Is Bigger Always Better? How Targeting Aid Windfalls Affects Capture and Social Cohesion

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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 aid targeting is successful depends primarily on the threat of contestation by 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.

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1 Introduction

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 such as money, food, and non-food items (Wahlberg, 2008; Barrett, 2006). The World Bank has supported approximately 400 cash transfer projects targeting the poor in 94 countries valued at almost $30 billion (Wong, 2012). In 2015 alone, UN agencies channeled over $900 million dollars in global humanitarian funding specifically for the distribution of non-food items in emergency settings (OCHA, 2015). In recent years, the World Food Program has targeted 54 percent of 4.4 million metric tons of food aid (World Food Program, 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.[1] The main goal of this paper is to examine the effects of aid targeting on

[1]For one review of the aid literature, see (Wright and Winters, 2010). For exceptions to the lack of literature on individual-level aid targeting, see Winters, 2014; Jablonski, 2014; Alatas et al., 2012. Much of the literature on aid targeting has employed cross-national research to explain how aid is targeted at the country or regional level. Micro-level research on aid has tended to focus on the effectiveness of specific interventions, which often aim to improve outcomes for specific marginalized groups but do not examine the effects of targeting *per se* (see, for example, Beath, Christia and Enikolopov, 2013; Fearon, Humphreys and Weinstein, 2009; Blattman and Annan, 2015).

distributional outcomes and social cohesion within communities when some individuals are eligible to receive aid 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 often play a role in targeted aid programs because successful targeting is challenging for aid agencies, especially for those operating in low-income or fragile countries.[2] 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, Grosh and Hoddinott, 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 induce them to turn to communities for assistance at different stages of the targeting process (Alatas et al., 2013; Jablonski, 2014).[3]

While community involvement in targeting can result in greater satisfaction with the program and other benefits (Winters, 2014; Alatas et al., 2012), it can also have unwelcome consequences. It is now widely appreciated that involving communities in aid distributions can increase the scope for mis-targeting (Kilic, Whitney and Winters, 2013) and elite capture (Findley et al., 2017; Alatas et al., 2013; Platteau, 2004). Additionally, targeting can result in heightened conflict and jealousy within communities, undermining social cohesion.[4] One program implemented by Oxfam in three East African countries helps to illustrate the variation in patterns of capture and social tensions that can result from aid targeting. As Jaspars and Shoham (1999) detail, targeting in Tanzania was successful in that aid was dis-

[2]A 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, Grosh and Hoddinott, 2004).

[3]For a review of different approaches to targeting, see Coady, Grosh and Hoddinott (2004).

[4]As de Sardan (2014) notes with respect to a targeted cash transfer program in Niger: "Cash transfers are not the devil...They are sharpening conflicts that are already there."

tributed primarily to the most drought-affected households while maintaining a high level of community satisfaction with the program. In Kenya, communities were also pleased with the program but mis-targeting was extensive with nearly all households receiving benefits in the end. In South Sudan, however, there was extensive elite capture and communal in-fighting over the aid, resulting in local tensions that endured long after the program ended. Despite evidence of such variation in targeting outcomes, there remain few systematic explanations for why targeting is beneficial in some communities but not in others.

To guide our inquiry into how targeting aid within communities affects distributional outcomes and social cohesion, we first develop a simple formal model with four assumptions, three of which are relatively standard and one which is not. First, for the reasons described above, we assume that aid agencies do not have perfect control over targeting and that communities can influence how aid is allocated. Second, we assume that aid agencies seek to target vulnerable groups (such as the poor), which typically have less power within the community (NRC, 2013; OCHA, 2014; de Sardan et al., 2015). Third, we assume that elites hold formal positions of power within the community and therefore can influence how aid is distributed and capture a share for themselves. The fourth and novel assumption is that aid targeting creates a group of *de facto* non-beneficiaries—comprising other members of the community (distinct from elites) who are ineligible to receive aid—that can also influence distributional outcomes and try to capture aid. While there is qualitative evidence that non-beneficiaries play a role in influencing how aid is allocated (de Sardan et al., 2015; Morel, Watanabe and Wrobel, 2009), their role as an independent strategic actor has not (to our knowledge) been theorized. Building upon these assumptions, we contend that aid targeting can best be conceptualized as creating a bargaining environment in which three groups—the target group, the elites, and other non-beneficiaries in the community (the excluded group)—compete over the allocation of funds.

We formalize these intuitions in a three-player bargaining model allowing for bargaining breakdown and coalition formation. The main components of the model are the size of

the aid windfall, the relative influence of the different groups within the community, and the value of their relations with each other. Taking these into account, the elites offer a division of the aid to the excluded group and the target group, who in turn decide whether to accept the elite's offer or to contest it. Contestation occurs in equilibrium when the elite's concerns about further empowering another influential group are higher than the costs of contestation, which we formalize as the deterioration of group relations. In the case of contestation, groups may form coalitions, and the choice of coalitions determines the costs of contestation suffered and the probabilities of winning. We show that even when there is no direct risk of contestation by the target group, they nonetheless can affect distributional outcomes by the prospect of forming a coalition with one of the powerful players.

The central insights from the model are that, as the amount of the aid windfall increases, the target group will receive more of the aid to which it is entitled in competitive communities where the *excluded* group is both influential and has bad relations with other groups (hereafter 'competitive' communities). It is precisely in these communities that the threat of excluded group capture is high, which prompts elites to redistribute more to the target group to entice them to form a coalition. In contrast, in communities where the excluded group has good relations with the rest of the community or lacks influence (hereafter 'non-competitive' communities), bigger aid windfalls do *not* result in the target group receiving a bigger share of the aid allocation. In fact, they can lead to the target group receiving an even smaller share due to more capture by the other more influential groups. Furthermore, while bigger aid windfalls result in the target group getting more in competitive communities, this can come at the cost of social cohesion. A bigger aid windfall expands the set of parameters that result in contestation, making it more likely. This highlights that the efficiency objectives of targeted aid programs—to improve the economic welfare of the most vulnerable—can run counter to do the "do no harm" goals of maintaining social cohesion.

We test our predictions using a regression discontinuity design and original survey data from a post-conflict community-driven reconstruction project implemented in the Indone-

sian province of Aceh. In recent years, community-driven development and reconstruction programs have achieved prominence as a means for promoting economic welfare and social cohesion at the community level in post-conflict settings (Mansuri and Rao, 2004; World Bank, 2007; Fearon, Humphreys and Weinstein, 2009; Casey, Glennerster and Miguel, 2012). The BRA-KDP program studied here aimed to improve these outcomes following 30 years of separatist conflict between the Free Aceh Movement (*Gerakan Aceh Merdeka*, or GAM) and the central government of Indonesia. BRA-KDP is a relevant setting for testing the model as targeting clearly created three groups—a target group comprising civilian conflict victims, an excluded group of former GAM combatants, and elites—poised to interact strategically over the aid allocation. Another important feature of our empirical setting is that the amount of aid each village received (and thus the stakes of contestation) was based on an arbitrary cutoff in a continuous measure of village population. This is an advantage over existing studies in that it enables us to use a regression discontinuity design to estimate the causal effect of the aid amount, mitigating concerns that the size of the aid windfall is endogenous to community characteristics. Our main empirical goal is to estimate how more aid affects distributional outcomes—especially what the target group receives—and social cohesion, conditional on whether villages were historically competitive or not. We do this using data from an original survey conducted with 504 civilians, former-combatants, and village heads in 75 BRA-KDP villages.

