Government Transfers and Civil Conflict: Experimental Evidence from the Philippines*

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Abstract

We exploit a randomized experiment to estimate the effect of a large conditional cash transfer program on civil conflict in the Philippines. We find that the program caused a substantial decrease in reported conflict incidents in treatment villages. We also find evidence that the program reduced conflict in nearby villages through a spillover effect. These findings are consistent with a spatial model in which cash transfers reduce violence by increasing insurgents' cost of recruitment, so that affected villages export fewer combatants to other villages.

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1 Introduction

A large body of research shows that civil conflict has a wide range of negative effects on the welfare of affected populations. In addition to direct casualties, conflict causes lower economic growth (Abadie and Gardeazabal, 2003; Lopez and Wodon, 2005) and education attainment rates (Leon, forthcoming), as well as adverse health outcomes, like low birth weight (Ghobarah et al., 2003; Camacho, 2005; Mansour and Rees, forthcoming). Conflict-affected countries have had substantially lower rates of poverty reduction and have made slower progress toward the Millennium Development Goals than peaceful countries (World Bank, 2012). As a result of these findings, the World Bank and others have called for increases in development assistance to conflict-affected countries, most prominently in the latest World Development Report (World Bank, 2012).

Yet the empirical evidence on the effects of aid and government spending on conflict is mixed. On the one hand, Berman et al. (2011) and Iyengar et al. (2011) find that small-scale aid and reconstruction spending disbursed by the US Army in Iraq led to a decrease in violence against US forces and civilians. On the other hand, there is evidence that infrastructure spending disbursed in the form of community-driven development projects increased violence in the Philippines (Crost and Johnston, 2010), and that US food aid has increased conflict in recipient countries (Nunn and Qian, 2012).

This paper contributes to the literature on government spending and conflict by estimating the effect of a large antipoverty program, the Philippines' *Pantawid Pamilyang Pilipino Program* (4Ps), on the intensity of violent conflict. The 4Ps program distributes cash transfers to approximately one million of the poorest households in the Philippines that meet a number of conditions, such as sending children to school and vaccinating them. Previous research suggests cash-transfer programs can improve the welfare of transfer recipients along many dimensions (DFID Policy Division, 2011). Moreover, evidence suggests that cash-transfers and other forms of development aid can increase support for governments among aid recipients (Manacorda et al., 2011; Beath et al., 2011). Both improved material condi-

tions and political support have been linked to a reduced likelihood of civil conflict. However, to date there is no evidence whether cash-transfer programs themselves are correlated with reduced civil violence. We examine that relationship in this paper.

To estimate 4Ps' impact on conflict, we exploit a randomized experiment conducted by the World Bank in 2009. In the experiment, 130 villages in 8 municipalities of the Philippines were randomly divided into a treatment group in which the 4Ps program was introduced in 2009, and a control group in which it was delayed until 2010. This setup allows us to estimate the causal effect of the program by comparing the intensity of violence in treatment and control villages in 2009. Using village-level conflict data from the Armed Forces of the Philippines and difference-in-differences estimation, we find that the program caused a significant reduction in conflict incidents in treatment villages. This result provides experimental evidence that government spending can reduce civil conflict.

Another contribution of our empirical analysis is that we systematically test for the presence of spillovers. Spillovers may be likely to occur when an intervention has a smaller geographic reach than the organizations involved in a conflict. For example, a project that "wins the hearts and minds" of the population may reduce conflict by making it difficult for insurgents to carry out attacks, since individuals are more likely to supply government forces with intelligence about the insurgency Berman et al. (2011). While this might reduce conflict in the villages that benefit from the project, it could increase conflict in neighboring villages if the insurgency responded simply by shifting its forces and carrying out attacks in these areas.

To clarify the issue of spillovers, we present a spatial model in which insurgents operate in two neighboring villages. For simplicity, we assume that insurgents recruit troops in only one of the villages, but can use them to carry out attacks in both. We show that interventions that increase the cost of recruitment in one village reduce conflict in both villages, while interventions that reduce the payoff from carrying out attacks in one of the villages reduce conflict in that village but increase it in the other. The sign of an intervention's spillover

effect is thus likely to depend on the mechanism through which it operates.

To empirically estimate the size and sign of the spillovers from the 4Ps program, we use a method similar to that of Miguel and Kremer (2004), which estimates the effect of proximity to treatment villages. To anticipate the results, we find that villages within 5 kilometers of treated villages experience a significant reduction in conflict. In terms of the theoretical model, the sign of the spillover effect suggests that 4Ps reduced conflict by making it more difficult for insurgents to recruit troops.

