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

Laura Paler* Camille Strauss-Kahn† Korhan Koçak‡

December 11, 2018

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.

Word Count: 13,863

*Assistant Professor, University of Pittsburgh.

†Ph.D. Candidate, Columbia University.

‡Ph.D. Candidate, Princeton University. The authors would like to thank Stephen Chaudoin, Muslahuddin Daud, Grant Gordon, Guy Grossman, Macartan Humphreys, Yotam Margalit, Lucy Martin, Adrian Morel, Cyrus Samii, Teuku Zkhradi Setiawan, Michael Ting, Makiko Watanabe, Jon Woon, as well as participants at the political economy breakfast at Columbia University, the Northeast Workshop in Empirical Political Science, The Center for Global Development speaker series, and the Global Politics Seminar at the University of Pittsburgh. Paler would also like to thank her collaborators on the Aceh Reintegration and Livelihood Surveys (ARLS), especially Patrick Barron, Macartan Humphreys, Yuhki Tajima, and Jeremy Weinstein.

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 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 (WFP), 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 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.

[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 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, Christia and Enikolopov, 2013; Fearon, Humphreys and Weinstein, 2009).

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 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 lead 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 and other benefits (Winters, 2014; Alatas et al., 2012), 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 and Shoham (1999) 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 and Mookherjee, 2006; Alatas et al., 2013) *or* non-beneficiary capture

[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).

(Galasso and Ravallion, 2005) but rarely study both together. In doing so, they overlook the fact that elites and non-beneficiaries can be independent 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 and Mookherjee, 2006; Galasso and Ravallion, 2005; Alatas et al., 2013). Alternatively, there is a growing literature concerned with the effects of targeting aid at vulnerable populations on rebel or government-initiated violence (Wood and 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.

This paper develops and tests a theory to explain when aid targeting will both be more effective at reaching its intended beneficiaries and have consequences for social cohesion. Our main innovation is to argue that targeting creates a situation in which three groups in a community—one weak group (the target group) and two stronger groups (elites and non-beneficiaries)—bargain over how the aid should be distributed. Despite the fact that bargaining is central to resource allocation in many settings, targeting has rarely been studied through a bargaining lens, much less through the lens of three-player bargaining. Critically, however, traditional bargaining theory cannot resolve the central dilemma of aid targeting—to ensure that aid reaches a weak group—because it predicts that the stronger players will receive almost all of the benefits (Rubinstein, 1982; Baron and Ferejohn, 1989). We present a model that shows that these bargaining dynamics are fundamentally altered when the target group can form a *coalition* with one of the more powerful players. Our approach yields the counter-intuitive insight that the target group will get a bigger share of the benefits to which it is entitled—despite its own weakness—when there is competition among two other, more powerful players.

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 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.[4] We

[4]Modeling 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."

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-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.

2 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, 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.[5] While aid agencies take steps to mitigate capture and mis-targeting, they are difficult to eliminate. We thus 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.

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 (Norwegian Refugee Council (NRC), 2013; Office for the Coordination of Humanitarian Affairs (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

[5]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).

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.

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, 2004; Angeles and Neanidis, 2009; Alatas et al., 2013; 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 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 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, Whitney and Winters, 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 and Ravallion (2005) 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 and 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.

2.1 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 $\alpha = (\alpha_L, \alpha_X, \alpha_T)$.[6] 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.[7] 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 $1 - p_{XT}$

[6]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.

[7]Conceptually, 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.

the outcome is $(1, 0, 0)$.[8] Either way, the game ends after T's choice of coalition and payoffs are realized.