As predicted, we find that, targeting a bigger aid windfall resulted in the target group receiving a bigger share in competitive communities and a smaller share in non-competitive communities. The evidence is also consistent with the prediction that this is due to the greater threat of excluded group capture in competitive communities. Specifically, we find that bigger aid windfalls caused the excluded group to benefit more—and elites to benefit less—in competitive communities. While these outcomes are consistent with the distributional patterns predicted by the model, we find little evidence of outright excluded group contestation in competitive communities. Rather, the results suggest that targeting a bigger

aid windfall resulted in more successful conflict resolution in those contexts, which perhaps reflects the fact that BRA-KDP staff intervened to mediate the most egregious instances of appropriation. While these results are reassuring in that they show that more benefits for the target group do not have to entail heightened social divisions, they nevertheless raise awareness that better targeting could come at the cost of social cohesion.

This paper makes several contributions to the literature on aid effectiveness. First, it highlights that bigger aid windfalls do not always result in better economic and social outcomes. In doing so, we complement existing research that shows how aid windfalls can result in the dissipation of economic benefits due to competition among powerful groups within communities (Svensson, 2000) or how humanitarian food aid exacerbates civil conflict due to the ease by which it can be appropriated by rebel groups (Nunn and Qian, 2014).

Moreover, this paper presents a novel answer to a question of central importance in the aid targeting literature: When does the target group, as a weak group, receive a greater share of the benefits to which it is entitled? Explaining distributions to weak players is a puzzle from the perspective of traditional bargaining approaches, which tend to model bargaining as occurring between powerful players or over-predict allocations to stronger players when power is asymmetrical. Existing explanations for distributions to the target group focus on norms of generosity (Harragin and Chol, 1998); the monitoring and enforcement abilities of aid agencies (Paul, 2006; Dietrich, 2013); or the notion that aid empowers the target group and enables it to hold elites or aid agencies accountable for how a targeted aid program is implemented (Winters, 2014; Alatas et al., 2012, 2013; Keen, 2008). While these are important explanations, we do not presume that it is always true that norms prevail in the face of material self-interest, that aid agencies can perfectly control the targeting process, or that vulnerable groups can effectively hold more powerful actors accountable.[5] This paper complements existing explanations by offering an alternative logic—that distributions to the

[5]See Dreze and Sen (1989); Galasso and Ravallion (2005) for more on why vulnerable groups struggle to enforce accountability in the context of targeted aid programs.

target group depend in fact on the bargaining dynamics among more powerful actors within the community.

This study is most closely related to others that have examined distributional outcomes from targeted aid programs within communities (Bardhan and Mookherjee, 2006; Galasso and Ravallion, 2005; Alatas et al., 2013; Kilic, Whitney and Winters, 2013). Existing studies of capture tend to focus on dynamics between two groups in a community, however. For instance, Galasso and Ravallion (2005) examine distributional outcomes between the poor and non-poor in a community-based targeting program in Bangladesh, finding that the non-poor capture more in villages with high income inequality (which they argue proxies for power differentials). In the Indonesian context, Alatas et al. (2013) do distinguish between two powerful groups—formal and informal elites—and find that formal elites capture more, which they attribute to greater reputational costs for informal elites. The authors do not, however, go so far as to theorize the strategic interaction among these different actors.[6] In another paper, Alatas et al. (2016) theorize the cost-benefit calculation for rich individuals deciding whether to apply for an aid program targeted at the poor but do not consider the role of community dynamics. Our paper calls attention to the importance of the excluded group as an independent player with its own interests. Taking into consideration the role of those excluded from aid not only helps to explain when the target group receives more but also presents a more complete picture of the nature and extent of capture. While the literature on elite capture has so far produced mixed results (Bardhan and Mookherjee, 2006; Niehaus et al., 2013; Alatas et al., 2013), our findings underscore that aid could in fact be getting appropriated by a third group within the community whose intentions and actions have so far been largely overlooked.

[6]The results in Alatas et al. (2013) are consistent with the predictions from our model in contexts in which informal elites are the excluded group and place high value on maintaining good relations with the rest of the community.

2 Theory

We begin by developing a simple theoretical framework to shed light on how community dynamics shape distributional outcomes within communities. We make four assumptions that we build into the model. The first three assumptions are relatively standard in the literature: that communities can influence distributional outcomes from a targeted aid program; that the target group is vulnerable; and that community elites can influence distributional outcomes and capture aid. Our fourth assumption, that there is also a group of non-beneficiaries in any community that can influence outcomes, is our main contribution. 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, Grosh and Hoddinott, 2004). In other cases, 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.[7] While aid agencies take steps to mitigate mis-targeting and capture, they are difficult to eliminate.[8] In our approach, we follow on Galasso and Ravallion (2005) in assuming that the aid agency has imperfect control over aid targeting, which shifts our focus to understanding the importance of community dynamics.

[7]The 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, 2006).

[8]Aid agencies often use complaints mechanisms (Bailey, 2008; Maxwell et al., 2011) and community sensitization (Mansuri and Rao, 2004) to try to improve targeting.

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 and Ravallion, 2005; Bardhan and Mookherjee, 2006; Dreze and Sen, 1989). Indeed, what is unique about targeting—and what makes it different from other distributional 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 as a vulnerable group by modeling them as a player that has relatively low levels of influence within the community.

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 often 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, 2004; Alatas et al., 2013; Galasso and Ravallion, 2005; Bardhan and Mookherjee, 2006). Dreze and 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 and most novel assumption—is that there exists yet another group in the community that can also influence how aid is allocated: the excluded group. By definition targeting creates a group of community members (distinct from elites) who do not meet the eligibility criteria and therefore should not receive benefits. Unlike elites, the excluded group does not have a formal role in the targeting process. There is nevertheless anecdotal evidence that such non-beneficiaries intervene in the aid allocation process to try to expropriate a share of the resources for themselves. 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). As one report on the program noted, "As long as you choose one group that receives aid and one that doesn't, there is tension."[9] The excluded group could consist, for example, of former militants in a program targeted at civilians (as in our empirical case); non-poor members of a community in a program targeted at the poor; 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.