2 Theory: Spillovers from Interventions that Affect Conflict

Empirical studies of conflict often face the problem that the likely presence of spillovers makes it difficult to extrapolate from the local effects of an intervention to its aggregate effects. For example, if an intervention makes it difficult for insurgents to launch attacks in a particular village, the rebels might move their forces to villages that are not affected by the intervention and increase the number of attacks there. If this is the case, a simple comparison of conflict levels between affected and unaffected villages would lead us to overestimate the effect of the intervention. On the other hand, an intervention may make it more difficult for insurgents to recruit combatants in a particular village. If they compensate this shortfall by redeploying forces from villages that were not affected by the intervention, the simple comparison will lead us to understate the intervention's aggregate effect.

To formalize this intuition, consider a simple model in which insurgents can recruit combatants in one village (village A) and can use them to carry out attacks in the same village and/or another village (village B). The insurgents face the following maximization problem:

$$\max_{x^A,x^B} R^A(x^A) + R^B(x^B) - C(X)$$

$$s.t.\ x^A + x^B \leq X$$

where x^A and x^B are the numbers of combatants (and other inputs) that are used to commit attacks in villages A and B, respectively. $R^A(x^A)$ and $R^B(x^b)$ are the returns to carrying out the attacks[1] and $C(X)$ is the cost of recruiting combatants and purchasing other inputs, e.g. arms, ammunition and logistical support. We assume that attacks have diminishing marginal returns, so that $R^A(.)$ and $R^B(.)$ have positive first derivatives and negative second derivatives. By combining the two first-order conditions, we get:

$$R^{A\prime} = R^{B\prime} = C'$$

Unsurprisingly, the insurgents distribute their attacks such that the marginal return to the last combatant is the same in both villages and equal to the marginal cost of recruiting and sustaining him/her.

This condition has important implications for spillover effects. If the cost of recruitment C' increases through an exogenous shock, the insurgents will choose lower levels of both x^A and x^B, so that the marginal effects of combatants, $R^{A\prime}$ and $R^{B\prime}$, increase to equal the higher cost. If, on the other hand, the return to combatants in village A, $R^A(x^A)$, decreases, the insurgents will increase the number of combatants in village B, in order to restore the equality of $R^{A\prime}$ and $R^{B\prime}$. Thus, the direction of spillovers from a shock that decreases conflict in village A depends on the mechanism through which it operates. If the shock increases the cost of recruiting, it will reduce conflict in village B. But if it reduces the payoff of attacks in village A, it will increase conflict in village B.

Of course, this is not the only mechanism through which cash-transfers to one village can have spillovers to nearby villages. It is also possible that the cash transfers to treatment villages boost the economy in nearby villages, which directly affects the cost of recruitment (or the willingness to cooperate with the government) there. Nevertheless, the model has

[1] These returns may be direct monetary benefits but may also be intangible benefits to achieving one's political goals.

two implications: First, we should expect spillovers in contexts in which insurgents can easily move combatants between localities and, furthermore, the direction of the spillover is theoretically ambiguous. Second, the observed conflict-reducing spillover effect of cash-transfers that we report in Section 5 is consistent with the hypothesis that the transfers increase the cost of recruitment in treated villages, but not consistent with the hypothesis that they increase the local cost of carrying out attacks.

3 Institutional Background

3.1 The 4Ps Program and Its Experimental Evaluation

4Ps is a conditional cash-transfer program implemented by the Philippine government and partly funded through loans from the World Bank. Since its inception in 2007, the program has financed transfers to approximately one million households in 782 cities and municipalities in 81 provinces in all 17 regions in the Philippines[2] and is currently the country's flagship antipoverty program.

The program's design is similar to many other conditional cash-transfer programs, such as Mexico's *Oportunidades* and Brazil's *Bolsa Familia*. In areas determined eligible by a poverty-mapping exercise, the program distributes cash-transfers to all households with children under 15 years that are below the provincial poverty line. In return, eligible households must fulfill a number of conditions such as regular pre- and post-natal health check-ups for pregnant women, and vaccinations, deworming and pre-school attendance for children. The size of the cash transfers depends on the number of children in the household and ranges between 800 and 1400 Philippine Pesos (PHP) per month (approximately 18-32 US Dollars). The maximum transfer amount corresponds to 23% of the national poverty line for a 5-member household. Given that only households below the poverty line are eligible, the transfer forms a substantial portion of a recipient household's income.

[2]These statistics were current as of January 2011. See Arulpragasam et al 2011, p. 1.

