Supporting Information

How Does Community Policing Affect Police Attitudes?
An Experimental Test and a Theory of Bureaucrat-Citizen Contact

American Political Science Review

Dotan Haim, Matthew Nanes, and Nico Ravanilla

Table of Contents

Supporting Information **SI 2**

A.1 Intervention and Sampling Details . . . . . SI 2

A.2 Descriptive Statistics . . . . . SI 5

A.3 Main Results . . . . . SI 11

A.4 Robustness . . . . . SI 14

A.5 Qualitative Methodology . . . . . SI 28

A.6 Ethics and Permissions . . . . . SI 31

A.7 Study Timeline . . . . . SI 33

A.8 Pre-Analysis Plans . . . . . SI 33

A.1 Intervention and Sampling Details

The One Sorsogon program we evaluate in this research was a large province-wide community policing initiative rolled out by the Sorsogon PNP. As we discuss in the main text, the impetus behind this initiative was to repair citizen-police relations in Sorsogon in the wake of the violent war against drugs occurring elsewhere in the Philippines. In this section, we begin by describing the nature of community-facing police activities in Sorsogon prior to the intervention. We then describe the One Sorsogon community policing program, which represented a wholesale change in the citizen-facing activities for regular beat officers assigned to treatment. Finally, we include details on sampling for the randomized intervention.

A.1.1 Community Policing Before One Sorsogon

As we describe in detail in SI Section A.6 on research ethics, the Sorsogon Police Chief who worked with us on the experiment was a known reformer who implemented several citizen outreach programs in the two years he was assigned to Sorsogon prior to our experiment. This included a program to engage with at-risk youth at schools, a program to connect students interested in law enforcement careers with similar students in other countries via Facebook, and efforts to improve the PNP's social media image. However, the previous versions of "community policing" in Sorsogon, and in the Philippines generally, were of limited scope, specifically with regard to which officers participated in these programs. Each municipal station had a single officer who was assigned as the "police community relations" (PCR) director, and whose role centered on public outreach. The Sorsogon PNP chief's previous citizen outreach programs were implemented almost exclusively by these specialized PCR officers (who were excluded from our sampling frame). The PNP did hold periodic meetings with barangay officials to identify local issues, referred to as "Enhanced Managing Police Operation" (EMPO) meetings, but these meetings were also led by the specialized PCR officers and were far less frequent than our POP meetings. On our baseline survey of the barangay leaders, nearly 40% reported no contact with the police in the six-month period prior to the intervention. On the plus side, the existence of the EMPO meetings provided a baseline level of familiarity with the model for station leadership and PCR directors that aided their ability to support the wider POP intervention.

The ordinary beat patrol officers in Sorsogon who were eligible for the intervention had very limited involvement in any of these programs. These officers virtually never engaged in the usual components of community policing, such as structured engagement with community leaders, extensive citizen-facing foot patrols, or leading town-hall meetings with ordinary citizens. Officers we spoke with agreed that our version of POP – especially the regularity of meetings and intensity of follow-ups to identified problems – constituted a wholesale change. Therefore, we view it as unlikely that our null findings stem from insufficient differences between existing and newly-implemented practices of community policing.

A.1.2 The "One Sorsogon" Community Policing Program

The core components of the intervention were 1) a two-day training program on community policing, 2) monthly "Problem-Oriented Policing" (POP) meetings with barangay leaders at the municipal station, 3) citizen-facing foot patrols in treatment barangays between the meetings, 4) monthly meetings with the Station Chief and PCR Director to discuss progress, and 5) a culminating town hall with barangay citizens. Treatment officers participated in all these activities, while control officers participated in none of them. Disaggregating the effects of the various aspects of community policing is certainly an important outstanding question in the literature, but we chose to bundle the treatment in order to stay true to how community policing is normally implemented, as well as to implement the strongest version of the program in the face of resource constraints.