While studies have acknowledged the importance of non-beneficiaries in distributional outcomes, their strategic role has not yet been theorized. In our model, the excluded group, like elites, is a relatively influential group and has the option to contest an aid allocation

[9]See `http://www.irinnews.org/report/100907/cash-transfers-good-people-bad-community` (last accessed March 28, 2016).

proposed by the elites. Whether the excluded group chooses to do so is partly a function of its pre-existing influence within the community and of the quality of its relations with other groups. We contend that, in contexts where an aid agency has imperfect control over the targeting process, targeting catalyzes a bargaining environment in which three groups within the community—two more influential groups (the elites and the excluded group) and one relatively less influential group (the target group)—compete over how to allocate the aid.

While the discussion so far pertains to characterizing the nature of the players, one additional and relevant factor is the size of the aid windfall. The amount of aid affects the stakes of contestation such that bigger windfalls increase the incentives to trying to capture a share of the pie. In considering how windfall size affects group dynamics, we build on a large literature on group-rent seeking contests. In the classic formulation, bigger windfalls deliver benefits when groups within a community are homogeneous but are dissipated (through rent-seeking) in the presence of multiple powerful and competing groups (Tornell and Lane, 1998; Svensson, 2000). Our contribution is to show how windfall size affects bargaining among powerful groups in a way that also affects distributions to a weak group.

2.1 Model

We model aid distribution as a bargaining game with coalition formation, where players move in the order of their influence (formally defined below). Given the size $S > 0$ of the windfall and a target group T, the strategic interaction begins when the elite L proposes a take-it-or-leave-it division of the aid windfall among the three players $\alpha = (\alpha_L, \alpha_X, \alpha_T)$.[10] 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 costly contestation in the village.

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

If the excluded group chooses to contest the elite's proposal, they can try to sway the target group to their side by making a counter-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 $1 - p_{XT}$ and the outcome is $(1, 0, 0)$. Either way, the game ends after T's choice of coalition and payoffs are realized.[11] See Figure 1 for an extensive form of the game.

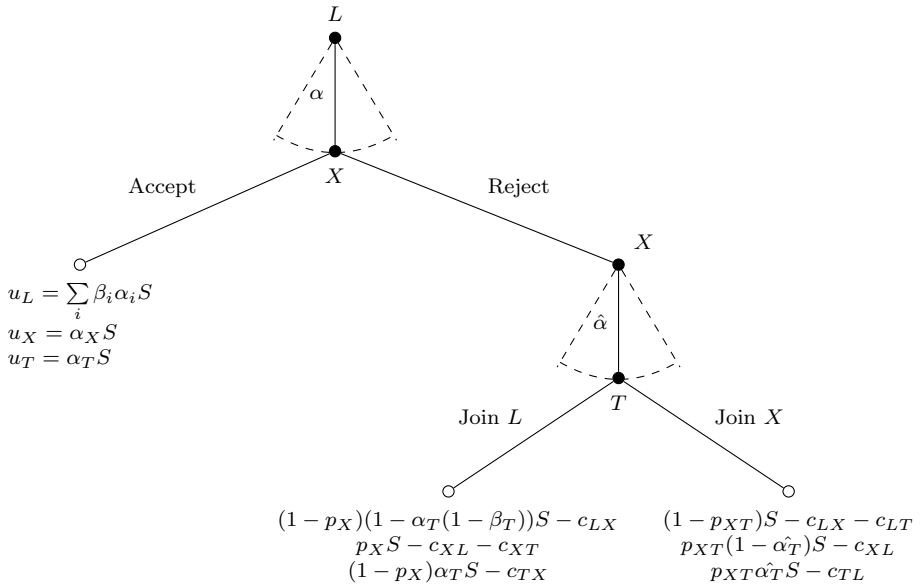

Figure 1: Extensive form of the game.

To consider how community dynamics affect distributional outcomes, we focus on two factors that are important but also conceptually distinct. The first factor is the quality of

[11] In the appendix we study a more general version of the model where T can make a counter-offer or contest both powerful groups at once, and show that the model we present here is functionally the same as that general version; and our results are the same in either case. We present the simpler version in the main paper, and keep the more general version in the appendix for the interested reader.

relations between the groups, meaning how beneficial their interactions with other groups are and therefore how important it is for them to maintain good relations. To capture the effect of relations between groups on bargaining outcomes, we suppose that each group i pays a cost $c_{ij} > 0$ for all groups j they face off against during contestation. This parameter can be thought of as reduced form for the value of continued interactions among the groups after the bargaining situation is resolved, which are lost when there is contestation. Thus, groups that have good relations with the rest of the community will face bigger 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. To understand how variations in the influence of groups affect 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 and Ravallion, 2005). 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 by the excluded group.[12]

These weights allow us to capture two distinct and diametrically opposed incentives for the elite. On one hand, when pressed, elites 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. On the other hand, they are concerned about rivals and fear giving resources to another group with high influence that might employ these resources to one day challenge the elite's political control.[13] Thus, we assume that weights assigned to the

[12]When $\beta_T \geq 1$, there is no elite capture. Depending on parameters, the aid is either shared between the excluded group and the target group, or the target group receives the whole windfall. We ignore this possibility because it is not relevant to our case, but we acknowledge that it may be relevant in other aid settings.

[13]An interesting extension of this model would be to look at a repeated version of this game

shares are lower for more influential groups, that β_i is strictly decreasing in the influence of group $i \in \{X, T\}$. Typically, with respect to the excluded group—which is another relatively influential group—the rivalry concerns dominate the distribution concerns and we have $\beta_X \leq 0$. For the target group vulnerability translates to low influence within the community. For them the elite's generosity concerns dominate and we have $\beta_T \geq 0$. In case of contestation, for sake of convenience we assume that β is only non-zero for groups in a coalition with the elites; for groups with whom the elite is in contestation, the continuation value is incorporated into the costs of contestation.

We assume that windfall size is large enough to make the threat of contestation by the excluded group credible.[14] Our solution concept is Subgame Perfect Nash Equilibrium. To 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 recruiting another or not while making an offer, they choose not to recruit.

Whether contestation occurs in equilibrium depends on the elite's proposal. Unless the elite's relations with others—particularly the excluded group—are extremely bad, they will try to prevent contestation by appeasing the excluded group. To avoid contestation the elite must make sure X is at least as well off accepting the offer than rejecting. There are two ways with which L can approach this problem; (a) L can either offer a large share to X and ignore T, or (b) L can give 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. We call the former the $\mathcal{A}$ppropriation

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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.

[14]Formally, $S > \max\{(c_{XL} - c_{TX} + c_{TL})/p_{XT}, (c_{XL} + c_{XT})/p_X, (c_{XT} + c_{TX} - c_{TL})/(p_{XT} - p_X)\}$. If this does not hold, there are parameter values for which the elite offers to keep the whole windfall for themselves, and the excluded group accepts this offer as the stakes are too low relative to the value of their relations with the elite.

equilibrium where the two influential groups share the aid between themselves, and denote that allocation by $\alpha^{\mathcal{A}}$. Similarly, we call the latter the $\mathcal{D}$ivision equilibrium where all three groups get some share of the aid, and denote this allocation by $\alpha^{\mathcal{D}}$.