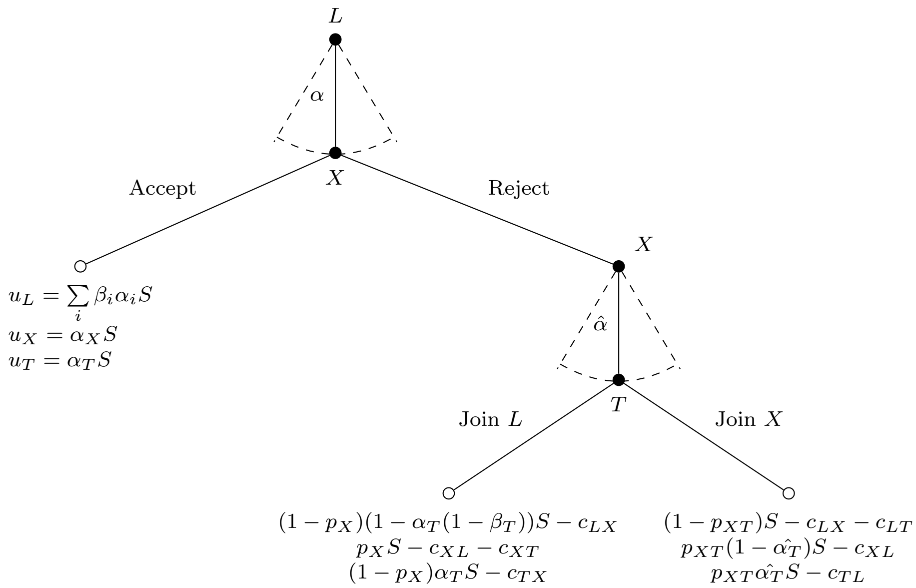

Figure 1: Extensive form of the game.

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

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

social benefits, such as trade, information-sharing, intermarriage, and social insurance. It is often argued that the better relations are, the more any 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, Gibbons and Murphy, 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 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.

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.[9] Thus, we

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

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.[10] 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.[11] 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.[12] 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

---

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.

[10]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 offering zero and a positive amount to another group, they offer zero.

[11]See Appendix A for the formal statement of this condition.

[12]This is consistent with work on the possibility of disagreement under complete information. For instance, Laengle and 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.

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 with the elite.[13]

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 $\alpha^{\mathcal{A}}_X = p_{XT} - \frac{c_{XL}}{S} + \frac{\max\{c_{TX}-c_{TL},0\}}{S}$ and $\alpha^{\mathcal{A}}_T = 0$, X and T accept, windfall is divided accordingly.*

[13] 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; $\beta_T > \frac{c_{TX}+c_{XT}-c_{TL}-c_{LT}}{(p_{XT}-p_X)S+c_{TX}+c_{XT}-c_{TL}}$.

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

Proof. In Appendix A. □

2.2 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.[14] 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

[14] We 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.

($\beta_X > \beta_X^*$) and/or has good relations with the other two groups ($c_{XL}+c_{LX}+c_{XT} \geq c^*(\beta_X, S)$ and $c_{XT} + c_{TX} > c_{TL}$).[15] 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.[16]

---

[15]When $\alpha_T > 0$, 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.

[16]When the equilibrium outcome is $\mathcal{A}$ppropriation, Ts share is always zero, regardless of the size of the windfall (left column of Figure 2). When the equilibrium outcome is $\mathcal{I}$nclusion, and Ts 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).

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.[17] Where 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 $\mathcal{I}$nclusion 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.[18]

> ***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.

---

[17] We 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.

[18] We 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.

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

2.3 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 and 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.[19] 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, 2000). 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

[19]Our model is closest in setup to Dal Bó and 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).

are in fact imbuing a weak group with out-sized power. We view the possibility of coalition formation as consistent with a large 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 and 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.[20]

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 (WFP), 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 from one case. In Section 6 we return to a discussion

[20]In 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.

of the relevance of our approach to targeted aid programs more broadly.

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 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 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.

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, Watanabe and Wrobel, 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, Watanabe and Wrobel, 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, Watanabe and Wrobel, 2009, 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, Watanabe and Wrobel, 2009, 30). BRA-KDP reports suggest that such actions by GAM generated tensions and community resentment (Morel, Watanabe and Wrobel, 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).[21]

Similarly, GAM's influence varied at the village-level both during and after the 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, Watanabe and Wrobel, 2009). GAM also established strongholds in areas where it lacked community support but that were of strategic importance, relying on

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

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, Watanabe and Wrobel, 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.

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

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 3, 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, Watanabe and Wrobel, 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 1. We use this binary coding in the main analysis because it provides the most direct test of the main model predictions. In Appendix I 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 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 E.

The PODES data also allows us to conduct a rough analysis of the factors that predict excluded group contestation threat. Appendix F 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 K.

4.2 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., 2009). 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).[22] 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 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.

4.3 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 and 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:

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

[22] 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. In Appendix D we provide a more detailed description of the assignment process and explanation as to why we do not estimate effects at other thresholds.