Before the community policing program began, PNP officers assigned to POP teams and one LGU representative from each participating barangay attended a two-day training workshop in Sorsogon City. The PIs collaborated

with PNP leadership and a Manila-based consultant to develop the workshop, which aimed to provide officers and local government unit (LGU) representatives with a comprehensive understanding of the theory of change behind community policing, as well as to explain their roles in the POP meetings they would attend over the subsequent seven-month intervention. At the training, treatment officers were paired with the barangay leaders in their assigned barangay, practiced using crime reports and barangay logbooks to identify the most pressing local public safety issues, learned techniques for empathetic communication, and focused on recognizing and reporting officer misconduct, including corruption

From December 2017 to May 2018, POP teams from the participating barangays met on a monthly basis in the municipal police station. During Meeting 1, each team discussed current public safety problems in the barangay, and identified one or two issues that would provide the focus for their intervention. Teams were provided with barangay-level data showing the types of crime that citizens experienced most often, along with anonymized logs of SMS tips sent to the PNP that referenced their barangay. Barangay leaders were encouraged to bring their barangay public safety logbooks to the meeting, which are overseen by the chief tanod and the barangay secretary.

In the second and third meetings, the teams were tasked with crafting a strategy to address their selected issue(s) and developing a proposed budget of up to 5,000 pesos (about $100 US) for implementation. After reviewing the plans for compliance with funder regulations, the research staff provided the requested budget to each POP team. In the fourth, fifth and sixth months of the intervention, teams dedicated their resources to implementing their projects. Some barangays used the opportunity to increase foot patrols and used their budgets for basic equipment like flashlights, whistles and rain ponchos. Other barangays took steps to improve relevant infrastructure, like constructing fences along highways to prevent stray dogs from causing traffic accidents, installing streetlights in strategic locations to reduce incidents of vagrancy and public intoxication, and adding road safety signage. To address more complex public safety issues, such as juvenile delinquency, barangays implemented activities to engage those at risk. One POP team chose to purchase sports equipment for at-risk youth to use after school, while another POP team created a community garden tended to by at-risk youth.

During the intervention period, all participating PNP officers were encouraged by their superiors to travel to their assigned barangay at least once between each POP meeting. Officers often helped the barangay leadership implement the solutions they decided on during POP meetings, and informally met with citizens to get their perspective on the focus public safety issue. Subsequently, participating barangays held culminating town halls to reinforce their progress and discuss ways to continue activities that would promote public safety in the future. The members of the POP team led the town halls by describing their activities and plans, after which there was a long period of Q&A and unstructured discussion. On average, these culminating town halls were attended by nearly 80 citizens and lasted more than three hours. We provide additional details about the intervention in the Supporting Information (SI). The key characteristic of the POP program is that each barangay's POP team decided individually what projects they would take on, but all teams operated within the same structure and shared the same goal of solving community-specific issues through preventive interventions.

Municipal police chiefs and "Police Community Relations" (PCR) directors were instructed by the Provincial Police Office (PPO) to conduct monthly check-ins with each pair of treatment officers assigned to a POP team to discuss progress and surface any citizen reports of police corruption. Officers were aware that the municipal chiefs and PCR directors also met monthly with the Provincial PNP Chief and his staff to discuss the progress of his flagship community policing program. One goal of having officers work in teams was to create the sense that corrupt behavior may be reported by officers' well-meaning colleagues, especially after the common knowledge about reporting practices was generated by the training. Importantly, the One Sorsogon program started shortly after President Duterte and the PNP Director-General stated that they were instituting a nationwide pause to the Drug War to instead focus on tackling police corruption (link).

As described above, a substantial portion of the treated officers' formal interactions as part of One Sorsogon were with barangay leaders rather than "ordinary" citizens. We designed the intervention this way in part to reflect

the types of "citizens" focused on by prior research on community policing. Community policing interventions, and POP in particular, often focus on increasing contact between police officers and "community leaders," who typically have a bigger picture understanding of crime in their community and can better assist the police in implementing reforms. Barangay captains and councilors are elected officials, and tanods are appointed by the barangay captain, but none of these positions is a full-time job, and these individuals are only loosely tied to municipal and national-level governments. For example, only 60% of barangay leaders said they had contacted the police in the *six-month* period prior to the intervention, suggesting that the intervention represented a substantial change to police officers' interactions with barangay leaders. In the context of police-community relations, the barangay leaders on the POP teams are best viewed as advocates for their barangay, rather than an arm of the national government. From the perspective of the PNP, barangay leaders were discussed as "citizen representatives," and they play a similar role to informal village leaders and community watch members in other parts of the Global South. In other words, we view the community leaders involved in the POP intervention as being comparable to a class of "citizens" discussed in much of the prior research on community policing.