Whether in equilibrium the elite offers $\alpha^{\mathcal{A}}$ or $\alpha^{\mathcal{D}}$ depends on the influence of the excluded group, β_X.[15] Specifically, we define $\beta_X^* \equiv \frac{\beta_T - p_X}{1-p_X}$ such that when $\beta_X < \beta_X^*$, the elite offers $\alpha^{\mathcal{D}}$, and otherwise offers $\alpha^{\mathcal{A}}$. This is because when β_X is low, 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. Therefore, in this case the elites prefer to give up some of the aid in exchange for an allocation that favors the target group relative to the excluded group.

Finally, there is a set of parameters for which there is contestation in equilibrium, *i.e.* when the relations between the elite and the excluded group are very bad, and the excluded group is sufficiently influential.[16] The intuition behind the existence of contestation is straightforward: when the excluded group is very influential and their relations with the rest of the village are extremely bad, the elite's concerns about empowering them dominate 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 with the elite.[17]

The equilibria of this game are summarized in the following proposition:

Proposition 1. *When $c_{XL} + c_{LX} \leq c^*(\beta_X, S)$, there is contestation in equilibrium. Other-*

[15] Since in our empirics we observe variation in the excluded group's influence but not in the target group's influence, we fix β_T and study variations in β_X.

[16] Formally, there is contestation when $c_{XL} + c_{LX} < c^*(\beta_X, S)$ where the latter term is defined in the appendix.

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

wise, there are two potential peaceful equilibria:

(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.*

(D) When $\beta_X < \beta_X^*$*, L offers* $\alpha_X^{\mathcal{D}} = p_X - \frac{c_{XL}+c_{XT}}{S}$ *and* $\alpha_T^{\mathcal{D}} = \frac{p_{XT}-p_X}{1-p_X} + \frac{c_{TX}+c_{XT}-c_{TL}}{(1-p_X)S}$*, X and T accept, windfall is divided accordingly.*

This result relates more broadly to the literature about negative externalities in bargaining situations (Laengle and Loyola, 2012, 2015). In classic bargaining models, there is no bargaining breakdown in equilibrium since there is always a solution that Pareto dominates it. But, consistent with other work on the possibility of disagreement under complete information (Chowdhury, 1998), Laengle and Loyola (2015) has shown that, in two-player ultimatum or dictator games when players derive negative externalities from the share of the windfall that the other gets, there can be bargaining breakdown in equilibrium. With this model we show that introducing a third, non-rival receiver opens an alternative to bargaining breakdown. When the excluded group is very powerful and has bad relations with others, the elite is not willing to let them capture a large portion the aid without a fight. They can, however, both agree to distribute the aid to the target group, which is not a threat to either.

Predictions

Our main interest is understanding when the target group receives anything in equilibrium despite their lack of influence. We study comparative statics on the share of the target group as a function of windfall size for different sets of parameters. We find that as the size of the windfall (the stakes of the game) increases, the target group will get more of the aid in places where the excluded group is both very influential ($\beta_X < \beta_X^*$) *and* has bad relations with the target group ($c_{XT} + c_{TX} < c_{TL}$), hereafter 'competitive' communities. This is true because, as the excluded group's relations with the rest of the community worsen, it becomes marginally cheaper for the elite to buy support from the target group than to assuage the

excluded group. Conversely, the target group obtains a smaller share where the excluded group is not very influential *or* has good relations with the rest of the community, hereafter 'non-competitive' communities. This is an important finding because it shows that more aid does not benefit the target group in non-competitive communities while it does benefit them in competitive communities but only due to the bargaining power of the excluded group.

Hypothesis 1. *As the amount of aid increases, the equilibrium share of the target group increases in competitive communities and (weakly) decreases in non-competitive communities.*

While our main interest is in understanding distributions to the target group, the model suggests that bigger aid windfalls will result in the excluded group capturing more of the aid—and elites capturing less—in competitive communities. This occurs for two reasons. First, in a $\mathcal{D}$ivision equilibrium, the elite is able to use their first-mover advantage to extract the costs that the excluded group would have to endure if there was contestation, $c_{XL} + c_{XT}$. As S increases, the excluded group's gains from contestation increase. However, their costs stay the same, and so does the amount L can extract. Hence, L needs to offer X a greater share of the pie to avoid contestation. Second, there is also a greater risk of contestation in competitive communities in equilibrium (see below), in which case the excluded group's share is always larger than when there is no contestation. Thus, we expect that, in competitive communities, both the absolute amount and the share of the aid received by the excluded group will increase as the size of the aid windfall gets bigger. One important implication of this result is that while it appears as if bigger windfalls result in more aid for the target group and less elite capture in competitive communities, we are missing a significant part of the capture story unless we take the excluded group into account.

Hypothesis 2. *As windfall size increases, the equilibrium share of the excluded group increases in competitive communities, while the equilibrium share of the elite decreases.*

As just mentioned, one of the reasons that the excluded group gets more in competitive communities is because there is more contestation in these contexts. To see why this is

the case we compare the utility gain for the elite from making an offer that will result in contestation versus peaceful resolution. We find that—holding everything else constant—better community relations (higher c) make avoiding contestation more desirable for the elite. This is not only because better relations make contestation more costly for the elite but also more costly for others, which in turn increases the amount that the elites can extract. Conversely, as the influence of the excluded group grows, the elite has fewer incentives to share aid with them and thus the utility gain from an offer that leads to contestation grows relative to one that averts it. These two observations imply that, *ceteris paribus*, competitive communities are more prone to contestation than non-competitive communities. Furthermore, in competitive communities, greater windfall size makes the utility gain of contestation greater for the elite. Overall, this means that as the aid amount increases, we should expect to see more contestation in competitive communities.

Hypothesis 3. *As windfall size increases, contestation becomes more likely in competitive communities.*

Importantly, while the target group might receive more of the benefits to which it is entitled in competitive communities, this prediction highlights that these are also the very communities that might experience the greatest deterioration in social relations. This is an important prediction to test since aid agencies—which typically operate under a "do no harm" principle—hope that their programs will not resulted in heightened social divisions.

3 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 Indonesian military and government. While the conflict evolved in several stages, civilians frequently suffered the brunt of hostilities as the Indonesian military sought to undercut popular support for GAM by terrorizing suspected civilian supporters. The conflict resulted in approximately 30,000

deaths as well as widespread instances of murder, torture, rape, internal displacement, and property destruction.

