FIGHTING FOR HEARTS AND MINDS: THE EFFECT OF DEVELOPMENT PROJECTS ON CIVIL CONFLICT

Benjamin Crost*　　　　**Patrick B. Johnston**†

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Abstract

"Hearts and minds" approaches to conflict resolution are based on the premise that development projects reduce violent conflict, either by attracting popular support for the government or by increasing individuals' relative returns to peaceful activities. But if insurgents are aware that development projects weaken their position, they have an incentive to oppose them, which may lead to increased violence. To formalize this intuition, we develop a theoretical model of bargaining and conflict in the context development projects. Our model predicts that development programs will cause an increase in conflict if governments cannot credibly commit to (1) ensuring the project's success in the face of insurgent opposition and (2) honoring agreements reached before the start of the project. To test the model, we estimate the causal effect of a large development program on conflict casualties in the Philippines. Identification is based on a regression discontinuity design that exploits an arbitrary poverty threshold used to assign eligibility for the program. Consistent with the model's predictions, we find that eligible municipalities suffered a substantial increase in casualties. The effect is split evenly between government troops and insurgents and only lasts for the duration of the project.

*Ph.D Candidate, Department of Agricultural and Resource Economics, University of California Berkeley.

†Research Fellow, International Security Program, Belfer Center for Science and International Affairs, Harvard Kennedy School.

"Make no mistake: Insurgents are constantly targeting these areas of success. They bomb new roads and throw acid on girls who go to school. But if we stick by the Afghan people, we will win over their hearts and minds." General David Petraeus

1 Introduction

Over the last six decades, civil conflict has destroyed immense amounts of physical and human capital and has led to the deaths of more than 16 million people (Fearon and Laitin 2003, 75). It has been associated with the spread of health pandemics (Elbe 2002; Murray et al. 2002; Ghobarah et al 2004); the degradation of the rule of law and the creation of new opportunities for criminality (Reno 1998; Collier et al 2004; Fearon 2004; Ross 2004 ; Angrist and Kugler 2008); the forced displacement of massive amounts of people (Salehyan 2007; Ibanez and Velez 2008); regional instability and conflict (Salehyan 2009); and poorly governed spaces hospitable to transnational terrorist organizations (Piazza 2008).

Unfortunately, civil conflict is not uncommon. Since the end of World War II over half of all countries have suffered at least one incidence of civil conflict. Roughly twenty percent have experienced over a decade of civil war (Blattman and Miguel 2010, 3-4). The urgency of civil conflict has never been greater: The proportion of conflict-affected countries, which increased steadily from 1945 through the mid-1990s, is once again on the rise (Harbom and Wallensteen 2010).

Over the last decade, development interventions have become popular countermeasures against violent conflict. Huge aid flows have been directed to conflict-affected areas on the premise that aid can help rebuild societies, reduce conflict and improve the lives of those in conflict-affected areas. But even though aid programs have become a popular tool for ameliorating conflict, little is known about their effectiveness. Berman et al. 2008 conduct a study of the Commanders' Emergency Reconstruction Program (CERP) in Iraq, in which evidence suggests that CERP aid reduced anti-coalition insurgent attacks. However, the violence-reducing effect of CERP obtained only after 2007, roughly coinciding with a troop surge that increased coalition presence and improved security in contested areas, suggesting the possibility of a conditional effect. Dube and Naidu (2010) examine the effect of a different type of aid intervention–military assistance–on conflict-related violence by guerrillas and paramilitary forces in Colombia. Their results suggest a different effect: while aid to Colombia had no significant effect on guerrilla attacks, it increased paramilitary attacks.

These contradictory results are likely explained, at least in part, by contextual variation in the countries and aid programs under study. However, two fundamental challenges have limited previous efforts to identify the relationship between aid programs and conflict. First, non-random aid assignment has limited opportunities for identification. Logistical and ethical issues have made randomized experiments difficult to implement and exacerbated the selection bias in observational data. Selection bias is especially problematic in conflict-affected areas because, depending on conditioning factors, endogenous aid assignment can introduce either an upward or a downward bias. On the one hand, development agencies might assign programs to peaceful areas where their staffs can operate safely. On the other, agencies that allocate aid based on need are likely to target programs at areas with high civil conflict incidence, since need and real or anticipated violence are likely to co-vary. Second, lack of high quality micro-data has made it difficult for researchers to study the relationship between aid and conflict. The most commonly-used indicators of conflict come from cross-national estimates of battle deaths per country-year, which do not have a high enough resolution to identify the causal effects of micro-level interventions.[1]

We address these challenges and contribute to the literature in two ways. First, we employ a regression discontinuity design to cleanly identify the causal effect of of a major development programcalled KALAHI-CIDSS on conflict violence in the Philippines. Eligibility for KALAHI-CIDSS, a World Bank-financed program, was restricted to the the poorest 25 percent of municipalities in participating Philippine provinces[2]. The arbitrary choice of this eligibility threshold created a discontinuity in the assignment of aid, which enables identification through the RD design.[3] A second advantage is the scope and precision of our data. As mentioned above, the data provide complete information on conflict incidents that involved AFP units operating across the country between 2001 and 2008. They contain information on the dates, location, participating units, and measurable outcomes of each incident, including which party was the initiator and how many government, insurgent, and civilian casualties occurred.[4]

Our analysis yields four main results.

First, the program exacerbated conflict violence in eligible municipalities. Municipalities just above the threshold suffered a statistically significant and substantively large increase in violence compared with municipalities just below the threshold. These increases cannot be explained by differences in pre-program violence or other observable characteristics.

Second, the increase in violence in eligible municipalities only lasted for the duration

[1]Examples of commonly used conflict measures are found in the Correlates of War Intra-State War Dataset and UCDP/PRIO Battle Deaths Dataset. On these data, see Sarkees and Schafer 2000 and Lacina and Gleditsch 2005.

[2]Municipal poverty rankings were derived from comprehensive local poverty indices based on pre-existing census and survey data, which we describe in detail below. For a full description of the poverty indices, see Balisacan et al. 2002; Balisacan and Edillon 2003. For more on poverty mapping methodology, see Elbers et al 2003.

[3]See Imbens and Lemieux (2008) for a primer on the theory and practice of regression discontinuity designs.

[4]For a fuller description of the conflict data, see Felter 2005.

of the program's implementation. We do not find evidence of a lasting effect on violence, either positive or negative.

Third, insurgents and government troops suffer the majority of program-related casualties. Civilians appear to have suffered less, experiencing only a small and statistically insignificant increase in casualties due to the program.

Fourth, the increase violence attributable to KALAHI-CIDSS was not one-sided. Eligible municipalities experienced a similar increase in the number of casaulties in insurgent-initiated and government-initiated attacks. While "hearts and minds" models predict that development programs will decrease insurgent violence (e.g., Berman et al. (2008)) and "greed" theories predict they will increase insurgent violence (Collier and Hoeffler (1998)), the dual increases in violence associated with insurgent attacks and government offensives is inconsistent with both models. To the contrary, our results are consistent with a bargaining models, which predicts that program eligibility leads to bargaining failure between local governments and insurgents (see, e.g., Fearon 1995; Powell 2004; 2006).

The remainder of the paper proceeds as follows. In Section 2, we outline a theoretical model of bargaining and conflict in the context of development projects and discuss the predictions it generates for the Philippine context. In Section 3, we give a brief overviews of the main insurgencies in the Philippines' and a detailed overview of the KALAHI-CIDSS program. Section 4 contains a description of our empirical strategy an overview of the data that we use for our empirical analysis. In Section 5, we present our main empirical results and discuss both their robustness and the robustness of the RD identifying assumptions to available evidence. Section 6 concludes.

2 Development Projects, Bargaining and Conflict: A Model

This section describes a simple theoretical model of bargaining between insurgents and the local government in a municipality that is scheduled to receive assistance in the form of a development project. The model draws heavily on the work of Fearon (1995) and Powell (2004, 2006), which shows that sudden shifts in expected power between conflicting parties can lead to a breakdown of bargaining. While the model is an over-simplified abstraction from the complex reality of interactions between local governments and insurgent groups, we believe that it captures a fundamental mechanism through which development projects - like the Philippines' KALAHI-CIDSS program of our empirical case-study - can increase violence in an ongoing civil conflict. There are two main reasons for modelling the interaction between insurgents and local governments as a bargaining game. First, bargaining failures are thought to be a central cause of civil conflict in many contexts (see Blattman and Miguel for a recent review of the conflict literature).