In addition to the randomization of PNP officers into the intervention, several aspects of One Sorsogon program were randomized across barangays. The component of the project that is the focus of this paper was part of a larger effort to evaluate community policing, including citizen-facing aspects. From the 298 barangays in our sample, 99 were assigned to receive POP teams that included PNP officers, while 99 received a version of the intervention that was led only by the LGU officials. The remaining 100 barangays were "control" units that received policing-as-usual. In addition to the POP teams, another aspect of One Sorsogon was that the PNP upgraded a previously underutilized hotline that citizens could use to contact the police, paired with a large-scale drive to advertise the hotline. We provide further details on the intervention and the research design we used to evaluate the citizen-facing aspects of the program in the PAP. The officer randomization only occurred *within* the first category of treatment barangays in the larger study (those that received POP teams including PNP officers), meaning we cannot leverage the barangay-level randomization for analyzing the effects on officers. The barangay-level randomization only matters insofar as it increases the representativeness of the study areas to the larger sample of barangays in Sorsogon (which speaks to external validity).

A.1.3 Sampling

The primary sampling unit for the geographical randomization is the barangay, the smallest administrative unit in the Philippines. As discussed in-text, we began by identifying the barangays in Sorsogon Province which have sufficient government control as to make the community policing intervention feasible and safe. From there, we randomly assigned 99 barangays to receive the POP treatment with only LGU members, 99 brangays to receive the POP treatment with both LGU and PNP members, and 100 barangays to receive the control condition of "policing as usual," i.e. largely reactive policing that generates little contact between civilians and police officers in non-emergency situations.

We then surveyed every police officer in Sorsogon province twice, once shortly before the intervention began and a second time shortly after it ended. The survey was conducted via in-person self enumeration: trained research staff visited each municipal police station at a pre-appointed time, explained the study to the police officers, and gave them tablet computers loaded with the survey. Officers completed the survey on their own and then returned the tablets to the staff. Staff remained available to answer any questions during the survey, but officers entered their responses themselves in an effort to ensure privacy.

For the officer-level randomization, we relied on officers who took the baseline officer survey as our sampling frame. We excluded non-uniformed personnel and the upper-level leadership in each municipal police station because the intervention did not fit their professional responsibilities. Officer sampling was blocked on police stations in order to assign each treatment officer to participate on a POP team in the jurisdiction where they were

assigned. Second, we blocked on officer rank. The PNP wanted to assign at least one "senior" officer to each team to ensure that officers had the experience necessary to successfully lead the intervention.

A.2 Descriptive Statistics

A.2.1 Summary Statistics

Tables A.1 and A.2 display descriptive statistics for the officers in our sampling frame and for the municipalities where the intervention was implemented, respectively. Municipality sociodemographic variables are from the 2010 Philippine Census, while the NPA presence variable is from AFP intelligence reports in December 2015. Officer characteristics are from our officer survey.

