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

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Abstract

A central challenge in development involves ensuring that 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. Our approach yields the counter-intuitive insight that the target group will actually benefit more in communities where elites and the excluded group compete to capture aid. We provide support for predictions using a regression discontinuity design and original survey data from an aid program implemented in Aceh, Indonesia. This article demonstrates the importance of understanding the role of community dynamics in shaping the economic and social outcomes of targeted aid programs.

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Introduction

One of the central challenges in development involves ensuring that aid—whether provided by international or domestic, governmental or nongovernmental actors—reaches those in greatest need. 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 humanitarian and development assistance are channeled through targeted aid programs to individuals, households, or groups. More than 85% of the aid intended for individuals now takes the form of targeted distributions of divisible goods like money and food (Barrett, 2006; Wahlberg, 2008). For instance, the World Bank has supported approximately 400 cash transfer projects targeting the poor in 94 countries valued at almost US$30 billion (Wong, 2012). In recent years, the World Food Programme (WFP) has targeted 54% of 4.4 million metric tons of food aid to vulnerable populations (WFP, 2011).

Despite the prevalence of aid targeting, its economic and social consequences have received relatively little attention in the literature.[1] The main goal of this article is to examine what happens after targeted aid arrives in a community and, especially, when it will reach those for whom it is intended when some individuals in a community are eligible to receive assistance and others are not.

In doing so, we argue that understanding the consequences of targeting aid depends on examining dynamics within the communities in which intended beneficiaries live. Communities play a role in almost all targeted aid programs because successful 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, & Hoddinott, 2004). Even in settings where aid agencies identify beneficiaries through more 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 (Alatas, Banerjee, Hann, Olken, & Tobias, 2012; Winters, 2014), it can also have unwelcome consequences such as elite

capture, nonbeneficiary capture, and heightened social divisions. One Oxfam program designed 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 mistargeting occurred. In South Sudan, there was both extensive elite capture and communal fighting over the aid, resulting in enduring local tensions.

Existing studies on targeting within communities have limited ability to explain such variation. For one, they often study either elite capture (Alatas et al., 2013; Bardhan & Mookherjee, 2006) *or* nonbeneficiary capture (Galasso & Ravallion, 2005), rarely considering both together. In doing so, they overlook the fact that elites and nonbeneficiaries 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 tend to focus on either economic *or* social outcomes but not on how these relate. For instance, research on targeting in nonconflict settings primarily examines economic welfare or capture, with little attention to impacts on social cohesion within recipient communities (Alatas et al., 2013; Bardhan & Mookherjee, 2006; Galasso & Ravallion, 2005). Alternatively, studies on targeting in conflict settings investigate consequences for violence between groups but not the conditions under which more aid reaches those who are targeted (Wood & Sullivan, 2015; Zurcher, 2017).

This article develops and tests a theory to explain when aid targeting will both be more effective at reaching its intended beneficiaries and impact 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 nonbeneficiaries)—bargain over the distribution of aid. Despite the fact that bargaining is central to resource allocation in many settings, targeting has rarely been studied through such a 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 stronger players will receive almost all of the benefits (Baron & Ferejohn, 1989; Rubinstein, 1982). We present a model that shows how these bargaining dynamics are altered when the target group can form a *coalition* with one of the more powerful players. Our approach yields the counterintuitive 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 nonbeneficiaries (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 (where better relations equal higher 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 when it is both influential *and* has bad relations with other groups. It is in precisely such "high threat" communities—communities with a high threat of excluded group contestation—that elites offer the target group more 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. It also underscores the sobering fact that it is hard to improve targeting without also increasing mistargeting: 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 note 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 as aid agencies—which typically operate under a "do no harm" principle—hope that their efforts to improve economic outcomes do not come 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 nonconflict settings. We test predictions in the context of one postconflict community-driven development (CDD) project implemented in the Indonesian province of Aceh. The *Badan Reintegrasi-Damai Aceh, Kecamatan Development Program* (BRA-KDP) program 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 (RDD) 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 (smaller) share of the benefits in communities with a high (lower) threat of *excluded* group contestation. We also show that bigger aid windfalls resulted in the excluded group getting more, and elites less, in high threat communities. While our findings on social cohesion are more suggestive, they indicate that bigger windfalls reduced acceptance of former GAM combatants overall but improved conflict resolution in high threat villages. 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 actually have yielded social benefits.

This article makes several contributions to research on aid effectiveness in conflict and nonconflict settings. First, it sheds 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 nonbeneficiaries 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.

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: (a) communities can influence distributional outcomes; (b) there is a target group that is vulnerable; (c) elites have some authority over distributions and can try to capture aid for themselves; and (d) there are other community members who should be excluded from receiving benefits but who can also try to capture a share of the aid. Recognizing that targeted aid programs create *three* players—the target group, the elites, and the excluded group—that can influence distributional outcomes is the main innovation of our approach. Before turning to the details of the model, we explain these assumptions and characterize the players.

