Aid Under Fire: Development Projects and Civil Conflict

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Abstract

An increasing amount of development aid is targeted to areas affected by civil conflict; some of it in the hope that aid will reduce conflict by weakening popular support for insurgent movements. But if insurgents know that development projects will weaken their position, they have an incentive to oppose them, which may exacerbate conflict and derail projects. We formalize this intuition in a theoretical model of bargaining and conflict in the context of development projects. Our model predicts that development projects cause an increase in violent conflict if governments cannot (1) ensure the project's success in the face of insurgent opposition and (2) credibly commit to 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, which lasts only for the duration of the project and is split evenly between government troops and insurgents.

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1 Introduction

Over the last six decades, civil conflict has led to the deaths of more than 16 million people and the destruction of immense amounts of physical and human capital (Fearon and Laitin 2003). It has been associated with the spread of pandemics (Elbe 2002; Murray, King, Lopez, Tomijima, and Krug 2002; Ghobarah, Huth, and Russett 2004); the degradation of the rule of law (Reno 1998; Collier, Hoeffler, and Soderbom 2004; Fearon 2004; Ross 2004; Angrist and Kugler 2008) and the forced displacement of hundreds of thousands of people (Salehyan 2007; Ibanez and Velez 2008). Unfortunately, civil conflict is widespread. Since the end of World War II over half of all countries have suffered at least one incidence of civil conflict (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, 3-4).

In recent years, donors and governments have targeted an increasing amount of development aid to conflict-affected areas, some of it in the hope that aid will reduce conflict by weakening popular support for insurgent movements. The previous literature suggests two main mechanisms through which development aid might reduce conflict. First, successful development projects may increase the population's support for the government, making individuals more willing to supply the government with intelligence on insurgents' plans and whereabouts. This would make it harder for insurgents to launch attacks, leading to a reduction in violence (Berman, Shapiro, and Felter 2008). Second, development projects may increase individuals' economic opportunities, making them less likely to join insurgent groups and participate in conflict (Collier and Hoeffler 2004). Some recent empirical evidence suggests that conflict can indeed be reduced through these mechanisms. Berman, Shapiro, and Felter (2008)show that increased spending on reconstruction programs was correlated with reductions in violence against coalition forces in Iraq. Miguel, Satyanath, and Sergenti (2004) find that positive shocks to economic growth - in the form of good rainfall - caused a decrease in conflict in Africa. Dube and Vargas (2007) show that increases in world market prices of agricultural goods reduced conflict in Colombia, presumably because they lead to increases in the return to peaceful economic activities.

Based on these empirical findings, one might expect that development projects can reduce conflict, either by winning the "hearts and minds" of the population or by increasing individuals' returns to peaceful activities.

This paper offers a simple but frequently overlooked explanation for why the opposite can be the case: if insurgents know that development projects will weaken their position, they have an incentive to oppose them, which may exacerbate conflict. This hypothesis is supported by a large body of anecdotal evidence which suggests that insurgents in many countries frequently attack aid workers and infrastructure projects. A recent report on civil counterinsurgency strategies[1] warns that "insurgents strategically target government efforts to win over the population. Indeed, the frequency with which insurgents attack schools, government offices, courthouses, pipelines, electric grids, and the like is evidence that civil [counterinsurgency] threatens them" (Gompert, Kelly, Lawson, Parker, and Colloton 2009).

To formalize the mechanism behind these anecdotes, this paper develops a simple theoretical model of bargaining and conflict around development projects. Based on the work of Fearon (1995) and (Powell 2004; Powell 2006), the model shows that development projects can cause conflict if (1) a successful project changes the future balance of power in favor of the government, (2) the insurgents have the ability to hinder the project's successful implementation by violent means, and (3) governments cannot commit to honoring agreements reached before the start of the project. The first two conditions ensure that the insurgents have an incentive to use violence to derail the project, the third condition ensures that governments cannot pay off insurgents in return for allowing the project's peaceful implementation. We explore this logic in more detail in Section 2.

To test our theoretical model, we estimate the causal effect of a large development program–the Philippines' KALAHI-CIDSS–on casualties in armed civil conflict. During the period 2003-08, KALAHI-CIDSS was the Philippines' flagship anti-poverty program with a budget of $180 million, financed through a loan from the World Bank. Two fundamental challenges have limited previous efforts to identify the relationship between development aid and conflict. First, aid allocation is usually non-random, making it hard to pin down the direction of causality. This is especially problematic in conflict-affected areas because aid assignment could either be positively or negatively correlated with unobserved determinants of conflict. On the one hand, agencies that allocate aid based on need are likely to target conflict-affected areas, since these are home to the poorest and most vulnerable populations. On the other, development agencies might assign programs to more peaceful areas out of concern about the safety of their staff. Second, lack

[1] The term civil counterinsurgency is used to describe efforts to weaken popular support for insurgent movements by raising the living-standards of the population.

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.[2]

Our empirical analysis addresses these challenges and contributes to the literature in two ways. First, we employ a Regression Discontinuity Design (RDD) to cleanly identify the causal effect of the KALAHI-CIDSS program on conflict violence. Eligibility for the program was restricted to the poorest 25 percent of municipalities in participating Philippine provinces[3]. This eligibility threshold created a discontinuity in the assignment of aid, which allows us to identify its causal effect by comparing municipalities just above and just below the threshold.[4] Our second contribution lies in the scope and precision of our data, which provide information on all conflict incidents that involved units of the Armed Forces of the Philippines (AFP) between 2001 and 2008. The data 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.[5]

Consistent with the predictions of our model, our empirical analysis finds that the KALAHI-CIDSS program exacerbated violent conflict in eligible municipalities. Municipalities just above the eligibility threshold suffered a large and statistically significant increase in violence compared to municipalities just below the threshold. This effect cannot be explained by differences in pre-program violence or other observable characteristics. Our model is further supported by the finding that the increase in violence only lasted for the duration of the program - while insurgent attacks could still affect its success - and was stronger for municipalities that received larger amounts of aid. The majority of casualties was suffered by insurgents and government troops, while civilians appear to have suffered less. We further find that the program caused similar increases in violence initiated

[2]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.

