69 percent of civilian (victim) households and 58 percent of former combatants received some assistance from BRA-KDP with the average amount totaling about 630,000 rupiah (about USD \$63) for each group, which suggests that excluded group capture was consequential. The overwhelming majority of funds were used for private goods, with about 95 percent of all recipients reporting that they primarily received goods in the form of cash that was then put towards livelihood activities (Barron et al., 2009b, Morel et al., 2009).

Our first hypothesis is that, as the amount of aid increases, the target group will obtain a greater share of the benefits in villages with a high threat of excluded group contestation. To test the prediction, we divide the total amount (in monetary terms) of goods that a respondent reported receiving by the size of the village's aid windfall to obtain a measure of per capita share of the aid windfall.¹¹² Table 3.2 presents the results for the civilian subsample.¹¹³ The columns present results from six different models in which we fit linear and quadratic regressions separately on either side of the threshold, both with and without village pre-treatment controls and district fixed effects, for our preferred bandwidth of ± 150 .

¹¹²Because we have a representative sample, a bigger share for respondents that belong to the target group implies a bigger share for other group members.

¹¹³We use data from the full civilian subsample here because victim-hood was broadly defined in many villages; we show in Appendix B.8 that we observe the same pattern of results if we define conflict victims more narrowly using objective or subjective criteria.

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	DV: Per capita windfall share for target group members					
	Linear spline			Quadratic spline		
	(1) no controls	(2) controls	(3) controls + district f.e.	(4) no controls	(5) controls	(6) controls + district f.e.
Bigger windfall * High threat (γ)	0.97*** (0.34) 0.006	1.38*** (0.41) 0.001	1.08*** (0.39) 0.007	1.58*** (0.54) 0.004	1.93*** (0.55) 0.001	1.36** (0.52) 0.010
Bigger windfall (τ)	-0.46* (0.26) 0.081	-0.50* (0.25) 0.050	-0.29* (0.17) 0.091	-0.86* (0.47) 0.073	-0.98** (0.47) 0.041	-0.37 (0.35) 0.292
High threat (δ)	-0.57* (0.29) 0.056	-0.95*** (0.35) 0.008	-0.44 (0.31) 0.165	-0.83* (0.50) 0.099	-1.20** (0.48) 0.015	-0.79* (0.43) 0.071
Marginal effect of a bigger aid windfall in 'high threat' villages	0.51** (0.22) 0.023	0.88*** (0.30) 0.004	0.80** (0.30) 0.010	0.72*** (0.25) 0.006	0.95** (0.38) 0.013	1.00** (0.39) 0.011
N	317	312	312	317	312	312
Band	150	150	150	150	150	150

Notes: *** $p < .01$, ** $p < .05$, * $p < .10$ based on a two-tailed test. All results are from survey weighted least squares linear and quadratic regressions fitted separately on either side of the threshold. Standard errors are clustered at the village level.

Table 3.2: Effect of Targeting a Bigger Aid Windfall on Target Group Benefits

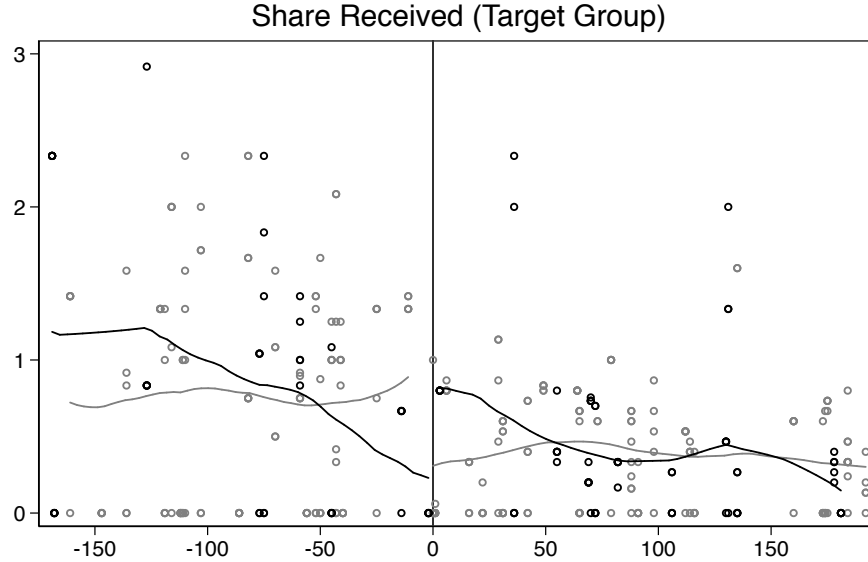


Figure 4: Local polynomial regression showing the effect of targeting a bigger aid windfall on the share received by the target group in high threat (black line) versus lower threat (gray line) villages.