Second, there is strong anecdotal evidence of negotiations between local governments and insurgents over the implementation of the KALAHI-CIDSS program, so that a bargaining model is well suited to describe the context of our empirical case-study.

The intuition behind the model is the following: The central government, in our case the Department for Social Welfare and Development (DSWD), plans to implement a development project in a municipality. If the program is successfully implemented, it will shift the balance of power towards the local government and away from insurgents. One possible mechanism for this shift in power is suggested by the "hearts and minds" model of counter-insurgency (Berman et al.). A successful project will increase the population's support for the government, making individuals in the project area more willing to supply the government with information about insurgent's plans and whereabouts, which makes it harder for insurgents to operate. Another possible mechanism is that a successful project will decrease poverty and increase returns to peaceful economic acitivities, making it harder for insurgents to find recruits (this mechanism is suggested by the model of Dal Bo and Dal Bo, 2004, and the empirical findings of Dube and Vargas 2010). Regardless of the precise mechanism, insurgents are aware that a successful program will decrease their ability to inflict damage on the government and thus decrease their bargaining power in future negotiations. They therefore have an incentive to launch attacks in order to hinder the program's implementation. This alone would not be enough to explain an increase in violence if the government could pay off insurgents for allowing it to peacefully implement the project. However, since a successful project increases the government's bargaining power, it has an incentive to renege on bargaining agreements reached before the project's start. As described by Fearon and Powell, the government's inability to commit to an agreement can lead to conflict by making it impossible to reach a mutually acceptable agreement.

The model is a simple two-party sequential bargaining model with a finite number of rounds. In each round the insurgents demand a transfer m_t from the government. If the government accepts the transfer, it pays the transfer and receives a payoff of $-m_t$ from the current round. If it rejects, the insurgents launch attacks on government facilities. This is costly to both the government, which receives a payoff of $-c_t$, and the insurgents, who receive a payoff of $-d_t$. By allowing insurgents to conduct costly attacks after their demands have been rejected, the model assumes that they are able to overcome the commitment problem and make credible threats. While we do not model the precise mechanism by which insurgents commit to attacking, a large amount of anecdotal evidence suggests that insurgent organizations are often able to follow up their threats with violent attacks when extorting individuals and companies.

The timing of the game is as follows. At the beginning of period *1*, it becomes known that the municipality is eligible for the project. The insurgents choose m_1, which the government either accepts or rejects. At the end of period *1*, a move of nature decides whether the project is successfully implemented or fails. In periods *2* to N bargaining takes place as in period *1*, but there are no more moves of nature.

The model's first key assumption is that conflict in the first period affects the probability that the project is successfully implemented. Anecdotally, there are at least two potential mechanisms for this. First, insurgents can use violent attacks to disrupt the preparations for the project and threaten the security of project staff, leading the implementing agency to withdraw. Second, even if the project continues, insurgents can hinder its successful implementation by destroying project infrastructure. In the case of KALAHI-CIDSS there is anecdotal evidence for both mechanisms. In some municipalities, insurgents launched attacks during the program's preparation phase, which led the program's implementing agency, DSWD, to abort implementation in four inititally eligible municipalities due to concerns about the safety of its staff[5]. In other municipalities, insurgents attacked construction work that was being funded through the project. Here, we define p^c as the probability that the program is successfully implemented conflict occurs in period *1* and p^b as the probability that of successful implementation if there is no conflict in period *1* and assume that $p^c \leq p^b$.

The model's second key assumption is that the government's cost of conflict in later rounds depends on whether the program was succesfully implemented, so that. We thus write the government's cost of conflict in period two as $c_2(K)$, where $K = 1$ if KALAHI-CIDSS is being succesfully implemented and $K = 0$ if it is not. As mentioned above, there are two possible mechanisms to explain the program's effect on the government's cost of conflict. First, a successful project is likely to increase the population's support for the government. This makes individuals in the project area more likely to supply the government with information about insurgent's plans and whereabouts, making it easier for the government to defend itself against attacks. The second possible mechanism is that a successful project will decrease poverty and increase returns to acitivities in the peaceful economy, making it harder for insurgents to find individuals willing to carry out risky attacks (Dal Bo and Dal Bo, 2004, and Dube and Vargas 2010). Here, we remain agnostic about which of these mechanisms causes the change in the government's cost of conflict and merely assume that $c_t(1) < c_t(0)$, so that the program reduces the government's cost of conflict.

We now solve the model by backward induction. In periods *2* to N, conflict does not affect the government's future cost of conflict and the goverment will accept any demand less than or equal to $c_t(K)$. Since $c_t(K)$ is known to the insurgents, they maximize their payoff by demanding $m_t = c_t(K)$, which the government accepts. Thus, in any subgame perfect equilibrium, both parties' payoffs from rounds *2* to N only depend on whether the project was successfully implemented in round 1: the insurgents' payoff will

[5] Authors' interview with KALAHI-CIDSS Program Manager Camilo Gudmalin, Department for Social Welfare and Development, Quezon City, Philippines, May 28, 2010.

be $C(K) = \sum_{t=2}^{N} \beta^{t-1} c_t(K)$, and the government's payoff will be $-C(K)$.

Thus, if the government accepts the insurgent's offer in period *1*, its total payoff is:

$$U^{gov}(accept) = m_1 - p^b C(1) + \left(1 - p^b\right) C(0)$$

where β is the common discount factor. If the government rejects the offer, its expected payoff is

$$U^{gov}(reject) = c_1 - p^c C(1) + (1 - p^c) C(0)$$

The government accepts the insurgents' first-round offer if $U^{gov}(accept) \geq U^{gov}(reject)$, so that the largest demand it is willing to accept is:

$$m_1^{\star} = c_1 + \Delta p \left(C(1) - C(0)\right) \tag{2.1}$$

Here, $\Delta p = p^b - p^c$, which denotes the amount by which first-round conflict reduces the probability that the program is successfully implemented. The first term on the right-hand side, c_1, is the smallest offer the government is willing to accept if Kalahi-CIDSS does not affect its second-round bargaining power, or if conflict does not affect the probability that the program is successfully implemented. The second term is the additional payment the government is willing to make in order to appease the insurgents into accepting the program. Of course, $m_1^{\star}$ is the smallest demand the insurgents will make under any circumstances, since by demanding less they would unnecessarily reduce their payoff. Thus, the insurgents either demand $m_1^{\star}$, avoid first-round conflict and receive a payoff of

$$U^{ins}(m_1^{\star}) = m_1^{\star} + p^b C(1) + \left(1 - p^b\right) C(0)$$

or make a higher demand, engage in first-round conflict and receive:

$$U^{ins}(m_1 > m_1^{\star}) = -d_1 + p^c C(1) + (1 - p^c) C(0)$$

Insurgents are only willing to avoid conflict if $U^{ins}(m_1^{\star}) \geq U^{ins}(m_1 > m_1^{\star})$. Thus first-round conflict will only be avoided if:

$$m_1^{\star} \geq -d_1 + \beta \Delta p \left(C(0) - C(1)\right) \tag{2.2}$$

The combination of equations 1 and 2 yields the condition for a successful bargaining agreement:

$$2\Delta p \left(C(0) - C(1)\right) \leq c_1 + d_1 \tag{2.3}$$

If this condition fails, the highest transfer the government is willing to make in exchange for peace cannot compensate the insurgents for their loss of bargaining power in future periods. The condition shows that conflict is more likely if the project causes a larger reduction in insurgents' future bargaining power, $c_{t\geq 2}$. If the project has no effect on $c_{t\geq 2}$, or if conflict has no effect on the probability of the project's implementation, bargaining will always be successful, since we assume that conflict is costly to at least one party, so that $c_1 + d_1 > 0$. As in the models of Fearon (1995) and Powell (2004, 2006), bargaining fails if the (potential) shift in bargaining power from one round to the next is large compared to the inefficiency of conflict. The model's key testable predictions are that eligibility for development projects initially increases the probability of violent conflict, since bargaining can break down in the first period, but that this effect disappears after some time, since bargaining is always successful in the second period. The model also predicts that conflict is more likely if insurgents can credibly threaten the program's implementation, i.e. if first-round conflict has a large effect on the program's succesful implementation. Another key prediction is that conflict can occur before the start of the project's implementation and that being eligible for a project can lead to conflict even if the project is in the end not implemented.