[3]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.

[4]See Imbens and Lemieux (2008) for a primer on the theory and practice of Regression Discontinuity Designs.

[5]For a fuller description of this dataset, see Felter 2005.

by insurgents and government troops, suggesting that the effect is not the result of a one-sided offensive by either party.

The remainder of the paper proceeds as follows. In Section 2, we develop a formal theoretical model of bargaining and conflict in the context of development projects. In Section 3, we give brief overviews of conflict in the Philippines and of the KALAHI-CIDSS program. Section 4 contains a description of the data and of the empirical strategy we use to identify the causal effect of the KALAHI-CIDSS program on conflict. In Section 5, we present our main empirical results and the results of robustness tests. Section 6 concludes by highlighting the implications of our results for future research and policy.

2 Development Projects, Bargaining and Conflict: A Model

This section develops a simple theoretical model of bargaining between an insurgent organization and a local government in a municipality that is scheduled to receive aid in the form of a development project. The model draws heavily on the work of Fearon (1995) and Powell (2004), Powell (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 that we analyze empirically - 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 2010 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[6], so that a bargaining model is well suited to describe the context of our empirical analysis.

The intuition behind the model is the following: The central government, in our case the Philippines' 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

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

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, Shapiro, and Felter 2008): 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 launch successful attacks. Another possible mechanism is that a successful project will decrease poverty and increase returns to peaceful economic activities, making it harder for insurgents to find recruits (this mechanism is suggested by the model of Dal Bo and Dal Bo forthcoming, and the empirical findings of Dube and Vargas (2007). Regardless of the precise mechanism, insurgents are aware that a successful project 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 project's implementation. This alone would not be enough to explain an increase in violence if the government could pay off insurgents in return for allowing the project's peaceful implementation. However, this may not be possible since the government cannot credibly commit to honoring bargaining agreements reached before the project's start. Since a successful project increases the government's bargaining power, it will have an incentive to renege on any existing agreement after the project is completed, in order to reach an agreement with more favorable terms. As described by Fearon (1995) and Powell (2004), this inability to credibly commit to a bargaining agreement can lead to conflict by making it impossible to reach a mutually acceptable agreement before the project's start.

To describe the mechanism behind this intuition more formally, consider 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 this demand, it pays the transfer and receives a payoff of $-m_t$ from the current round. If the government rejects the demand, the insurgents launch attacks on government facilities (these could include but are not limited to individuals and infrastructure associated with the project). These attacks are 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 implicitly 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, anecdotal evidence suggests that insurgent organizations are often able to follow up their threats with violent attacks when extorting individuals and companies (e.g. (Lobrigo, Imperial, and Rafer 2005); (Holden and Jacobson 2007).

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 attacking project staff and 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. In four initially eligible municipalities, insurgent attacks caused the program's implementing agency to abort implementation due to concerns about the safety of its staff[7]. In other municipalities, insurgents attacked construction work that was being funded through the project (DSWD 2009). For the purpose of the model, we define p^c as the probability that the project is successful if conflict occurs in period *1* and p^p as the probability that the project is successful if there is no conflict in period *1.* To keep things simple, we do not model the precise mechanism through which conflict affects the project's implementation, but merely assume that $p^c \leq p^p$.

The model's second key assumption is that the government's cost of conflict in later rounds depends on whether the program was successfully implemented. We thus define the government's cost of conflict in later periods as $c_t(K)$, where $K = 1$ if the project was successful and $K = 0$ if it failed. 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 may 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 (Berman, Shapiro, and Felter 2008). Second, a successful project may decrease poverty and increase the return to peaceful activities, making it harder for insurgents to find

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

recruits to carry out risky attacks (Dal Bo and Dal Bo forthcoming; Dube and Vargas 2007). 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 a successful project reduces the government's cost of conflict.

The model can be solved by backward induction. In periods *2* to N, conflict does not affect the government's future cost of conflict and the government 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 from rounds *2* to N is $C(K) = \sum_{t=2}^{N} \beta^{t-1} c_t(K)$, and the government's payoff is $-C(K)$, where β is the common discount factor.

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

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

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 highest demand it is willing to accept is:

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

The first term on the right-hand side, c_1, is the highest demand 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

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

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

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

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

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

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

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

If this condition fails, conflict will occur since 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, $C\left(\cdot\right)$. If the project has no effect on $C\left(\cdot\right)$, 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, 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 bargaining conditions also shows 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 probability of success. As a consequence, the model predicts that conflict only lasts as long as it affects the project's probability of success, which is only in round 1. In later rounds, after the project has been implemented (or failed), neither party has an incentive to engage in conflict, so that bargaining is always successful. A related prediction is that being eligible for a project can lead to conflict even if the project is eventually 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 the project. If the government could fix its second-round offer in the first round, the game would collapse into a single-round bargaining game with a peaceful outcome. 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 2008). 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 that other models make about the effect of development projects on conflict and compare these predictions to those of our model.

The "hearts and minds" model of conflict, as described by Berman, Shapiro, and Felter (2008), 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 (forthcoming), 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 activity, the authors find that increases in the returns to labor cause a decrease in conflict, because fewer individuals are willing to participate in conflict, making it harder for insurgents to find recruits. On the other hand, increases in the returns to capital 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 predict that a development project's effect on conflict materializes only after the project has started and persists as long as the project affects the economy. These predictions differ from those of our model, which predicts that a development project can cause conflict even before its implementation has begun and that conflict only lasts as long as it is being implemented. Our model follows those of Berman, Shapiro, and Felter (2008), and Dal Bo and Dal Bo (forthcoming), in 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 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.