The peace agreement reached in 2005 contained provisions to reintegrate GAM combatants and to provide assistance to civilian conflict victims (Tajima, 2010). The Aceh Peace Reintegration Agency (*Badan Reintegrasi-Damai Aceh*, or BRA) was established to manage this process. In an effort to reach conflict-affected communities, BRA partnered with the World Bank-supported Kecamatan Development Program (KDP). The resultant BRA-KDP program was designed to deliver assistance to local conflict-affected communities. The central component of the program was the disbursement to more than 1,700 villages of aid windfalls ranging in size from 60 to 170 million rupiah (about USD $6,000-$17,000).[18]

The target group in BRA-KDP was civilian conflict victims. In order to identify beneficiaries, BRA-KDP opted for a community-based targeting approach. Each village organized a series of meetings to select the criteria for identifying the most conflict-affected households and which households met those criteria. While in some villages all community members viewed themselves as conflict-affected, BRA-KDP socialized communities to try to identify households that were *most* conflict-affected and provide those households with the greatest share of benefits (Morel, Watanabe and Wrobel, 2009). Importantly, conflict victims were indeed 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 rely on the community to determine who qualifies for assistance, we won't get the benefits we deserve" (Morel, Watanabe and Wrobel, 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.

Importantly, former GAM combatants were explicitly excluded from BRA-KDP. In many

[18]BRA-KDP resembles a game with perfect information in that the amount of aid going to each village was heavily publicized by implementation teams within recipient communities.

villages, former combatants constituted a powerful and organized group (Morel, Watanabe and Wrobel, 2009). Anecdotal evidence from BRA-KDP implementers suggests that there were indeed instances in which ex-combatants tried to influence the allocation of BRA-KDP funds and to capture a share for themselves. The notion that former combatants felt entitled to BRA-KDP assistance is evident 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, Watanabe and Wrobel, 2009, 28).

Finally, anecdotal evidence suggests that elites also played a distributional role in BRA-KDP. BRA-KDP actually presents a difficult context for identifying elite capture because the program was implemented by facilitators employed directly by the program—and not affiliated with the village government—precisely to minimize the scope for elite capture. Nevertheless, it appears 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, Watanabe and Wrobel, 2009, 27).

4 Empirical Strategy

4.1 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.[19]

[19]Due to the lack of availability of data from other sources, survey data is commonly used in evaluations of community-driven reconstruction programs (Mansuri and Rao, 2004) and

The surveys were implemented in 2008, approximately 12 months after the BRA-KDP program ended, and were conducted face-to-face by trained enumerators from a professional survey firm. Sampling followed a multi-stage cluster sampling approach. Strata were first formed by sub-district and sub-district population. Villages within sub-districts were the primary sampling units and were selected with a fixed probability. Five households were randomly sampled within each selected village, either from a complete list of all households in the village or by using a random walk method. Finally, individuals were sampled within households where any male or female member of the household between the ages of 18-65 was eligible for selection.[20] All analysis employs survey weights to account for unequal probabilities of inclusion in the study. Question-wording for all survey questions used in the analysis can be found in Appendix B.

Coding 'competitive' versus 'non-competitive' villages

The central empirical objective is to investigate the effect of windfall size on aid allocations and social cohesion conditional on pre-existing community dynamics. According to the model, outcomes depend on whether the threat of capture by the excluded group is credible, which is predominantly the case when it has high influence ($\beta_X < \beta_X^*$) *and* antagonistic community relations ($c_{XT}+c_{TX} < c_{TL}$) (labeled 'competitive' communities). Conversely, villages in which GAM has low influence or good relations with other groups in the community yield a low threat of excluded group capture and are considered 'non-competitive'.

We use data from the village head survey to code villages as competitive or non-competitive.

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other targeted aid programs (Galasso and Ravallion, 2005; Bardhan and Mookherjee, 2006).

[20]For more on the sampling methodology, see the research design memo for the Aceh Reintegration and Livelihood Surveys available on [AUTHOR'S WEBSITE]. For the ex-combatant sample, whose data we use in Appendix G, a complete list of all ex-combatants living in each village was unenumerated in collaboration with village and former-GAM leaders. Ex-combatants were then sampled from the list with a fixed probability.

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. We proxy for GAM influence using a question about whether the village was a GAM stronghold ('basis GAM') during that period. Importantly, 'basis GAM' was a designation made by the Indonesian military and was orthogonal to whether or not communities actually supported GAM's presence there. Our assumption is that in communities where GAM had a stronger presence during the conflict, former combatants will be more influential in the community. We proxy for the nature of community relations with a survey question that inquired into whether the majority of villagers actually supported GAM-TNA during this period or did not (meaning they supported the Indonesian military or neither side). We consider relations between GAM and the community to be historically 'good' in villages where GAM had at least majority support.

We combine these two measures to create a binary indicator where competitive villages—those in which GAM is influential and had bad relations with other groups in the community—are coded as one (1) while all other villages are considered non-competitive and coded as zero.[21]

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

The table shows the over-lapping measures of GAM influence and relations taken at the village-level. Villages in which GAM is both influential and has bad community relations are considered 'competitive', all other villages are considered 'non-competitive.

Table 1: Measure of village competitiveness

[21]While it is beyond the scope of this paper to explain why villages vary in their competitiveness, we provide some preliminary analysis of the correlates in Appendix E.

4.2 Regression discontinuity design and estimation

A key determinant of the threat of excluded group contestation in the model is the size of the aid windfall. One benefit of our empirical context is that we have exogenous variation in windfall size, which gives us causal leverage over an important model parameter. 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., 2009). BRA-KDP used two measures to determine aid amounts at the village-level.[22] 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.[23] 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).[24] 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 received an aid windfall in the amount

[22]For a more detailed description of the assignment process, see Appendix C.

[23]We note that the data we use to classify villages as competitive or not is different than the data the World Bank used in the assignment process.

[24]The 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. We discuss external validity implications below.

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 2 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 competitive or non-competitive.

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 and Lemieux, 2008). Our main empirical goal is to estimate the effect of an increase in windfall size on aid allocations in competitive and non-competitive villages. To do this we estimate 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.[25] 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.[26] V_j is the binary indicator which equals one for competitive villages and zero for non-competitive 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 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

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

[26]This is a 'sharp' RD in that by all World Bank accounts the cutoff completely determined assignment.