The randomized experiment we exploit was conducted by the World Bank in 2009. In the experiment, 130 villages were randomly divided into 65 treatment villages, in which the 4Ps program was introduced in 2009 and 65 control villages, in which the program's start was delayed until 2010. The details of the experiment are described in (Redaelli, 2009). The experimental sampling followed a three-step procedure. First, four eligible provinces—Lanao del Norte, Mountain, Negros Oriental, and Occidental Mindoro—were selected from a pool of eight provinces that were scheduled to begin receiving the 4Ps program in 2009. These provinces were non-randomly selected on the basis of geography to ensure that the evaluation would cover areas in each of the country's three major island groups, Luzon, Visayas, and Mindanao (Redaelli, 2009, p. 20). Within each of these provinces, two eligible municipalities were randomly selected to participate in the evaluation. Finally, half of the villages within each of these eight municipalities were randomly assigned to the treatment group and the other half to the control group. Table 1 contains information on the treatment assignment of villages in each of the 8 participating municipalities. Overall, the experimental villages contain 47,627 households, out of which 24,651 were eligible for the 4Ps program (Redaelli, 2009).

3.2 Civil Conflict in the Philippines

The Philippines is home to multiple long-running insurgencies with distinct motives and characteristics. The country's largest and most active insurgent organization during the 2001-2010 period of study was the New People's Army (NPA). The NPA's strength averaged approximately 7000 fighters over this period, and the group was active in 63 of the country's 73 provinces (Felter, 2005). Over 60 percent of the operational incidents reported by units of the Armed Forces of the Philippines' (AFP) in the field involved elements of the NPA, referred to by the Philippine military as the Communist Terrorist Movement (CTM). In the villages that took part in the 4Ps experiment, the CTM was involved in 72.1 percent of the reported incidents. The country's second-largest insurgent movement is the Moro Islamic Liberation

Front (MILF), an Islamist separatist movement active in the southwestern provinces on the island of Mindanao. Between 2001 and 2010, the MILF was involved in 11 percent of nationwide incidents and 9.6 percent of incidents in the villages under study. The remaining incidents involved insurgent splinter groups and criminal groups that the AFP refers to as Lawless Elements, who were involved in just under 19 percent of nationwide incidents and 18.3 percent of incidents in the villages under study. Finally, the al-Qaeda-affiliated Abu Sayyaf Group (ASG) were involved in 5 percent of the incidents reported by the military nationwide, but in none of the incidents in the villages under study. [3]

4 Empirical Strategy

Our empirical strategy exploits a randomized experiment at the village level conducted by the World Bank as part of an assessment of the 4Ps program's impact on poverty, education, and health outcomes. 130 villages in 8 municipalities were randomly divided into 65 treatment villages and 65 control villages. In 2009, households in the treatment villages received conditional cash-transfers through the 4Ps, while households in the control villages did not receive transfers

We estimate the causal effect of the 4Ps program on civil conflict through a "difference-in-differences" approach, using data from the period 2001-09. Our baseline specification is:

$$Y_{it} = \beta_0 + \beta_1 Treat_i + \beta_2 Treat_i * Y2009_t + \beta_3 X_{it} + \lambda_t + \epsilon_{it} \tag{1}$$

where Y_it is the number of conflict incidents in village i at time t, $Treat_i$ is an indicator variable for villages assigned to the treatment group, and $Y2009_t$ is an indicator variable for observations in 2009, the year in which the 4Ps program was active in the treatment villages but not the control villages. The model further controls for year fixed effects, (which make it

[3]This is because the ASG only operates in remote areas of Basilan and Jolo provinces, which did not take part in the experimental evaluation since 4Ps was already operating both provinces by late 2008.

unnecessary to control for the uninteracted indicator for the program time period, $Y2009_t$) and a set of observed village characteristics X_it. The causal effect of the 4Ps program is captured by the parameter β_2, associated with the interaction between the indicators for the treatment group and for the program time period. The parameter β_1 captures baseline differences in levels of conflict between treatment and control villages. If the randomization was successful, we would expect no significant baseline differences, so that testing for a non-zero β_1 constitutes a robustness test of the randomization process.

4.1 Estimating Spillover Effects

The model in section 2 suggests that conditional cash-transfers in one village can affect conflict in neighboring villages through a spillover effect. To estimate the size and sign of a possible spillover effect, we follow an approach similar to that of Miguel (2004) and estimate the following equation:

$$Y_{imt} = \beta_0 + \beta_1 Treat_i + \beta_2 Treat_i * Prog_t + \beta_3 X_{it} + \gamma_d N^T_{dit} * Prog_t + \phi_d N^T_{dit} + \lambda_t + \theta_{mt} + \epsilon_{it} \quad (2)$$

where N^T_{dit} is the number of treatment villages within d kilometers of village i. The spillover effect is captured by the parameter $gamma_d$ which is associated with the number of villages within d kilometers that are currently receiving the 4Ps program (the interaction of N^T_{dit} and the treatment time-period). This identification strategy is based on the proximity of villages to each other, which may be correlated with unobserved village and municipality characteristics (for example, villages in more densely populated regions are likely to be closer together, so that they are likely to have a higher value of N^T_{dit}). We therefore control for the uninteracted variable N^T_{dit}, to control for time-fixed village characteristics, such as population density, that may be correlated with proximity to treatment villages. In order to control for time-varying shocks that differently affect more or less densely populated regions, we further

control for municipality-by-year fixed effects, $\theta_m t$.