Finally, models of bargaining with asymmetric information make predictions that are similar to those of our model. Suppose, for example, that local governments know the exact benefits they will receive from successfully implementing a development project while the insurgents do not. This means that insurgents do not know the government's willingness to pay to avoid conflict, which may make it optimal to make demands that are rejected with positive probability. 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 (1991). Translated to the present context, their results suggest that asymmetric information can cause conflict over the implementation of development projects, but that conflict decreases over time as insurgents learn about the government's true willingness to pay to avoid conflict. While these predictions are similar to those of our model–conflict initially increases in municipalities eligible for a development project, but returns to baseline levels over time - we believe that asymmetric information models cannot plausibly explain conflict around development projects, especially in countries with long-running conflicts. For example, insurgents in the Philippines have been attacking road construction 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, the information asymmetry should disappear over time as insurgents learn about the government's willingness to pay to avoid attacks. The fact that insurgents keep attacking infrastructure projects even after years of conflict is difficult to explain by asymmetric information and suggests that a different mechanism is at work.

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 we study in this paper, 2001-2008, the two largest insurgent organizations active in the country were the New People's Army (NPA) and the Moro Islamic Liberation Front (MILF). Below we briefly describe these organizations and a third category of insurgents, the so-called "lawless elements".

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 estimated at 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). The NPA operates mostly in rural areas - its military strategy relies on small guerrilla fronts that are deployed in and around villages. Due to its guerilla tactics and lack of a broad ethnic or religious constituency, the NPA's activities require significant support from the Philippines' rural poor who supply most of the group's recruits and logistical support[8]. 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 in our data for which the 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, due to disagreement about the means by which to pursue independence. After the split, the MILF escalated armed conflict against the government while the MNLF signed a peace accord in 1996 that created the Autonomous Region of Muslim Mindanao (ARRM). The MILF's core grievances stem from government efforts to re-title lands

[8]Chapman (1987) and Jones (1989) provide detailed histories of the NPA. For an insider perspective on the war from an AFP officer's perspective, see Corpus (1989). From an NPA leadership perspective, see Sison and Werning (1989).

considered by the southern Muslim population to be part of their ancestral homeland and the group reportedly enjoys broad support in the Muslim population. (Kreuzer and Werning 2007).

With an estimated 10,500 fighters under arms, the MILF is larger than the NPA. Furthermore, its tactics are more manpower intensive. 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). 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.

3.1.3 Lawless Elements (LE)

The term "lawless elements" refers to small, loosely-allied bands of guerrilla and criminal groups operating across the Philippines. Some of these groups are local manifestations of the NPA, the MILF, or the Abu Sayyaf Group (ASG)[9]. Many others are criminal organizations that employ guerrilla-like tactics but use violence primarily as part of criminal activities such as kidnapping-for-ransom rather than to pursue political objectives. During the period 2001-2008, Lawless Elements were involved in roughly 25% of all conflict incidents involving AFP units

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.[10] Plans to expand KALAHI-CIDSS are currently being made, with the aim of doubling 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

[9]The ASG, a high profile southern Philippine terrorist organization with well established links to al-Qaeda, does not figure in our analysis because the provinces in which the ASG operates are not eligible for KALAHI-CIDSS.

[10]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 National Statistical Coordination Board - Standard Geographic Codes.

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.[11]

KALAHI-CIDSS follows a standard CDD template. First, each participating municipality receives a block grant for small-scale infrastructure projects. Within the municipality, each village *(barangay* in Tagalog) 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. Once funding has been allocated, community members are encouraged to monitor or participate in project implementation.[12]

The amount of aid distributed through KALAHI-CIDSS is substantial. Participating municipalities receive PhP300,000, or approximately \$6000, per village in their municipality. The average municipality has approximately 25 villages, making the average grant 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 anti-poverty program that would target aid to the poorest populations in the Philippines. Aid was targeted following a two-stage 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 estimate municipal poverty levels using data from the 2000 National Census,

[11]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.

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

the Family Income and Expenditure Survey (FIES) and rural accessibility surveys (Balisacan et al. 2002; Balisacan and Edillon 2003). Poverty levels were estimated following the poverty mapping method of Elbers et al (2003).[13] The first step of this method is to estimate the relationship between measures of household expenditure, which are only available for a subset of municipalities, and variables from census data and accessibility surveys, which are available for all municipalities. 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. Based on the estimated poverty levels, only the poorest 25% of municipalities within each participating province were eligible for aid from 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

[Insert 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 phases with 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).[14] 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).

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

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

3.2.3 Eligibility and 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 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. In some other cases, initially eligible municipalities were dropped from the program because of concerns about the security of program staff[15].

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 poverty mapping methodology based on a combination of data from FIES and the 2000 Census of the Philippines (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

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

conflict, so that any change in conflict across the threshold can be attributed to the KALAHI-CIDSS program[16].

The running variable in our RD regressions is the distance of the municipality's poverty rank from the provincial eligibility threshold[17]. 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[18]. 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 municipality has a normalized poverty rank of one. 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, X_i is the municipality's normalized poverty rank and c is the threshold that determines 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[19]

[Figure 2 about here]

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

[17]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.

[18]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.

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

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. 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.

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 casualties 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 agreement 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.[20]

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 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

[20]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.

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.[21] *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.[22]

As additional controls, we include municipality-level pre-treatment de-

[21]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.

[22]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 2006). 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.

mographic 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.[23] The second is a 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.[24]

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[25] 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.