Table A.1: Summary Statistics - Survey

	Baseline			**Endline**		
Variable	N	Mean	SD	N	Mean	SD
Treated	691	0.28	0.45	705	0.33	0.47
Age	679	35	6.9	678	35	6.8
Male	688	0.76	0.42	704	0.77	0.42
Completed College	687	0.18	0.39	701	0.2	0.4
Catholic	688	0.91	0.29	0		
Rank (Senior)	691	0.2	0.4	705	0.18	0.38
Years Served	691	10	6.5	703	9.8	6.4
Years Served in Mun	684	4.3	4.4	699	4.4	4.6
Lives in Province	691	0.87	0.33	705	0.88	0.33
Lives in Municipality	691	0.54	0.5	705	0.56	0.5
Share Concerns	625	1.5	0.84	698	1.3	0.85
Officer Attitude Index	528	-0.038	0.54	568	-0.0098	0.55
Officer Attitude Idx (alt)	676	-0.021	0.53	698	-0.0066	0.56
Trust Index	665	-0.13	0.83	680	0.0037	0.82
Trust Index (alt)	678	-0.14	0.84	694	-0.0021	0.82
Trust (Safe)	682	71	26	690	75	26
Trust (Intent)	674	76	22	693	80	22
Trust (Info)	671	77	19	693	79	20
Empathy Index	665	-0.065	0.77	676	-0.016	0.8
Empathy Index (alt)	684	-0.058	0.77	700	-0.018	0.8
Empathy (Complaints)	669	62	27	680	62	29
Empathy (Reports)	680	82	18	696	84	19
Accountability Index	581	-0.04	0.67	612	0.0092	0.63
Accountability Idx (alt)	609	-0.039	0.67	641	0.0073	0.63
Accountability (Care)	683	87	17	694	90	15
Accountability (Hyp1 - Punish)	678	3	1.4	695	2.8	1.3
Accountability (Hyp1 - Report Self)	632	77	28	667	80	25
Accountability (Hyp1 - Report Other)	624	76	28	663	78	26
Accountability (Hyp2 - Punish)	680	2.5	1.3	692	2.5	1.3
Accountability (Hyp2 - Report Self)	631	71	29	655	72	28
Accountability (Hyp2 - Report Other)	627	69	29	645	71	28
Corruption Idx	614	0.059	0.8	660	-0.0091	0.76
Corruption Idx (alt)	664	0.048	0.79	690	-0.0059	0.76
Corruption (Hyp1 - Self)	661	93	18	691	94	17
Corruption (Hyp1 - Other	661	90	22	688	91	22
Corruption (Hyp2 - Self)	664	78	27	690	81	27
Corruption (Hyp2 - Other	663	76	28	688	77	30

Table A.2: Summary Statistics - Municipalities

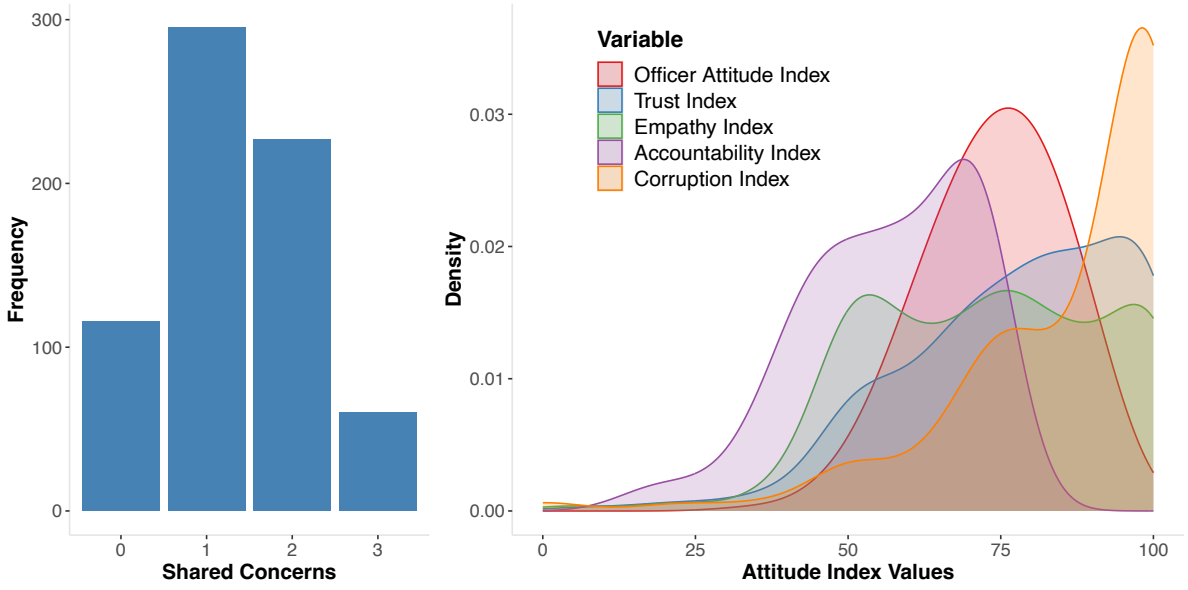
Variable	N	Mean	SD	Min	Max
Population (mun)	17	34,136	27,277	9,338	86,728
Household Size (mun)	17	3.3	0.18	2.8	3.5
Education (mun)	17	1.8	0.13	1.7	2.1
Percent Catholic (mun)	17	0.95	0.011	0.93	0.98
Home Quality (mun)	17	0.38	0.0074	0.37	0.39
Avg. NPA Presence 2015 (mun)	17	0.34	0.36	0	1.2