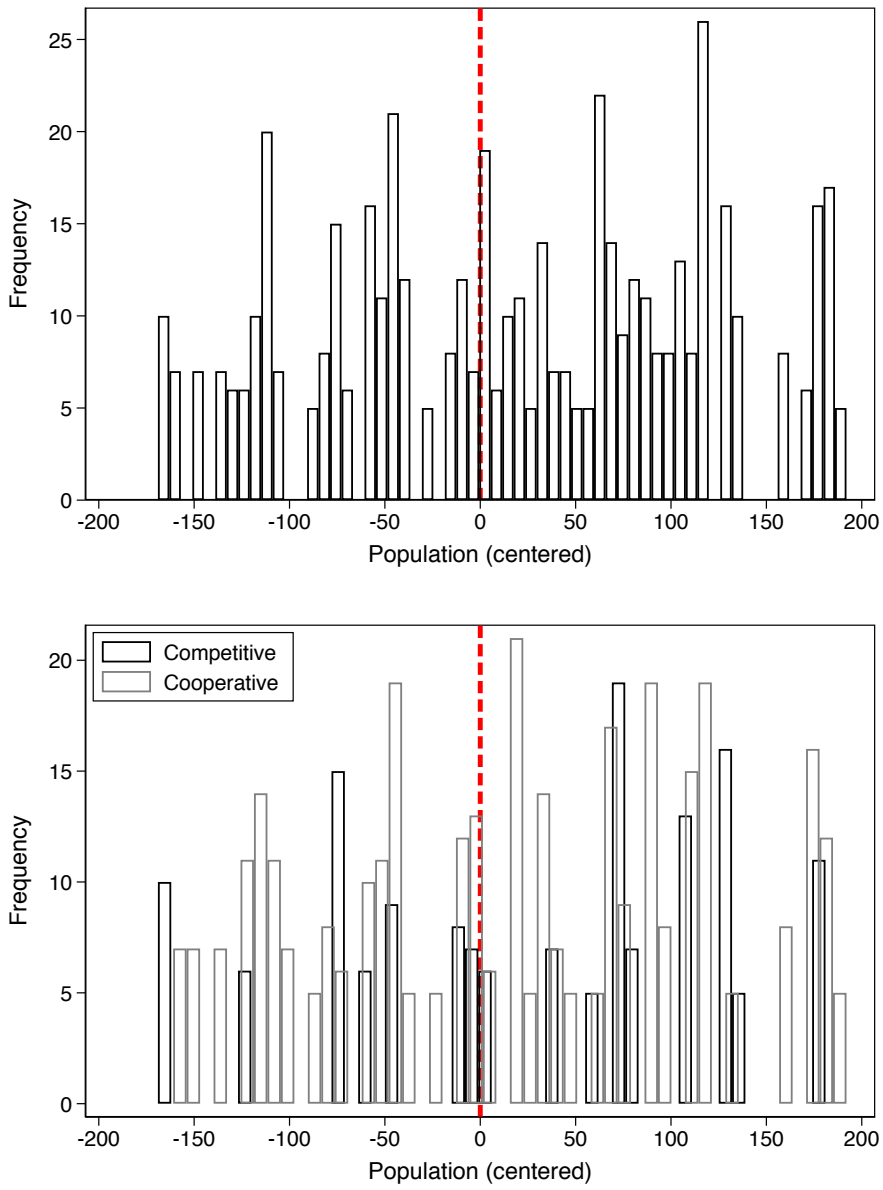

Figure 2: Distribution of individual-level observations around the population threshold centered at zero. Top panel shows the full sample; bottom panel shows the distribution in competitive and non-competitive villages.

on either side of the threshold.[27] The coefficient γ identifies the effect of a bigger windfall in competitive relative to non-competitive villages while τ captures the effect of targeting a

[27]For 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: $\beta_5\tilde{P}_j^2 + \beta_6 Z_j\tilde{P}_j^2 + \beta_7 X_j\tilde{P}_j^2 + \beta_8 Z_j V_j\tilde{P}_j^2$.

bigger windfall in non-competitive communities. To save on space, we only present τ, δ, γ, and the marginal effect of windfall size in competitive communities ($\tau + \gamma$) in our tables.[28]

We also include in our regressions X'_{jm}, a vector of m village-level controls, to account for possible confounding in our measure of village competitiveness. We obtain a rich set of pre-treatment controls using data from the 2000 PODES survey, an extensive survey conducted regularly in every Indonesian village. We control for village poverty; terrain and proximity to a forest; remoteness from services, markets and population centers; government capacity; security; and the presence of criminal networks. We present regressions with and without controls, in some specifications we also control for district fixed effects. Descriptive statistics for all variables used in the analysis can be found in Appendices D.

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

The key identifying assumption of an RDD is the continuity of potential outcomes at the threshold (Hahn, Todd and Van der Klaauw, 2001; de la Cuesta and Imai, 2016). Following the literature, we check this assumption by testing for discontinuities in our m pre-treatment village-level controls and our measures of village competitiveness at the threshold. The results, presented in Appendix F 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 F).

[28]We 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.

5 Results

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 D, 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., 2009; Morel, Watanabe and Wrobel, 2009).

Our first hypothesis is that, as the amount of aid increases, the target group will obtain a greater share of the benefits in competitive villages and a smaller share of the benefits in non-competitive villages. 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.[29] Table 2 presents the results for the civilian subsample. We use data from the full civilian subsample here because victimhood was broadly defined in many villages; we show in Appendix G that we observe the same pattern of results if we define conflict victims more narrowly using objective or subjective criteria. 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 $\pm$ 150.

There are three main findings presented in this table, which are also shown graphically in Figure 3. 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

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

benefits in competitive communities. Across all six main specifications, the coefficients are positive and significant. The point estimates across the models 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, what the target group receives is (weakly) decreasing in non-competitive 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 competitive *relative* to non-competitive communities. These differences are statistically and substantively significant. Appendix G presents results in terms of the actual amounts received by those in the target group. The evidence shows that, as windfall size increases, those in the target group in competitive communities received 1.28 to 2.51 million rupiah (USD $128-251) more than their counterparts in non-competitive communities.

	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 * Competitive (γ)	0.97***	1.38***	1.08***	1.58***	1.93***	1.36**
	(0.34)	(0.41)	(0.39)	(0.54)	(0.55)	(0.52)
	0.006	0.001	0.007	0.004	0.001	0.010
Bigger windfall (τ)	-0.46*	-0.50*	-0.29*	-0.86*	-0.98**	-0.37
	(0.26)	(0.25)	(0.17)	(0.47)	(0.47)	(0.35)
	0.081	0.050	0.091	0.073	0.041	0.292
Competitive (δ)	-0.57*	-0.95***	-0.44	-0.83*	-1.20**	-0.79*
	(0.29)	(0.35)	(0.31)	(0.50)	(0.48)	(0.43)
	0.056	0.008	0.165	0.099	0.015	0.071
Marginal effect of a bigger aid windfall in competitive villages	0.51**	0.88***	0.80**	0.72***	0.95**	1.00**
	(0.22)	(0.30)	(0.30)	(0.25)	(0.38)	(0.39)
	0.023	0.004	0.010	0.006	0.013	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 2: Effect of Targeting a Bigger Aid Windfall on Target Group Benefits

While these results support the first hypothesis, we consider the possibility that they