[23]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*.

[24]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.

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

5 Results

In this section, we present results from the regression discontinuity approach based on the poverty threshold that determined eligibility for the Philippines' KALAHI-CIDSS program. As mentioned above, aid from 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). Next, 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. Our regression discontinuity design compares eligible and ineligible municipalities close to the eligibility threshold to estimate the causal effect of the KALAHI-CIDSS program. For most of our analysis, we restrict the sample to contain only the four municipalities closest to the eligibility threshold in each participating province: the two richest municipality that are still eligible for the program and the two poorest that are not. In the language of regression discontinuity design, our running variable is the distance from the provincial eligibility threshold and we choose a rectangular kernel with a bandwidth of two ranks. This restriction makes sure that the identifying assumption of the regression discontinuity design holds: that eligible and ineligible municipalities within our sample do not differ on observed and unobserved characteristics, except for program eligibility. To test this assumption we present evidence 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 casualties 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 present results of negative binomial and linear regressions that exploit the discontinuity described above. In addition to the comparison of eligible and ineligible municipalities across the threshold, the regressions exploit the timing of the program's implementation. Specifically, the program's causal effect is estimated as the "double-difference" of casualties in eligible and ineligible municipalities, before and after the roll-out of KALAHI-CIDSS. As is standard for the regression discontinuity design, all regressions control for the running variable, which in our case is the distance from the eligibility threshold in ranks. All standard errors are adjusted for clustering at the province level. To test the robustness of our results to the choice of bandwidth, we also present estimates based on larger and smaller bandwidths.

5.1 Summary Statistics and Balance Tests

[Table 3 about here]

To get an idea of the intensity of conflict in the Philippines in the period of observation, 2001-08, Table 3 reports summary statistics of some conflict outcomes. The first column reports average outcomes per municipality per year for the whole of the Philippines, the second column reports outcomes for our restricted sample of municipalities within two ranks of the eligibility threshold for KALAHI-CIDSS.

[Table 4 about here]

As a robustness test of the identifying assumption of the regression discontinuity design, Table 4 compares observable variables of eligible and ineligible municipalities in our restricted sample. All of these variables were measured in 2000 or 2001, at least two years before the start of the KALAHI-CIDSS program. Significant differences between eligible and ineligible municipalities would point to a violation of the identifying assumption that no unobserved variable changes discontinuously across the eligibility threshold. To test this, we conduct t-tests for equality of means of eligible and ineligible municipalities in our sample. The results show that none of the variables are significantly different at the 10% level, which increases our confidence in the identifying assumption of the RD design. 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 The Effect of KALAHI-CIDSS on Conflict Casualties

5.2.1 Graphical Evidence

[Figure 3 about here]

Figure 3 displays the time trend of conflict casualties in eligible and ineligible municipalities within our restricted sample of municipalities within two ranks of 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 regression. 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 period after the program's start, 2003-2008. The figure shows that eligible and ineligible municipalities experienced similar numbers of conflict casualties in the pre-program period. However, the number of casualties in eligible municipalities increased sharply in January 2003 - the first full month after the first announcement of eligibility for KALAHI-CIDSS - while the number of casualties in ineligible municipalities remained virtually unchanged. The difference in casualties between eligible and ineligible municipalities then becomes smaller but increases again around in the second half of 2005, close to the start of KALAHI-CIDSS Phases IIIB and IV.

5.2.2 Quantitative Evidence

[Table 5 about here]

Table 5 presents regression estimates of the effect of KALAHI-CIDSS on conflict casualties. The estimated causal effect of eligibility for 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 program a municipality was covered. For municipalities covered in Group A (Phases 1 and 2), the program period is 2003-2005, since implementation began in 2003. For municipalities in Phase Group B, the case is slightly more complicated. Implementation in Phase IIIA began in 2004, so that the program period for municipalities covered in that phase is 2004-2006. For the remaining municipalities, implementation began in 2006, so that 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 IIIA, while the remaining 13 (87%) participated in phases IIIB and IV. We thus assume that the 6 non-participating but eligible municipalities would have participated in phase IIIA with a probability of 13% and in phases IIIB or IV with a probability of 87%. To these municipalities we therefore 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 IIIA and a value of 0.87 for the years 2007/08 when implementation was only ongoing in phases IIIB and IV. For the year 2006, we assign a value 1 since implementation was ongoing in all participating municipalities in Group B.

Columns 1-3 of Table 5 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 the interaction of Eligible and Program) ranges from 0.66 to 0.93 in the negative binomial regressions. The effect is strongly statistically significant and robust to the inclusion of municipality fixed effects and clustering of standard errors at the province level. Since in the negative binomial regression the mean is 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, according to our preferred fixed effects specification, eligibility for KALAHI-CIDSS caused a 90% increase in the number of conflict casualties, which is clearly a large effect relative to the baseline level of violence. In absolute terms, municipalities barely eligible for the program were likely to experience approximately 0.9 more conflict-related casualty per year than similar municipalities that narrowly missed the cutoff for eligibility. This means that, over the three-year program period, an eligible municipality experienced close to 3 additional casualties. Assuming a constant treatment effect, the 182 municipalities that received KALAHI-CIDSS experienced almost 500 excess casualties. Given that leading datasets 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 effect only persists as long as the project is being implemented. The point estimates of the effect in the post-program period (the coefficient associated with the interaction of Eligible and Post-Program) are much smaller than the effect during the program-period. In the models that include control variables 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 only increases while the project is ongoing, since that is when insurgents can still hinder the project's successful implementation. The results of the fixed effects linear regression in column 4 demonstrate that robustness of our estimation to different assumptions about the functional form.