First, we assume that communities can influence the distributional outcomes of targeted aid programs. In some cases, aid agencies opt for community-based targeting approaches, knowingly relinquishing some control in exchange for a process that is more sensitive to local context and information (Coady et al., 2004). In other cases, aid agencies face logistical constraints that lead them to rely (at least to some extent) on community assistance, with confirming lists of beneficiaries or managing distributions, for instance. 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] Although aid agencies take steps to mitigate capture and mistargeting, they are difficult to eliminate. We thus follow on Galasso and Ravallion (2005) in assuming that the aid agency has imperfect control over 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 (de Sardan et al., 2015; Norwegian Refugee Council, 2013; Office for the Coordination of Humanitarian Affairs, 2014). Vulnerable groups are targeted precisely because they are often the most in need and at risk of otherwise being marginalized from resource allocation. While targeting can help to empower recipients (Winters, 2014), we follow on existing research that suggests it is unlikely to erase existing power asymmetries within the community (Bardhan & Mookherjee, 2006; Dreze & Sen, 1989; Galasso & Ravallion, 2005). Indeed, what is unique about targeting—and what differentiates it from other distributive contexts—is that it makes a weak group a relevant player despite its lack of formal bargaining strength. We reflect the weakness of the target group by modeling it as a player that has relatively little influence within the community.

Our third assumption is that elites, as individuals with formal political authority, are often in a position to influence how aid is allocated and to capture a share for themselves. When aid agencies involve communities in targeting, they typically turn first to local 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 (Alatas et al., 2013; Angeles & Neanidis, 2009; Bardhan & Mookherjee, 2006; Platteau, 2004). Dreze and Sen (1989) 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. (p. 107)

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 nonbeneficiaries, or individuals who live in the community but who do not meet the eligibility criteria and therefore should not receive benefits (Duffield, 1996). We refer to all such nonbeneficiaries as members of the "excluded group." Who comprises this excluded group depends on the nature of the program, but could be the nonpoor 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 nonbeneficiaries also often intervene to try to expropriate a share of the resources for themselves (de Sardan et al., 2015; Kilic, Whitney, & Winters, 2013). For instance, in one cash transfer program in Niger, nonbeneficiaries 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 rich nonbeneficiaries were more likely to try to capture aid intended for the poor in villages with higher income inequality. Wood and Sullivan (2015) show that in conflict settings, rebel groups often aim to appropriate aid targeted at vulnerable civilian populations. Importantly, although the problem of nonbeneficiary capture is well recognized, much of the literature—especially that on nonconflict 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 three different groups within a community. 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 preexisting influence within the community and on the quality of its relations with other groups. It is the interaction of bigger windfalls and these features of local context that make excluded group contestation more likely, which in turn drives elites to make the target group a better offer.

Model

We model aid distribution as a bargaining game between the elite 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 a counter-offer $\hat{\alpha} = (\hat{\alpha}_L, \hat{\alpha}_X, \hat{\alpha}_T)$. 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

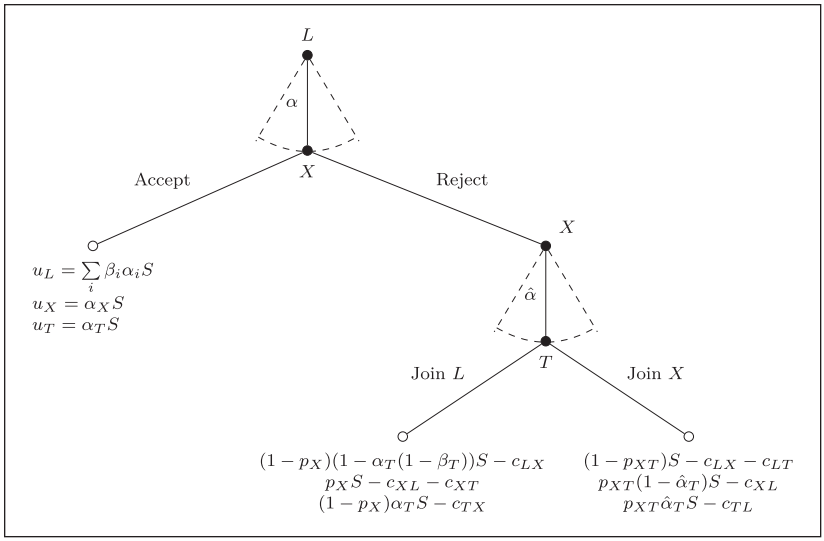

Figure 1. Extensive form of the game.

with probability p_X, the excluded group wins and gets the whole windfall, $(0,1,0)$. Similarly, if T sides with X, they win with probability p_{XT} and the outcome is $(0,1-\hat{\alpha}_T,\hat{\alpha}_T)$, and with probability $1-p_{XT}$, the outcome is $(1,0,0)$.[8] Either way, the game ends after *T*'s choice of coalition and payoffs are realized.