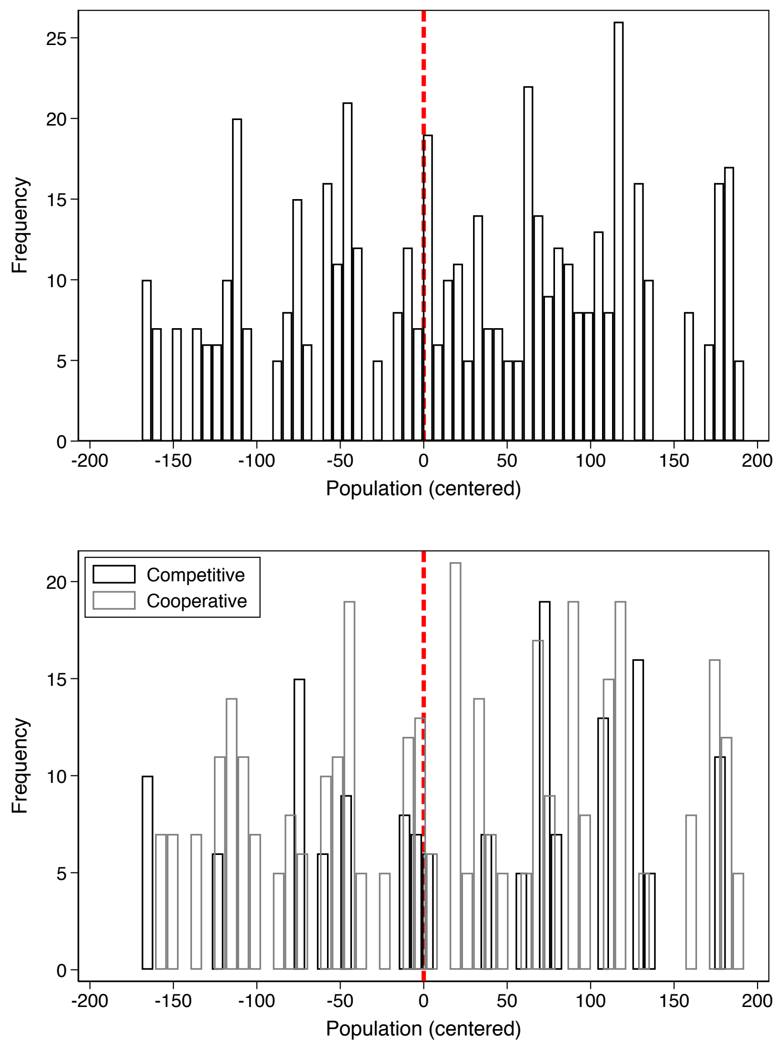

Figure 3: Distribution of individual-level observations around the population threshold centered at zero. Top panel shows the full sample; bottom panel shows the distribution in high threat and lower threat villages.

where Y_{ij} refers to the outcome for individual i in village j.[23] 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.[24] V_j is the binary indicator which equals one for

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

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

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 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.[25] The coefficient γ identifies the effect of 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.[26] 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 G 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, Cattaneo and Titiunik, 2014).

The key identifying assumption of an RDD is the continuity of potential outcomes at the threshold (Imbens and 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 G 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 G).

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

[26]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

5.1 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 E, 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 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.[27] Table 2 presents the results for the civilian subsample.[28] 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.

The table shows three main findings, also shown in Figure 4. First, looking at the final

[27] 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.

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

	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***	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
High threat (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 'high threat' 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