5 Results

5.1 Data, Summary Statistics and Balance Tests

We use three different sources of data for our empirical analysis. Data on conflict incidents was compiled from unclassified portions of the reports submitted by units of the Armed Forces of the Philippines deployed to conduct counterinsurgency and other internal security operations in the field. The database includes information on every operational incident reported by the AFP during the period of observation of 2001–2009. In total, it contains information on almost 26,000 unique incidents.[4] The dependent variable is an annual count of conflict incidents per village. Data on the treatment assignment of villages comes from 4Ps program data maintained by the Philippine Department of Social Welfare and Development (DSWD). Data on village characteristics comes from the Philippines' 2000 National Census.

Table 2 presents summary statistics and balance tests for village-level control variables. The control variables consist of the villages population as well as indicators for the presence of paved streets, electricity, a communal water system and at least one store. All variables are from the 2000 National Census of the Philippines, except for the conflict incidents variable, which is the annual average over the pre-treatment period 2001-08.

The first two columns show means for treatment and control villages separately. Column 5 shows p-values of t-tests for differences in. The results show that treatment villages had slightly more conflict incidents in the pre-treatment period and slightly worse infrastructure than control villages, as they are less likely to have paved streets, electricity and stores, and more likely to have a communal water system.[5] However, these differences are not

[4]Felter (2005) provides a comprehensive overview of the AFP data. Replication data will be made available through the Empirical Studies of Conflict (ESOC) Project.

[5]Communal water systems are more likely to be present in poorer villages, while richer villages are likely to have piped water access to individual household

statistically significant at conventional significance levels, which increases our confidence that the randomization was successful.

Figure 2 shows graphical evidence of the effect of the 4Ps program on conflict. The top panel compares the trends in the average number of incidents experienced by treatment and control villages over the period of observation, 2001–2009, while the bottom panel plots the differences between the groups. The figure shows that treatment and control group had relatively steady and almost identical levels of conflict in the early pre-treatment period, 2001–2006. In 2007–2008, both groups experienced an upward trend, which was slightly steeper for the treatment group. In 2009, when the program was implemented in treatment villages, conflict in these villages dropped sharply; in control villages, by contrast, conflict continued on the same upward trend that it had followed during the previous years. To test whether the difference in conflict levels in the late pre-treatment period constitutes evidence for a failure of randomization, we conduct a robustness test for its statistical significance, which we report together with the main results in the next subsection.

The summary statistics show that the baseline level of conflict in the study area is relatively low. In the period from 2001 through 2008, villages experienced only about 0.1 conflict incidents per year. As a whole, the 130 experimental villages experienced a total of about 13 incidents per year. This is partly because villages are geographically small units—there are over 40,000 villages in the Philippines—and partly because the Philippine conflict tends to be of lower intensity when compared to other conflict-affected countries, like Afghanistan or Iraq.

The low level of violence does not mean that the conflict is economically insignificant. In addition to the lives and resources lost to violence, the mere presence of insurgents distorts economic incentives, by increasing entrepreneurial risks and/or imposing an implicit tax from extortion and bribes paid to insurgents for protection. From an empirical perspective, the disadvantage of a relatively small study area is that our results are necessarily based on only a small number of incidents. While this is unfortunate, it is part of the necessary trade-off

involved in using experimental rather than observational data. As we will show below, the estimated effect of the 4Ps program is large enough to be robustly statistically significant even though the estimate is based on relatively few incidents.

5.2 The Causal Effect of Cash Transfers on Conflict: Experimental Estimates

As mentioned above, we identify the causal effect of 4Ps on conflict using data from a randomized control trial of 130 villages in eight randomly-selected municipalities in four provinces that began in early 2009. All villages were untreated from 2001 until 2008, so that treatment status differs only in 2009. Since the dependent variable is a count of the number of incidents, we use negative binomial regressions for our estimations.[6] Table 3 displays the results of Equation 1 in section 4. To make interpretation easier, we report marginal effects instead of coefficients (note, however, that the asterisks in Table 3 denote significance of the underlying coefficient). The effect of treatment is identified by the interaction of the treatment indicator and the program year (2009) indicator. The results show that the effect of the 4Ps program is negative, large, statistically significant and robust to the inclusion of control variables (Column 2), municipality fixed effects (Column 3) and municipality-by-year fixed effects (Column 4). Standard errors in all specifications are adjusted for clustering at the province level. The point estimates suggest that the program reduced conflict by 0.2 incidents per village per year. If this effect could be extrapolated to all of the approximately 14,000 villages covered by the program, it would add up to a total reduction of approximately 2800 incidents per year (of course the program's actual effect may well be smaller, since not all areas are affected by conflict to the same extent, so that the program's effect cannot necessarily be extrapolated beyond the experimental villages).

