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Aid Under Fire: Development Projects and Civil Conflict†

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We estimate the causal effect of a large development program on conflict in the Philippines through a regression discontinuity design that exploits an arbitrary poverty threshold used to assign eligibility for the program. We find that barely eligible municipalities experienced a large increase in conflict casualties compared to barely ineligible ones. This increase is mostly due to insurgent-initiated incidents in the early stages of program preparation. Our results are consistent with the hypothesis that insurgents try to sabotage the program because its success would weaken their support in the population. (*JEL* D74, F35, I32, I38, O15, O17, O18, O19)

Over the past two decades, the world has seen enormous progress towards eradicating poverty. In 1990, 43 percent of people in developing countries lived in extreme poverty, defined as subsisting on incomes below $1 a day. By 2010, that number had fallen to 21 percent. Only one group of countries has bucked this trend: those affected by conflict. The estimated one-and-a-half billion people living in conflict-affected countries are substantially more likely to be undernourished, less likely to have access to clean water and education, and face higher rates of childhood mortality (World Bank 2012). Not a single low-income conflict-affected country has yet achieved the Millennium Development Goals.

Continued progress towards eradicating poverty will require that the substantial gains made elsewhere be extended to conflict-affected countries. To help achieve this, governments and multilateral donor organizations are increasingly directing development aid to conflict-affected countries worldwide. The prevailing view is that

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the effects of this aid are positive—it improves the welfare of recipients and can help reduce conflict and build peace (World Bank 2012; USAID 2007). However, some experts argue that aid can inadvertently fuel conflict and contribute to a vicious cycle of poverty and violence (Bryer and Cairns 1997; Goodhand 2002; Polman 2010).

To date, there is little empirical evidence on the effect of aid on conflict, and the existing evidence is mixed. Across countries, Nielsen et al. (2011) find that decreases in overall aid flows cause conflict in recipient countries, suggesting that aid can have a stabilizing effect. In a within-country analysis in Iraq, Berman, Shapiro, and Felter (2011) find that small-scale reconstruction projects funded by the US military led to fewer insurgent attacks, but no evidence that large-scale programs had a conflict-reducing effect. In contrast, Nunn and Qian (2014) find that US food aid *increased* conflict in recipient countries. The overall mixed evidence suggests that the effect of aid on conflict depends largely on the way in which aid is disbursed (Berman et al. 2013). In order to design aid interventions that do not exacerbate conflict—and perhaps even reduce it—it is therefore important to understand how and why different types of aid affect conflict.

This article improves our understanding of the effect of aid on conflict by estimating the causal effect of a large community-driven development (CDD) program—KALAHI-CIDSS, the Philippines' flagship development program—on civil conflict deaths. CDD programs have become one of the most popular tools for delivering development aid over the last decade. In 2012, the World Bank supported 400 CDD programs in 94 countries, valued at close to $30 billion (Wong 2012), including programs in conflict-affected countries such as Afghanistan, Indonesia, Thailand, East Timor, India, Peru, Colombia, Rwanda, and Sierra Leone.[1] Recent evidence suggests that CDD programs can promote social cohesion in post-conflict settings (Fearon, Humphreys, and Weinstein 2009), though the existence of overall positive effects of CDD on social capital and institutions is disputed (Casey, Glennerster, and Miguel 2012; Avdeenko and Gilligan 2013). Proponents of the CDD approach argue that its inclusive nature and focus on institutional change make it particularly well suited for conflict settings (Barron 2011; USAID 2007; World Bank 2006).[2] Skeptics warn that efforts to change historically rooted institutions may in fact lead to more conflict (Easterly 2006). Unfortunately, neither side has strong empirical evidence to back up its claims. Despite the widespread use of CDD programs in conflict-affected countries, little is known about CDD's impact on conflict.

We fill this gap in the literature by estimating the effect of the KALAHI-CIDSS program by a regression discontinuity (RD) design, which exploits the fact that eligibility for the program was restricted to the poorest 25 percent of municipalities in participating provinces. This eligibility threshold created a discontinuity in aid assignment, which allows us to identify its causal effect by comparing municipalities just above and just below the threshold. This empirical strategy allows us

[1] Over the entire past decade, the World Bank spent approximately $50 billion on CDD programs (Mansuri and Rao 2012), which make up around 10 percent of its lending portfolio (Barron 2011). CDD programs are also funded by many other large multilateral and bilateral donor agencies, including the Asian Development Bank, Inter-American Development Bank, USAID, JICA, and DFID.

[2] For example, the World Bank's website lists "ten reasons why the CDD approach makes sense in a conflict or post-conflict setting" (http://go.worldbank.org/D8VIYP0YQ0, retrieved April 2013).

to overcome the problem of endogenous aid allocation and cleanly identify the program's causal effect on conflict casualties.

In addition to contributing to the growing literature on the effects of CDD programs, we also contribute to the literature on aid and conflict more generally by exploring the mechanisms through which CDD affects conflict. We do this by exploiting unique, detailed conflict data from the Armed Forces of the Philippines (AFP) between 2002 and 2006. The data were compiled from the AFP's original conflict incident reports, which contain comprehensive information on all conflict incidents reported by units operating across the country. These data are similar to the US military's "Significant Activities" (SIGACTS) database, which scholars have used to advance the study of conflict based on US experiences in Afghanistan and Iraq (Berman, Shapiro, and Felter 2011; Iyengar, Monten, and Hanson 2011; Beath, Christia, and Enikolopov 2011). The detailed nature of the Philippine conflict data allows us to precisely estimate the timing of the program's effect over its implementation cycle and to separately estimate its effect on conflict with different insurgent organizations. It also allows us to estimate who among the conflicting parties (insurgents or government forces) suffers the excess casualties and who initiates the violence. Taken together, this detailed evidence sheds new light on the mechanisms that link aid and conflict, which may eventually help design more effective aid interventions that alleviate poverty without exacerbating conflict.

Our results strongly suggest that KALAHI-CIDSS led to an increase in violent conflict. After the program's start, barely eligible municipalities experienced a large and statistically significant increase in casualties compared to barely ineligible ones. We find no differences in preprogram casualties or other observable characteristics that could explain this effect. Further results show that the effect was concentrated in the early stages of program preparation, before funds were disbursed and before eligible municipalities had committed to participating. The effect was strongest for casualties suffered by government forces as a result of insurgent-initiated attacks. Interestingly, the program appears to have exacerbated conflict with certain types of armed groups—the communist New People's Army (NPA) and the Muslim-separatist Moro-Islamic Liberation Front (MILF)—but not others, notably so-called "Lawless Elements," which comprise armed criminal groups that lack a clear political motivation.

These results are consistent with a model in which aid increases conflict because insurgents tried to sabotage programs for political reasons: successful implementation of government-supported projects would weaken insurgents' position among the population. This explanation is in line with the finding of Beath, Christia, and Enikolopov (2011) that a successful community-driven development program increased support for the government in Afghanistan. It might be precisely this positive (from the government's point of view) effect of CDD projects that leads insurgents to attack them and thereby exacerbate conflict in the short term.[3] We discuss the consistency of our evidence with this mechanism and possible alternative explanations in Section V.

[3] See Powell (2012) for a detailed theoretical account of how expected future shifts in power can lead to conflict.