At this point it should be noted that bargaining only fails because the government cannot credibly commit to not using the increase in bargaining power it gains from KALAHI-CIDSS. If the government could fix its second-round offer in the first round, the game would collapse into a single-round bargaining game in which the peaceful solution is Pareto-optimal. In addition, bargaining failure depends on the assumption of discrete time. If bargaining took place in continuous time (in other words, if bargaining rounds become infinitely short), the government would be able to devise a continuous stream of payments that the insurgents prefer to conflict (e.g. Schwarz and Sonin 2007). While it is thus possible to devise a continuous-time model in which development projects do not lead to a breakdown of bargaining, we believe that the discrete time assumption is better suited for the present context because negotiations with insurgents pose considerable logistical challenges so that there are likely to be substantial lags between successive rounds of bargaining.

2.1 Comparison With Other Models of Conflict:

This section reviews the predictions other models of conflict make about the effect of development projects on conflict and compares them to those of our model. The standard "hearts and minds" model of conflict, as described by Berman et al., predicts that development projects cause a decrease in conflict. The model's key assumption is that development projects increase the population's support for the government, which leads individuals to be more willing to share information about insurgents with the armed forces. Insurgents therefore find it more difficult to launch attacks in areas affected by development projects, which leads to a decrease in conflict. A related model is that of Dal Bo and Dal Bo, who take a general equilibrium approach to modeling conflict. In their model, conflict is a consequence of low returns in the peaceful economy and high returns in the "conflict economy", i.e. in appropriating the economy's output by violent means. Assuming that conflict is a labor intensive actrivity, Dal Bo and Dal Bo (2004) find that increases in the returns to labor cause a decrease in conflict, because fewer individuals are willing to participate in conflict, which makes it harder for insurgents to find recruits. Increases in the returns to capital on the other hand cause an increase in conflict, because they increase the value of the total output to be fought over. For the case of development projects, their model's predictions therefore depend on whether the project increases the return to labor or the return to capital. Other general equilibrium models, like the one of Grossman (1999), predict that economic growth can increase conflict by increasing the amount of resources to be fought over, regardless of whether growth favors labor or capital. Regardless of the sign of the effect, general equilibrium models predicts that a development project's effect on conflict should materialize only after the project has started and should persist as long as the project's effect on the economy. These predictions differ from our model's, which predicts that conflict can increase even before the project's implementation has started and returns to pre-project levels as the project ends. Our model obviously has a similar starting point as those of Berman et al. (2008), and Dal Bo and Dal Bo (2004), assuming that development projects can reduce insurgents' capacity to launch successful attacks, either by making it harder to find recruits or making it harder to operate clandestinely. The main point of departure is that our model explicitly incorporates the strategic interaction between insurgents and the government, which can lead to conflict over a project's implementation.

A type of models which make similar prediction to ours are models of bargaining with asymmetric information. Suppose, for example, that municipalities governments know the exact benefits they will receive from successfully implementing the project while the insurgents do not. If in surgents do not know the highest demand that each government will accept, it may be optimal for them to make a demand that is rejected by some fraction of governments. In a dynamic game, the government will have an additional incentive to reject high demands in order to affect insurgents' beliefs about its willingness to pay to avoid conflict in later rounds. Multiple-round games of asymmetric information have been described by Fudenberg and Tirole (1983). Translated to the present context, their results suggest that asymmetric information can cause conflict over the implementation of development projects, but that the likelihood of conflict decreases over time as insurgents learn about the government's true willingness to pay to avoid conflict. While asymmetric information models make predictions that are similar to those of our model - conflict initially increases in municipalitys eligible for a development project, but returns to baseline levels over time - we do not believe that they can plausibly explain conflict around development projects in the present context. It is well known that insurgents in the Philippines have been attacking road construction projects and other infrastructure projects for over 30 years. While asymmetric information might explain this type of attack for the first few years of the conflict, we would expect the information asymmetry to disappear over time as insurgents learn about the government's willingess to pay to avoid attacks.

3 Empirical Setting: Conflict and Development Projects in the Philippines

3.1 Violent Conflict in the Philippines

Civil conflict in the Philippines has been ongoing for over four decades, caused more than 120,000 deaths, and cost the country an estimated $2-3 billion (Schiavo-Campo and Judd 2005). During the period of study, 2001-2008, insurgent organizations with distinct goals and structures were active across the country's varied human and physical terrain. The two largest of them are the New People's Army (NPA) and the Moro Islamic Liberation Front (MILF).

3.1.1 New People's Army (NPA)

As the armed wing of the Communist Party of the Philippines (CPP), the New People's Army is a class-based movement that seeks to replace the Philippine government with a communist system. Since taking up arms in 1969, the NPA has relied on pinprick ambushes and harassment tactics rather than conventional battlefield confrontations against government armed forces. The NPA's current strength is an estimated 8000 armed insurgents, down from a 1986 peak of approximately 25,000 insurgents, who exerted influence in 63 of the (then) 73 Philippine provinces (Felter 2005, 37-39). Since NPA military strategy is not manpower intensive, the organization's leadership manages to spread its influence by deploying small guerrilla fronts in and around villages throughout the Philippines' rural countryside. Due to the NPA's organization in small, local guerrilla fronts and the group's lack of a broad ethnic or religious constituency, the NPA's organizational structure requires significant support from the Philippines' rural poor. Indeed, rural populations supply most of the group's recruits and logistical support[6]. According to our dataset, the NPA is by far the most active insurgent organization in the Philippines. During the period 2001-08, the NPA was involved in 65% of all incidents for which the

[6]Chapman (?) and Jones (?) provide detailed histories of the NPA. For an insider perspective on the war from an AFP officer's perspective, see Corpus (?). From an NPA leadership perspective, see Sison and Werning (?).

enemy organization was reported (about 10% of incident reports do not report an enemy organization).

3.1.2 Moro Islamic Liberation Front (MILF)

The Moro Islamic Liberation Front is a separatist movement fighting for an independent Muslim state in the *Bangsamoro* region of the southern Philippines. The MILF was formed in 1981, when the group's founders defected from the Moro National Liberation Front (MNLF), another longstanding southern Philippines insurgent movement with similar goals to the MILF's. MNLF and MILF leaders disagreed on the means by which to pursue independence. After the split, the MILF escalated armed conflict against the government. While the MNLF and the government signed a peace accord in 1996 that created the Autonomous Region of Muslim Mindanao (ARRM), the MILF has remained reluctant to end. The MILF's core grievances stem from government efforts to retitle lands considered by the southern Muslim population to be part of their ancestral homeland and the group reportedly enjoys broad support from the communities in which they operate. (Kreuzer and Werning **?**).

The goals, ideology, organization, and tactics of the MILF differ significantly from those of the NPA. In contrast to the NPA's secular class-based organizational structures, religion is an extremely important part of the MILF's organization. Moreover, the ways in which the MILF organizes its fighters and conducts military operations also differ from the NPA: While the NPA relies mainly on small unit guerrilla tactics, it is not uncommon for MILF commanders to mass their forces into larger units to fight semi-conventional battles against government forces (Felter 2005, 35-37). Indeed, the MILF has the manpower necessary to do this. At its peak strength in 1999, the group had an estimated 15,700 rebels under arms. While MILF strength declined after a major government offensive in 2000, August 2004 estimates suggested that the MILF still had more than 10,500 armed fighters in its ranks (Felter 2005, 36). However, because of its narrow geographic focus, the MILF not a major cause of conflict in our data, being involved in only 10% of all reported incidents.