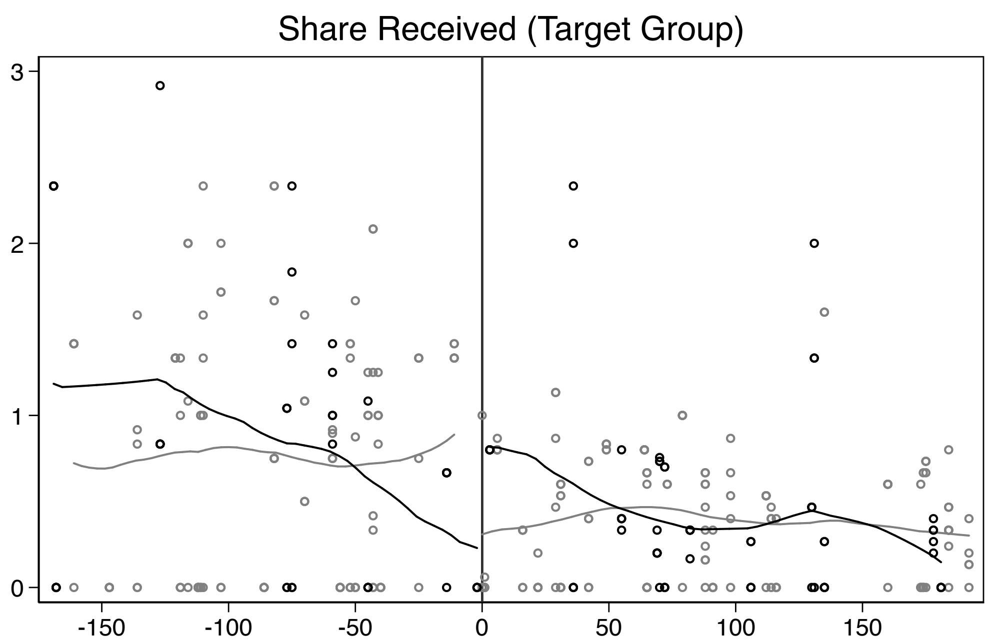

Figure 4: Local polynomial regression showing the effect of targeting a bigger aid windfall on the share received by the target group in high threat (black line) versus lower threat (gray line) villages.

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 H 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.[29]

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 benefits into an index using inverse covariance weighting.[30]

[29]There 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.

[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 * High threat	-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
High threat	-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 'high threat' 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

	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*	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
High threat	-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
Marginal effect of a bigger aid windfall in 'high threat' villages	-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)

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 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 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.

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 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.

	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	-0.37	-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
High threat	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 'higher threat' 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

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 Appendices H, G, and I). 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.

5.2 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 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 E). Yet, the findings in Panel A of Table 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.

	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	-0.38*	-0.42**	0.27	-0.19	-0.31
	(0.19)	(0.19)	(0.19)	(0.31)	(0.32)	(0.28)
	0.578	0.050	0.033	0.386	0.563	0.274
N	317	312	312	317	312	312
Panel B: Conflict resolved satisfactorily						
Bigger windfall	0.08	0.06	0.04	0.04	0.22	0.16
	(0.09)	(0.09)	(0.10)	(0.17)	(0.18)	(0.14)
	0.373	0.534	0.645	0.804	0.233	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 6: Effect of Targeting a Bigger Aid Windfall on Social Cohesion (Unconditional on Threat)

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 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 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

	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	0.08	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
High threat	-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 'high threat' 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 * High threat	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
High threat	-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 'high threat' 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 7: Effect of Targeting a Bigger Aid Windfall on Social Cohesion

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 its demands in all known cases (Morel, Watanabe and Wrobel, 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.

6 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—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, Watanabe and Wrobel, 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 2 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 and Ibanez, 2003;

Barron, Diprose and Woolcock, 2007; Angeles and Neanidis, 2009; Caeyers and Dercon, 2012; de Sardan et al., 2015; Kilic, Whitney and Winters, 2013; Zurcher, 2017). While our scope conditions rule out some targeted aid programs—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 for of aid targeting (Mansuri and 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 and Ravallion, 2005; Kilic, Whitney and Winters, 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.[31] 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, Watanabe and Wrobel, 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.

7 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

[31]Interestingly, our predictions for lower threat communities are consistent with the findings in Alatas et al. (2013), 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.

investigates how the economic and social outcomes 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 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 (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 and 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 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 and Mookherjee, 2006; Niehaus et al., 2013; Alatas et al., 2013). 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, 2000; Wright and 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.

References

Alatas, Vivi, Abhijit Banerjee, Rema Hann, Benjamin Olken and Julia Tobias. 2012. "Targeting the Poor: Evidence from a Field Experiment in Indonesia." *American Economic Review* 102(4):1206–1240.

Alatas, Vivi, Abhijit Banerjee, Rema Hanna, Ben Olken, Ririn Purnamasari and Matthew Wai-Poi. 2013. "Does Elite Capture Matter? Local Elites and Targeted Welfare Programs in Indonesia." *NBER Working Paper Series* (18798).

Angeles, Luis and Kyriakos Neanidis. 2009. "Aid effectiveness: the role of the local elite." *Journal of Development Economics* 90(120-134).

Aspinall, Edward. 2009. *Islam and Nation.* Stanford University Press.