Each player derives utility from the windfall amount 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 elite care not only about short-run benefits from the windfall but also their relative power in the long run. The elite's 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 among the groups. Better relations bring economic and social benefits, such as trade, information-sharing, intermarriage, and social insurance. It is often argued that the better relations are, the more any 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 (e.g., Baker et al., 2002). We follow on this logic to assume

that, ceteris paribus, better relations make contestation *less* likely. We capture the costs of contestation as a loss of above-mentioned benefits, supposing that each group i pays a cost $c_{ij} > 0$ for all groups j they face off against during contestation. Thus, groups that have good relations with the rest of the community will face higher costs of contestation.

A second feature of community interactions pertains to the influence of different groups in the community, particularly whether groups are weak or strong. By influence, we refer to attributes including but not limited to group size or access to resources that improve a group's abilities to affect outcomes. To understand how variation in group influence 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 assigned to the share of group i (Galasso & Ravallion, 2005). We fix the weight the elite assign to their own share to one, $\beta_L = 1$. We assume that the elite 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 on keeping others happy or because they want to comply 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 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 (as 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 (as 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, the elite 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 Supplemental Appendix A such that when $c_{XL} + c_{LX} + c_{XT} \leq c^*(\beta_X, S)$, there is no possibility to find a negotiated solution. The intuition behind unavoidable contestation is straightforward: when the excluded group is very influential *and* has

bad relations with other groups, the elite's concerns about empowering them overcome their incentives to maintain good relations. In this case, the elite 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 relations are not bad enough for it to be inevitable. We now focus on this intermediate situation and look at how different parameters affect the target group's 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 Appropriation 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 Inclusion case and denote $\alpha^{\mathcal{I}}$).

Whether the elite 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 $\beta_X^* = (\beta_T - p_X)/(1 - p_X)$ such that when the excluded group's influence is relatively high ($\beta_X < \beta_X^*$), the elite offers $\alpha^{\mathcal{I}}$, and otherwise offers $\alpha^{\mathcal{A}}$. This is because, when β_X is low (excluded group influence is high), the elite's incentives to withhold the windfall from a very influential X become stronger; so much so that they are willing to take a smaller share themselves. The intermediate equilibrium outcome is summarized in the following proposition:

Proposition 1.
($\mathcal{A}$) When $\beta_X \geq \beta_X^*$, *L offers* $\alpha_X^{\mathcal{A}} = p_{XT} - \frac{c_{XL}}{S} + \frac{\max\{c_{TX} - c_{TL}, 0\}}{S}$ *and* $\alpha_T^{\mathcal{A}} = 0$, *$X$ and T accept, windfall is divided accordingly.*
($\mathcal{I}$) When $\beta_X < \beta_X^*$, *L offers* $\alpha_X^{\mathcal{I}} = p_X - \frac{c_{XL} + c_{XT}}{S}$ *and* $\alpha_T^{\mathcal{I}} = \frac{p_{XT} - p_X}{1 - p_X} + \frac{c_{TX} + c_{XT} - c_{TL}}{(1 - p_X)S}$, *$X$ and T accept, windfall is divided accordingly.*
Proof. In Supplemental Appendix A.

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

	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.

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.

Our main prediction is that what the target group receives differs in "high threat" communities—where the excluded group is both influential ($\beta_X < \beta_X^*$) *and* has bad relations with other groups—and in "lower threat" communities—where the excluded group is not influential ($\beta_X > \beta_X^*$) and/or has good relations with the other two 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 elite's 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, the elite have a greater incentive to offer the target group a bigger share of the aid 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), the elite lack such incentives and the share received by the target group is (weakly) decreasing in those contexts.[16]

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 elite want to avoid contestation in equilibrium, bigger windfalls mean that they must now offer the excluded group a bigger share. Specifically, in an Inclusion equilibrium, the elite 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. Because the amount L can extract is equal to that cost, it follows that the share they can keep for themselves decreases in S. 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, they could also bring a general loss in social cohesion. Given that aid agencies often hope their programs will also enhance—or at least not undermine—social cohesion, contestation is an unwelcome outcome that merits investigation.