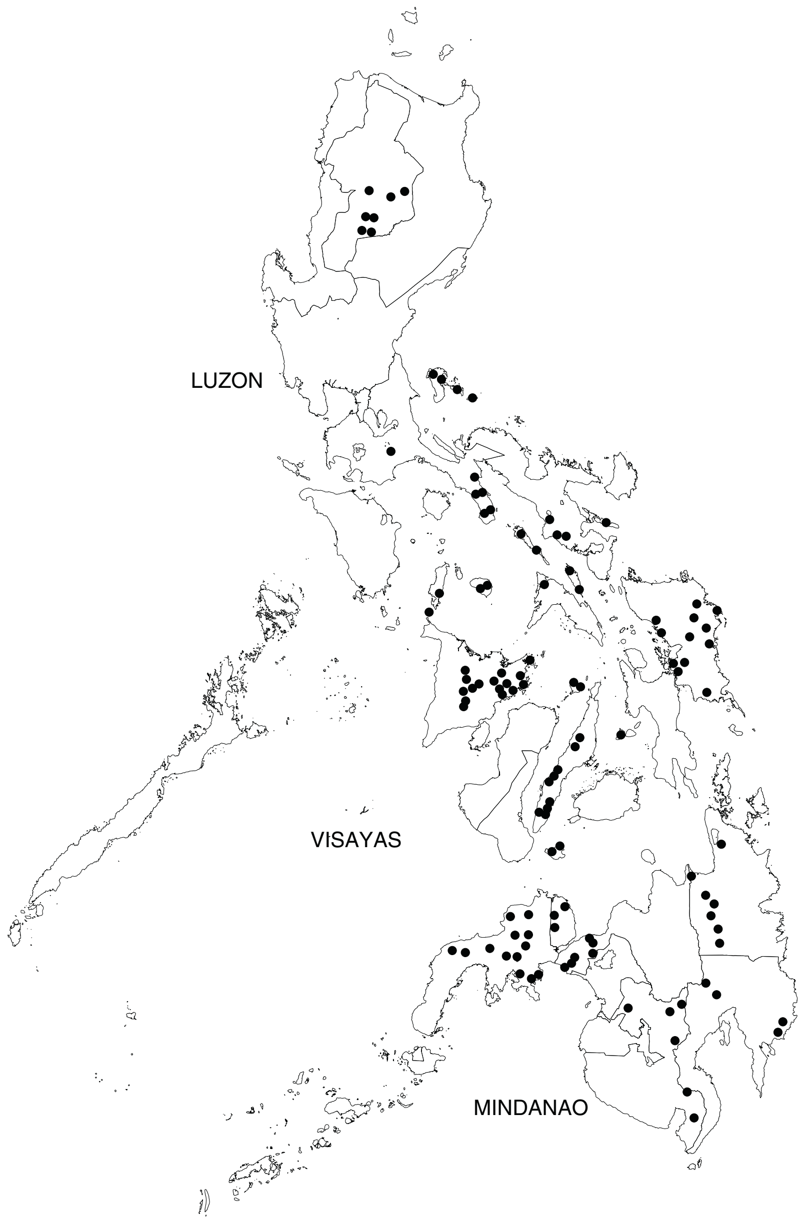

Figure 1. Map of KALAHI-CIDSS Municipalities: Initial Phase Beginning in 2003

I. Institutional Background: The KALAHI-CIDSS Program

This article studies the KALAHI-CIDSS program, a community-driven development (CDD) program implemented by the Philippine government's Department of Social Welfare and Development (DSWD) and funded through World Bank loans. The program aims to enhance local infrastructure, governance, participation, and social cohesion. Between 2003 and 2008, more than 4,000 villages in 184 municipalities across 42 provinces received aid through KALAHI-CIDSS, making it the largest development program in the country during this period.[4] Plans are currently being made to expand KALAHI-CIDSS, with the aim of doubling the number of recipient municipalities during the program's next phase. Figure 1 shows a map of the municipalities that received KALAHI-CIDSS aid in the program's initial phase beginning in 2003, which is the focus of our analysis.

KALAHI-CIDSS followed a standard CDD template (Parker 2005). In the six months before the program's official start, inhabitants of eligible municipalities were informed of the project and its procedures in a "social preparation phase." Municipal administrators had to decide if the municipality would participate in the program and set up the required institutional infrastructure, such as regular village meetings. While most eligible municipalities ended up participating in the program, several of them opted out or were dropped by DSWD, some due to concerns about the security of project personnel. After the program's official start, participating municipalities received a block grant for small-scale infrastructure projects. Each village in participating municipalities held a series of meetings in which village members drafted project proposals. Villages then sent democratically elected representatives to participate in municipal-level intervillage fora, in which proposals were evaluated and funding allocated. Once funding was allocated, community members were encouraged to monitor or participate in project implementation. The amount of aid distributed through KALAHI-CIDSS was substantial. Participating municipalities received PhP300,000 (approximately US$6,000) per village. The average municipality has approximately 25 villages, making the average grant approximately $150,000, or about 15 percent of an average municipality's annual budget. Over the course of the program, the project cycle was repeated three times, so that participating municipalities received on average close to half a million US dollars.

KALAHI-CIDSS was rolled out sequentially in two phases. Our analysis focuses on the initial phase, which covered 22 provinces. The municipal poverty ranking for this phase was made public in December 2002, and the social preparation phase began immediately. The actual program implementation began in June 2003 and lasted for three years. This timetable was determined before it was known which municipalities would be eligible and was the same for all municipalities in the initial phase. We do not include later phases in our analysis because of concerns about endogenous timing. In later phases, DSWD officials decided the program's start date separately for each eligible municipality. This made it possible to time the program in a way that avoids more violent periods, which could bias our results. Including later phases would also introduce measurement error, since we do not know what the program's start date would have been in ineligible municipalities or eligible ones that did not participate in the program.

II. Empirical Strategy

To estimate the causal effect of KALAHI-CIDSS on the intensity of violent conflict, we use a regression discontinuity (RD) design, made possible by the arbitrary eligibility threshold used to target the program. Targeting followed a two-stage

[4] As of March 2010, there were 80 provinces and 1,496 municipalities in the Philippines.

approach. First, 42 eligible provinces were selected, among them the country's 40 poorest based on estimates from the Family Income and Expenditure Survey (FIES). Of these provinces, 22 were covered in the program's initial phase, which is the focus of our analysis. 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, Edillon, and Ducanes 2002; Balisacan and Edillon 2003). The estimation was carried out by an independent consulting firm in order to make sure that it was based on objective criteria. In each eligible province, municipalities were ranked according to their poverty level, and only the poorest quartile was eligible for KALAHI-CIDSS.

Each municipality's poverty level was estimated as a weighted combination of variables from the 2000 census and variables from a database on road conditions from the Department of Transportation. The road database is no longer available, so we are unable to replicate the raw poverty scores and use them as the running variable of our RD design. Instead, we directly use the rankings published by Balisacan, Edillon, and Ducanes (2002) and Balisacan and Edillon (2003) to generate the running variable, which is the distance of the municipality's poverty rank from the provincial eligibility threshold. 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. This threshold number was then subtracted from the municipality's actual poverty rank to obtain its relative poverty rank. For each participating province, the richest eligible municipality has a relative poverty rank of zero and the poorest ineligible municipality has a relative poverty rank of one. Since the poverty ranking was used exclusively to target KALAHI-CIDSS and no other program, the only variable that should change discontinuously at this threshold is eligibility for KALAHI-CIDSS.

Figure 2 shows the distribution of the running variable. Since 22 provinces were eligible for the first phase of the program, 22 municipalities have relative poverty ranks of 0 or 1, one for each province. Poverty ranks further away from the eligibility threshold occur less frequently, because smaller provinces do not have enough municipalities to fill those ranks.

Our goal is to estimate the discontinuous change in conflict casualties across the eligibility threshold. Assuming that unobserved variables are a smooth function of the running variable, this change reflects the causal effect of the KALAHI-CIDSS program. One limitation of our research design is that the running variable is discrete. This makes it impossible to observe municipalities that are arbitrarily close to the threshold on both sides, so that we have to extrapolate the trend of the outcome away from the threshold.[5] This extrapolation potentially makes our results sensitive to the functional form we choose for the running variable. To explore this possibility, we follow the suggestion of Lee and Lemieux (2010) and use two different functional forms. The first is a regression that flexibly controls for quadratic trends on both sides of the threshold:

$$Y_{ipt} = \beta_0 + \tau D_{ip} + \beta_1 X_{ip} + \beta_2 X_{ip}^2 + \beta_3 D_{ip} X_{ip} + \beta_4 D_{ip} X_{ip}^2 + \alpha_p + \gamma_t + \varepsilon_{ipt}.$$

[5] While this is a conceptual problem of discrete running variables, it is shared in practice by all RD designs implemented in finite samples.