5.3 Robustness Tests

5.3.1 Balance on Pre-Treatment Violence

The crucial identifying assumption of the Regression Discontinuity design is that municipalities on both sides of the eligibility threshold do not differ in unobserved variables that determine the intensity of conflict. This assumption might fail if the poverty mapping exercise that determined eligibility was manipulated in order to target the program to municipalities with higher or lower levels of pre-program conflict. 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. While we are not aware of any anecdotal evidence of manipulation of the poverty mapping exercise, we present two pieces of evidence in support of the identifying assumption.

First, Figure 3 shows that eligible and ineligible municipalities experienced similar levels of conflict before eligibility for KALAHI-CIDSS was announced. If eligible and ineligible municipalities differed in unobserved characteristics that determine conflict, we would expect them to experience different levels of violence before the program was announced, which does not appear to be the case. To the contrary, the fact that the increase in conflict in eligible municipalities coincides exactly with the start of the program's roll-out 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 5. In Columbus 1 and 2, the coefficient associated with "Eligible" is an estimate of the difference in annual casualties in eligible and ineligible municipalities before the start of the program. The estimates show that the difference is not statistically significant and close to zero. 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.

5.3.2 Robustness to Choice of Bandwidth

[Table 6 about here]

We now test the robustness of our results to the choice of bandwidth. Our baseline estimates are based on a bandwidth of 2, meaning that the sample only included municipalities within two ranks of the provincial eligibility threshold (i.e. the richest two municipalities that were still eligible and the poorest two that were ineligible). Table 6 shows results of regressions based on bandwidths of 1 and 3. Overall, the estimates of the program's effect are very robust to changes in the bandwidth. The point estimates range between 0.73 and 1.07, which is comparable to the baseline results which ranged between 0.66 and 0.93, and are statistically significant at the 5% level. Thus, it does not appear that our estimates are strongly influenced by the choice of bandwidth.

5.3.3 Robustness to Outliers

[Table 7 about here]

One concern is that our results are driven by a small number of observations with very large numbers of casualties. To rule this out, Table 7 reports the results of Probit regressions of the probability of having any casualties at all in a given year. The estimated marginal effect of KALAHI-CIDSS on the probability of having any conflict casualties is approximately 13 percentage points, which is large compared to the observed probability of 29%. To rule out that our results are driven by a small number of incidents with a very high number of casualties, Table 8 reports the results of regressions that use the number of violent incidents (incidents with at least one casualty) as the dependent variable. The results of the negative binomial regressions in columns 1-3 show that eligibility for KALAHI-CIDSS increases the number of violent incidents by between 37 and 64 percentage points, though the estimate in the fixed effects specification is only statistically significant at the 10% level. Overall, the results in Table 7 lead us to conclude that KALAHI-CIDSS affects the probability as well as the intensity of conflict and that this result is not entirely driven by a small number of municipalities that experience severe conflict, or a small number of severe incidents.

5.4 The Effect of Project Size on Conflict Casualties

[Table 8 about here]

We now analyze the relationship between a project's size and its effect on conflict. To do this, we exploit the fact that the amount of aid an eligible municipality received was a function of the number of villages (called *barangays* in Tagalog) it contains[26]. As mentioned in Section 3, the amount of aid an eligible municipality received from KALAHI-CIDSS was determined by multiplying its number of villages by PhP300,000 (about US$6000). This means that municipalities with more villages received larger amounts of aid. Assuming that larger amounts of aid cause a larger (potential) shift in power between the government and insurgents, our model predicts that eligible municipalities with many villages will experience a larger increase in conflict than municipalities with few villages. The regressions reported in Table 8 test this hypothesis by including an interaction between eligibility, the program time-period and the number of villages in the municipality. Of course, the number of villages is likely to be correlated with both area and population, which might also affect the size of the program's effect on conflict. To control for this, we also include interactions between eligibility, the program time-period and the municipality's area and population (both linear and squared), as well as its population density. The results suggest that holding area and population constant, municipalities with a larger number of villages experienced a larger increase in conflict from being eligible for KALAHI-CIDSS. The point estimates are in the range of 0.04 to 0.05 suggest that having an additional village - which increased the grant size by PhP300,000, or US$6000 - increased the project's effect on casualties by approximately 4 to 5 percentage points. This result suggests that larger grants caused more conflict, which is consistent with our model's prediction that conflict is more likely when the (potential) shift in power between government's and insurgents is large.

5.5 Who Suffers and Who Initiates the Violence?

[Table 9 about here]

Table 9 reports estimated effects of KALAHI-CIDSS on casualties from the three groups involved in the conflict: Government, insurgents and civilians. The results show that all groups suffered more casualties in KALAHI-eligible municipalities than in the similar ineligible municipalities. The largest effect is on insurgents, who suffered an approximate increase of 113% from a baseline of 0.31 casualties. At approximately 63%, the estimated effect is

[26]Barangays are the smallest administrative unit in the Philippines, with an average of 25 barangays per municipality

smaller for government troops. However, government troops suffer a higher number of casualties on average, so that the absolute effect is almost as large as for insurgent casualties. Civilians appear to suffer fewer casualties, both overall and as a result of the project. 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 and civilians are not directly involved.