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

Discussion of the Model

Showing that competition between two stronger players can have distributive benefits for a weak player is counterintuitive from the perspective of canonical bargaining models, which predict that bargaining between weak and

strong players will result in the latter getting almost all of the benefits (Baron & Ferejohn, 1989; Rubinstein, 1982). 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 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). Although 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, consistent with the notion that targeting can be empowering (Winters, 2014). A related concern might be that, by allowing T to join a coalition, we are in fact giving a weak group outsized 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), being pivotal in their support for one party over another (Smith & De Mesquita, 2012), or 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. Practically speaking, it is common in targeted aid programs for donors to publicize beneficiary criteria and the aid amount, making incomplete information less of a concern (United Nations Children's Fund, 2005; WFP, 2005). More importantly, a key contribution of our model is to demonstrate how community dynamics affect targeting and capture *even in situations of complete information*. Although we could get similar predictions using information asymmetries, we show that the dynamics described do not depend on uncertainty or information advantages among players and that they would not be solved simply by increasing transparency in the targeting process.

Finally, we emphasize that the model is relatively general and 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 CDD, conditional cash transfer, and humanitarian aid programs—in both conflict and nonconflict settings. In what follows, we provide empirical support for the model's predictions based on evidence from one case. In the "Alternative Explanations and External Validity" section, we return to a discussion of the relevance of our approach to targeted aid programs more broadly.

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 US$6,000-US$17,000) to more than 1,700 villages. The program also sought to target those funds to civilian conflict victims.

To identify civilian 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 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, & Wrobel, 2009, p. 19). Following the determination of eligible beneficiaries, villagers developed proposals that were then voted on at community meetings. Communities had discretion over how to allocate funds but were instructed to prioritize proposals submitted by the most conflict affected.

Elites played a distributional role in BRA-KDP, despite the program's efforts to minimize the possibility of elite capture by using external facilitators for implementation. Nevertheless, anecdotal evidence suggests that village elites still managed to influence the decision-making process. As one villager stated with respect to BRA-KDP community meetings,

"Meetings are normally attended only by village authorities. Hamlet heads, religious figures, community leaders and village government officials attend." And, according to another, "It is always a group of people who are close to the village authorities that monopolize the benefits" (Morel et al., 2009, p. 27).

By targeting civilian conflict victims, BRA-KDP also created an excluded group of nonbeneficiaries consisting of former GAM combatants. Although ex-combatants were not supposed to benefit directly from the program, in many villages, they felt entitled to receive some of the aid. In the words of one former commander:

> Everyone should understand that returning GAM are heroes. We should receive money. There are 1,000 combatants here . . . and there's potential for them to conduct criminal acts if BRA-KDP doesn't target them. GAM are conflict-affected people as well and therefore we should also get money. (Morel et al., 2009, p. 28)

BRA-KDP personnel documented numerous instances in which GAM threatened or took actions to try to appropriate a share of the funds. These actions included extortion, theft, protest, threats and demands, and, in rare instances, physical intimidation (Morel et al., 2009, pp. 27-33). These methods are consistent with how, during the conflict, GAM often demanded that villages pay "taxes" to finance its operations (Aspinall, 2009). As stated by one villager:

> There is a rumor here that GAM have requested 20 percent of the [BRA-KDP] project funds. I think the money should go to them first, not the community. Because once they have received something, the process will go more smoothly. (Morel et al., 2009, p. 30)

BRA-KDP reports suggest that such actions by GAM generated tensions and community resentment (Morel et al., 2009).

Although 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 differ in the extent to which they supported GAM during the conflict, with implications for the quality of relations postconflict. For much of the conflict, GAM enjoyed relatively high levels of community

support in eastern 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 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 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 (Aspinall, 2009; Schulze, 2004), although even then its influence within the community varied depending on factors such as the strength of other forms of local authority (Morel et al., 2009). GAM also established strongholds in areas where it lacked community support but that were of strategic importance, relying on 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 postconflict period (Morel et al., 2009). In the next section, we explain how we use our data to capture such village-level variation in both GAM strength and the quality of its relations with others in the community, which in turn determine whether GAM posed a high or low threat of contestation to targeted aid in the postconflict period.

Empirical Strategy

The Data

Our main data come from original 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. Respondents were selected through multistage cluster sampling in which villages were first sampled within strata and then civilians and ex-combatants were randomly sampled within villages (see Supplemental Appendix B for details on the sampling strategy). Question wording for all survey questions used in the analysis can be found in Supplemental 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 above, we proxy for GAM influence using a question about whether a village was a GAM stronghold ("basis GAM") during that period. In doing so, we draw on the qualitative evidence that ex-combatants remained more influential in communities where they also had a stronger presence during the conflict (Morel et al., 2009). We proxy for the nature of community relations with a survey question that inquired into whether the majority of villagers actually supported GAM during this period. 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 (see Table 1). We use this binary coding in the main analysis because it provides the most direct test of the main model predictions shown in Figure 2. In Supplemental Appendix I, we show that the empirical results follow the predictions when we disaggregate this measure into its component parts.