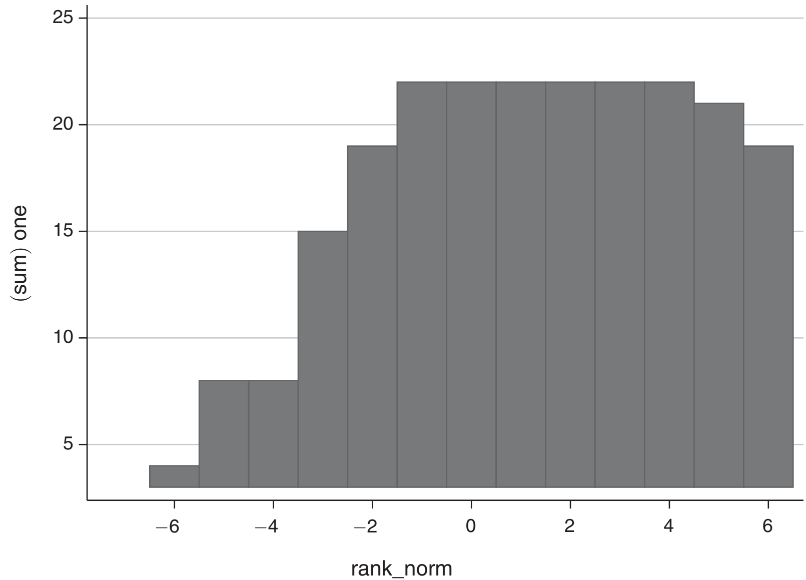

Figure 2. Number of Municipalities by Relative Poverty Rank

Notes: The figure displays the number of municipalities with each relative poverty rank. A relative poverty rank of zero indicates the richest municipality in a province that is still eligible for the KALAHI-CIDSS program. A rank of one indicates the poorest ineligible municipality.

Y_{ipt} denotes the number of conflict casualties suffered by municipality i in province p in month t. X_{ip} denotes the municipality's relative poverty rank. D_{ip} is an indicator that takes the value 1 if the municipality is eligible for the program and 0 if it is not. α_p and γ_t are province and time fixed effects. The RD estimator is given by the parameter τ.

The second specification is a nonparametric local linear regression. This approach estimates the following linear regression, while using kernel weights to give higher weight to observations that are closer to the threshold:

$$Y_{ipt} = \beta_0 + \tau D_{ip} + \beta_1 X_{ip} + \beta_2 D_{ip} X_{ip} + \alpha_p + \gamma_t + \varepsilon_{ipt}.$$

Our baseline specification uses a triangular kernel with a bandwidth of 6. Since there is currently no universally agreed-upon method for selecting the optimal bandwidth for local linear regressions, we follow Ludwig and Miller (2007) and also report results for a wide range of bandwidths between 2 and 6 ranks in the online Appendix. The unit of observation is the municipality-month. Since our outcome of interest is a discrete "count" variable, we estimate OLS regressions as well as Poisson models. To deal with the potential serial correlation of conflict in our sample, the standard errors of all specifications are clustered at the municipality level.

When interpreting the results of this approach, it should be kept in mind that not all eligible municipalities participated in the program. Some municipalities declined to participate, and several others were dropped from the program by the implementing agency due to concerns about the security of its staff. Our results therefore constitute an "intention-to-treat" effect—the effect of *eligibility* for the program regardless of later participation status. One might think that it would be preferable to estimate the local average treatment effect of program participation by "fuzzy" RD design that uses eligibility as an instrument for participation. However, we show in Section IV that a large part of the increase in violence occurred during the program's social preparation phase, which took place in all eligible municipalities before it was clear whether they would actually participate in the program. It thus appears that eligibility alone could cause an increase in violence even in municipalities that did not ultimately participate in the program. We also show that the largest increase in violence was suffered by municipalities that did not end up participating in the program, which is consistent with anecdotal evidence that insurgent groups used violence to intimidate the population and prevent municipalities from participating.[6] This violates the exclusion restriction of the IV estimator; as a consequence, we estimate only the intention-to-treat effect.

A. *Robustness Tests*

One concern with our approach is that the poverty ranking may have been manipulated to target the program towards certain municipalities, which would violate the identifying assumption of the RD estimator. To explore this possibility, we test for smoothness of observable characteristics across the eligibility threshold. If the assumptions of the RD estimator hold, we should not observe a discontinuous change in observable preprogram characteristics (including preprogram casualties) across the eligibility threshold. The results of these tests are reported in Section IV. As an additional robustness test, we examine whether there are discontinuities in conflict at other "pseudo-thresholds" where eligibility for the program did not change, which might occur if our results were due to misspecification of the relationship between running variable and outcome. The results of these tests are reported in the online Appendix.[7]

III. Data

Data on our outcome of interest—casualties in civil conflict—come from the Armed Forces of the Philippines (AFP). They were derived from the original incident reports of AFP units operating throughout the Philippines from 2001 through 2008. The incident-level data are similar to the US military's SIGACTS data, which have been used to study the conflicts in Afghanistan and Iraq (Berman, Shapiro, and Felter 2011; Iyengar, Monten, and Hanson 2011; Beath, Christia, and Enikolopov 2011). The full dataset contains information on the incident's date, location, involved groups, initiating party, and 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 (JOC) by units deployed across the country. In total, the database documents

[6] Author interview with DSWD senior officials and project managers.

[7] One disadvantage of our design is that we cannot use the density of the running variable to test for possible manipulations of the poverty ranking. Since there are as many municipalities that are one rank below the eligibility cutoff as there are one rank above it, the density of the running variable is smooth by construction. It is therefore impossible to detect manipulations by testing for "bunching" of municipalities at the threshold.

more than 21,000 unique incidents during this period, which led to just under 10,000 casualties nationwide. The depth, breadth, and overall quality of the AFP's database makes it a unique resource for conflict researchers.[8]

One potential weakness of the data stems from the fact that AFP troops are themselves party to the conflict and may have incentives to selectively report casualties. This might upwardly bias our estimates if the AFP overreported casualties in municipalities eligible for KALAHI-CIDSS. For example, the AFP may have been opposed to the way in which the program was implemented and may therefore have tried to discredit it by overstating problems with program security. While this should be kept in mind when interpreting our results, there are several factors that limit the extent of misreporting. First, the field reports underlying our data are only for internal use by the AFP and were not used to make public claims about the program. Their main purpose is to chronicle military activities and assist in developing intelligence assessments and planning future AFP operations. Inaccurate information included in field reports might jeopardize future military operations and put the lives of AFP troops at risk, so the AFP's chain of command has a strong incentive to enforce accurate reporting. Our focus on casualties as the outcome of interest further limits the possibility of misreporting.[9] Casualties are relatively easy to verify, which limits the ability of AFP units to misreport casualties, even if they had an incentive to do so. Somewhat reassuringly, tests reported in Section IV indicate that our results are strongest for government casualties, which are easiest to verify by the JOC. The information on which party initiated the incident—government or insurgents—is more subjective and therefore more prone to misreporting. However, previous studies make similar distinctions based on comparable data reported by US military units in the SIGACTS data (e.g., Berman, Shapiro, and Felter 2011; Iyengar, Monten, and Hanson 2011; Beath, Christia, and Enikolopov 2011), and we follow this approach to ensure that our results are comparable with those of the literature.

Another concern is that eligible municipalities may experience more casualties simply because more AFP troops are present to engage in conflict with insurgents. It is, for example, possible that the AFP moved its troops towards eligible municipalities to enhance the security of the program. If this was the case, the program may not have increased aggregate conflict in the country as a whole, but merely shifted it from one location to another by acting as a "conflict magnet." To explore this possibility, we estimate the program's spillover effect on conflict in nearby municipalities. If the program's effect was mainly due to troop movements, we would expect conflict to decrease in municipalities where troops were withdrawn, which are likely to be nearby. The results of this analysis are presented in the online Appendix and show no evidence of a spillover effect.

[8] A subset of this dataset covering the 22 provinces covered by the first round of KALAHI-CIDSS for the years 2002–2005 is used in this analysis and available for replication. Additional releasable information from this military database is maintained by the Empirical Studies of Conflict Project (ESOC, https://esoc.princeton.edu).