A.2.2 Outcome Measures and Index Construction

The "Shared Concerns" outcome was calculated in four steps. First, we asked officers: "Below is a list of public safety concerns. What do you consider to be the three most important public safety concerns in the municipality where you are assigned." Officers selected from the 13 public safety concerns displayed in Table 1 in the article. Second, we asked the same question (specific to the barangay) on our survey of the citizen barangay leaders, which included the barangay captain, barangay counselor in charge of peace and order, and the chief tanod. Third, we aggregated the ranking of these issues from the barangay leader survey to the municipality-level. Fourth, for each officer, we counted how many of the officer-identified issues matched the four most common issues from the aggregated barangay leader survey responses in the same municipality. Figure A.1(a) shows the distribution of the *Shared Concerns* measure at baseline. In Section A.4.2, we show that the results are robust to alternative ways of measuring this variable.

Figure A.1: Outcome Distributions

(a) Shared Concerns (b) Attitude Indices



The above figures show the distribution of the outcome variables at baseline. The left panel shows the distribution of the shared concerns (knowledge) variable, which counts how many of the most important public safety issues identified by officers match the issues identified by community leaders in the same municipality. The right panel shows the distribution of the raw attitudinal indices.

Item	Question	Options
	TRUST	
trust_safe	How safe for officers is the municipality where you are assigned.	0-Very Dangerous; 100-Very Safe
trust_intent	Citizens care about the well-being of police officers.	0-Strongly disagree; 100-Strongly agree
trust_info	Information that I receive from civilians is likely to be accurate.	0-Strongly disagree; 100-Strongly agree
Trust Index		Index of: *trust_safe, trust_intent, trust_info*
	EMPATHY	
empathy_complain	When people complain about the police, it's because they have a good reason.	0-Strongly disagree; 100-Strongly agree
empathy_report	Most things that people report to the police are worth taking seriously.	0-Strongly disagree; 100-Strongly agree
Empathy Index		Index of: *empathy_complain, empathy_report*
	HYPOTHETICAL SCENARIOS	
	A police officer stops a motorist for speeding. The officer agrees to accept a personal gift of half of the amount of the fine in exchange for not issuing a citation.	
h2_corrupt_self	Do you consider this behavior to be serious misconduct?	0-Not at all serious; 100-Very serious
h2_corrupt_other	Do most police officers consider this behavior to be serious misconduct?	0-Not at all serious; 100-Very serious
h2_punish	If an officer engaged in this behavior and was discovered doing so, what if any discipline do you think will follow?	0-None; 1-Verbal reprimand; 2-Written reprimand; 3-Period of suspension without pay; 4-Demotion in rank; 5-Dismissal
h2_report_self	Do you think you would report a fellow police officer who engaged in this behavior?	0-Definitely not; 100-Definitely yes
h2_report_other	Do you think most police officers would report a fellow police officer who engaged in this behavior?	0-Definitely not; 100-Definitely yes
	A police officer routinely accepts free meals, cigarettes, and other items of small value from merchants on his beat. He does not solicit these gifts and is careful not to abuse the generosity of those who give gifts to him.	
h3_corrupt_self	Do you consider this behavior to be serious misconduct?	0-Not at all serious; 100-Very serious
h3_corrupt_other	Do most police officers consider this behavior to be serious misconduct?	0-Not at all serious; 100-Very serious
h3_punish	If an officer engaged in this behavior and was discovered doing so, what if any discipline do you think WILL follow?	0-None; 1-Verbal reprimand; 2-Written reprimand; 3-Period of suspension without pay; 4-Demotion in rank; 5-Dismissal
h3_report_self	Do you think you would report a fellow police officer who engaged in this behavior?	0-Definitely not; 100-Definitely yes
h3_report_other	Do you think most police officers would report a fellow police officer who engaged in this behavior?	0-Definitely not; 100-Definitely yes
	ACCOUNTABILITY	
account_care	The police leadership takes citizen complaints about officers seriously.	0-Strongly disagree; 100-Strongly agree
Accountability Index		Index of: *account_care, h2_punish, h2_report_self, h2_report_other, h3_punish, h3_report_self, h3_report_other*
	CORRUPTION	
Corruption Index		Index of: *h2_corrupt_self, h2_corrupt_other, h3_corrupt_self, h3_corrupt_other*
	NPA SYMPATHY	
npa_sympathy	What percentage of civilians in the municipality where you are assigned would you estimate are sympathetic to the NPA?	0%; 100%