Controls. Although we have exogenous variation on windfall size (described next), the threat of excluded group contestation is not exogenous. There could in fact be numerous confounding factors, so to address concerns about omitted variable bias, we employ a rich set of pretreatment 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 Supplemental Appendix E.

The PODES data also allow us to conduct a rough analysis of the factors that predict excluded group contestation threat. Supplemental 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

Table 1. Measure of Village-Level Threat of Excluded Group Contestation.

		Village was a GAM stronghold (2001-2005)	
		No	*Yes*
Majority of village supported GAM during the conflict (2001-2005)	*Yes*	$j = 11$ $i = 75$ Lower threat = 0	$j = 18$ $i = 134$ Lower threat = 0
	No	$j = 24$ $i = 145$ Lower threat = 0	$j = 22$ $i = 150$ High threat = 1

The table shows the overlapping 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.
GAM = Gerakan Aceh Merdeka.

head time in office (which could proxy for elite strength). These correlations help to confirm the validity of our threat measure. Although we do not present regressions displaying controls in the main text, these results are available in Supplemental Appendix K.

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 subdistricts to participate in BRA-KDP, with all villages in those subdistricts guaranteed some amount of aid (Barron, Humphreys, Paler, & Weinstein, 2009). BRA-KDP used two measures to determine aid amounts at the village level. First, it used a continuous measure of *subdistrict* conflict intensity and employed arbitrary cutoffs to categorize subdistricts 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.

Although the BRA-KDP assignment process in fact created multiple thresholds, the analysis in this article focuses on the one for which we have a sufficiently large sample near the threshold and which passes the McCrary (2008) density test (see Appendix G).[22] Specifically, we focus our analysis on the cutoff between small and medium-sized villages in high conflict-affected subdistricts. All villages with 0 to 299 people received 120 million rupiah (about US$12,000) while all villages with 300 to 599 people received 150 million rupiah (about US$15,000)—an increase of 30 million rupiah (about US$3,000) at the cutoff of 300 persons. This is equivalent to an increase in 100,000 rupiah (US$10) per capita, or 560,000 rupiah (US$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.

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 design (Imbens & Lemieux, 2008). Our main empirical goal is to estimate the effect of an increase in windfall size on aid allocations in high threat and lower threat villages. To do this, we estimate weighted least squares regressions of the following form:

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

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 high threat villages and zero for lower threat villages and $\tilde{P}_j$ is the continuous measure of population centered at 300. 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. 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] In our results below, we also present the estimated marginal effect of a bigger windfall in high threat communities. All regressions include X'_{jm}, the vector of m village-level controls obtained from the PODES 2000 data.

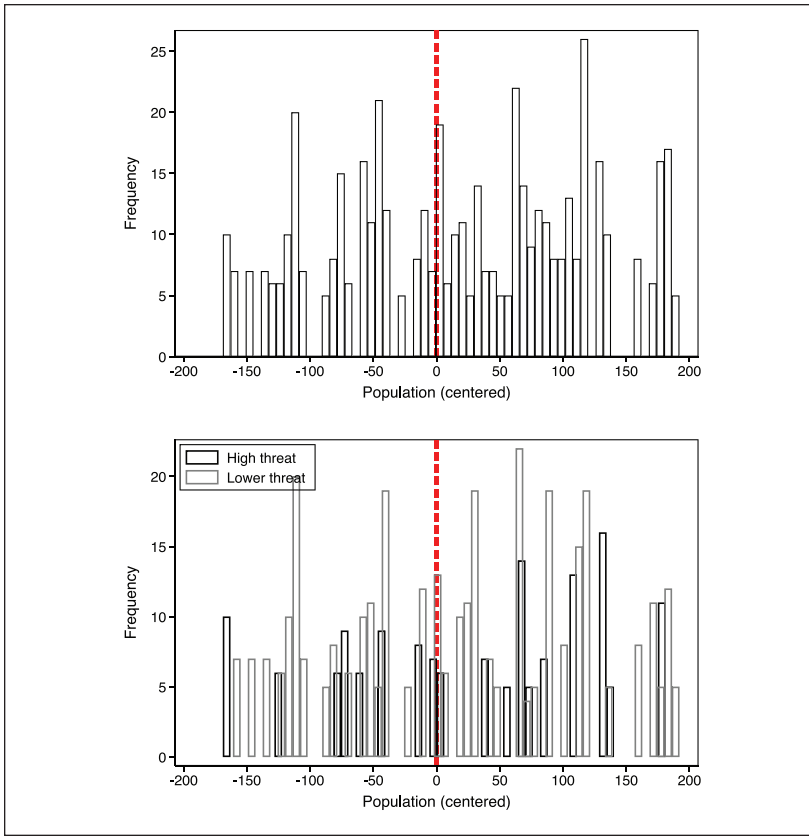

Figure 3. Distribution of individual-level observations around the population threshold centered at zero.
Top panel shows the full sample; bottom panel shows the distribution in high threat and lower threat villages.