[9] We find the same qualitative results when using raw incident counts as the outcome of interest, though the point estimates are slightly smaller and less precisely estimated.

Table 1—Summary Statistics

	Mean	SD
Panel A. Outcome variables		
Total casualties (/month)	0.087	0.63
Insurgent casualties	0.028	0.29
Government casualties	0.038	0.38
Civilian casualties	0.021	0.29
Casualties in insurgent-initiated incidents	0.058	0.52
Casualties in government-initiated incidents	0.029	0.34
Casualties in incidents involving the NPA	0.050	0.47
Casualties in incidents involving the MILF	0.011	0.29
Casualties in incidents involving "Lawless Elements"	0.024	0.28
Panel B. Control variables		
Population	30,704	19,617
Fraction of villages with highway access	0.660	0.298
Ethnic fractionalization index	0.327	0.301
Religious fractionalization index	0.326	0.236
Muslim population fraction	0.047	0.158
Insurgents present in municipality	0.369	0.484
Fraction of HH with access to electricity	0.363	0.177
Fraction of HH with access to indoor plumbing	0.470	0.187
Fraction of HH with access to running water	0.338	0.228
Fraction of houses with strong roofing material	0.490	0.210
Fraction of houses with strong wall material	0.585	0.220
Education of household head (years)	6.30	1.20
Observations	7,992	7,992
Municipalities	222	222

Notes: Panel A reports summary statistics of monthly conflict casualties reported by units of the Armed Forces of the Philippines (AFP) in the period 2002–2006. Panel B reports summary statistics of control variables. Data on insurgent presence comes from an internal intelligence assessment conducted by the AFP in 2001. All other variables come from the 2000 Census of the Philippines. The sample is restricted to municipalities within six poverty ranks of the provincial eligibility threshold for the KALAHI-CIDSS program.

IV. Results

A. *Summary Statistics*

Table 1 reports summary statistics of casualties and control variables in our sample of municipalities within six ranks of the program's eligibility threshold. The table shows that the conflict is of relatively low overall intensity, with approximately 0.078 casualties per municipality per month, or 0.94 casualties per municipality per year. The majority of the casualties are suffered by government forces and occur as a result of incidents initiated by insurgents. Out of the three main insurgent groups, incidents involving the communist New People's Army (NPA) account for the vast majority of casualties. The conflict with the Muslim-separatist Moro-Islamic Liberation Front (MILF) has a smaller geographic reach (the MILF is only active in parts of the southern island of Mindanao and the Sulu Sea), and therefore accounts for a smaller fraction of total casualties. The third group, referred to in the AFP data as Lawless Elements (LE), consists of organized criminal groups without a political agenda. The number of casualties in conflict with these groups is larger than with the MILF but substantially smaller than with the NPA. Turning to the control variables, 37 percent of municipalities have some insurgent presence. Data on this variable come from an internal intelligence assessment conducted by the AFP in 2001. The

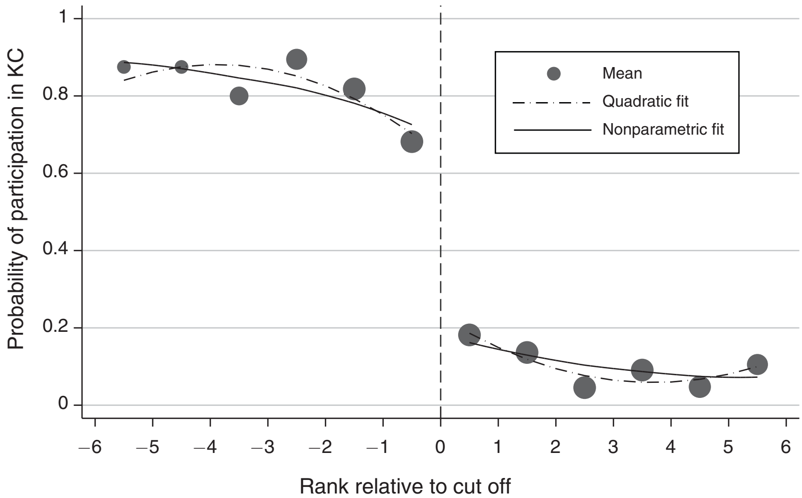

FIGURE 3. THE EFFECT OF ELIGIBILITY ON PARTICIPATION

Notes: The figure presents the relationship between the probability of participating in the KALAHI-CIDSS program and the running variable of the RD design, which is the distance between the municipality's poverty rank and the provincial eligibility threshold. Scatter dots represent means. Dashed lines are quadratic fits, separately estimated on both sides of the eligibility threshold. Solid lines are nonparametric fits from a local linear regression that uses triangular kernels with a bandwidth of 6, separately estimated on both sides of the eligibility threshold.

other control variables are derived from the 2000 Census of the Philippines. As expected based on the program's targeting, the summary statistics of the control variables show that municipalities in the sample are relatively poor. For example, in the average municipality less than 40 percent of households have access to electricity or running water.

B. *Eligibility and Participation*

Figure 3 plots a municipality's observed probability of participating in KALAHI-CIDSS against its relative poverty rank. The size of the scatter dot represents the number of municipalities with the corresponding relative poverty rank. The dashed line represents a quadratic fit to the data; the solid line represents a local linear fit with a triangular kernel and a bandwidth of 6. The graph shows that the probability of participation decreases sharply across the eligibility threshold, though some eligible municipalities did not participate and were replaced by municipalities above the threshold. The corresponding RD regressions in Table 2 show that the probability of participation increased by between 35 and 48 percentage points across the eligibility threshold. As discussed in Section II, participation in the program is potentially endogenous to the conflict, since several municipalities dropped out of the program during the social preparation phase. Consistent with this hypothesis, we find that municipalities with a relative poverty rank of 0, who are least likely to participate in the program, saw the largest increase in violence during the social preparation phase (as shown in Figure 4). Since this potentially violates the exclusion restriction of a "fuzzy" RD design, our empirical analysis estimates the

Table 2—Effect of Eligibility on Participation in KALAHI-CIDSS

	Local Linear		Quadratic		Control mean
	(1)	(2)	(3)	(4)	(5)
Panel A. Probit					
Eligible	0.527***	0.569***	0.363*	0.405*	0.102
	(0.123)	(0.116)	(0.194)	(0.195)	(0.116)
Panel B. OLS					
Eligible	0.517***	0.512***	0.409**	0.410**	0.102
	(0.116)	(0.172)	(0.172)	(0.027)	(0.116)
Controls	No	Yes	No	Yes	No
Observations	222	222	222	222	128

Notes: Columns 1–4 report results of the RD design described in Section II. The running variable is the distance between the municipality's poverty rank and the provincial eligibility threshold. Local linear regressions control for flexible trends of the running variable on each side of the eligibility threshold and use triangular kernel weights with a bandwidth of six ranks. Quadratic regressions control for flexible quadratic trends of the running variable on both sides of the threshold. For probit models, reported values are marginal effects. Control variables are shown in Table 1. All regressions include province fixed effects. Column 5 reports the mean outcome in ineligible municipalities.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