3.2 The KALAHI-CIDSS Program

KALAHI-CIDSS is a major development program in the Philippines. Designed to enhance local infrastructure, governance, participation, and social cohesion, KALAHI-CIDSS has been the Philippines' flagship development program since 2003. As of mid-2009, more than 4000 villages in 184 municipalities across 40 provinces had received KALAHI-CIDSS aid.[7] Plans to expand KALAHI-CIDSS are currently being made, with the aim of dou-

[7] As of March 2010, there were 80 provinces and 1496 municipalities in the Philippines. For a complete list of all Philippine administrative units, see the National Statistical Coordination Board's website at

bling the number of recipient municipalities during the program's next phase.

Run by the Philippine government's Department of Social Welfare and Development and funded through World Bank loans, KALAHI-CIDSS aims to promote local governance reform and development by supporting bottom-up infrastructure and institution-building processes. As a community-driven development (CDD) program, KALAHI-CIDSS is representative of a common type of development intervention. The World Bank lends more than two billion dollars annually for CDD projects (Mansuri and Rao 2004) and donors are increasingly making use of CDD programs in conflict-affected countries. Over the last decade, for example, CDD programs have been launched in Afghanistan, Angola, Colombia, Indonesia, Nepal, Rwanda, and Sudan.[8]

KALAHI-CIDSS follows a standard CDD template. First, each participating municipality receives a block grant for small-scale infrastructure projects. With the municipalities, each village, or *barangay,* holds a series of meetings in which community members draft project proposals. Villages then send democratically elected representatives to particpate in municipal inter-*barangay* fora, in which proposals are evaluated and funding is allocated. Proposals are funded until each municipality's block grant has been exhausted. Finally, once funding has been allocated, community members are encouraged to monitor or participate in project implementation.[9]

KALAHI-CIDSS is a significant intervention. Particpating municipalities receive PhP300,000, or approximately \$6000, per village in their municipality. The average municipality has approximately 25 villages, so the average grant is approximately \$150,000, or about 15% of an average municipality's annual budget. Over the course of the program, the project cycle is repeated three times–occasionally four–meaning that on average, participating municipalities receive a total of between \$450,000 and \$600,000 dollars.

3.2.1 Targeting

KALAHI-CIDSS was designed in the early 2000s as a nationwide CDD program that would target aid at the poorest of the poor. It was targeted following a two-staged approach. First, 42 eligible provinces were selected, among them the 40 poorest based on estimates from the Family Income and Expenditure Survey (FIES). To identify the poorest municipalities within eligible provinces, a team of economists was hired to conduct a municipal poverty mapping exercise using data from the 2000 National Census, the Family Income and Expenditure Survey (FIES) and rural accessibility surveys (Balisacan et al. 2002; Balisacan and Edillon 2003). The poverty index was calculated using follow-

National Statistical Coordination Board - Standard Geographic Codes.

[8]For an overview, see World Bank 2006. . See also Mansuri and Rao2004. For an assessment of the impact of Indonesia's CDD program, called KDP, on local corruption and public goods provision, see Olken 2007;2010. On KALAHI-CIDSS and social capital, see Labonne and Chase 2009.

[9]See Parker 2005 for a detailed overview of the KALAHI-CIDSS process.

ing the poverty mapping method of Elbers et al (2003).[10] The first step of the poverty mapping is to regress measures of household expenditure, which are only available for a subset of municipalities on data from the census and accessibility surveys. The estimated relationship between census and accessibility variables and poverty in this subset of municipalities was then used to predict poverty levels for all municipalities. Within each eligible province, only the poorest 25% of municipalities were eligible to participate in KALAHI-CIDSS. The arbitrary nature of this eligibility cutoff enables us to identify the program's casual effect through a regression discontinuity design.

[Table 1 about here]

Table 1 shows which variables were used in calculating the poverty index and the weights that they were assigned. For the first ten variables, the weights were determined by the regression of the poverty mapping approach, the weights of the last two variables were chosen by the researchers.

3.2.2 Timeline

[Figure 1 about here]

Figure 1 shows the timeline of the program, which was rolled out on a staggered schedule. Participating provinces were first divided into two groups, Group A and Group B. Eligible municipalities in Group A and Group B provinces were then divided into phaseswith different start dates.

[Table 2 about here]

Table 2 displays the start dates and the number of municipalities that participated in each phase of the program. Group A municipalities learned their eligibility status in December 2002 and began receiving project aid in either January 2003 (Phase I) or June 2003 (Phase II).[11] Group B municipalities were informed of their eligibility status in October 2003 and implementation began in October 2004 (Phase IIIA), January 2006 (Phase IIIB), or August 2006 (Phase IV).

3.2.3 Participation

In each eligible municipality, implementation of the program was preceded by a "social preparation phase" in which the program was introduced to the public and preparations were made for its implementation. During this time, eligible municipalities were required to ratify a memorandum of understanding and put in place basic institutional mechanisms

[10] Details can be found in Balisacan et al. 2002; Balisacan and Edillon 2003.

[11] Phase I was a pilot phase whose municipalities were outside of the bottom quartile of poverty.

required for implementation. If an eligible municipality failed to meet these conditions by the time KALAHI was scheduled to be launched, it was declared ineligible for the program. There were some cases in which eligible municipalities failed to comply with program requirements and were replaced by municipalities that were not initially eligible.

4 Empirical Strategy

4.1 Regression Discontinuity Design

To test our theoretical model of bargaining and conflict in the context of the Philippines, we use a regression discontinuity (RD) design to estimate the causal effect of KALAHI-CIDSS - the country's flagship anti-poverty program in the period 2003-2008 - on the intensity of violent conflict. The RD approach is made possible by the arbitrary eligibility threshold used to target the program. Targeting followed a two-staged approach. First, 42 eligible provinces were selected, among them the 40 poorest based on estimates from the Family Income and Expenditure Survey (FIES). The poverty levels of all municipalities within the eligible provinces were estimated using a povery mapping methodology based on a combination of data from FIES and the 2000 Census (Balisacan et al. 2002, 2003). In each eligible province, municipalities were ranked according to their poverty level and only the bottom quartile was eligible for KALAHI-CIDSS. The arbitrary cutoff at the 25th percentile of poverty created a discontinuity that we exploit to identify the program's causal effect on violent conflict. In essence, we estimate the causal effect by comparing the outcomes of municipalities just below the eligibility threshold with those of municipalities just above it. The identification assumption of the RD design is that municipalities close to the threshold on either side do not differ in unobserved variables that affect conflict, so that any change in conflict across the threshold can be attributed to the KALAHI-CIDSS program[12].

The fact that not all eligible municipalities participated in the program might suggest the use of a "fuzzy" RD design that uses eligibility as an instrument for participation. However, our theoretical model suggests that eligibility itself affects conflict and that participation is an endogenous outcome. We therefore estimate the "intention to treat" effect - the effect of eligibility regardless of later participation status.

Formally, the RD estimator of the causal effect of eligibility for KALAHI-CIDSS is

$$\tau_{RDD} = \lim_{x \downarrow c} [Y_i | X_i = x] - \lim_{x \uparrow c} [Y_i | X_i = x]$$

where Y_i is municipality i's outcome (e.g. the number of conflict incidents in the municipality), X_i is the municipality's poverty index and c is the threshold that determines

[12]Robustness tests for this identification assumption, using pre-treatment conflict and other observable variables, are presented in the Results section of the paper.

assignment (i.e. the 25th percentile of each municipality's poverty index). Verbally, the estimated causal effect is the difference in the limits of the expected outcome as we approach the eligibility threshold from above and below. In practice, linear regressions are fitted on both sides of the threshold and the limits are estimated by extrapolating the regression lines[13]

The running variable in our RD regressions is the distance of the municipality's poverty rank from the provincial eligibility threshold[14]. Since only municipalities in the poorest quartile were eligible, the provincial threshold was calculated by dividing the number of municipalities in each province by four and then rounding to the nearest integer[15]. This threshold number was then subtracted from the municipality's actual poverty rank to obtain the municipality normalized poverty rank. For each participating province, the richest eligible municipality has a normalized poverty rank of zero and the poorest ineligible municipaliy has a normalized poverty rank of one.