Baker, George, Robert Gibbons and Kevin Murphy. 2002. "Relational Contracts and the Theory of the Firm." *Quarterly Journal of Economics* 117(1):39–84.

Bardhan, Pranab and Dilip Mookherjee. 2006. "Pro-poor Targeting and Accountability of Local Governments in West Bengal." *Journal of Development Economics* 79:303–327.

Baron, David P. and John A. Ferejohn. 1989. "Bargaining in Legislatures." *The American Political Science Review* 83(4):1181–1206.

Barrett, Christopher B. 2006. Food Aid's Intended and Unintended Consequences. ESA Working Paper 06-05 Food and Agriculture Organization.

Barron, Patrick, Macartan Humphreys, Laura Paler and Jeremy Weinstein. 2009. "Community-Based Reintegration in Aceh: Assessing the Impacts of BRA-KDP." *Indonesian Social Development Paper* 12.

Barron, Patrick, Rachel Diprose and Michael Woolcock. 2007. "Local Conflict and Development Projects in Indonesia." *World Bank Policy Research Working Paper* 4212.

Beath, Andrew, Fotini Christia and Ruben Enikolopov. 2013. "Empowering Women through Development Aid: Evidence from a Field Experiment in Afghanistan." *American Journal of Political Science* 107(3):540–557.

Caeyers, Bet and Stefan Dercon. 2012. "Political Connections and Social Networks in Targeted Transfer Programmes: Evidence from Rural Ethiopia." *Economic Development and Cultural Change* 60(4):639–675.

Calonico, Sebastian, Matias Cattaneo and Rocio Titiunik. 2014. "Robust data-driven inference in the regression-discontinuity design." *The Stata Journal* 14(4):909–946.

Coady, David, Margaret Grosh and John Hoddinott. 2004. "Targeting Transfers in Developing Countries: Review of Lessons and Experience." *World Bank Publications* .

Dal Bó, Ernesto and Robert Powell. 2009. "A Model of Spoils Politics." *American Journal of Political Science* 53(1):207–222.

de Sardan, Jean Pierre Olivier. 2014. "Manna, standards and suspicions." *Revue Tiers Monde* 3:197–215.

de Sardan, Jean Pierre Olivier, Oumarou Hamani, Nana Issaley, Younoussi Issa, Hannatou Adamou and Issaka Oumarou. 2015. "Cash transfers in Niger: the manna, the norms and the suspicions." *(working paper)* .

DeNardo, James. 1985. *Power in numbers : the political strategy of protest and rebellion*. Princeton, NJ: Princeton University Press.

Dietrich, Simone. 2013. "Bypass or Engage? Explaining Donor Delivery Tactics in Foreign Aid Allocation." *International Studies Quarterly* 57:698–712.

Dreze, Jean and Amartya Sen. 1989. *Hunger and Public Action*. Oxford University Press.

Duffield, Mark R. 1996. "The Symphony of the Damned: Racial Discourse, Complex Political Emergencies and Humanitarian Aid." *Disasters* 20(3):173–193.

Fearon, James, Macartan Humphreys and Jeremy Weinstein. 2009. "Can Developmen Aid Contribute to Social Cohesion after Civil War? Evidence from a Field Experiment in Post-Conflict Liberia." *American Economic Review: Papers & Proceedings* 99(2):287–291.

Galasso, Emanuela and Martin Ravallion. 2005. "Decentralized Targeting of an Antipoverty Program." *Journal of Public Economics* 89:705–727.

Harragin, Simon and Chold Changath Chol. 1998. The Southern Sudan Vulnerability Study. Technical report Save the Children UK.

Imbens, Guido and Thomas Lemieux. 2008. "Regression Discontinuity Designs: A Guide to Practice." *Journal of Econometrics* 142:615–635.

Jablonski, Ryan. 2014. "How Aid Targets Votes: Th Impact of Electoral Incentives on Foreign Aid Distribution." *World Politics* 66(2):293–330.

Jaspars, Susanne and Jeremy Shoham. 1999. "Targeting the Vulnerable: A Review of the Necessity and Feasibility of Targeting Vulnerable Households." *Disasters* 23(4):359–372.

Kilic, Talip, Edward Whitney and Paul Winters. 2013. "Decentralized Beneficiary Targeting in Large-Scale Development Programs." *World Bank Policy Research Working Paper* (6713).