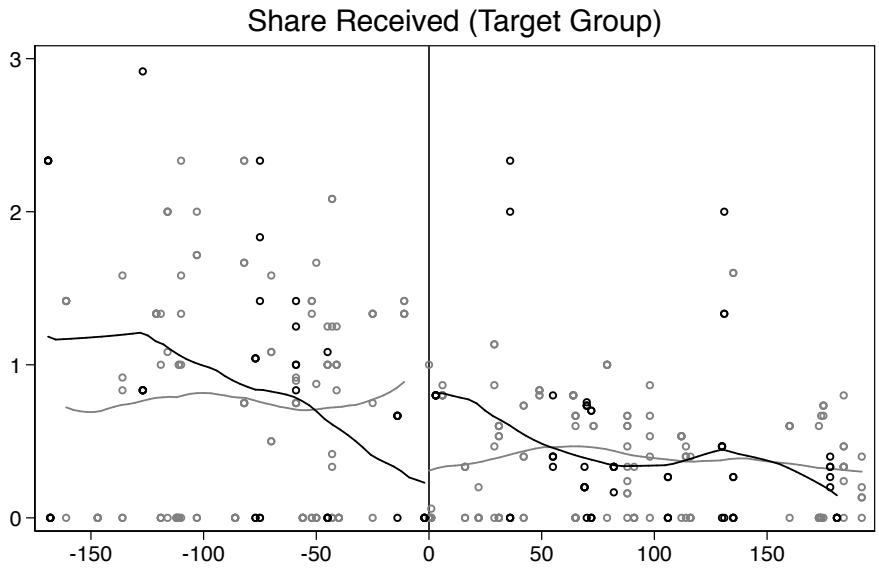

Figure 3: Local polynomial regression showing the effect of targeting a bigger aid windfall on the share received by the target group in competitive (black line) versus non-competitive (gray line) villages.

would not be an accurate reflection of what the target group actually received if there was secondhand redistribution, a form of capture in which the target group is compelled to give those goods to others in the community following the initial allocation. To investigate this, we use a question from the survey that asked respondents what happened to the BRA-KDP goods within one month of receiving them. No one reported that they had been given or taken away. This strongly suggests that the findings presented here reflect the genuine final allocation of goods in BRA-KDP.

The theoretical model shows that, as the amount of aid increases, the target group receives a greater share of the benefits in competitive communities because of the bargaining dynamics between elites and the excluded group as the two other influential groups within the community. The second hypothesis predicts that the excluded group will receive a greater share of the windfall in competitive communities—where their threat of contestation is credible—while elites will receive a smaller share. To assess the extent to which 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). For ease of analysis, we combine the two measures pertaining to elite benefits into an index using inverse covariance weighting (Anderson, 2008).[30]

The main results on perceived ex-combatant benefits are presented in Table 3, where the results in the final row show the marginal effect of targeting a bigger aid windfall in competitive 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 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 $\pm$ 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 competitive communities.

As predicted by the second hypothesis, the results suggest that the reverse is true for elites. The model predicts that, as windfall size increases, there will be less *elite* capture in competitive 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 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.

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

	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 * Competitive	-0.11	1.01***	0.87***	0.27	0.97***	1.05***
	(0.35)	(0.31)	(0.33)	(0.38)	(0.36)	(0.29)
	0.752	0.002	0.010	0.471	0.008	0.000
Bigger windfall	-0.20	-0.30	-0.36**	-0.15	0.06	-0.30*
	(0.25)	(0.20)	(0.16)	(0.27)	(0.23)	(0.16)
	0.430	0.131	0.025	0.583	0.780	0.063
Competitive	-0.01	-0.50*	-0.55**	0.04	-0.32	-0.34
	(0.24)	(0.26)	(0.23)	(0.24)	(0.29)	(0.21)
	0.979	0.059	0.019	0.883	0.266	0.116
Marginal effect of a bigger aid windfall in competitive villages	-0.31	0.71***	0.51*	0.13	1.04***	0.76***
	(0.25)	(0.26)	(0.28)	(0.27)	(0.30)	(0.22)
	0.207	0.007	0.067	0.640	0.001	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: Effect of Targeting a Bigger Aid Windfall on Perceived Excluded Group Benefits

All in all, the results thus far are generally consistent with the main predictions of the model that the target group does better in competitive communities, primarily due to the threat of excluded group contestation. While the target group gets more in competitive communities, our third hypothesis highlights the concern that this could come at the cost of social cohesion. While the threat of excluded group contestation is what results in the target group receiving more, bargaining breakdown and excluded group contestation are detrimental to social cohesion since the costs of contestation are deteriorated relations between the excluded group and the rest of the community.

While we do not have direct measures of whether contestation occurred, we use survey questions that proxy for relationship deterioration and the sense that village divisions tend to endure. To measure the deterioration in relations between community members and former combatants, we use inverse covariance weighting to create an 'index of GAM acceptance' that aggregates five survey measures. These measures capture civilian willingness to accept GAM in various roles, including as members of village associations, as village leaders, and as

	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 * Competitive	0.78*	1.71***	0.84	1.28***	1.59***	0.99**
	(0.41)	(0.55)	(0.51)	(0.48)	(0.55)	(0.46)
	0.059	0.002	0.101	0.009	0.005	0.033
Bigger windfall	-0.87***	-0.92***	-0.45*	-0.93***	-0.55	0.54*
	(0.14)	(0.25)	(0.24)	(0.20)	(0.34)	(0.28)
	0.000	0.000	0.064	0.000	0.108	0.056
Competitive	-0.62**	-2.19***	-1.53***	-0.64	-2.06***	-2.62***
	(0.31)	(0.48)	(0.58)	(0.41)	(0.50)	(0.55)
	0.048	0.000	0.010	0.116	0.000	0.000
M.E. bigger windfall in comp. vils	-0.09	0.79*	0.39	0.35	1.03**	1.52***
	(0.39)	(0.44)	(0.38)	(0.44)	(0.48)	(0.38)
	0.824	0.072	0.317	0.425	0.033	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 4: Effect of Targeting a Bigger Aid Windfall on Excluded Group Benefits (ex-combatant sample)

close friends. Overall, as can be seen in Appendix D reported acceptance of former GAM is high. Moreover, the results in Panel A of Table 6 suggest that targeting a bigger aid windfall had little effect on relations with former combatants.