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The table shows three main findings, also shown in Figure 4. First, looking at the final row of the table, there is strong evidence that targeting a bigger aid windfall resulted in the target group receiving a greater share of the benefits in high threat communities. Across all six main specifications, the coefficients are positive and significant and suggest that targeting a bigger aid windfall caused a .5-1 percentage point increase in the share of the windfall for the target group. Second, the coefficients on *Bigger windfall* (τ) are negative and at least marginally significant in five out of the six columns. This is consistent with the prediction that, as the amount of aid increases, the share received by the target group is (weakly) decreasing in lower threat communities. Finally, the findings in the first row show that, as windfall size increases, the target group indeed received a greater share of the benefits in high threat *relative* to lower threat communities. These differences are statistically and substantively significant. Evidence in Appendix B.8 shows that, as windfall size increases, those in the target group in high threat communities received 1.28 to 2.51 million rupiah (USD \$128-251) more than their counterparts in lower threat communities.¹¹⁴

Our second hypothesis is that the excluded group receive a greater share of the windfall (and elites a smaller share) *within high threat communities*. To assess whether the excluded group and elites benefited from BRA-KDP, we use three measures from the survey that ask: “When the community has to make a decision about how to allocate resources in the village, sometimes some groups benefit more than others. Generally, do you think that [ex-GAM combatants/friends and family of the village leader/people that are well-connected with local government]” do much or somewhat better than others (coded 1), about the same as others (coded 0), or much or somewhat worse than others (coded -1). We combine the two measures pertaining to elite

¹¹⁴There is also no evidence from the survey that BRA-KDP goods had been given or taken away one month after receiving them, allaying concerns about forced redistribution after the initial allocation.

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benefits into an index using inverse covariance weighting.¹¹⁵

	DV: Perceived benefits for excluded group					
	Linear spline			Quadratic spline		
	(1) no controls	(2) controls	(3) controls + district f.e.	(4) no controls	(5) controls	(6) controls + district f.e.
Bigger windfall * High threat	-0.11 (0.35) 0.752	1.01*** (0.31) 0.002	0.87*** (0.33) 0.010	0.27 (0.38) 0.471	0.97*** (0.36) 0.008	1.05*** (0.29) 0.000
Bigger windfall	-0.20 (0.25) 0.430	-0.30 (0.20) 0.131	-0.36** (0.16) 0.025	-0.15 (0.27) 0.583	0.06 (0.23) 0.780	-0.30* (0.16) 0.063
High threat	-0.01 (0.24) 0.979	-0.50* (0.26) 0.059	-0.55** (0.23) 0.019	0.04 (0.24) 0.883	-0.32 (0.29) 0.266	-0.34 (0.21) 0.116
Marginal effect of a bigger aid windfall in 'high threat' villages	-0.31 (0.25) 0.207	0.71*** (0.26) 0.007	0.51* (0.28) 0.067	0.13 (0.27) 0.640	1.04*** (0.30) 0.001	0.76*** (0.22) 0.001
N	315	310	310	315	310	310
Band	150	150	150	150	150	150

Notes: *** $p < .01$, ** $p < .05$, * $p < .10$ based on a two-tailed test. All results are from survey weighted least squares linear and quadratic regressions fitted separately on either side of the threshold. Standard errors are clustered at the village level.

Table 3.3: Effect of Targeting a Bigger Aid Windfall on Perceived Excluded Group Benefits

The main results on perceived ex-combatant benefits are presented in Table 3.3, where the results in the final row show the marginal effect of targeting a bigger aid windfall in high threat villages. The coefficients in this row are positive and significant at least at the 90 percent confidence level in four of the six main specifications, suggesting that former combatants indeed receive more in such contexts. These findings are consistent with those in Table 3.4, which reports results from the ex-combatant subsample on what they actually received from BRA-KDP. While the ex-combatant sample is small ($n=117$ in the ± 150 bandwidth) and more susceptible to false positives, the findings nonetheless are consistent with the perceptions results and with the prediction that a bigger aid windfall causes ex-combatants to capture a greater share of the windfall in high threat communities.