[Figure 2 about here]

Figure 2 plots the observed probability of participating in KALAHI-CIDSS against the normalized poverty rank. The graph shows that the probability of participation decreases sharply at the eligibility threshold, though some eligible municipalities did not participate and were replaced by municipalities above the threshold. The probability of participation is somewhat lower for municipalities at the eligibility threshold, i.e., those with a normalized poverty rank of zero. A possible explanation is that the implementing agency had room for discretion on the margins when calculating the number of eligible municipalities per province. The standard procedure for determining the number of eligible municipalities per province was to divide the number of municipalities in each province by four and then to round to the nearest integer, but in some cases the number was rounded down due to budget constraints, particularly if the municipality at the threshold did not express a strong interest in participating in KALAHI-CIDSS.

[13] For further details, see Imbens and Lemieux 2008.

[14] Unfortunately, data for the last two variables used for the poverty ranking - density of good barangay roads and road distance to the provincial center of trade, both in 2000 - are no longer available, so that we are unable to reproduce the poverty index that formed the basis of the ranking. However, our regressions control for the remaining ten Census variables used for the ranking. Balance tests show that there are no discontinuous breaks in these variables or other observable municipal characteristics at the eligibility threshold. To avoid bias from omitting the road density and road distance variables (or any other unobserved municipal characteristics) some of the regressions presented in the Results section include municipality fixed effects.

[15] In cases where dividing a province's number of municipalities by 4 ended on .5, the number of eligible municipalities was rounded down more often than up, so in calculating municipalities' normalized poverty rankings, we follow suit and round down at .5. Doing so improves the accuracy with which the normalized poverty rank predicts participation in KALAHI-CIDSS but otherwise does not affect the empirical results.

4.2 Data

Three types of data are used in this paper: Program data from the Department for Social Welfare and Development (DSWD), the government agency responsible for implementing KALAHI-CIDSS; armed conflict data from the Armed Forces of the Philippines (AFP); and population data from the Philippines 2000 National Census. All variables in our analysis are measured at the municipality level.

4.2.1 Program Data

We use KALAHI-CIDSS program data from the Philippines Department for Social Welfare and Development. These data include information on municipalities' eligibility for KALAHI-CIDSS, whether or not eligible municipalities participated in the program, and the phase or timing of the program's roll out. These data are available from 2003 through 2009, the full duration of the program to date.

4.2.2 Conflict Data

Our data on civil conflict and violence come from the Armed Forces of the Philippines' (AFP) records of civil conflict-related incidents. The data were derived from the original incident reports of deployed AFP units that operated across the country from 2001 through 2008. With authorization from the AFP's Chief of Staff, researchers were hired and trained to compile and code the field reports to an unclassified database. The incident-level data contains information on the date, location, the involved insurgent group or groups, the initiating party, and the total number of casualties suffered by government troops, insurgents, and civilians (see Felter 2005). The data are comprehensive, covering every conflict-related incident reported to the AFP's Joint Operations Center by units deployed across the country. In total, the database documents more than 21,000 unique incidents during this period, which led to just under 10,000 casualties. The depth, breadth, and overall quality of the AFP's database makes it a unique resource for conflict researchers and enables credible assessment of the average impact of KALAHI-CIDSS on the dynamics of insurgent and counterinsurgent violence.

The outcome of interest for our analysis is the number of casaulties in conflict incidents. We believe that this outcome best captures the true intensity of conflict; better than other outcomes such as the number of conflict incidents. In particular, there is reason to believe that the number of incidents does not tell us much about the actual intensity of conflict. Even in municipalities where local governments have negotiated peace agreements with insurgents, AFP units still have an incentive to conduct patrols and other operations, if only to convince their superiors that their deployment in the municipality serves a useful purpose. It is therefore likely that AFP units will encounter insurgents on a regular basis regardless of whether a local peace agreeement is in place or not, making the number of incidents a weak measure of conflict. However, the intensity with which AFP and insurgent units engage each other in combat, and as a consequence the resulting number of casualties, clearly depends on whether a (possibly informal) peace agreement is in place or not. We therefore believe that using incidents as the outcome of interest is likely to understate the effect of KALAHI-CIDSS on the intensity of conflict, and instead use the number of casualties as the outcome of interest.

4.2.3 Other Data

Data from the Philippines' 2000 National Census are also used. The primary purposes for using these data are to test the plausibility of the RD identifying assumption and to check the sensitivity of the results to alternative specifications. These variables are described in more detail below.

4.3 Variables

Our main dependent variable is *total casualties*. This variable measures the total number of people killed and wounded in conflict-related incidents per municipality-year from 2001-2008 as documented in the AFP's field reports. The total casualties variable is calculated as the sum of government casualties, insurgent casualties, and civilian casualties.[16]

To study the dynamics of civil conflict–who suffered and inflicted the casualties–we break down the total casualties variable by individual parties to the conflict.

To this end, the first variable we use is *government casualties. Government casualties* measures the number of government-affiliated troops killed and wounded in action, per municipality-year, from 2001-2008, as documented in the AFP's field reports. The variable counts the casualties suffered by all Philippine government armed forces conducting internal security operations during the study period, including "elite" units such as Special Forces and Scout Rangers units; conventional, or "regular," units such as infantry battalions; and local auxiliary units, such as Citizen Armed Force Geographical Units (CAFGUs), that were administered by the AFP.

The second variable we use is *insurgent casualties. Insurgent casualties* measures the number of insurgents killed and wounded in action, per municipality-year, from 2001 to 2008, as documented in the AFP's field reports. The variable counts the casualties suffered by all insurgent movements operating in a given municipality.

The third variable we use is *civilian casualties. Civilian casualties* measures the total number of civilians killed and wounded in conflict-related incidents, per municipality-year, from 2001 to 2008, as documented in the AFP's field reports. The variable counts

[16]These data were originally made available by the AFP's Chief of Staff and the staff in the Office of the Deputy Chief of Staff for Operations J3 in their unclassified form. For a full description of the conflict data, see Felter 2005, 48-67.

the total number of casualties suffered by civilians in a given municipality but does not distinguish between insurgent-inflicted civilian casualties and government-inflicted civilian casualties.

To test whether insurgencies with differing aims and organizational structures behaved differently in response to the aid intervention, we measure conflict intensity by insurgency. These variables measure the total number of people killed and wounded–government, insurgent, and civilian–per municipality-year, from 2001-2008, in conflict-related incidents involving the communist guerrilla movement the New People's Army and the Muslim separatist movement the Moro Islamic Liberation Front (MILF). These variables are named *Casualties - NPA incidents*, and *Casualties - MILF incidents.*

We also use a number of municipality characteristics as controls. *Municipality Population* measures the total number of residents per municipality in year 2000 as measured by the Philippines' 2000 National Census. As Table 4 shows, the average population of control and treatment municipalities was 29,578.[17] *Highway access* captures the percentage of villages per municipality with access to a national highway. Taken from the *barangay* characteristics section of the Philippines' 2000 National Census, the data show that 68 percent of the villages in municipalities included in our analysis were recorded as having national highway access in 2000. *Timber* measures the amount of land per municipality, in squared kilometers, covered with timber. Data on timber come from the Philippines' National Statistics Coordination Board. *Affected by NPA* is an indicator for whether the NPA reportedly had a local presence. These estimates were made in 2001, two years before the beginning of the KALAHI-CIDSS treatment. Importantly, the affectation data are based on intelligence estimates of insurgent *presence,* rather than *violence,* providing a separate measure of insurgent activity.[18]

As additional controls, we include municipality-level pre-treatment demographic characteristics. The first is an index of *ethnic fractionalization.* Computed using microdata from the 2000 National Census, this variable gives the probability that two individuals drawn randomly from a municipality are from different ethnic groups.[19] The second is a

[17] This is the average population of municipalities in eligible provinces using a normalized poverty rank bandwidth of of three. The average population for all Philippine municipalities in 2000 was 47,043. The lower average population for municipalities' covered in this study reflects conventional wisdom on poverty in the Philippines–rural areas tend to be the most stricken with poverty.