[6]We use negative binomial instead of poisson models since the incidents variable exhibits overdispersion. However, we find qualitatively similar results in poisson models

5.3 Tests for Pre-Treatment Differences

In Table 3, the parameter associated with the treatment indicator captures the baseline difference in conflict between treatment and control group over the entire period of observation except for the program year of 2009. This difference is small and not statistically significant, which suggests that the randomization was successful, so that treatment and control villages do not differ in unobserved variables that affect conflict. However, the steeper increase in incidents in treatment groups in 2007 and 2008 raises the possibility that treatment and control villages may have experienced unobserved shocks in the late pre-treatment period, so that they may have differed in unobserved variables right before the start of the experiment. To test this, Table 5 presents estimates of the difference in conflict between treatment and control villages in the years leading up to the experiment. The results show that, while the number of incidents was higher in treatment villages in 2007 and 2008, the difference was not statistically significant. We therefore conclude that there is no evidence for a failure of randomization that resulted in unobserved differences between treatment and control villages before the start of the experiment.

5.4 Robustness to Outliers

One concern is that our results are driven by a small number of outliers that experience a large numbers of incidents in a given year. To test whether this is the case, we estimate the effect of the 4Ps program on the probability of a village experiencing *any* conflict incidents in a given year. Table 4 reports results of probit models that estimate this effect. The dependent variable used in these models is an indicator that takes a value of 1 if the village experienced at least one conflict incident in the current year. The results in Table 4 show that the 4Ps program caused a 2-10 percentage point decrease in the probability of experiencing conflict in a given year, which suggests that our results are not driven by outliers. These results are statistically significant at the one-percent level and robust to the inclusion of control variables, village fixed effects and municipality-by-year fixed effects.

5.5 Spillovers

Table 6 reports estimates of the spillover effect of the 4Ps program on neighboring villages, which are obtained from the regression model described in Equation 2 of section 4. The spillover effects are captured by the interaction between the number of treated villages within 5 (or within 5 and 10) kilometers of the village and the indicator for the treatment year of 2009.

The results show that the 4Ps program substantially reduced conflict in nearby villages through a spillover effect. The point estimates suggest that a treated village reduced the number of incidents in village within 5 kilometers by 0.03 incidents and in villages within 5 to 10 kilometers by 0.01 incidents, though the latter estimate is not statistically significant at conventional levels. Since each treatment village has on average 6 other villages within 5 kilometers, the total spillover effect adds up to about 0.18.

Thus, the aggregate reduction of conflict due to the spillover effect is about as large as the program's direct effect in the treated village itself. As outlined in Section 2, this spillover effect is consistent with a model in which the 4Ps program reduces conflict by making it more difficult for insurgents to recruit combatants in treated villages, so that these villages can "export" fewer combatants to carry out attacks in other villages[7].

6 Conclusion

This paper presents an experimental evaluation of the effect of a large conditional cash transfer program—the Philippines' *4Ps*—on the intensity of violence in civil conflict. Previous evaluations suggest that cash-transfer programs have positive effects on many dimensions of household welfare, which makes them one of the most popular anti-poverty interventions of the past decade. However, even though cash-transfer programs operate in many conflict-

[7]Unfortunately, we cannot rule out other plausible mechanisms. It is for example possible that the cash transfers to treatment villages boost the economy in nearby villages, which directly affects individuals' willingness to participate in conflict there

affected areas, to date there is little rigorous evidence on how they affect conflict.

Our experimental results suggest that the program caused a substantial reduction in the number of conflict incidents in the program area. In addition to finding a direct reduction of conflict in villages that received cash transfers, we also found evidence of a spillover effect, through which the program reduced conflict in nearby villages. This spillover effect is consistent with a simple model in which cash transfers reduce conflict by making it more difficult for insurgents to recruit combatants. As a result, villages where households receive cash transfers export fewer combatants to nearby villages, which reduces conflict there as well. However, while our results are consistent with this explanation, we cannot rule out that the spillover effect operated through a different channel—perhaps cash transfers directly improved the welfare of individuals in nearby villages by boosting the local economy.