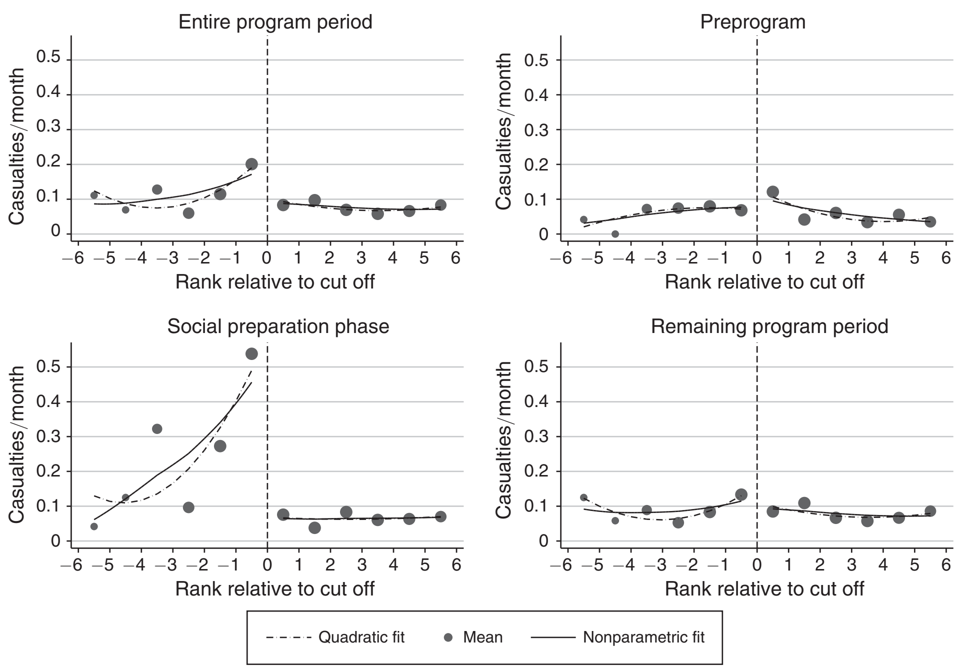

Figure 4. The Effect of Eligibility on Casualties

Notes: The figure presents the relationship between the number of casualties experienced during the program period and the running variable of the RD design, which is the distance between the municipality's poverty rank and the provincial eligibility threshold. Scatter dots represent means. Dashed lines are quadratic fits, separately estimated on both sides of the eligibility threshold. Solid lines are nonparametric fits from a local linear regression that uses triangular kernels with a bandwidth of 6, separately estimated on both sides of the eligibility threshold.

"intention to treat" effect—the effect of eligibility regardless of later participation status. We further explore the relationship between violence in the social preparation phase and program participation in Section IVF.

C. *Balance Tests*

We now test for smoothness of pretreatment observable variables across the threshold. These tests estimate the same equations used to estimate the program's effect on casualties but use different control variables as the dependent variable. The parameter associated with eligibility reflects the discontinuous change in the control variables across the eligibility threshold. Under the identifying assumption of the RD estimator—that assignment close to the threshold is as good as random—the control variables should not change discontinuously across the threshold. The results in Figure 5 and Table 3 are consistent with this assumption. In the local linear specification, the "effect" of eligibility is not statistically significant for any of the control variables. In the quadratic specification, only the religious fractionalization index is statistically significant at the 10 percent level. We do not believe that this reflects a systematic manipulation of the running variable, since the quadratic trend for this variable appears to have been strongly influenced by the outlying data point at -3 poverty ranks, which led to an overfitting of the inverted u-curve. Also, given that we tested 12 variables in two specifications it is not surprising that one null-hypothesis is rejected at the 10 percent level.

D. *The Effect of KALAHI-CIDSS on Conflict Casualties*

Figure 4 displays the relationship between conflict casualties and the relative poverty rank. The top left panel shows the relationship for the entire duration of the program. The graph shows that barely eligible municipalities—the ones whose poverty ranks put them just below the eligibility threshold—suffered more total casualties than barely ineligible municipalities, which suggests that the program had a causal effect on casualties.

The first row of Table 4 presents the corresponding regression results from the estimating equations in Section II. It shows that the program had a significant positive (in sign) effect on total casualties during the three years after the program's start. Depending on the specification, the effect size ranges between 0.083 and 0.141 casualties per month, which corresponds to between 1.0 and 1.7 casualties per year. This effect corresponds to a 110–185 percent increase in casualties relative to the mean in ineligible municipalities of approximately 0.076 per month or 0.91 per year. If we assumed the effect was constant across all 184 municipalities that received the program, the program would have caused between 550 and 930 excess casualties during the three years in which it was active.

E. *Timing of the Violence*

To analyze the timing of the program's effect, we separately analyze the effect of eligibility on casualties during three time periods: the preprogram period, the social preparation phase, and the remaining program period. The social preparation phase

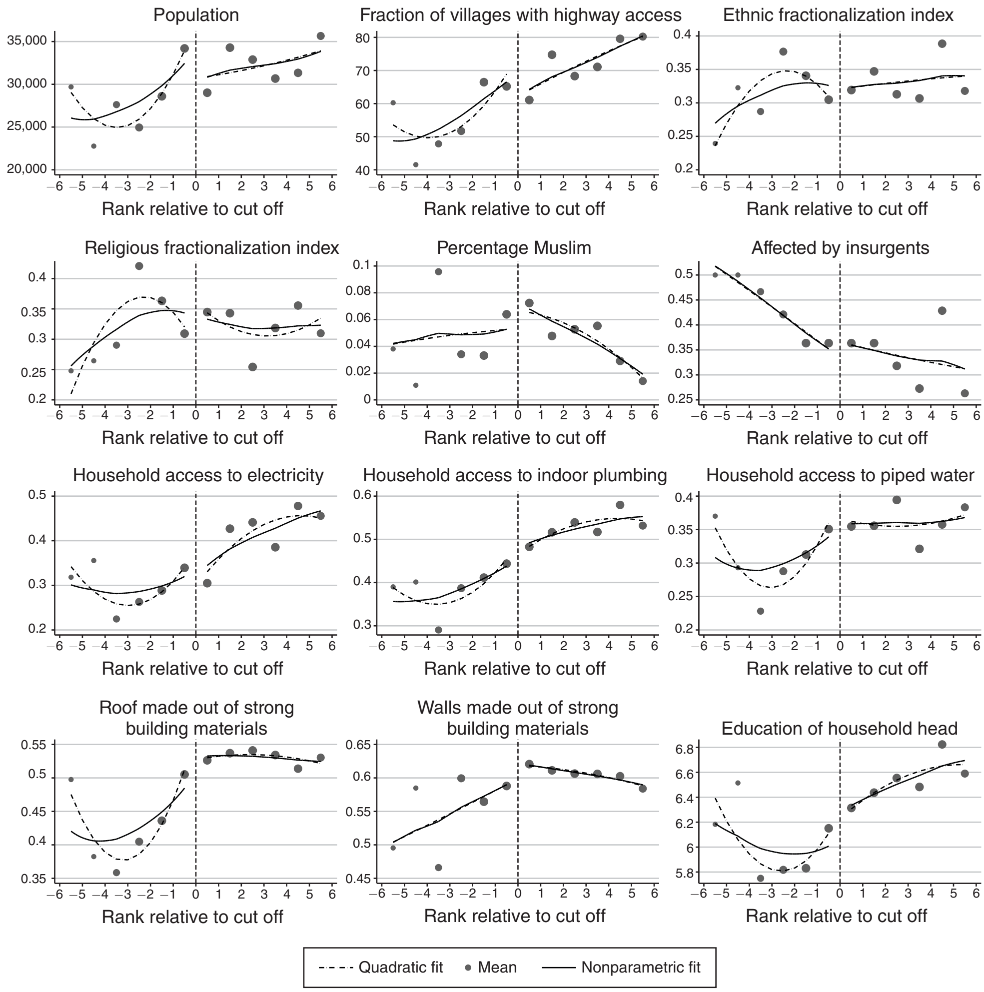

Figure 5. Robustness Tests for Continuity of Observables across the Threshold

Notes: The figure presents the relationship between the running variable of the RD design, which is the distance between the municipality's poverty rank and the provincial eligibility threshold, and a number of control variables. Scatter dots represent means of bins with a bandwidth of one. Dashed lines are quadratic fits, separately estimated on both sides of the eligibility threshold. Solid lines are nonparametric fits from a local linear regression that uses triangular kernels with a bandwidth of 6, separately estimated on both sides of the eligibility threshold.

took place from December 2002 to May 2003. During this phase, the program was introduced to the population, and municipal officials decided whether their municipality would participate in it. We define the preprogram period as the 12 months before the start of the social preparation phase. The remaining program period is defined as June 2003 to June 2006.

The results in Figure 4 and Table 4 show that the program's effect is concentrated in the social preparation phase. During this period, eligible municipalities experienced between 0.37 and 0.54 excess casualties per month. The program's estimated effect during the remaining program period was substantially smaller and not statistically significant in any specification.