¹¹⁵While we have data on what ex-combatants actually received from BRA-KDP (see below), we do not have data on what elites actually received.

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	DV: Per capita windfall share for ex-combatants					
	Linear spline			Quadratic spline		
	(1) no controls	(2) controls	(3) controls + district f.e.	(4) no controls	(5) controls	(6) controls + district f.e.
Bigger windfall * High threat	0.78* (0.41) 0.059	1.71*** (0.55) 0.002	0.84 (0.51) 0.101	1.28*** (0.48) 0.009	1.59*** (0.55) 0.005	0.99** (0.46) 0.033
Bigger windfall	-0.87*** (0.14) 0.000	-0.92*** (0.25) 0.000	-0.45* (0.24) 0.064	-0.93*** (0.20) 0.000	-0.55 (0.34) 0.108	0.54* (0.28) 0.056
High threat	-0.62** (0.31) 0.048	-2.19*** (0.48) 0.000	-1.53*** (0.58) 0.010	-0.64 (0.41) 0.116	-2.06*** (0.50) 0.000	-2.62*** (0.55) 0.000
Marginal effect of a bigger aid windfall in 'high threat' villages	-0.09 (0.39) 0.824	0.79* (0.44) 0.072	0.39 (0.38) 0.317	0.35 (0.44) 0.425	1.03** (0.48) 0.033	1.52*** (0.38) 0.000
N	117	117	117	117	117	117
Band	150	150	150	150	150	150

Notes: *** $p < .01$, ** $p < .05$, * $p < .10$ based on a two-tailed test. All results are from survey weighted least squares linear and quadratic regressions fitted separately on either side of the threshold. Standard errors are clustered at the village level.

Table 3.4: Effect of Targeting a Bigger Aid Windfall on Excluded Group Benefits (ex-combatant sample)

The model predicts that the reverse will be true for elites; in other words, as windfall size increases, there will be less *elite* capture in high threat communities as elites are forced to give the target and excluded groups a greater share of the windfall in order to forestall excluded group contestation. The coefficients in the final row of Table 3.5 are generally negative and are significant in two of the quadratic spline specifications. While this is somewhat weaker evidence for the second hypothesis it nonetheless suggests support for the predictions of the model in light of the findings already presented.

All in all, the results thus far are generally consistent with the main predictions of the model in showing that the target group receives a bigger share of the aid in communities with a high threat of excluded group contestation. These results are highly robust to alternative specifications, bandwidths, and extended analyses (see

	DV: Perceived benefits for elites					
	Linear spline			Quadratic spline		
	(1) no controls	(2) controls	(3) controls + district f.e.	(4) no controls	(5) controls	(6) controls + district f.e.
Bigger windfall * High threat	-0.62 (0.61) 0.308	-0.37 (0.80) 0.645	-1.09 (0.86) 0.209	-1.56*** (0.48) 0.002	-1.72** (0.73) 0.020	-2.95*** (0.80) 0.000
Bigger windfall	0.26 (0.30) 0.387	0.46 (0.32) 0.148	0.55* (0.30) 0.074	0.57* (0.33) 0.087	1.18*** (0.37) 0.002	1.24*** (0.38) 0.001
High threat	0.35 (0.26) 0.175	-0.06 (0.58) 0.917	0.53 (0.68) 0.433	0.31 (0.31) 0.325	0.27 (0.52) 0.608	1.45** (0.60) 0.017
Marginal effect of a bigger aid windfall in 'higher threat' villages	-0.36 (0.54) 0.500	0.09 (0.64) 0.887	-0.54 (0.69) 0.439	-0.99*** (0.35) 0.005	-0.54 (0.62) 0.382	-1.71** (0.66) 0.012
N	312	307	307	312	307	307
Band	150	150	150	150	150	150

Notes: *** $p < .01$, ** $p < .05$, * $p < .10$ based on a two-tailed test. All results are from survey weighted least squares linear and quadratic regressions fitted separately on either side of the threshold. Standard errors are clustered at the village level.