[18] These data were originally made available in unclassified form by the AFP's Office of the J2. See Felter 2005, 39. The primary limitation of these data is a lack of comparable data on MILF presence. While having the same kind of data on MILF presence would be ideal, the NPA data are extremely useful. They provide a measure, however crude, of the density of insurgent "control" within municipalities–a variable posited to be important in previous theoretical work but that is nearly always unobserved in empirical studies of conflict. (Kalyvas **?**). Of all of the Philippines' insurgent movements, however, the NPA provides the most leverage empirically since it operates nationwide. Despite the incompleteness of the insurgent presence data, the pre-treatment NPA presence variable can consequently help us determine whether pre-existing insurgent presence influences either the intervention or the outcomes of interest.

[19] This variable is similar to the commonly employed ethnolinguistic fractionalization (ELF) index used in Fearon and Laitin's (2003) study of civil war onset, which was based on 1964 country-level data from *Atlas Narodov Mira.*

similar index measuring religious differences, also based on year 2000 census microdata, which we call *religious fractionalization*. We also include a control for *percentage Muslim* that measures the percentage share of Muslims, by municipality, based on 2000 census data.[20]

Finally, we control for most of the variables that were used to calculate the municipal poverty index used to determine eligibility for KALAHI-CIDSS. The variables used to calculate the poverty index are shown in Table 1. We control for the first ten of these variables, which come from the 2000 Census: *Age 0-6*, *Age 7-14*, *Age 15-25* and *Age 25+*, denote the proportion of the municipal population that falls into the respective age range. *Electricity*, *Water-sealed toilet* and *Level III water system* denote the proportion of households that have access to the respective facilities. *Strong walls* and *Strong roof* denote the proportion of households whose dwelling has walls or a roof made of "strong" materials[21] Unfortunately, data for the last two variables used for the poverty ranking–density of good barangay roads and road distance to the provincial center of trade, both in 2000–are no longer available and consequently cannot be controlled for in our regressions or used to reproduce the poverty indices. However, the balance tests in the next section show that there are no discontinuous breaks at the eligibility threshold in any observable municipal characteristics including pre-treatment conflict, which suggests that the identifying assumption of the RD design holds. To avoid bias from omitting these variables (or any other unobserved municipal characteristics) some of the regressions presented in the next section include municipality fixed effects.

5 Results

In this section we present results from the regression discontinuity approach based on the arbitrary poverty threshold that determined eligibility for the Philippines' KALAHI-CIDSS program. As mentioned before, KALAHI-CIDSS was targeted following a two-staged approach. First, 42 eligible provinces were selected, among them the 40 poorest based on estimates from the Family Income and Expenditure Survey (FIES). The poverty levels of all municipalities within the eligible provinces were estimated using a poverty mapping approach based on a combination of data from FIES and the 2000 Census (Balisacan et al. 2002, 2003). Within each province, municipalities were ranked according to their poverty level and only the bottom quartile was eligible for KALAHI-CIDSS (i.e. the number of eligible municipalities was calculated by dividing the total number of municipalities in the province by four and rounding to the nearest integer). Our regression

[20] Area experts have suggested that predominantly Muslim areas have unique conflict dynamics due to clan-based social and political structures and exogenous historical circumstances. See, e.g., Abinales 2000; Kreuzer 2005; 2009.

[21] Materials counted as "strong" are galvanized iron or aluminium, concrete, clay tiles amd asbestos for roofs and concrete, brick, stone, wood, galvanized iron or aluminium, asbestos and glass for walls

discontinuity design compares eligible and ineligible municipalities close to the eligibility threshold to estimate the causal effect of the KALAHI-CIDSS program.

The rest of the section is organized as follows: To test the identifying assumption of the RD design we first present summary statistics and regressions showing that municipalities near the eligibility threshold on either side do not differ significantly on any observable characteristics, including levels of conflict prior to the start of KALAHI-CIDSS.

We then present a graphical comparison of trends in conflict casualties in eligible and ineligible municipalities near the eligibility threshold. This comparison shows that the the number of casualites in eligible municipalities sharply increases after eligibility for KALAHI-CIDSS is announced, while the number of casualties in ineligible municipalities remains virtually unchanged.

To obtain a quantitative estimate of the causal effect of eligibility of KALAHI-CIDSS on on conflict violence, we use OLS and fixed effects regressions that exploit the discontinuity described above. These regressions estimate the effect of being eligible for the program while controlling for the running variable–municipalities' normalized poverty rank. As is standard in RD designs, we allow the effect of the running variable to differ on both sides of the threshold by adding an interaction term between eligibility and the normalized poverty rank.

Finally, in addition to the comparison of eligible and ineligible municipalities across the threshold, the regressions exploit the timing of the introduction by including an interaction between being above the poverty threshold and an indicator for the time period after roll-out of KALAHI-CIDSS had begun. In the specifications with municipality fixed effects, the identification comes from the "double-difference" of casualties in eligible and ineligible municipalities, before and after the roll-out of KALAHI-CIDSS. All standard errors are adjusted for clustering at the province level.

5.1 Testing Balance on Observables

To test whether the eligible and ineligible municipalities in our sample differ on observable variables, Table 3 presents balance tests for a number of pre-treatment variables measured in year 2000 or 2001. These tests determine whether any observable variables change discontinuously across the eligibility threshold. If this were the case, it might point to a violation of the identifying assumption that no unobserved variable changes discontinuously across the eligibility threshold.

[Table 3 about here]

To test this, we conduct t-tests for equality of means between eligible and ineligible municipalities in our sample. We conduct these tests for the census variables that determined eligibility, as well as for a set of additional control variales. The results show that none of the variables are significantly different at the 10% level. This increases our confidence that the identifying assumption of the RD design - that municipalities on both sides of the threshold do not differ in unobserved variables that determine conflict - holds. To further rule out possible bias from discontinuous changes in variables across the threshold, some of the regressions presented below include municipality fixed effects to control for all unobserved time-invariant differences between eligible and ineligible municipalities.

5.2 Development Projects and Increased Violence

5.2.1 Graphical Evidence

[Figure 3 about here]

Figure 3 displays the time trend of conflict casualties in eligible and ineligible municipalities just above and just below the eligibility threshold. The scatter plot marks the average number of conflict casualties in a given month, the fitted line is obtained by local quadratic regressions. To clarify the effect of KALAHI-CIDSS, the figure displays two fitted lines, one for the pre-program period 2001-2002 and one for the post-program period 2003-2008. The figure shows that eligible and ineligible municipalities experience similar numbers of conflict casualties in the pre-program period. However, the number of casualites in eligible municipalities sharply increases in January 2003, the first full month after the first announcement of eligibility for KALAHI-CIDSS, while the number of casualties in ineligible municipalities remains virtually unchanged. The difference in casualties between eligible and ineligible municipalities then becomes smaller but increases again around 2005/06 when KALAHI-CIDSS is rolled out in Phase III and IV municipalities.

5.2.2 Quantitative Evidence

[Table 4 about here]

Table 4 presents regression estimates of the effect of KALAHI-CIDSS on conflict violence during the whole period of observation. The estimated causal effect of eligibility of KALAHI-CIDSS on conflict casualties is the regression coefficient associated with the interaction of eligibility and the program time-period. Since the program was scheduled to last for 3 years, we define the program time period as the three years after the start of the program. The program time-period thus depends on which phase of the K-C program a municipality was covered. For municipalities covered in Group A (Phases 1 and 2), the program period is 2003-2005, since implementtion began in 2003. For municipalities in Phase Group B, the case is slightly more complicated. Implementation in Phase IIIA began in 2004, so the program period for municipalities covered in that phase is 2004-2006. For the remaining municipalities, implementation began in 2006, so the program period is 2006-2008. One difficulty comes from the fact that we do not know when implementation was scheduled to begin in the eligible municipalities on Group B that did not participate in the program. To deal with this issue, we assume that the non-participating municipalities in Group B would have been assigned to phases with the same probability as the participating municipalities. In our sample, out of the 15 participating municipalities in Group B, only 2 (13%) participated in phase 3A, while the remaining 13 (87%) participated in phases 3B and 4. We thus assume that the 6 non-participating but eligible municipalities would have participated in phase 3A with a probability of 13% and in phases 3B or 4 with a probability of 87%. For these municipalities, we thus assign a value of 0.13 to the interaction of eligible and program in the years 2004/05 when implementation had only started in phase 3A and a value of 0.87 for the years 2007/08 when implementation was only still ongoing in phases 3B and 4. For the year 2006 we assign a value 1 since implementation was ongoing in all participating municipalities in Group B.