TABLE 3—ROBUSTNESS TESTS FOR CONTINUITY OF OBSERVABLES ACROSS THE THRESHOLD

	Parameter associated with eligibility		
	Local linear (1)	Quadratic (2)	Control mean (3)
Population	3,108.0	5,745.7	32,239.6
	(3,305.2)	(6,134.2)	(1,872.3)
Percentage of villages with highway access	0.057	0.099	0.723
	(0.055)	(0.094)	(0.025)
Ethnic fractionalization	−0.018	−0.046	0.332
	(0.033)	(0.054)	(0.027)
Religious fractionalization	−0.021	−0.085*	0.321
	(0.030)	(0.050)	(0.020)
Percent Muslim	−0.014	−0.001	0.046
	(0.023)	(0.044)	(0.014)
Affected by insurgents in 2001	−0.005	−0.037	0.336
	(0.088)	(0.157)	(0.042)
Household access to electricity	0.004	0.070	0.414
	(0.031)	(0.056)	(0.016)
HH access to water-sealed toilet	−0.034	0.005	0.527
	(0.030)	(0.051)	(0.016)
HH access to running water	−0.014	0.024	0.361
	(0.034)	(0.059)	(0.021)
House with strong roof material	−0.030	0.022	0.531
	(0.026)	(0.046)	(0.018)
House with strong wall material	−0.033	−0.007	0.606
	(0.028)	(0.051)	(0.019)
Education of household head (years)	−0.21	0.025	6.53
	(0.17)	(0.31)	(0.094)
Observations	222	222	128

Notes: Columns 1–2 report results of the RD design described in Section II. The running variable is the distance between the municipality's poverty rank and the provincial eligibility threshold. The dependent variable is given in the row (each row reports the result of a different regression). Reported values are the coefficients associated with eligibility for KALAHI-CIDSS. Local linear regressions control for flexible trends of the running variable on each side of the eligibility threshold and use triangular kernel weights with a bandwidth of six ranks. Quadratic regressions control for flexible quadratic trends of the running variable on both sides of the threshold. All regressions include province fixed effects. Column 3 reports the mean outcome in ineligible municipalities.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

The estimate of the program's "effect" during the preprogram phase serves as a robustness test for possible violations of the RD assumptions. Here, the concern is that the poverty ranking or the timing of the rollout was manipulated in order to give the program to municipalities that were more (or less) affected by conflict. If this were the case, we would expect eligible municipalities to have significantly more (or fewer) casualties even before the program becomes active. The results in Figure 4 and the second row of Table 4 show no evidence of this. Barely eligible municipalities experienced slightly fewer casualties in the preprogram period, but the difference is not statistically significant. This increases our confidence that the poverty ranking was not manipulated in a way that would bias our results.

Table 4—Regression Discontinuity Estimates of the Effect of Eligibility for KALAHI-CIDSS on Conflict Casualties

	Local linear		Quadratic		Control mean
	(1)	(2)	(3)	(4)	(5)
Panel A. Poisson					
Entire program period	0.083**	0.091***	0.100**	0.116***	0.076
	(0.037)	(0.031)	(0.045)	(0.043)	(0.008)
Preprogram period	−0.030	−0.007	−0.059	−0.036	0.059
	(0.038)	(0.032)	(0.048)	(0.043)	(0.011)
Social preparation phase	0.416***	0.488***	0.366***	0.492***	0.065
	(0.145)	(0.135)	(0.136)	(0.163)	(0.014)
Remaining program period	0.024	0.031	0.049	0.061	0.078
	(0.033)	(0.029)	(0.044)	(0.041)	(0.009)
Panel B. OLS					
Entire program period	0.096**	0.090**	0.141**	0.120*	0.076
	(0.046)	(0.042)	(0.069)	(0.065)	(0.008)
Preprogram period	−0.029	−0.024	−0.067	−0.069	0.059
	(0.045)	(0.040)	(0.065)	(0.063)	(0.011)
Social preparation phase	0.446**	0.449**	0.544*	0.496*	0.065
	(0.202)	(0.187)	(0.295)	(0.272)	(0.014)
Remaining program period	0.025	0.018	0.060	0.045	0.078
	(0.037)	(0.037)	(0.057)	(0.059)	(0.009)
Controls	No	Yes	No	Yes	No
Municipalities	222	222	222	222	128

Notes: Columns 1–4 report results of the RD design described in Section II. The running variable is the distance between the municipality's poverty rank and the provincial eligibility threshold. Local linear regressions control for flexible trends of the running variable on each side of the eligibility threshold and use triangular kernel weights with a bandwidth of six ranks. Quadratic regressions control for flexible quadratic trends of the running variable on both sides of the threshold. For Poisson models, reported values are marginal effects. Standard errors are clustered at the municipality level. Control variables are shown in Table 1. All specifications include year and province fixed effects. Column 5 reports the mean outcome in ineligible municipalities.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

We show in the online Appendix that the above results are robust to varying the bandwidth of the RD estimator. If anything, the estimates become larger as we decrease the bandwidth, so that the estimates in Table 4 constitute conservative estimates of the program's effect.

F. *Violence in the Social Preparation Phase and Participation*

As mentioned in the introduction, our preferred explanation for the program's effect is that insurgents use violence to derail it for political reasons. This explanation is consistent with the program's strong effect during the social preparation phase, when municipal leaders were still deciding whether to participate in the program, and DSWD was deciding whether to drop municipalities from the program. If this explanation is correct, we would expect eligible municipalities who experienced more violence during the social preparation phase to be more likely to drop out of the program. Consistent with this hypothesis, Figures 3 and 4 show that eligible municipalities close to the threshold, who were most likely to drop from the

Table 5—Conflict Intensity and Program Participation

	Participating (1)	Nonparticipating (2)	p-value of difference (3)
Preprogram	0.057	0.097	0.43
	(0.011)	(0.031)	
Social preparation phase	0.145	0.824	0.052
	(0.042)	(0.292)	
Remaining program period	0.073	0.167	0.136
	(0.011)	(0.040)	
Municipalities	76	18	94

Notes: The table reports means of monthly casualties in municipalities eligible for KALAHI-CIDSS. Standard errors in parentheses, clustered at the municipality level.

program, also experienced the most casualties during the social preparation phase. Table 5 explores this relationship further by showing that eligible municipalities who dropped from the program experienced substantially more casualties during the social preparation phase than eligible municipalities who remained in the program. Of course, this should not be interpreted as a causal effect of violence on participation, since both variables are likely correlated with unobserved factors. Nevertheless, this evidence is consistent with the hypothesis that the program's effect is due to insurgents' efforts to prevent eligible municipalities from participating.

G. *Disaggregation by Incident Characteristics*

The top two panels in Figure 6 suggest that the discontinuous change across the eligibility threshold is particularly strong for casualties in insurgent-initiated incidents, and much less pronounced for casualties in government-initiated incidents. The corresponding results in Table 6 support this conclusion. The program's effect is mainly due to its effect on casualties in insurgent-initiated incidents, which ranges between 0.088 and 0.118 and is statistically significant in all specifications. The program's estimated effect on casualties in government-initiated incidents is much smaller (sometimes even negative) and not statistically significant in any of the specifications.

The bottom three panels in Figure 6 and the corresponding results in Table 6 show that the program's effect is strongest on government forces, who suffer between 0.048 and 0.070 excess casualties per month. The effect on insurgent casualties is smaller, between 0.019 and 0.037, and not statistically significant in all specifications. For civilian casualties, the point estimate of the program's effect ranges between 0.017 and 0.029. This is large relative to the mean of 0.016 civilian casualties in ineligible municipalities, though the estimates are also not statistically significant in all specifications.