Table 3.5: Effect of Targeting a Bigger Aid Windfall on Perceived Elite Benefits

Appendices B.8, B.7, and B.9). All in all, our findings show that targeting a bigger aid windfall does lead to more effective aid targeting in communities where the threat of excluded group contestation is high.

4.2 Contestation and social cohesion

While the evidence so far shows that targeting bigger aid windfalls result in better targeting in high threat communities, we next investigate whether doing so comes at the cost of social cohesion. We remind readers that the distributive results presented above are *not* dependent on contestation actually occurring, rather the threat of contestation is sufficient to produce these outcomes. Yet, contestation is a possible and important mechanism, which motivates our investigation.

We first test our third hypothesis that bigger windfalls—unconditional on local context—increase the likelihood of contestation and, consequently, a deterioration of

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group relations. To measure relations between the excluded group and target group, we use inverse covariance weighting to create an index of ‘GAM acceptance’ that aggregates five survey measures that capture civilian willingness to accept GAM in various roles, including as members of village associations, as village leaders, and as close friends. We also employ a more general question from the survey that asked whether individuals felt that conflict in their village was resolved satisfactorily (coded 1) or tended to endure (coded 0), which less directly proxies for a persistent deterioration in relations. If bigger windfalls resulted in more contestation and worsened relations, we expect to see a negative coefficient on both measures.

In general, there are high levels of reported acceptance of former GAM (see Appendix B.5). Yet, the findings in Panel A of Table 3.6 provide weak evidence that targeting a bigger aid windfall did in fact undermine acceptance of former GAM combatants. In five out of six specifications the coefficients are negative and in two of them the effect is significant at at least the 90 percent confidence level. There is little indication of any significant effects for our measure of conflict resolution in Panel B.

Additionally, we explore whether bigger windfalls resulted in more contestation in high versus lower threat villages. While the model does not yield the specific prediction of a differential effect in high versus lower threat villages, it is possible that contestation is more likely in high threat villages. This is important to investigate empirically to shed more precise light on whether greater effective targeting in high threat villages indeed comes at the expense of less social cohesion in high threat villages.

The results in Panel A of Table 3.7 suggest that bigger windfalls reduced GAM acceptance in lower threat villages (as indicated by the negative coefficients on *Bigger windfalls*), while the lack of a statistically significant interaction implies similar effects in high threat villages. Of greater interest are the results in the final row of Panel

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	Linear spline			Quadratic spline		
	(1) no controls	(2) controls	(3) controls + district f.e.	(4) no controls	(5) controls	(6) controls + district f.e.
Panel A: Index of Ex-combatant Acceptance						
Bigger windfall	-0.10 (0.19) 0.578	-0.38* (0.19) 0.050	-0.42** (0.19) 0.033	0.27 (0.31) 0.386	-0.19 (0.32) 0.563	-0.31 (0.28) 0.274
N	317	312	312	317	312	312
Panel B: Conflict resolved satisfactorily						
Bigger windfall	0.08 (0.09) 0.373	0.06 (0.09) 0.534	0.04 (0.10) 0.645	0.04 (0.17) 0.804	0.22 (0.18) 0.233	0.16 (0.14) 0.257
N	313	308	308	313	308	308
Band	150	150	150	150	150	150
Controls	No	Yes	Yes	No	Yes	Yes
District fixed effects	No	No	Yes	No	No	Yes

Notes: *** $p < .01$, ** $p < .05$, * $p < .10$ based on a two-tailed test. All results are from survey weighted least squares linear and quadratic regressions fitted separately on either side of the threshold. Standard errors are clustered at the village level.

Table 3.6: Effect of Targeting a Bigger Aid Windfall on Social Cohesion (Unconditional on Threat)

B, which suggest that a bigger aid windfall had a *positive* effect on perceptions of conflict resolution in high threat villages.