We also examine the effect of targeting a bigger aid windfall on perceptions that tensions in the village tend to endure, consistent with the notion of a persistent deterioration in relations. The results in Panel B of Table 6 suggest that, in competitive communities, a bigger aid windfall increased perceptions that conflict is resolved satisfactorily and does not persist. This suggests that there was little outright contestation in competitive villages with big windfalls. One possible explanation for the lack of outright contestation might be the involvement of the aid agency in facilitating conflict resolution. Indeed, BRA-KDP program management actively intervened to mediate social conflict and diffuse tensions throughout implementation (Morel, Watanabe and Wrobel, 2009, 31). Of known attempts by former combatants to extort funds in eight sub-districts, further socialization and intervention led GAM to withdraw its demands in all known cases. While contestation was avoided in BRA-

	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 * Competitive	-0.62	-0.37	-1.09	-1.56***	-1.72**	-2.95***
	(0.61)	(0.80)	(0.86)	(0.48)	(0.73)	(0.80)
	0.308	0.645	0.209	0.002	0.020	0.000
Bigger windfall	0.26	0.46	0.55*	0.57*	1.18***	1.24***
	(0.30)	(0.32)	(0.30)	(0.33)	(0.37)	(0.38)
	0.387	0.148	0.074	0.087	0.002	0.001
Competitive	0.35	-0.06	0.53	0.31	0.27	1.45**
	(0.26)	(0.58)	(0.68)	(0.31)	(0.52)	(0.60)
	0.175	0.917	0.433	0.325	0.608	0.017
Marginal effect of a bigger aid windfall in competitive villages	-0.36	0.09	-0.54	-0.99***	-0.54	-1.71**
	(0.54)	(0.64)	(0.69)	(0.35)	(0.62)	(0.66)
	0.500	0.887	0.439	0.005	0.382	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 5: Effect of Targeting a Bigger Aid Windfall on Perceived Elite Benefits

KDP due to the aid agency's intervention, the model nevertheless highlights the potential for its occurrence in certain contexts, with serious consequences for social cohesion.

The results presented here are highly robust to alternative specifications, bandwidths, and extended analyses (see Appendices G and F). All in all, we find that, as the amount of aid increases, the target group receives a greater share in competitive communities but a smaller share in non-competitive ones. The results are consistent with the predicted distributional patterns: targeting a bigger aid windfall causes the excluded group to get more—and elites to get less—in competitive communities. We do not, however, see evidence of outright contestation by the excluded group. If anything it seems that community members experienced greater conflict resolution in such contexts. This is reassuring in the sense that more aid for the target group does not have to come at the expense of social cohesion.

One potential concern with our empirical test is external validity. Our results are based on one targeted aid program in Aceh.[31] Moreover, they are based on effects estimated at a

[31]We note that other studies also focus on estimating the effects of targeting in one or a small

specific threshold between small and medium sized villages. As with all regression discontinuity designs, this raises questions about the external validity of such a 'local' treatment effect. Indeed, we might expect effects to differ in bigger villages where community relations matter less or in places where the social cleavages are less salient or cut across one another. Importantly, predictions from the model could be tested in any context where there is scope for community dynamics to influence distributional outcomes and the target group is vulnerable. Our goal is to encourage further investigation into the role of the excluded group and the importance of community dynamics in understanding the outcomes of targeted aid programs.

6 Conclusion

This paper examines how targeting aid at vulnerable groups within communities affects distributional outcomes—especially the amount received by the target group—and social cohesion. We focus on the importance of community dynamics and argue that targeting creates a bargaining environment in which three groups in the community—the target group, an excluded group, and elites—compete over how to divide the funds. By highlighting the potential for the excluded group to contest the distribution, we provide a new explanation for when the target group—as a vulnerable group—receives more of the aid to which it is entitled.

Overall, the theory and findings presented here have three main implications. First, they suggest that more attention should be given to the influence of community dynamics and bigger windfalls on targeting outcomes, as it is the interaction of both that yields counter-intuitive dynamics. Second, they emphasize the importance of paying attention to the role and importance of the excluded group in explaining the outcomes of targeted programs. The

number of aid programs in a single country (Galasso and Ravallion, 2005; Bardhan and Mookherjee, 2006; Alatas et al., 2013).

analysis suggests that whether aid is distributed to the target group occurs primarily because of the coercive power of the excluded group. This leads to the third, and most surprising, observation that targeting may be more successful in competitive environments because elites have greater incentives to distribute aid to the target group. This observation runs counter to preconceived notions that the target group will benefit more in non-competitive villages and suggests that the final allocation of aid to vulnerable populations and the excluded group might follow a logic of conflict resolution rather than of social justice.

Taken together, these results highlight a possible tension between the economic objectives of targeting—efficiently reaching the target group to improve their welfare—and the humanitarian goal of not exacerbating divisions within communities. One important policy implication for aid agencies is that they need to be aware of local context and existing social divisions when designing aid programs. This paper emphasizes that aid targeting should avoid reinforcing existing social divisions and, if possible, cut across them (Pottier, 1996). Further research is needed on how targeting can be accomplished in a way that ensures it achieves the desired economic and social outcomes. Nevertheless, the findings presented here underscore the importance of recognizing that targeting can induce distributional conflict among different groups within a community and it is ultimately the nature of group power dynamics and competition that drive the outcomes of that process.

	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 * Competitive	0.41	0.08	0.09	0.65	0.25	0.53
	(0.39)	(0.71)	(0.72)	(0.47)	(0.77)	(0.69)
	0.302	0.909	0.902	0.168	0.748	0.446
Bigger windfall	-0.17	-0.41*	-0.48*	0.01	-0.27	-0.50
	(0.21)	(0.22)	(0.26)	(0.27)	(0.23)	(0.33)
	0.420	0.073	0.067	0.967	0.244	0.131
Competitive	-0.23	0.25	0.46	-0.58	-0.07	-0.29
	(0.36)	(0.63)	(0.60)	(0.45)	(0.70)	(0.58)
	0.529	0.697	0.446	0.200	0.917	0.618
Marginal effect of a bigger aid windfall in competitive villages	0.24	-0.32	-0.39	0.67*	-0.02	0.02
	(0.33)	(0.61)	(0.59)	(0.38)	(0.71)	(0.50)
	0.472	0.598	0.517	0.085	0.973	0.962
N	317	312	312	317	312	312
Panel B: Conflict resolved satisfactorily						
Bigger windfall * Competitive	0.55*	0.63**	0.77***	0.22	0.36	0.38
	(0.28)	(0.25)	(0.23)	(0.36)	(0.30)	(0.26)
	0.052	0.011	0.001	0.548	0.232	0.145
Bigger windfall	-0.08	-0.09	-0.14	-0.07	0.08	0.03
	(0.07)	(0.10)	(0.11)	(0.11)	(0.12)	(0.12)
	0.283	0.401	0.213	0.514	0.523	0.783
Competitive	-0.42*	-0.82***	-0.85***	-0.25	-0.57**	-0.56**
	(0.25)	(0.22)	(0.21)	(0.36)	(0.29)	(0.23)
	0.099	0.000	0.000	0.491	0.048	0.018
Marginal effect of a bigger aid windfall in competitive villages	0.48*	0.54***	0.62***	0.15	0.44	0.42*
	(0.27)	(0.20)	(0.17)	(0.34)	(0.27)	(0.21)
	0.083	0.007	0.001	0.668	0.105	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 6: Effect of Targeting a Bigger Aid Windfall on Social Cohesion

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