We now separately analyze the program's effect on conflict with different armed groups: the communist New People's Army (NPA), the Muslim-separatist Moro-Islamic Liberation Front (MILF), and so-called Lawless Elements (LE), a term used to describe organized criminal groups without a political agenda. Figure 7 and Table 7 show that the program caused an increase in conflict with the two politically motivated groups. The effect on conflict with the NPA is between 0.029 and

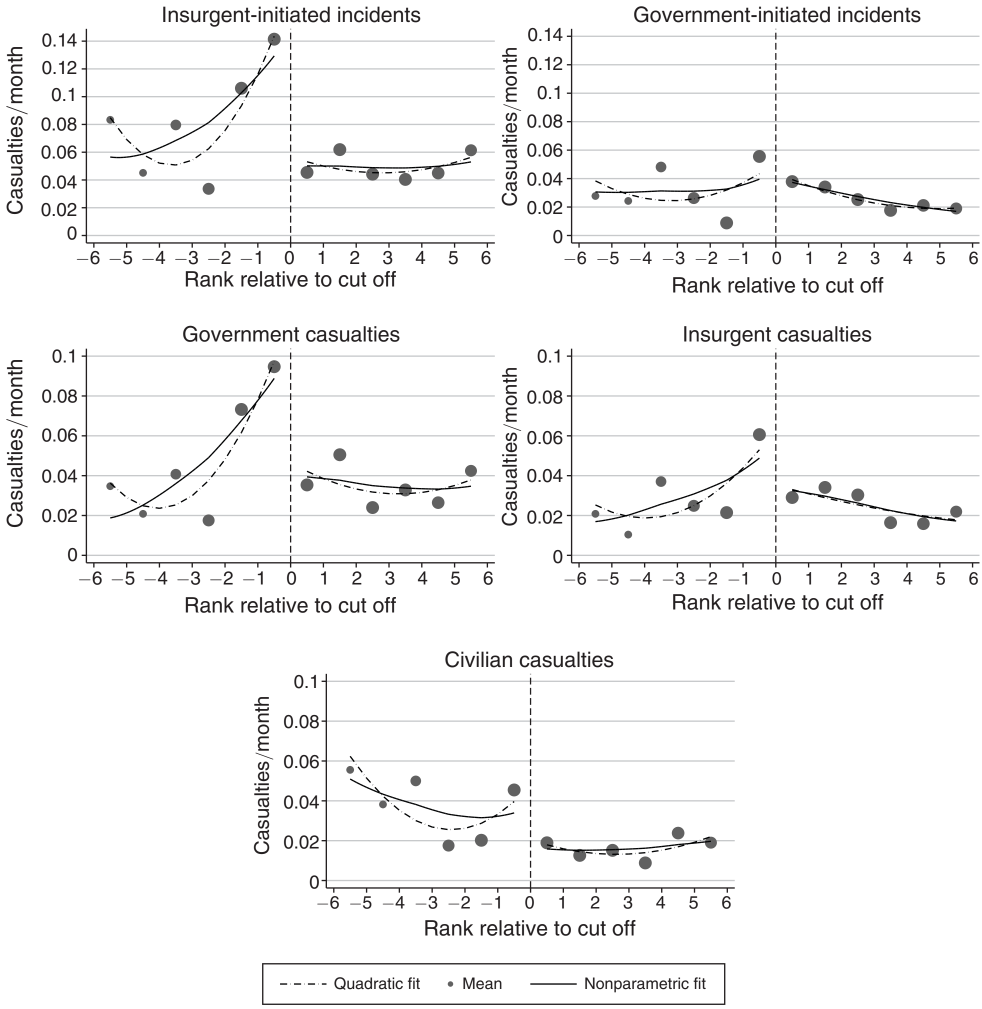

Figure 6. Who Initiates the Violence, and Who Suffers the Casualties?

Notes: The figure presents the relationship between the number of casualties experienced by different groups during the program period and the running variable of the RD design, which is the distance between the municipality's poverty rank and the provincial eligibility threshold. Scatter dots represent means. Dashed lines are quadratic fits, separately estimated on both sides of the eligibility threshold. Solid lines are nonparametric fits from a local linear regression that uses triangular kernels with a bandwidth of 6, separately estimated on both sides of the eligibility threshold.

0.079 casualties/month. The effect on conflict with the MILF is somewhat smaller in absolute value, ranging between 0.019 and 0.046, and not statistically significant. However, the point estimates are very large compared to the mean of 0.004 casualties in ineligible municipalities. The point estimates of the program's effect on conflict with Lawless Elements are substantially smaller and have the opposite sign.

V. Discussion

Designing effective aid interventions for conflict-affected countries requires an understanding of the mechanisms through which aid can exacerbate conflict. The

Table 6—Who Suffers the Casualties?

	Regression discontinuity results				
	Local linear		Quadratic		
	Poisson (1)	OLS (2)	Poisson (3)	OLS (4)	Control mean (5)
Casualties in insurgent-initiated incidents	0.088***	0.092***	0.104***	0.118**	0.049
	(0.027)	(0.033)	(0.038)	(0.049)	(0.006)
Casualties in government-initiated incidents	−0.002	−0.004	0.016	−0.001	0.026
	(0.013)	(0.019)	(0.018)	(0.030)	(0.005)
Casualties suffered by government forces	0.048**	0.053**	0.055**	0.070**	0.035
	(0.020)	(0.024)	(0.025)	(0.034)	(0.005)
Casualties suffered by insurgents	0.021*	0.019	0.037**	0.029	0.025
	(0.012)	(0.015)	(0.016)	(0.025)	(0.004)
Casualties suffered by civilians	0.025***	0.017	0.029*	0.021	0.016
	(0.009)	(0.014)	(0.016)	(0.024)	(0.003)
Controls	Yes	Yes	Yes	Yes	No
Observations	7,992	7,992	7,992	7,992	4,608
Municipalities	222	222	222	222	128

Notes: Columns 1–4 report results of the RD design described in Section II. The running variable is the distance between the municipality's poverty rank and the provincial eligibility threshold. Local linear regressions use triangular kernel weights with a bandwidth of six ranks. For Poisson models, reported values are marginal effects. Local linear regressions control for flexible trends of the running variable on each side of the eligibility threshold and use triangular kernel weights with a bandwidth of six ranks. Quadratic regressions control for flexible quadratic trends of the running variable on both sides of the threshold. Standard errors are clustered at the municipality level. All specifications include the control variables shown in Table 1 and year and province fixed effects. Column 5 reports the mean outcome in ineligible municipalities.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

mechanism suggested by early models of conflict is that aid increases the amount of resources that can be appropriated by violent means, therefore increasing the incentive for violence (Hirshleifer 1989; Grossman 1991; Skaperdas 1992). A related mechanism is that rebel groups divert aid to fund their own operations (Nunn and Qian 2014). Both of these mechanisms are supported by the qualitative evidence that aid shipments are frequently stolen or "taxed" by rebel groups.

An alternative mechanism is that aid increases conflict because insurgents try to sabotage aid programs for political reasons. This explanation is consistent with the finding that successful aid and antipoverty programs can increase popular support for the government (Beath, Christia, and Enikolopov 2011; Manacorda, Miguel, and Vigorito 2011). Rebel groups are likely to benefit from antigovernment sentiment generated by failure of the state to provide needed goods and services, since it makes it easier for them to recruit followers and gain access to information and support needed to carry out clandestine operations (e.g., Berman, Shapiro, and Felter 2011). They may therefore prefer that aid programs fail and use violence to sabotage them. This mechanism is supported by the qualitative evidence that insurgent groups frequently denounce aid programs as ineffective (or even harmful) or as thinly disguised counterinsurgency measures. For example, the New People's Army issued several statements denouncing the KALAHI-CIDSS program as "counterrevolutionary and anti-development" (Bayan 2012; Rosal 2003). The logic behind this