We interpret this result as consistent with a story in which distributive outcomes in high threat villages were due to the *threat* of excluded group contestation rather than contestation itself. Moreover, the findings suggest that distributions to GAM might have even helped to serve as a form of conflict resolution. In other words, there could have been a number of communities that were on the brink of contestation but that just managed—through their own efforts or with assistance from the program implementers—to reach a solution that appeased the excluded group, helping to ameliorate tensions and create a stronger impression of satisfactory conflict resolution. This is especially plausible in the BRA-KDP case given that staff actively intervened to mediate tensions when they arose. Indeed, of known attempts by former combatants to extort funds in eight sub-districts, such intervention led GAM to withdraw

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	Linear spline			Quadratic spline		
	(1) no controls	(2) controls	(3) controls + district f.e.	(4) no controls	(5) controls	(6) controls + district f.e.
Panel A: Index of Ex-combatant acceptance						
Bigger windfall * High threat	0.41 (0.39) 0.302	0.08 (0.71) 0.909	0.09 (0.72) 0.902	0.65 (0.47) 0.168	0.25 (0.77) 0.748	0.53 (0.69) 0.446
Bigger windfall	-0.17 (0.21) 0.420	-0.41* (0.22) 0.073	-0.48* (0.26) 0.067	0.01 (0.27) 0.967	-0.27 (0.23) 0.244	-0.50 (0.33) 0.131
High threat	-0.23 (0.36) 0.529	0.25 (0.63) 0.697	0.46 (0.60) 0.446	-0.58 (0.45) 0.200	-0.07 (0.70) 0.917	-0.29 (0.58) 0.618
Marginal effect of a bigger aid windfall in 'high threat' villages	0.24 (0.33) 0.472	-0.32 (0.61) 0.598	-0.39 (0.59) 0.517	0.67* (0.38) 0.085	-0.02 (0.71) 0.973	0.02 (0.50) 0.962
N	317	312	312	317	312	312
Panel B: Conflict resolved satisfactorily						
Bigger windfall * High threat	0.55* (0.28) 0.052	0.63** (0.25) 0.011	0.77*** (0.23) 0.001	0.22 (0.36) 0.548	0.36 (0.30) 0.232	0.38 (0.26) 0.145
Bigger windfall	-0.08 (0.07) 0.283	-0.09 (0.10) 0.401	-0.14 (0.11) 0.213	-0.07 (0.11) 0.514	0.08 (0.12) 0.523	0.03 (0.12) 0.783
High threat	-0.42* (0.25) 0.099	-0.82*** (0.22) 0.000	-0.85*** (0.21) 0.000	-0.25 (0.36) 0.491	-0.57** (0.29) 0.048	-0.56** (0.23) 0.018
Marginal effect of a bigger aid windfall in 'high threat' villages	0.48* (0.27) 0.083	0.54*** (0.20) 0.007	0.62*** (0.17) 0.001	0.15 (0.34) 0.668	0.44 (0.27) 0.105	0.42* (0.21) 0.051
N	313	308	308	313	308	308
Band	150	150	150	150	150	150

Notes: *** $p < .01$, ** $p < .05$, * $p < .10$ based on a two-tailed test. All results are from survey weighted least squares linear and quadratic regressions fitted separately on either side of the threshold. Standard errors are clustered at the village level.

Table 3.7: Effect of Targeting a Bigger Aid Windfall on Social Cohesion

its demands in all known cases (Morel et al., 2009, 31). This supports the conclusion that actual contestation was rare in BRA-KDP and that its most severe social consequences might have been avoided.

In sum, there is suggestive evidence that bigger windfalls reduced GAM acceptance overall but possibly resulted in more enduring conflict resolution in higher threat

villages that avoided contestation. These findings are broadly consistent with the third hypothesis but underscore that whether or not bigger windfalls harm social cohesion in high threat villages could depend on how close those communities are to bargaining breakdown and how capable they are of avoiding it. Thus, while the results for our context are reassuring, they do not alter the central insights of the model that aid targeting can have detrimental social outcomes.

5 Alternative Explanations and External Validity

Our theory and evidence show that targeting is more effective in villages with sufficiently big windfalls and with a high threat of excluded group contestation. Consistent with the predictions from the model, we find that both the target and excluded group benefit more in villages with a high threat of excluded group contestation and that receive bigger aid windfalls. We also find suggestive evidence that bigger windfalls reduced social cohesion—namely acceptance of former combatants—on average, but that the distributive arrangements reached in high threat villages might have had the effect of conflict resolution.

One possible alternative mechanism for the results presented above is that ex-GAM combatants in high threat villages used their leverage to obtain more benefits for the target group in order to build social capital. If social capital-building were the motivation, we might expect to see both greater distributions to the target group and *improved* relations in high threat communities, rather than the deteriorated relations predicted by the model.