Our findings contribute to the growing literature on the effects of aid and government interventions on conflict. They are consistent with previous findings that under certain conditions, interventions that "win hearts and minds" or increase the opportunity cost of joining an insurgency can reduce conflict (Berman et al., 2011; Iyengar et al., 2011; , n.d.). On the other hand, they are in contrast with the finding that another development project in the Philippines—the community-driven development program KALAHI-CIDSS—*increased* conflict (Crost and Johnston, 2010).

The difference in the effects of these two programs may be explained by the different ways in which they disburse funds. KALAHI-CIDSS disburses aid for small infrastructure projects through a participatory democratic process. As a result, it creates highly visible targets—the infrastructure itself as well as the community meetings needed to carry out the project. As suggested by the model of Crost and Johnston (2010), insurgents may have an incentive to attack these targets in order to derail the project. By contrast, the 4Ps program targets households and individuals directly and disburses aid in cash primarily through electronic transfers to beneficiaries' bank accounts. This gives insurgents fewer high-

profile targets and makes it more difficult to derail the project.[8] While this explanation is somewhat speculative it suggests that, because of their ease of implementation and lack of high-profile targets, conditional cash transfers may be a more effective way to disburse aid to conflict-affected areas than community-driven development projects.

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[8] There is anecdotal evidence, reported by Crost and Johnston (2010) that insurgents were able to prevent implementation of the KALAHI-CIDSS project in a number of areas, but no analogous evidence for the 4Ps program