One central concern with regression discontinuity designs is the choice of bandwidth. All main analyses presented in this article employ a bandwidth of ±150, which restricts our analysis to 63 villages. In Supplemental 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, & Titiunik, 2014).

The key identifying assumption of an regression discontinuity design (RDD) is the continuity of potential outcomes at the threshold (Imbens &

Lemieux, 2008). Following the literature, we check this assumption by testing for discontinuities in our m pretreatment village-level controls and our measures of excluded group threat at the threshold. The results, presented in Supplemental 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 their population scores. To check this, we implement a McCrary density test and find no evidence of sorting (see Supplemental Appendix G).

Results

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 Supplemental Appendix E, show that about 69% of civilian (victim) households and 58% of former combatants received some assistance from BRA-KDP, with the average amount totaling about 630,000 rupiah (about US$63) for each group. The overwhelming majority of funds were used for private goods, with about 95% of all recipients reporting that they primarily received goods in the form of cash that was then put toward livelihood activities (Barron et al., 2009; Morel et al., 2009).

Our first hypothesis is that, as the amount of aid increases, the target group will obtain a greater share of the benefits in villages with a high threat of excluded group contestation. To test the prediction, we divide the total amount (in monetary terms) of goods that a respondent reported receiving by the size of the village's aid windfall to obtain the 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 pretreatment controls and district fixed effects, for our preferred bandwidth of ± 150.

The table shows three main findings, also depicted in Figure 4. First, looking at the final row of the table, there is strong evidence that 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 0.5 to 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

Table 2. Effect of Targeting a Bigger Aid Windfall on Target Group Benefits.

	DV: Per capita windfall share for target group members					
	Linear spline			Quadratic spline		
	(1)	(2)	(3)	(4)	(5)	(6)
	No controls	Controls	Controls + district fixed effects	No controls	Controls	Controls + district fixed effects
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)
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)
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)
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)
n	317	312	312	317	312	312
Band	150	150	150	150	150	150

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. DV = dependent variable. *$p < .10$. **$p < .05$. ***$p < .01$ based on a two-tailed test.

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. Analysis in Supplemental 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 (US$128-US$251) more than their counterparts in lower threat communities.[29]

Our second hypothesis is that the excluded group receives 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]

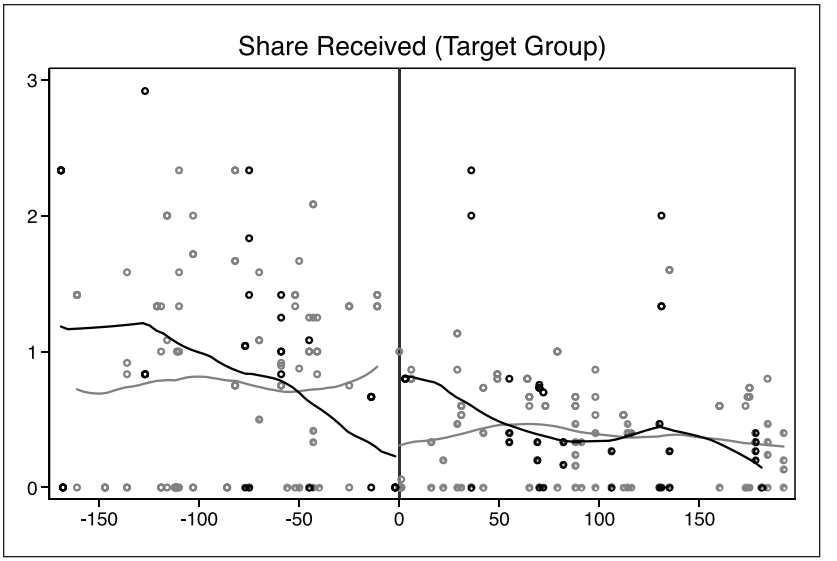

Figure 4. Local polynomial regression showing the effect of targeting a bigger aid windfall on the share received by the target group in high threat (black line) versus lower threat (gray line) villages.

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% confidence level in four of the six main specifications, suggesting that former combatants indeed received 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. Although the ex-combatant sample is small (n = 117 in the $\pm$150 bandwidth) and more susceptible to false positives, the findings nonetheless are consistent with the perceptions results and support the second hypothesis.

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 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. Although 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.

Table 3. Effect of Targeting a Bigger Aid Windfall on Perceived Excluded Group Benefits.

	DV: Perceived benefits for excluded group					
	Linear spline			Quadratic spline		
	(1)	(2)	(3)	(4)	(5)	(6)
	No controls	Controls	Controls + district fixed effects	No controls	Controls	Controls + district fixed effects
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)
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)
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)
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)
n	315	310	310	315	310	310
Band	150	150	150	150	150	150

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. DV = dependent variable. *$p < .10$. **$p < .05$. ***$p < .01$ based on a two-tailed test.