The results in Table 9 also show which group, government or insurgents, initiates the violence caused by KALAHI-CIDSS. Our theoretical model of bargaining and conflict does not make predictions about this - it simply states that conflict occurs if both parties cannot agree on a peaceful bargaining solution and remains agnostic about 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 causes an increase in violence originating from both groups. The increase of insurgent-initiated violence is slightly larger than that of government-initiated violence, but the difference is fairly small. Overall, the results suggest that the violence around the KALAHI-CIDSS program was not the result of a one-sided offensive by either the government or the insurgents

6 Conclusion

In recent years, donors and governments have targeted an increasing amount of development aid to areas affected by civil conflict, some of it in the hope that aid will reduce conflict by weakening popular support for insurgent movements. This paper has presented a simple mechanism through which development aid can have the unintended effect of *increasing* conflict: If a successful project will weaken the position of insurgent groups in the future, they have an incentive to oppose it, which may exacerbate conflict. To analyze this mechanism, we have developed a theoretical model of bargaining and conflict in the context of development projects. The model predicts that development projects can cause conflict if (1) a successful project changes the future balance of power in favor of the government, (2) the insurgents have the ability to hinder the project's successful implementation by violent means, and (3) governments cannot commit to honoring agreements reached before the start of the project. The first two conditions ensure that the insurgents have an incentive to use violence to hinder the project's implementation, the third condition ensures that governments cannot pay off insurgents in return for allowing the project's peaceful implementation.

Our empirical analysis tests the model by estimating the causal effect of a large development program - called KALAHI-CIDSS - on conflict casualties in the Philippines. During the period 2003-08, KALAHI-CIDSS was the Philippines' flagship anti-poverty program with a budget of $180 million, financed through a loan from the World Bank. To overcome the problem of endogenous targeting of aid, we employ a regression discontinuity design that compares municipalities just above and just below the poverty threshold that determined eligibility for the program. Using detailed data on all conflict incidents involving the Armed Forces of the Philippines between 2001 and 2008, our estimates show that eligibility for KALAHI-CIDSS caused a large and statistically significant increase in conflict casualties. Consistent with the predictions of our model, this effect only persists for the duration of the program - while insurgents can still hinder its implementation - and is stronger for municipalities that received larger amounts of aid. We further find that the majority of casualties was suffered by insurgents and government troops, while civilians appear to have suffered less. Eligible municipalities experienced a similar increase in the number of casualties in insurgent-initiated and government-initiated attacks, suggesting that the effect is not due to a one-sided offensive by either party.

Our results have implications for future research and policy. First, they highlight the potential pitfalls of extrapolating from the effect of natural experiments (in the sense of largely uncontrollable phenomena like rainfall and world-market prices) to the effect of local human interventions. Since conflict results from a strategic interaction between (at least) two parties, interventions that can be influenced by either party can have very different effects from interventions that are truly exogenous. For example, the recent literature suggests that shocks to world-market prices of agricultural goods reduce conflict by increasing the population's return to peaceful activities (Dube and Vargas 2007). Based on this finding, it is tempting to conclude that development projects that increase people's economic opportunities will have a similarly conflict-reducing effect. However, while insurgents cannot affect world-market prices, they *can* use violence to sabotage development projects. Development projects can therefore cause an increase in conflict violence; and our theoretical model outlines the conditions under which they are most likely to do so.

How this insight affects the targeting and design of development projects depends on the projects' goals. If a project's main goal is to reduce poverty and alleviate the suffering of populations in conflict-affected areas, one way of avoiding conflict is to make sure that the project does not affect the balance of power between governments and insurgents. One possible way of doing this is to cooperate with both governments and insurgents in designing the project and delivering the aid. (an example of this is the recent collaboration of Japan's development agency JICA with the MILF in extending aid to parts of Mindanao in the southern Philippines). If the project's goal is to reduce conflict by weakening insurgents, one way of reducing violence is to focus aid on a smaller number of projects but heavily defending these. This would ensure that insurgents have less ability to sabotage project (and face higher costs if they do), which should help deter violent attacks. To ensure that projects are implemented successfully, it may also be desirable to weaken insurgent capacity before the start of the project by military means, following a "clear, hold, build" strategy (2007). Of course, these policy conclusions are speculative, since they are derived from theory and have not been tested empirically. We hope that future research will be able to test these implications of our model and will help design development interventions that can operate in conflict-affected areas without exacerbating violent conflict.

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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: Summary Statistics of Conflict Outcomes, 2001-08

	All Philippines	RD Sample
Incidents	1.41	2.18
	(3.92)	(3.72)
Violent incidents (casualties > 0)	0.30	0.49
	(0.92)	(1.00)
Casualties	0.66	0.97
	(3.16)	(2.57)
AFP casualties	0.32	0.49
	(1.96)	(1.62)
Insurgent casualties	0.20	0.31
	(1.16)	(1.15)
Civilian casualties	0.17	0.20
	(1.47)	(0.89)
Cas. in AFP initiated inc.	0.28	0.40
	(1.89)	(1.51)
Cas. in insurgent initiated inc.	0.38	0.56
	(2.03)	(1.80)
Observations	14650	1285
Municipalities	1632	160

Reported values are means, standard deviations are in parenthesis

Table 4: Balance of Observed Variables Across Eligibility Threshold

	Eligible	Ineligible	Difference
Population ('000)	30.2	28.56	1.6
			(2.9)
Area (km^2)	0.029	0.036	$7.8{\times}10^{-3}$
			($5.2{\times}10^{-3}$)
Highway Access (%)	69.1	67.2	1.9
			(4.7)
Forest (km^2)	$8.4{\times}10^{-3}$	$8.2{\times}10^{-3}$	$2{\times}10^{-4}$
			($2.4{\times}10^{-3}$)
Affected by NPA in 2001 (%)	41.8	42.0	-0.2
			(7.9)
Percent Muslim	3.2	4.1	-0.9
			(2.2)
Ethnic fractionalization	0.32	0.29	0.03
			(0.05)
Religious fractionalization	0.32	0.30	0.03
			(0.04)
Municipalities	81	79	160