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Figures and Tables

Figure 1: Map of 4Ps Study Areas

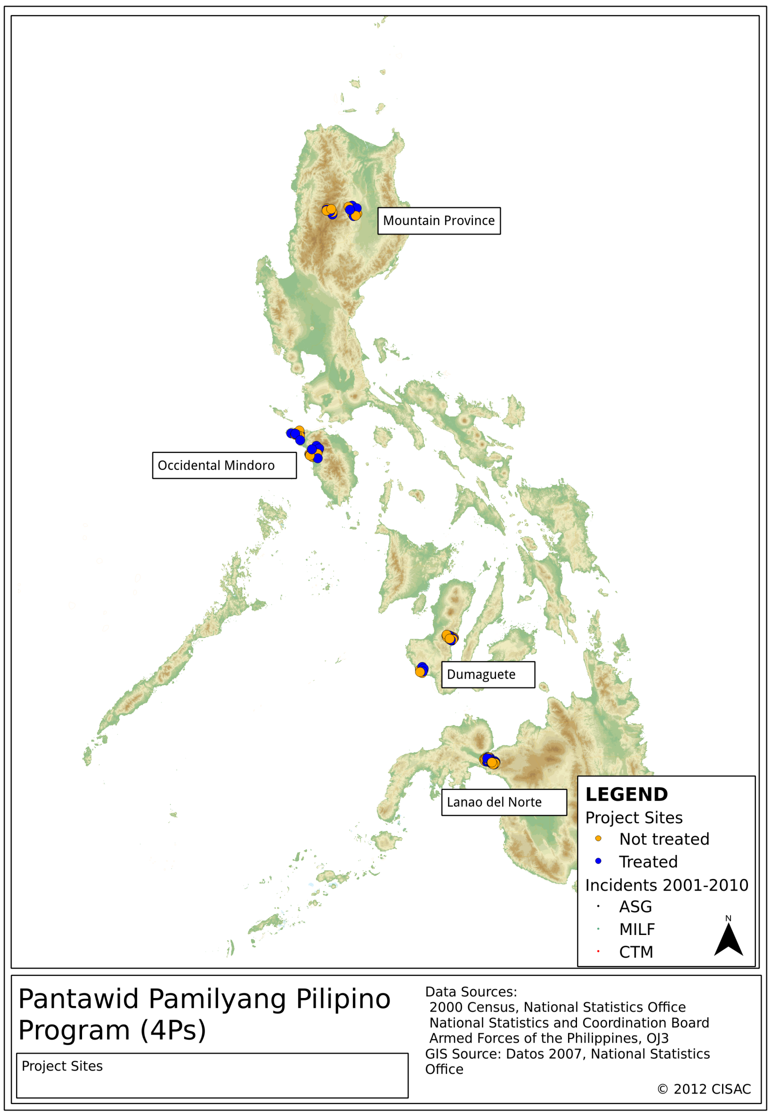

Figure 2: Time Trends of Conflict in Treatment and Control Villages

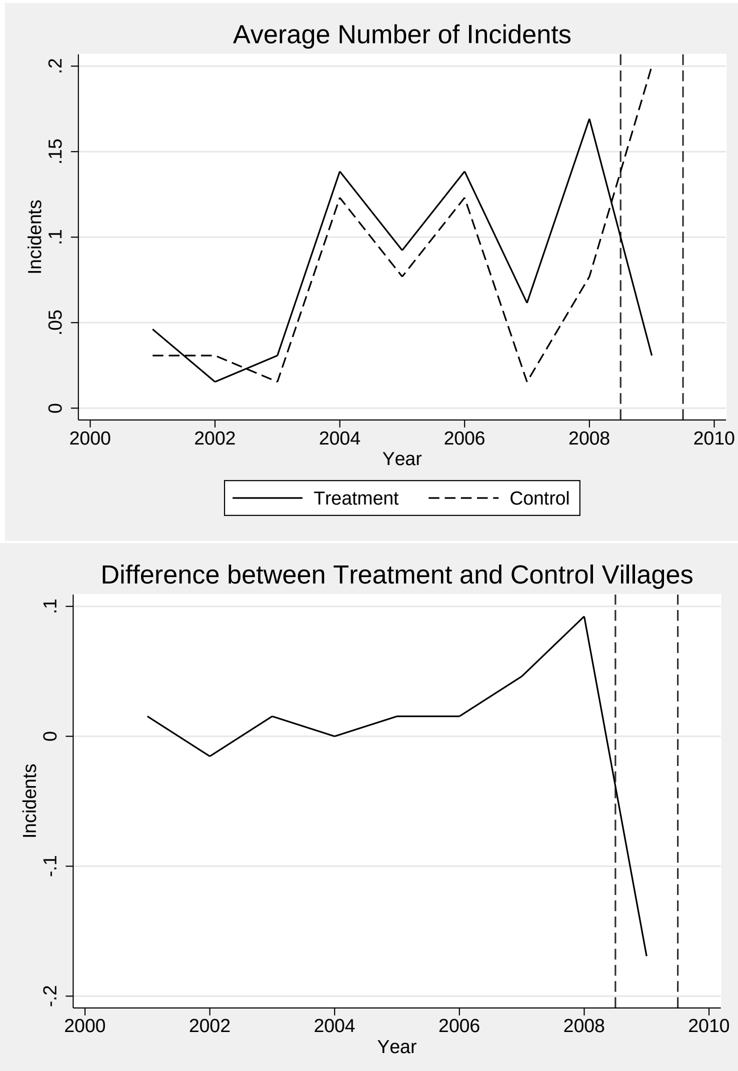

Table 1: 4Ps Experimental Sample

Region	**Province**	**Municipality**	**Treatments**	**Controls**
CAR	Mountain Province	Paracelis	4	5
CAR	Mountain Province	Sadanga	4	4
Region IV-B	Occidental Mindoro	Paluan	6	6
Region IV-B	Occidental Mindoro	Santa Cruz	5	6
Region VII	Negros Oriental	Jimalalud	15	13
Region VII	Negros Oriental	Basay	5	5
Region X	Lanao del Norte	Lala	13	14
Region X	Lanao del Norte	Salvador	13	12

Table 2: SUMMARY STATISTICS AND BALANCE TESTS

Variable	Treatment	Control	Difference	P-Value
Conflict Incidents	.087	.063	.023	.52
Population	1475	1419	55	.81
Paved Streets	.215	.313	.097	-.21
Communal Water System	.169	.154	.015	.81
Electricity	0.55	0.66	-0.11	0.21
Store	0.785	0.800	-0.015	.83
Observations	65	65	130	130

Summary statistics and balance tests of conflict incidents and village level control variables. The conflict incidents variable is the annual average over the pre-treatment period 2001-2008. All other variables are from the 2000 National Census of the Philippines.

Table 3: Direct Effect of Cash-Transfers on Treated Villages

	Negative Binomial Regressions Dependent Variable: Number of Incidents			
	(1)	(2)	(3)	(4)
Treatment x Program (t=2009)	-0.175***	-0.216***	-0.213***	-0.202***
	(0.144)	(0.140)	(0.082)	(0.055)
Treatment	0.028	0.016	0.030	0.034
	(0.045)	(0.031)	(0.052)	(0.048)
Population (1000)		0.032***	0.032**	0.034***
		(0.023)	(0.009)	(0.001)
Paved streets		-0.046***	-0.098***	-0.097***
		(0.023)	(0.018)	(0.010)
Electricity		-0.11***	-0.075**	-0.072***
		(0.032)	(0.044)	(0.022)
Communal Water System		0.067	-0.033	0.035
		(0.049)	(0.051)	(0.048)
Store		0.152***	0.007	0.017
		(0.090)	(0.018)	(0.019)
Municipality Fixed Effects	No	No	Yes	Yes
Municipality-by-Year FE	No	No	No	Yes
Observations	1300	1300	1300	1300
Villages	130	130	130	130

Reported values are marginal effects of negative binomial regressions with the annual number of incidents as the dependent variable. The period of observation is 2001-2009. The 4Ps experiment took place in 2009. *, ** *** denote statistical significance of the *underlying coefficient* at the 10%, 5% and 1% levels. Standard errors are clustered at the province level. All specifications include year fixed effects.