We see little evidence for this alternative mechanism, however. First, the results presented above do not specifically show relations with ex-combatants improved in high threat communities. Second, if GAM were acting in a purely altruistic way—

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championing the interests of the target group at the expense of its own material gain—then we would not expect to see evidence of it also taking a bigger share for itself in high threat communities, which we do. Finally, if GAM were acting in a more narrowly altruistic way—championing both its interests and those of the target group—there is no reason to expect that this would succeed in building social capital. Indeed, there were many BRA-KDP villages in which GAM pushed for an equal division of aid among all civilian and conflict-affected households, akin to threatening contestation on behalf of both the target group and itself. While community members acquiesced to avoid tension, such actions by the excluded group produced lingering resentment (Morel et al., 2009, 19). This pattern is consistent with evidence from other contexts that non-beneficiaries often seek to appropriate aid for themselves and do so at the expense of their community relations (e.g. de Sardan et al., 2015).

Another potential concern with this study might be that the theoretical model developed here is only relevant to our immediate empirical context of Aceh. As such, it might be the case that our model is not relevant to understanding outcomes in other targeted aid programs or that our findings would not extend beyond the case of Aceh.

We believe that the theory developed here can in fact shed light on targeting outcomes in a wide variety of settings. We show in Section 1 that the assumptions under-pinning our model are common features of targeted aid programs. In other words, it is widely recognized that community dynamics matter; aid is targeted at vulnerable groups; elites can formally or informally influence distributions, raising concerns about elite capture; and non-beneficiaries try to obtain a share for themselves in targeted aid programs (e.g. Rao & Ibáñez, 2005, Barron et al., 2007, Angeles & Neanidis, 2009, Caeyers & Dercon, 2012, de Sardan et al., 2015, Kilic et al., 2013, Zurcher, 2017). While our scope conditions rule out some targeted aid programs—

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namely those in which aid organizations distribute benefits directly to the target group without using local intermediaries—there are still many situations in which we expect it to be relevant.

To that end, while we investigate bargaining and contestation in the context of community-driven development, we do not believe our approach is limited to CDD. While CDD is a common form of aid targeting (Mansuri & Rao, 2004) and thus important to understand in its own right, we expect that the dynamics observed here could play out in any context in which community members have means—whether through informal or formal, peaceful or violent channels—to challenge elite decision-making. This builds on the observation that similar dynamics to those modeled here have also been reported in conditional cash transfer, employment, and humanitarian aid programs (de Sardan et al., 2015, Zurcher, 2017).

We also do not view our model as limited to conflict settings, insofar as many non-conflict settings meet our scope conditions and are prone to elite capture, non-beneficiary capture, and heightened social divisions (de Sardan et al., 2015, Galasso & Ravallion, 2005b, Kilic et al., 2013). Importantly, one of the benefits of the model is that it provides a framework for thinking about how our empirical findings in Aceh might generalize to different empirical contexts. A central feature of the model—and what makes it broadly relevant to both conflict and non-conflict settings—is that it crystallizes predictions about the effectiveness of aid targeting for different types of local contexts. In contexts where the excluded group is strong and has bad relations—which might be more common in conflict-affected environments—the model predicts that both the target group and excluded group will receive a bigger share of the aid on average. Conversely, in communities where the excluded group has good relations with elites and/or the target group—which might be more common in non-conflict settings—the model predicts less effective aid targeting and more elite

and/or excluded group capture.¹¹⁶ All in all, while the explanatory power of our model can only be uncovered through more empirical testing in different contexts, we hope that the theory and evidence presented here will motivate future research in conflict and non-conflict contexts alike.

Finally, we note that another possible concern is that there are sometimes multiple aid programs implemented in the same communities, either sequentially or simultaneously. We believe that there is good reason to view dynamics in each targeted aid program as independent rather than interrelated, especially in contexts where resources are scarce and aid programs are sufficiently separated in time. That does, in fact, describe the context in which BRA-KDP was implemented (Morel et al., 2009). While theorizing and testing the interdependence of dynamics from multiple aid programs is beyond the scope of this paper, this is an important avenue for future research and we believe that the model presented here lays the foundation for such an investigation.