All in all, the results thus far are generally consistent with the main predictions of the model in showing that the target group received 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 Supplemental 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.

Contestation and Social Cohesion

While the evidence so far shows that targeting bigger aid windfalls results 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

Table 4. Effect of Targeting a Bigger Aid Windfall on Excluded Group Benefits (Ex-Combatant Sample).

	DV: Per capita windfall share for ex-combatants					
	Linear spline			Quadratic spline		
	(1)	(2)	(3)	(4)	(5)	(6)
	No controls	Controls	Controls + district fixed effects	No controls	Controls	Controls + district fixed effects
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)
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)
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)
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)
n	117	117	117	117	117	117
Band	150	150	150	150	150	150

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. DV = dependent variable.
*$p < .10$. **$p < .05$. ***$p < .01$ based on a two-tailed test.

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 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 Supplemental 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 least at the 90% confidence level. There is little indication of any significant effects for our measure of conflict resolution in Panel B.

In addition, we explore whether bigger windfalls resulted in more contestation in high versus lower threat villages. While the model does not yield the

Table 5. Effect of Targeting a Bigger Aid Windfall on Perceived Elite Benefits.

	DV: Perceived benefits for elites					
	Linear spline			Quadratic spline		
	(1)	(2)	(3)	(4)	(5)	(6)
	No controls	Controls	Controls + district fixed effects	No controls	Controls	Controls + district fixed effects
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)
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)
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)
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)
n	312	307	307	312	307	307
Band	150	150	150	150	150	150

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. DV = dependent variable. *$p < .10$. **$p < .05$. ***$p < .01$ based on a two-tailed test.

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 indicate that a bigger aid windfall had a *positive* effect on perceptions of conflict resolution in high threat villages.

We interpret this result as consistent with a story in which distributive outcomes in high threat villages were due to the *threat* of excluded group contestation rather than contestation itself. Moreover, the findings suggest that distributions to GAM might have even helped to serve as a form of conflict resolution. In other words, there could have been a number of communities that were on the brink of contestation but that just managed—through their own efforts or with assistance from the program implementers—to reach a solution that appeased the excluded group, helping to ameliorate tensions and create a stronger impression of satisfactory conflict resolution. This is

Table 6. Effect of Targeting a Bigger Aid Windfall on Social Cohesion (Unconditional on Threat).

	Linear spline			Quadratic spline		
	(1)	(2)	(3)	(4)	(5)	(6)
	No controls	Controls	Controls + district fixed effects	No controls	Controls	Controls + district fixed effects
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)
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)
n	313	308	308	313	308	308
Band	150	150	150	150	150	150

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.
*$p < .10$. **$p < .05$. ***$p < .01$ based on a two-tailed test.

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 subdistricts, such intervention led GAM to withdraw its demands in all known cases (Morel et al., 2009, p. 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, although 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.

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, in such villages, both the target and excluded group benefitted more. We also find suggestive

Table 7. Effect of Targeting a Bigger Aid Windfall on Social Cohesion.

	Linear spline			Quadratic spline		
	(1)	(2)	(3)	(4)	(5)	(6)
	No controls	Controls	Controls + district fixed effects	No controls	Controls	Controls + district fixed effects
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)
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)
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)
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)
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)
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)
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)
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)
n	313	308	308	313	308	308
Band	150	150	150	150	150	150

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.
*$p < .10$. **$p < .05$. ***$p < .01$ based on a two-tailed test.

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 facilitated 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 that 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—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 households, akin to threatening contestation on behalf of both the target group and itself. While community members acquiesced to avoid tension, such actions by the excluded group produced lingering resentment (Morel et al., 2009, p. 19). This pattern is consistent with evidence from other contexts that nonbeneficiaries 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 might be that the theory and evidence presented here are only relevant to our immediate empirical context of Aceh. We believe that the theory can in fact shed light on targeting outcomes in a wide variety of settings. We show in the "Theory" section that the assumptions underpinning 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 formally or informally influence distributions and capture aid, and that nonbeneficiaries try to obtain a share for themselves in targeted aid programs (e.g., Angeles & Neanidis, 2009; Barron, Diprose, & Woolcock, 2007; Caeyers & Dercon, 2012; de Sardan et al., 2015; Kilic et al., 2013; Rao & Ibanez, 2003; Zurcher, 2017). Although 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 CDD, we do not believe our approach is limited to CDD. Although CDD is a common form of aid delivery (Mansuri & Rao, 2004) and thus important to understand in its own right, we expect that the dynamics observed here could play out in any context in which community members have means—whether through informal or formal, peaceful or violent channels—to challenge elite decision making. This builds on the observation that similar dynamics to those modeled here have 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 nonconflict settings meet our scope conditions and are prone to elite capture, nonbeneficiary capture, and heightened social divisions (de Sardan

et al., 2015; Galasso & Ravallion, 2005; Kilic et al., 2013). Importantly, one of the benefits of the model is that it provides a framework for thinking about how our empirical findings might generalize to different empirical 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 nonconflict settings—the model predicts less effective aid targeting and more elite and/or excluded group capture.[31] All in all, although 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 nonconflict contexts alike.