Table 4: Robustness to Outliers

	Probit Regressions Dependent Variable: Incidents>0			
	(1)	(2)	(3)	(4)
Treatment x Program (t=2009)	-0.041***	-0.033***	-0.021***	-0.096***
	(0.019)	(0.012)	(0.003)	(0.011)
Treatment	0.015	0.006	0.009	0.036
	(0.018)	(0.013)	(0.012)	(0.050)
Population (1000)		0.011**	0.0076*	0.033*
		(0.0064)	(0.0037)	(0.0017)
Paved streets		-0.0055	-0.016***	-0.064***
		(0.014)	(0.005)	(0.022)
Electricity		-0.054***	-0.027***	-0.11***
		(0.020)	(0.007)	(0.027)
Communal Water System		0.047*	0.0031	0.015
		(0.041)	(0.0091)	(0.032)
Store		0.037***	-0.0046	0.020
		(0.015)	(0.0059)	(0.029)
Municipality Fixed Effects	No	No	Yes	Yes
Municipality-by-Year FE	No	No	No	Yes
Observations	1300	1300	1300	1300
Villages	130	130	130	130

Reported values are marginal effects of probit regressions with an indicator for having any incidents in a given year as the dependent variable. The period of observation is 2001-2009. The 4Ps experiment took place in 2009. *, ** *** denote statistical significance of the *underlying coefficient* at the 10%, 5% and 1% levels. Standard errors are clustered at the province level. All specifications include year fixed effects.

Table 5: Test for Equality of Pre-Treatment Trends

	Negative Binomial Regressions Dependent Variable: Number of Incidents			
	(1)	(2)	(3)	(4)
Treatment x Program (t=2009)	-0.159***	-0.213***	-0.202***	-0.206***
	(0.125)	(0.145)	(0.084)	(0.076)
Treatment x (t=2005)	0.0028	-0.018	-0.0016	-0.025
	(0.032)	(0.020)	(0.036)	(0.032)
Treatment x (t=2006)	-0.0022	-0.035	-0.054	-0.063
	(0.047)	(0.038)	(0.076)	(0.074)
Treatment x (t=2007)	0.098	0.078	0.076	0.081
	(0.104)	(0.082)	(0.061)	(0.052)
Treatment x (t=2008)	0.050	0.033	0.051	0.024
	(0.038)	(0.066)	(0.053)	(0.052)
Treatment	0.012	0.014	0.027	0.038
	(0.051)	(0.049)	(0.067)	(0.072)
Control Variables	No	Yes	Yes	Yes
Municipality Fixed Effects	No	No	Yes	Yes
Municipality-by-Year FE	No	No	No	Yes
Observations	1300	1300	1300	1300
Villages	130	130	130	130

Reported values are marginal effects of negative binomial regressions with the annual number of incidents as the dependent variable. The period of observation is 2001-2009. *, ** *** denote statistical significance of the *underlying coefficient* at the 10%, 5% and 1% levels. Standard errors are clustered at the province level.

Table 6: Spillovers

	Negative Binomial Number of Incidents		Probit Any Incidents	
	(1)	(2)	(3)	(4)
Treatment x Program (t=2009)	-0.227***	-0.218***	-0.100***	-0.092***
	(0.089)	(0.067)	(0.016)	(0.013)
# treated within 5 km x Program (t=2009)	-0.034***	-0.029***	-0.031***	-0.029***
	(0.003)	(0.007)	(0.010)	(0.006)
# treated between 5 and 10 km x Program (t=2009)	-0.013	-0.005	-0.011	-0.007
	(0.011)	(0.021)	(0.011)	(0.010)
Treatment	0.034	0.026	0.044	0.035
	(0.060)	(0.052)	(0.063)	(0.051)
# treated within 5 km	-0.011	0.003	-0.006	0.002
	(0.021)	(0.012)	(0.018)	(0.008)
# treated between 5 and 10 km	0.004	0.009	-0.001	0.001
	(0.009)	(0.026)	(0.010)	(0.013)
Control Variables	No	Yes	Yes	Yes
Municipality Fixed Effects	No	No	Yes	Yes
Municipality-by-Year FE	No	No	No	Yes
Observations	1300	1300	1300	1300
Villages	130	130	130	130

Reported values are marginal effects of negative binomial regressions with the annual number of incidents as the dependent variable. The period of observation is 2001-2009. The 4Ps experiment took place in 2009. *, ** *** denote statistical significance of the *underlying coefficient* at the 10%, 5% and 1% levels. Standard errors are clustered at the province level. All specifications include year fixed effects.