Table 5: The Effect of KALAHI-CIDSS on Conflict Casualties

	Negative Binomial Regression			OLS
Outcome: Conflict Casualties (/Year)	(1)	(2)	(3)	(4)
Eligible*Program	0.91***	0.66**	0.93***	1.31***
	(0.26)	(0.31)	(0.30)	(0.44)
Eligible*Post-Program	0.45	-0.28	-0.09	-0.04
	(0.55)	(0.46)	(0.55)	(0.56)
Eligible	0.08	0.07		
	(0.23)	(0.22)		
Population (/1000)		0.024***		
		(0.007)		
Area (kmˆ2)		5.9*		
		(3.1)		
Pct. Barangays with Highway Acc.		-0.40		
		(0.40)		
Timber		0.068		
		(0.065)		
Affected by NPA in 2001		1.05***		
		(0.27)		
Ethnic Fractionalization		1.76***		
		(0.41)		
Religious Fractionalization		-0.77		
		(0.67)		
Percent Muslim Population		1.93***		
		(0.51)		
Constant	-0.36	1.5		
	(0.23)	(5.9)		
Additional Controls	No	Yes	Yes	Yes
Municipality Fixed Effects	No	No	Yes	Yes
Observations	1285	1285	1285	1285

Note: Robust standard errors in parentheses. Standard errors are clustered at the province level. The sample is restricted to municipalities within 2 ranks of the provincial eligibility threshold for KALAHI-CIDSS. All regressions control for the running variable (distance from threshold in ranks), fully interacted with eligibility and the project and post-project time-periods. 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 6: Robustness Tests: Choice of Bandwidth

	Negative Binomial Regressions on Casualties (/year)					
	Bandwidth = 3 ranks			Bandwidth = 1 rank		
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible*Project	0.97***	0.91***	0.73**	1.06***	1.07***	0.90***
	(0.22)	(0.30)	(0.29)	(0.22)	(0.29)	(0.31)
Eligible*Post-Project	0.41	0.19	-0.17	0.62	0.13	-0.09
	(0.40)	(0.34)	(0.43)	(0.41)	(0.38)	(0.41)
Eligible	0.05	-0.08		-0.14	-0.17	
	(0.17)	(0.43)		(0.27)	(0.39)	
Constant	-0.48			0.15		
	(0.35)			(0.28)		
Controls	No	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	No	No	Yes	Yes	No	Yes
Observations	1865	1865	1865	657	657	657

Note: Robust standard errors in parentheses. Standard errors are clustered at the province level. All regressions control for the running variable (distance from threshold in ranks), fully interacted with eligibility and the project and post-project time-periods. All regressions include year fixed effects. Asterisks denote statistical significance at the 1% (***), 5% (**) and 10% (*) levels. Control variables are the same as in Table 4.

Table 7: Robustness Tests: Robustness to Outliers

	Probability of cas.>0		Number of incidents with cas.>0		
	(1)	(2)	(3)	(4)	(5)
Eligible*Program	0.123*	0.132**	0.64**	0.60***	0.37*
	(0.069)	(0.062)	(0.26)	(0.19)	(0.21)
Eligible*Post-Program	0.071	-0.050	0.44	0.23	-0.27
	(0.125)	(0.087)	(0.52)	(0.44)	(0.38)
Eligible	-0.019	-0.039	-0.19	-0.07	
	(0.096)	(0.11)	(0.49)	(0.45)	
Mean	0.29		0.49		
Controls	No	Yes	No	Yes	Yes
Municipality FE	No	No	No	No	Yes
Observations	1285	1285	1285	1285	1285

Note: Robust standard errors in parentheses. Standard errors are clustered at the province level. The sample is restricted to municipalities within 2 ranks of the provincial eligibility threshold for KALAHI-CIDSS. All regressions control for the running variable (distance from threshold in ranks), fully interacted with eligibility and the project and post-project time-periods. All regressions include year fixed effects. In columns (1) and (2), reported values are marginal effects. Asterisks denote statistical significance of the underlying coefficient at the 1% (***), 5% (**) and 10% (*) levels. Control variables are the same as in previous tables.

Table 8: Effect of Project Size on Casualties

	Negative Binomial Regressions on Casualties (/year)		
	(1)	(2)	(3)
Eligible*Project	0.43	0.78	0.85
	(0.59)	(0.83)	(0.96)
Eligible * Project * # of villages	0.041**	0.047**	0.047**
	(0.019)	(0.019)	(0.019)
Eligible * Project * area	2.80	8.0	5.5
	(3.10)	(11.5)	(19.2)
Eligible * Project * area squared		-24.4	-15.4
		(56.7)	(77.4)
Eligible * Project * population	-0.019	-0.052	0.049
	(0.012)	(0.040)	(0.045)
Eligible * Project * pop. squared		3.5×10^{-7}	3.3×10^{-7}
		(3.6×10^{-7})	(3.9×10^{-7})
Eligible * Project * pop. density			-5.3×10^{-5}
			(3.3×10^{-4})
Eligible*Post-Project	-0.24	-0.22	-0.17
	(0.40)	(0.40)	(0.43)
Municipality Fixed Effects	Yes	Yes	Yes
Observations	1285	1285	1285

Note: Robust standard errors in parentheses. Standard errors are clustered at the province level. All regressions control for the running variable (distance from threshold in ranks), fully interacted with eligibility and the project and post-project time-periods. All regressions include year fixed effects. Asterisks denote statistical significance at the 1% (***), 5% (**) and 10% (*) levels. Control variables are the same as in Table 4.