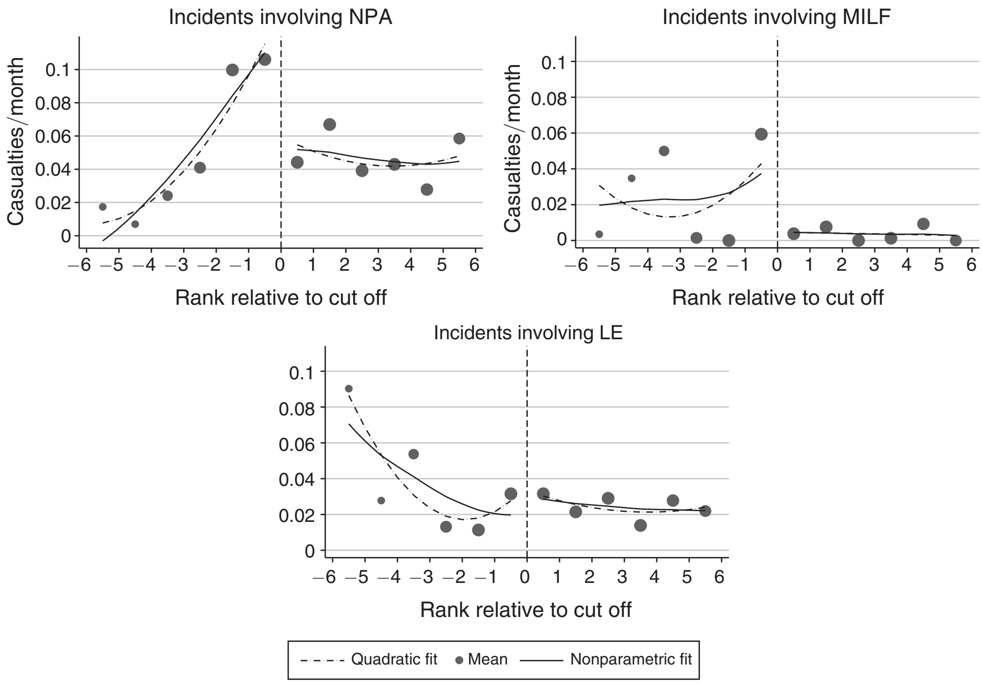

Figure 7. The Program's Effect on Different Armed Groups

Notes: The figure presents the relationship between the number of casualties experienced in incidents involving different armed groups during the program period and the running variable of the RD design, which is the distance between the municipality's poverty rank and the provincial eligibility threshold. Scatter dots represent means. Dashed lines are quadratic fits, separately estimated on both sides of the eligibility threshold. Solid lines are nonparametric fits from a local linear regression that uses triangular kernels with a bandwidth of 6, separately estimated on both sides of the eligibility threshold.

mechanism is laid out in bargaining models of the type described by Powell (2012). If a successful aid program shifts the balance of power in favor of the government, it reduces insurgents' bargaining power and decreases their payoffs in later bargaining rounds. This gives them an incentive to engage in conflict in earlier rounds in order to prevent the project from succeeding. If governments can credibly commit to future payments, they can in theory pay off insurgents for letting aid projects proceed. However, if governments can renege on commitments made in early bargaining rounds, insurgents may prefer to engage in costly attacks on aid projects to increase their bargaining power in future rounds.

We argue that our results are most consistent with the latter mechanism: that the KALAHI-CIDSS program increased conflict because insurgents had an incentive to derail it for political reasons because they feared that successful implementation would increase popular support for the government and, thus, undermine their own position and influence among the population. One piece of evidence is that eligibility for the program increased conflict only during the program's social preparation phase. During this phase, eligible municipalities had to decide whether to participate in the program and demonstrate that they had the institutional capacity to successfully implement it. This made the program particularly vulnerable to insurgent violence and intimidation during this period. Several eligible municipalities

TABLE 7—WHICH CONFLICTS ARE AFFECTED?

	Regression discontinuity results				
	Local linear		Quadratic		
	Poisson (1)	OLS (2)	Poisson (3)	OLS (4)	Control mean (5)
Casualties in incidents involving the NPA	0.043*	0.062**	0.029	0.079*	0.046
	(0.025)	(0.031)	(0.027)	(0.046)	(0.007)
Casualties in incidents involving the MILF	0.019	0.039	0.021	0.046	0.004
	(0.022)	(0.026)	(0.085)	(0.039)	(0.001)
Casualties in incidents involving lawless elements	−0.002	−0.011	−0.001	−0.004	0.024
	(0.012)	(0.014)	(0.018)	(0.027)	(0.004)
Controls	Yes	Yes	Yes	Yes	No
Observations	7,992	7,992	7,992	7,992	4,608
Municipalities	222	222	222	222	128

Notes: Columns 1–4 report results of the RD design described in Section II. The running variable is the distance between the municipality's poverty rank and the provincial eligibility threshold. Local linear regressions control for flexible trends of the running variable on each side of the eligibility threshold and use triangular kernel weights with a bandwidth of six ranks. Quadratic regressions control for flexible quadratic trends of the running variable on both sides of the threshold. For Poisson models, reported values are marginal effects. Standard errors are clustered at the municipality level. All specifications include the control variables shown in Table 1 and year and province fixed effects. Column 5 reports the mean outcome in ineligible municipalities.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

were dropped from the program during the social preparation phase, either because they failed to comply with program conditions or because the program's implementing agency (DSWD) had concerns about the local security situation. By contrast, no municipality was dropped once the program's implementation had begun. This suggests that the violence was motivated by an effort to prevent eligible municipalities from participating in the program. If insurgents were mainly motivated by the chance to appropriate the program's resources, we would expect violence to be highest during the program's implementation, when program funds were disbursed. The hypothesis that violence was used to derail the program is also consistent with the finding that the municipalities that saw the most violence in the social preparation phase were most likely to drop from the program.

Another possible explanation is that insurgents attempted to extort payments from municipal officials in return for letting the program proceed and launched attacks during the social preparation phase in order to make their threats credible. While it is difficult to completely rule out this explanation, it is not easily reconciled with the fact that the program affected conflict only with politically motivated organizations. We would expect apolitical criminal organizations (the so-called Lawless Elements) to be similarly motivated by incentives to extort. However, the Lawless Elements did not respond to the program, which suggests that pure extortion was not a major source of the program's effect.

Our findings differ from the earlier results of Berman, Shapiro, and Felter (2011), who found that some reconstruction projects funded by the US Army led to a decrease in violence in Iraq. A possible explanation is that the projects found to be violence-reducing by Berman, Shapiro, and Felter (2011) were relatively small and implemented by the US Army in the vicinity of a substantial presence of US combat forces, as a complement to small unit military operations (Berman et al. 2013). Insurgents may therefore have had little opportunity to interfere with the projects, which would explain why they did not cause an increase in violence. The KALAHI-CIDSS program, in contrast, was larger in scale, implemented by a civilian agency, and had a six-month lag between announcement and implementation. Insurgents therefore most likely had greater opportunities to act on their politically motivated incentives and sabotage the program, which would explain its conflict-increasing effect.

The Philippine setting is unique in that multiple insurgent groups along with criminal elements operate simultaneously and under similar conditions. This combined with extraordinarily precise data on the location, type, and perpetrator of conflict related casualties increases the likelihood that these results are relevant across other cases and should inform CDD and other aid programs implemented in conflict areas in general.

VI. Conclusion

Development aid is an important tool for reducing the widespread poverty and human suffering in conflict-affected areas. However, it is unclear how the increasing influx of aid affects conflict in recipient countries. Previous evidence suggests that some forms of aid delivery, food aid in particular, can increase conflict (Nunn and Qian 2014), while other forms can have conflict-reducing or stabilizing effects (Berman, Shapiro, and Felter 2011; Nielsen et al. 2011).

This article presents evidence on one of the most popular aid interventions, community-driven development (CDD) programs, which are increasingly used to deliver aid to conflict-affected areas. The evidence suggests that a large-scale CDD program in the Philippines caused an increase in conflict casualties, and that the program's effect was concentrated in its early stages, before funds were disbursed and before eligible municipalities committed to participating in the program. The effect was strongest for casualties suffered by government forces as a result of insurgent-initiated attacks. The program equally affected conflict with the communist New People's Army (NPA) and the Muslim-separatist Moro-Islamic Liberation Front (MILF) but did not affect conflict with so-called Lawless Elements, which consist of armed criminal groups without political motivation.

These results are consistent with the hypothesis that the program increased conflict because insurgents tried to sabotage it to avoid undermining their political position and popular support. This explanation is consistent with the finding of Beath, Christia, and Enikolopov (2011) that a successful community-driven development increased support for the government in Afghanistan. It might be precisely this positive (from the government's point of view) effect of CDD projects that leads insurgents to attack them and thereby exacerbate conflict in the short term.

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