6 Conclusion

It is widely appreciated that while targeted aid programs hold the promise of better economic welfare for populations in need they can also have adverse effects in the form of elite capture, mis-targeting or non-beneficiary capture, or heightened social divisions. Thus, a central challenge of targeting aid involves ensuring that assistance reaches those for whom it is intended without harming social cohesion within recipient communities. This paper investigates how the economic and social outcomes

¹¹⁶Interestingly, our predictions for lower threat communities are consistent with the findings in Alatas et al. (2013b), who show that formal elites are more likely to capture aid targeted at the poor than informal elites, which they attribute to greater reputational costs for the latter. While the authors do not theorize the strategic interaction, their results are consistent with ours insofar as informal elites constitute an excluded group that value on maintaining good relations.

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of targeted aid programs depend on the interaction of windfall size and community dynamics. Our central finding is that targeting will be more effective at reaching vulnerable populations when non-beneficiaries are willing and able to challenge elite authority to try to appropriate a share of the aid for themselves. It is this competition over resources between two more powerful actors—elites and non-beneficiaries—that can have surprising distributive benefits for the target population.

This finding contributes to research on aid targeting by offering a novel explanation for a central dilemma at the heart of aid targeting: When is the target group—as a weak group—ever going to get more of the benefits to which it is entitled in the presence of more powerful actors who might seek to appropriate benefits for themselves? Existing answers to this question tend to focus on norms of generosity (Harragin & Chol, 1998); the monitoring and enforcement abilities of aid agencies (Paul, 2006b, Dietrich, 2013); or the notion that aid empowers the target group and enables it to hold elites or aid agencies accountable (Winters, 2014). While important, these explanations rest on sometimes questionable assumptions—that norms prevail over material-self-interest, that aid agencies have perfect control over targeting, and that vulnerable groups can effectively hold more powerful actors accountable. We provide an explanation for when targeting is more likely to be effective that allows for self-interested actors, imperfect agencies, and a weak target group, which are ubiquitous features of targeted aid programs.

Another contribution of this paper is to highlight that bigger windfalls can improve targeting in some communities but at the cost of social cohesion. This finding is relevant to a growing literature on aid and conflict interested in how targeted aid windfalls affect interactions among vulnerable populations, rebel groups, and the government (Wood & Sullivan, 2015, Zurcher, 2017) but that has not yet fully theorized the strategic dynamics. As Zurcher (2017, 519) notes in a recent review article, one

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of the most important avenues for future research on this subject is studying systematically which local environments are more or less conducive to benefiting from aid. Our paper presents one of the first attempts to crystallize these conditions by focusing on the relationship between windfall size and community dynamics.

The findings presented here also have implications for understanding the consequences of targeted aid programs in non-conflict settings. Much of the existing literature on elite or non-beneficiary capture in non-conflict environments produces mixed results (Bardhan & Mookherjee, 2006b, Niehaus et al., 2013, Alatas et al., 2013b). This paper shows that accounting for three relevant groups within a community can provide a deeper understanding of when aid capture is likely to occur, how severe it is likely to be, and who—whether elites or non-beneficiaries—will capture more. It also highlights the need to consider the social consequences of targeted aid programs in non-conflict settings as it is still possible for relations to deteriorate in the face of competition over aid.

Finally, this paper sheds light on how windfall size affects economic and social outcomes within communities. While we might expect bigger aid windfalls to yield more benefits in poor communities, a large literature on the resource and aid curses suggests that introducing free commodities into resource-poor environments can increase corruption, rent-seeking, and conflict (Svensson, 2000b, Wright & Winters, 2010, Ross, 2013, Zurcher, 2017). We add nuance to this literature by showing how bigger windfalls can have contradictory effects, resulting in better economic welfare for the target group in some communities while at the same time increasing the risk of social conflict more broadly. All in all, the theory and evidence presented here underscore the importance of appreciating that targeted aid windfalls can induce distributional conflict among different groups within a community and that it is ultimately the nature of group dynamics that drives the outcome of that process.

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List of Annexes

Annex A - Additional Analysis for Papers 1 & 2

Annex B - Additional Analysis for Paper 3

Extra Documents

Supplement 1 - Dissertation Proposal

Supplement 2 - Field Manual

Supplement 3 - Study Protocole

All annexes are available and distributed as supplementary files to this document.