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

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, nonbeneficiary capture, and 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 article 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 nonbeneficiaries 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 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: When is the target group—as a weak group—ever going to get more of the benefits to which it is entitled in the presence of more powerful actors who might seek to appropriate benefits for themselves? Existing answers to this question tend to focus on norms of generosity (Harragin & Chol, 1998), the monitoring and enforcement abilities of aid agencies (Dietrich, 2013; Paul, 2006), or the notion that aid empowers the target group and enables it to hold elites or aid agencies accountable (Winters, 2014). Although 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 targeting effectiveness 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 article 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 but that has not yet fully theorized the strategic dynamics (Wood & Sullivan, 2015; Zurcher 2017). As Zurcher (2017, p. 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 article 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 nonconflict settings. Much of the existing literature on elite or nonbeneficiary capture in such environments produces mixed results (Alatas et al., 2013; Bardhan & Mookherjee, 2006; Niehaus, Atanassova, Bertrand, & Mullainathan, 2013). This article 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 nonbeneficiaries—will capture more. It also highlights the need to consider the social consequences of targeted aid programs in nonconflict settings as it is still possible for relations to deteriorate even in places without a history of violence.

Finally, this article sheds light on how windfall size affects economic and social outcomes within communities. Although 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 (Ross, 2018; Svensson, 2000; Wright & Winters, 2010; 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 also increasing the risk of social conflict more broadly. All in all, the theory and evidence presented here underscore the importance of appreciating that targeted aid windfalls can induce distributional conflict among different groups within a community and that it is ultimately the nature of group dynamics that drives the outcome of that process.

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Supplemental Material

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Notes

1. For one review of the aid literature, see Wright and Winters (2010). 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 not on the effects of targeting *per se* (see, for example, Beath, Christia, & Enikolopov, 2013; Fearon, Humphreys, & Weinstein, 2009). For exceptions, see Alatas, Banerjee, Hann, Olken, and Tobias (2012); Jablonski (2014); and Winters (2014).

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, & Hoddinott, 2004).
3. For a review of different approaches to targeting, see Coady et al. (2004).
4. Modeling contestation as worsened relations accords with anecdotal reports of heightened social divisions. For instance, de Sardan notes with respect to a program in Niger: "Cash transfers are not the devil . . . They are sharpening conflicts that are already there." https://www.thenewhumanitarian.org/feature/2014/12/02/cash-transfers-good-people-bad-community.
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).
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 nonconflict settings.
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 to depend on factors such as group size, wealth, or access to means of coercion.
9. An interesting extension of this model would be to look at a repeated version of this game where aid received in previous periods change the influence of groups in later periods. Although a complete analysis of a repeated bargaining game is beyond the scope of this article, 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 Supplemental 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.

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; $\beta_T > \frac{c_{TX} + c_{XT} - c_{TL} - c_{LT}}{(p_{XT} - p_X)S + c_{TX} + c_{XT} - c_{TL}}$.
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.
15. When $\alpha_T > 0$, whether T's share is increasing or decreasing in windfall size ($\partial\alpha_T / \partial S$) 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 Appropriation, T's share is always zero, regardless of the size of the windfall (left column of Figure 2). When the equilibrium outcome is Inclusion, and T's relations with L are better than their relations with X ($c_{XT} + c_{TX} \leq c_{TL}$), the surplus L must offer T to keep them from forming a coalition with X shrinks in relative terms (upper right quadrant).
17. We focus on the predictions for high threat communities to understand the trade-off between effective targeting and capture. For the full set of predictions for excluded group and elite capture, see Supplemental Appendix A.
18. We also show in Supplemental Appendix A that if contestation occurs due to bigger windfalls, the excluded group also gets a bigger share of the windfall in expectation.
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. 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).
20. In Supplemental 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.
21. Our fixed effects regressions, discussed below, allow us to investigate the effects of village-level variation in support for GAM within districts.
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. In Supplemental 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.
23. Whereas 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.
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.
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 victimhood was broadly defined in many villages; we show in Supplemental Appendix H that we observe the same pattern of results if we define conflict victims more narrowly using objective or subjective criteria.
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, we do not have data on what elites actually received.
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. Although the authors do not theorize the strategic interaction, their results are consistent with ours insofar as informal elites constitute an excluded group that values on maintaining good relations.

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