One possible concern with this strategy is that the start time of the program, i.e. assignment to phases, could have been affected by conflict in the municipality and could therefore be endogenous. It is, for example, possible that DSWD could have postponed implementation in a municipality that experienced high levels of conflict in 2004 by assigning the municipality to phase 3B or 4, even thought it was initially scheduled to be covered in phase 3A. This would lead to a downward bias in our estimates since implementation would be negatively affected by conflict (an upward bias is unlikely, since DSWD was trying to avoid implementation in municipalities with high levels of conflict and in fact stopped implementation in some municipalities due to security concerns). To deal with the problem of endogenous start dates we estimate a specification in which the indicators for receiving the program in a given year are replaced by the sample probability of receiving it, conditional on being eligible in Group B (in Group A, the start date for all municipalities was 2003, so the problem of endogenous start dates does not arise). Thus, we assign a value of In this specification, we assign a value of 0.13 to the program indicator to all municipalities in Group B in the years 2004/05 and a value of 0.87 for the years 2007/08. As previously, we assign a value of 1 for the year 2006, since implementation was ongoing in all participating municipalities in Group B. This approach is similar to using eligibility as an instrument for participating in a given year, which would estimate the Local Average Treatment Effect of participating in the KALAHI-CIDSS program. Our reason for not estimating this effect is that, our theoretical model predicts that eligibility itself causes an increase in conflict, regardless of whether the municipality eventually participates in the program. The effect of participation in KALAHI-CIDSS would therefore be an over-estimate of the effect that we are interested in, wich is the Intention To Treat effect.

Columns 1-3 of Table 4 report the results of negative binomial regressions with conflict casualties as the dependent variable. The results suggest that eligible municipalities were significantly more likely to suffer conflict casualties in the period in which the program was implemented. The point estimate of the causal effect (the coefficient associated with Eligible*Project) ranges from 0.89 to 1.05 in the negative binomial regressions. The effect is strongly statistically signiicant and tobust to the inclusion of municipality fixed effects and clustering of standard errors at the province level. Since the negative binomial regression models the mean as an exponential function of the parameters, their size can be approximately interpreted as the effect of a unit change in the explanatory variable on a percentage change in the outcome. This means that eligibility for Kalahi-CIDSS caused a 90-100% increase in the number of conflict casualties. The size of the estimated effect is clearly large relative to the baseline level of violence. In absolute terms, municipalities barely eligible for the program were likely to experience 0.7-0.8 more conflict-related casualties per year than similar municipalities that narrowly missed the cutoff for eligibility. This means that over the three-year program period, an eligible municipality epxerienced 2-2.5 additional casualties. Assuming a constant treatment effect, the 182 municipalities that received KALAHI-CIDSS experienced in the range of 400-450 additional casualties. Given that leading datasets on only require 25 annual battle-deaths for a violent dispute to be coded as a "civil conflict," the size of the program's effect is quite large. The results also show that, consistent with our theoretical model, the project's effect only persists as long as it is being implemented. The point estimates of the effect in the post-program period (the coefficient associated with Eligible*Post-Project) are much smaller than the effect during the program-period. In the models that control for unobserved heterogeneity by including province and municipality fixed effects, the point estimate of the post-program effect is negative and close to zero. The effect is not statistically significant in any of the models. This finding is consistent with the predictions of our theoretical model, that violence should only increase while the project is ongoing, since that is when insurgents can still hinder its successful implementation. The results of the fixed effects linear regression in column 4 demonstrate that the results of the negative binomial regressions are robust to different assumptions about the functional form.

5.3 Balance on Pre-Treatment Violence

The crucial identifying assumption of the RD design used here is that municipalities on both sides of the threshold are similar along unobserved variables that determine the intensity of conflict. This assumption might fail if the poverty mapping exercise that determined eligibility was manipualted in order to target the program to municipalities with higher or lower levels of pre-program conflict. For example, since there is some discretion about which census variables to use for the poverty mapping, it is possible that the variables were specifically chosen to make sure that a certain set of municipalities become eligible or ineligible. We are not aware of any anecdotal evidence of maipualtion of the poverty mapping exercise. Nonetheless, we present two pieces of evidence in support of the identifying assumption.

First, Figure 3 shows that eligible and ineligible municipalites experienced similar levels of conflict before eligibility for KALAHI-CIDSS was announced. If eligible and ineligible municipalites differed in unobserved characteristics that determine conflict, we would expect them to experience different levels of violence before the program was announced. This is not the case. To the contrary, the fact that the increase in conflict in eligible municipalities coincides with the announcement of eligibility suggests a causal effect of KALAHI-CIDSS.

To quantitatively test the hypothesis that eligible and ineligible municipalities experienced equal numbers of casualties in the pre-KALAHI-CIDSS period, we turn to the regression results in Table 4. In colums 1 and 2, the coefficient associated with "Eligible" is an estimate of the difference in annual casualties in eligible and ineligible municipalities in the pre-program period (2001-2002). The estimates show that the difference small and not statistically significant. Thus there is no evidence to suggest that eligible and ineligible municipalities differed on unobserved determinants of conflict prior to the start of the program, which suggests that our RD estimates measure the causal effect of the program on violence.

5.4 Who Suffers and who initiates the violence?

[Table 9 about here]

Table 5 reports estimated effects of KALAHI-CIDSS on casualties from the three groups involved in the conflict: Government, insurgents and civilians. The results show that both Philippine government forces and the insurgent movements suffered substantially more casualties in KALAHI-eligible municipalities than in the similar ineligible municipalities. Civilians appear to suffer less and we cannot reject the null hypothesis that the program does not affect the number of civilian casualties. These results are consistent with our model, in which insurgents and government forces engage in conflict over the division of the surplus from the program.

The results in Table 5 also show which group, government or insurgents, initiates the violence caused by eligibility for KALAHI-CIDSS. Our theoretical models of bargaining and conflict do not make predictions about this - it simply states that conflict occurs if both parties cannot agree on a peaceful bargaining solution and are agnostic as to who initiates the violence. Nevertheless, knowing who initiates the violence may yield insights about whether KALAHI-CIDSS gives one party the initiative in the ensuing conflict. The results show that the program equally affects violence originating from both groups. The point estimates are exactly identical so that we cannot reject the null hypothesis that both parties are equally likely to initiate violence in municipalities eligible for KALAHI-CIDSS.

6 Conclusion

To be written.

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Table 1: Variables Used to Determine KALAHI-CIDSS Eligibility

Variable	Weight
Proportion of Households with Electricity	4.41
Proportion of Households with Water-Sealed Toilets	2.83
Proportion of Households with Access to Level III Water Systems	4.56
Proportion of Houses with Roofs Made of Strong Material	4.27
Proportion of Houses with Walls Made of Strong Material	7.47
Proportion of Population Aged 0-6	23.7
Proportion of Population Aged 7-14	18.05
Proportion of Population Aged 15-25	5.96
Proportion of Population Aged >25	0.08
Educational Attainment of All Family Members Relative to Potential	8.28
Density of Good Barangay Roads that are Passable Year-Round	10
Road Distance to Provincial Center of Trade	10

Source:Balisacan and Edillon 2003

Table 2: Timetable of KALAHI-CIDSS

Phase / Duration	Duration	Municipalities	Barangays
I	Jan 2003 - June 2006	11	201
II	June 2003 - Dec 2006	56	1291
III A	Oct 2004 - Dec 2007	34	883
III B	Jan 2006 - Dec 2008	29	727
IV	Aug 2006 - July 2009	54	1127
Total	Jan 2003 - July 2009	184	4229