Laengle, Sigifredo and Gino Loyola. 2015. "The Ultimatum Game with Externalities." *Economic Computation & Economic Cybernetics Studies & Research* 49(4).

Mansuri, Ghazala and Vijayendra Rao. 2004. "Community-Based and -Driven Development: A Critical Review." *World Bank Research Observer* 19.

Maschler, Michael. 1963. "The power of a coalition." *Management Science* 10(1):8–29.

McCrary, Justin. 2008. "Manipulation of the Running Variable in the Regression Distontinuity Design: A Density Test." *Journal of Econometrics* 142:698–714.

Morel, Adrian, Makiko Watanabe and Rob Wrobel. 2009. "Delivering Assistance to Conflict-Affected Communities." *Indonesian Social Development Paper 13* .

Niehaus, Paul, Antonia Atanassova, Marianne Bertrand and Sendhil Mullainathan. 2013. "Targeting with Agents." *American Economic Journal: Economic Policy* 5(1):206–238.

Norwegian Refugee Council (NRC). 2013. "Mission statement and principles.".
URL: *http://www.nrc.no/globalassets/pdf/corporate/nrc-policy-paper-web.pdf*

Office for the Coordination of Humanitarian Affairs (OCHA). 2014. Humanitarian Needs Overview. Technical report United Nations Office for the Coordination of Humanitarian Affairs.

Paul, Elisabeth. 2006. "A Survey of the Theoretical Economic Literature on Foreign Aid." *Asian-Pacific Economic Literature* 20(1):1–17.

Platteau, Jean-Philippe. 2004. "Monitoring Elite Capture in Community-Driven Development." *Development and Change* 32(2):233–246.

Polachek, Solomon. 1980. "Conflict and Trade." *Journal of Conflict Resolution* 24(1):55–78.

Rao, Vijayendra and Ana Maria Ibanez. 2003. "The Social Impact of Social Funds in Jamaica: A Mixed-Methods Analysis of Participation, Targeting, and Collective Action in Community-Driven Development." *World Bank Policy Research Working Paper* 2970.

Ross, Michael. 2013. The Politics of the Resource Curse: A Review. In *Handbook on the Politics of Development*.

Rubinstein, Ariel. 1982. "Perfect equilibrium in a bargaining model." *Econometrica* pp. 97–109.

Schulze, Kirsten. 2004. The Free Aceh Movement (GAM): Anatomy of a Separatist Organization. Technical report East West Center.

Smith, Alastair and Bruce Bueno De Mesquita. 2012. "Contingent prize allocation and pivotal voting." *British Journal of Political Science* 42(2):371–392.

Svensson, Jakob. 2000. "Foreign Aid and Rent Seeking." *Journal of Internationl Economics* 51:437–471.

Tajima, Yuhki. 2018. "When do Communities Accept or Aid Former Combatants?" Working Paper.

United Nations Childrens Fund (UNICEF). 2005. Emergency Field Handbook. Tools, guidelines and methodologies United Nations Children's Fund Global: .
URL: *http://www.alnap.org/resource/8173*

Wahlberg, Katerina. 2008. "Food aid for the hungry.".
URL: *https://www.globalpolicy.org/world-hunger/46251-food-aid-for-the-hungry.html*

Winters, Matthew. 2014. "Targeting, Accountability, and Capture in Development Projects." *International Studies Quarterly* 58:393–404.

Wong, Susan. 2012. What Have Been The Impacts of the World Bank Community-Driven Development Programs: CDD Impact Evaluation Review and Research Implications. Technical report World Bank.

Wood, Reed and Christopher Sullivan. 2015. "Doing Harm by Doing Good? The Negative Externalities of Humanitarian Aid Provision during Civil Conflict." *Journal of Politics* 77(3):736–748.

World Food Programme (WFP). 2005. WFP's activities: Principles and NGO Involvement. Handbook 3 World Food Programme.
URL: *https://documents.wfp.org/stellent/groups/public/documents/manualguide/wfp085024.pdf*

World Food Programme (WFP). 2011. WFP Management Plan 2012-2014: Executive Summary. Follow-up briefing World Food Program.

Wright, Joseph and Matthew Winters. 2010. "The Politics of Effective Foreign Aid." *Annual Review of Political Science* 13:61–80.

Zurcher, Christoph. 2017. "What Do We (Not) Know About Development Aid and Violence: A Systematic Review." *World Development* 98:506–522.