Table 9: Who Suffers and Who Initiates the Violence? Conflict Casualties by Actor

	Parameters of Neg. Binom. Regression	Mean
Outcome:	(1)	(2)
AFP Casualties	0.61	0.49
	(0.39)	(0.05)
Insurgent Casualties	1.13**	0.31
	(0.45)	(0.03)
Civilian Casualties	0.57	0.20
	(0.51)	(0.02)
Cas. in AFP initiated inc.	0.75	0.40
	(0.58)	(0.04)
Cas. in insurgent initiated inc.	0.93**	0.56
	(0.40)	(0.05)
Control Variables	Yes	
Municipality Fixed Effects	Yes	
Observations	1285	1285

Note: Results in column 1 are parameter estimates associated with eligible*project in a fixed effects negative binomial regression. Robust standard errors in parenthesis, clustered at the province level. Results in column 2 are sample means. The sample is restricted to municipalities within 2 ranks of the provincial eligibility threshold for KALAHI-CIDSS. All regressions control for the running variable (distance from threshold in ranks), fully interacted with eligibility and the project and post-project time-periods. Asterisks denote statistical significance at the 1% (***), 5% (**) and 10% (*) levels.

Figure 1: Timeline of KALAHI-CIDSS Implementation

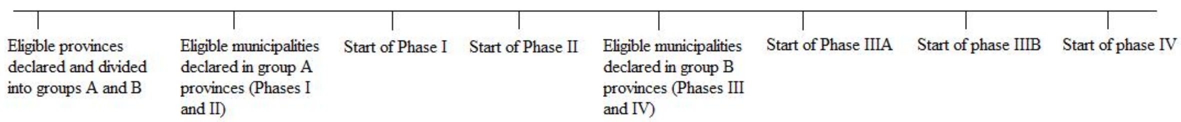


Figure 2: Probability of KALAHI-CIDSS Participation by Distance from Poverty Threshold

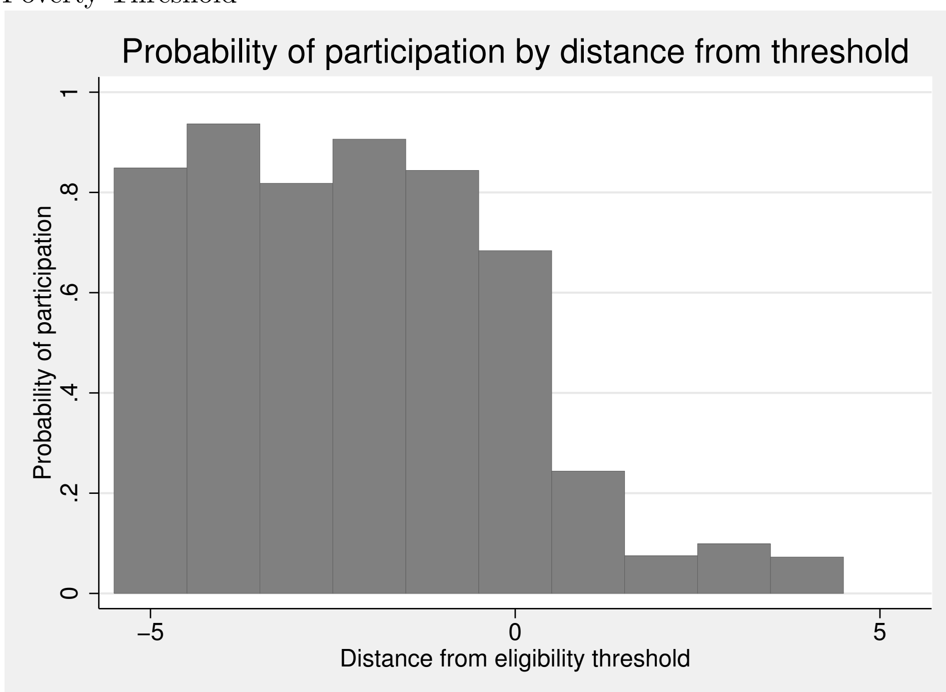
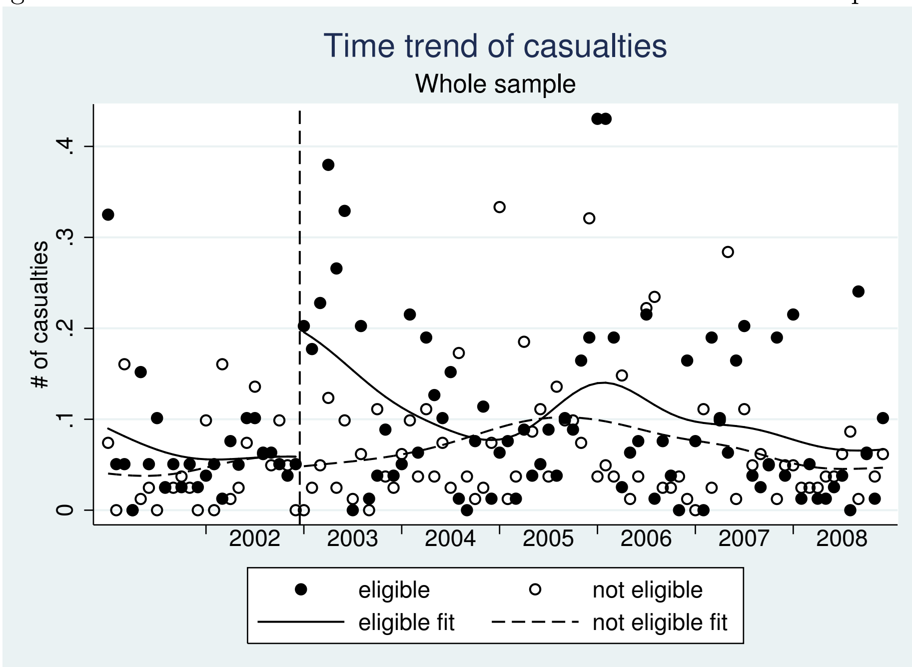


Figure 3: Time Trend of Casualties in Treatment and Control Municipalities



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