Source: Department of Social Welfare and Development

Table 3: Balance of Observed Variables Across Eligibility Threshold

DV: Other Variables	Eligible	Ineligible	Difference
Population ('000)	32.2	28.5	3.7
			(4.3)
Area (km^2)	0.037	0.033	4.2×10^{-3}
			(8.0×10^{-3})
Highway Access (%)	70.3	63.7	6.5
			(6.5)
Timber	5.95	5.92	-0.02
			(1.2)
Affected by NPA in 2001 (%)	39.0	44.9	-4.89
			(8.5)
Percent Muslim	3.4	4.4	-0.99
			(3.7)
Ethnic fractionalization	0.34	0.27	0.07
			(0.06)
Religious fractionalization	0.30	0.29	0.01
			(0.05)
Municipalities	41	41	82

Table 4: The Effect of KALAHI-CIDSS on Conflict Casualties

	Negative Binomial Regression			OLS
Outcome: Conflict Casualties (/Year)	(1)	(2)	(3)	
Eligible	-0.13	0.29		
	(0.27)	(0.40)		
Eligible*Program	1.05***	0.94***	0.89***	1.50***
	(0.28)	(0.31)	(0.31)	(0.39)
Eligible*Post-Program	0.61	-0.02	-0.10	0.16
	(0.41)	(0.41)	(0.41)	(0.46)
Population (/1000)		0.041*		
		(0.022)		
Area		-23.8**		
		(9.3)		
Pct. Barangays with Highway Acc.		-0.97		
		(1.14)		
Timber		0.11*		
		(0.061)		
Affected by NPA in 2001		-0.052		
		(0.34)		
Ethnic Fractionalization		3.24***		
		(0.99)		
Religious Fractionalization		-3.71		
		(3.11)		
Percent Muslim Population		1.06		
		(2.03)		
Constant	0.15	-21.4	0.045	0.81
	(0.28)	(22.0)	(0.363)	(0.09)
Additional Controls	No	Yes	Yes	Yes
Province Fixed Effects	No	Yes	Yes	Yes
Municipality Fixed Effects	No	No	Yes	Yes
Observations	657	657	657	657

Note: Robust standard errors in parentheses. Standard errors are clustered at the province level. The sample is restricted to the 2 municipalities clostest to the provincial eligibility threshold for KALAHI-CIDSS. All regressions include year fixed effects. Asterisks denote statistical significance at the 1% (***) 5% (**) and 10% (*) levels. Additional controls are the ten census variables used to determine eligibility for KALAHI-CIDSS. In the negative binomial regressions, the fixed effects refer to unconditional fixed effects.

Table 5: Who Suffers and who initiates the Violence? Conflict Casualties by Actor

	Negative Binomial	Sample Mean
Outcome:	(3)	
AFP Casualties	0.71**	0.63
	(0.36)	(0.08)
Insurgent Casualties	1.14**	0.31
	(0.43)	(0.04)
Civilian Casualties	0.48	0.23
	(0.48)	(0.04)
Cas. in AFP initiated inc.	0.84	0.42
	(0.59)	(0.06)
Cas. in insurgent initiated inc.	0.84**	0.69
	(0.33)	(0.09)
Control Variables	Yes	
Province Fixed Effects	Yes	
Municipality Fixed Effects	Yes	
Observations	657	657

Note: Robust standard errors in parentheses. Standard errors are clustered at the province level. Control variables are the same as in previous Tables. All regressions include year fixed effects.
The sample is restricted to the municipalities within 1 rank of the provincial eligibility threshold KALAHI-CIDSS. Asterisks denote statistical significance at the 1% (***), 5% (**) and 10% (*) levels.

Figure 1: Timeline of KALAHI-CIDSS Implementation

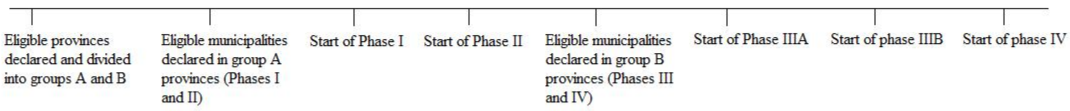


Figure 2: Probability of KALAHI-CIDSS Participation by Distance from Poverty Threshold

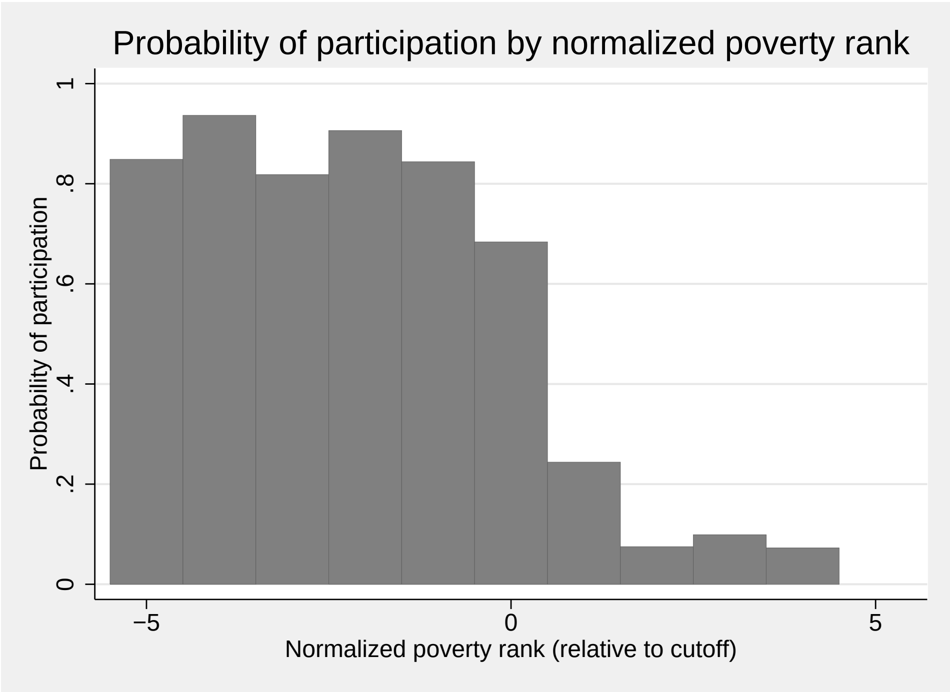
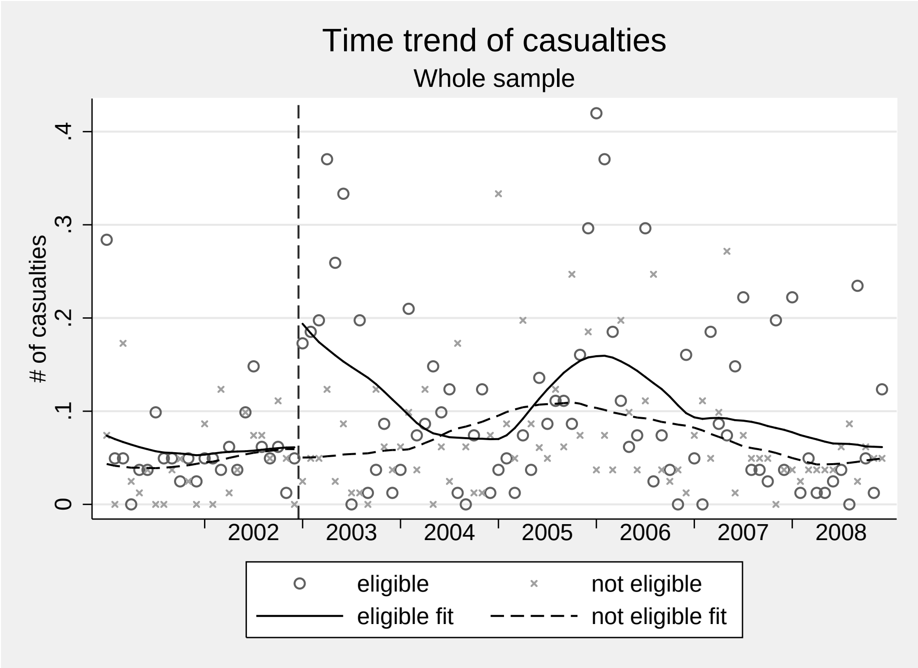


Figure 3: Time Trend of Casualties in Treatment and Control Municipalities



* Municipalities are within two ranks above and below the eligibility threshold