The attitudinal outcome indices — *Trust*, *Empathy*, *Accountability*, and *Corruption* — were created by adding the values of individual survey items (displayed above), and then dividing by the number of items. The umbrella *Officer Attitude Index* is an index of indices, created by adding the values of the other attitudinal indices and dividing by the number of constituent indices. Most individual survey items were captured using a slide bar in ISurvey that translated to a 100-point scale when the data were exported. The exceptions were *h2_punish* and *h3_punish*, part of the accountability index, which were measured on a 6-point ordinal scale. Following the PAP, we standardized each survey item before combining them into the index by subtracting the mean and dividing by the standard deviation of the variable at baseline. Figure A.1(b) displays the distribution of the attitudinal indices at baseline. For ease of interpretation, the figure displays versions of the indices created using the raw, non-standardized item responses.

In Table A.12, we use an alternative version of the main indices where values for missing items were imputed using other items from the same respondent. For example, the trust index is made up of three sub-items: *trust_info*,

trust_safe and *trust_intent*. To impute values of *trust_info*, we regress *trust_info* on *trust_safe* and *trust_intent* and extract the predicted values. We then use the same procedure to impute values for *trust_safe* and *trust_intent* before combining the items into the index. This method is described in the PAP.

A.2.3 Balance and Attrition

Table A.3 displays the balance of officer characteristics based on their treatment status. To account for the blocked randomization, we display outputs of regressions that predict treatment status based on each pre-treatment officer characteristic, including fixed effects for the blocking variables. In other words, the displayed estimates compare treatment officers to control officers in their same station and rank. None of these differences are statistically significant at conventional levels, though treatment officers were marginally less educated than control officers. In SI section A.4, we show that the main results hold when controlling for officer education.

In Table A.4, we analyze attrition. Officer turnover over the 8 months between our baseline survey and endline survey was relatively high. Of the 705 eligible officers we interviewed at baseline (rank PO or SPO), 190 dropped out from the sample before endline. The majority of attrition was due to officers being reassigned to different provinces, being assigned to training in the regional/national capitol, and retirements. Between baseline and endline, 183 new officers who would otherwise have been eligible to participate on a community policing team were hired or transferred to Sorsogon.

Table A.3: Balance Table

Variable	**Estimate**	**p-value**
Age	0.004	0.208
Male	0.026	0.466
Education	-0.071	0.041
Catholic	-0.047	0.564
Lives in Sorsogon	0.059	0.262
Commute	-0.013	0.336
Years Served	-0.001	0.836
Share Citizen Concerns	0.034	0.105
Officer Attitude Index	-0.030	0.286
Trust Index	-0.009	0.687
Empathy Index	-0.020	0.246
Accountability Index	-0.017	0.645
Corruption Index	0.011	0.692

Each row reports the estimates and p-values from regressions that predict treatment status based on pre-treatment officer attributes (demographic and attitudinal). All regressions include station and rank fixed effects to account for blocking. Standard errors are clustered at the station level.

As a result of this turnover, two comparisons are particularly important. First, were the officers who dropped out of our sample after baseline significantly different from the officers who remained in the sample? We find that these two groups were very similar. Most importantly, officers' treatment status did not significantly predict whether they dropped out (estimate=-0.06, p=.29). In Table A.4, this comparison can be seen for all demographic covariates where the Attrition Category is "Both Surveys" (the comparison group in these rows is officers who only took the baseline survey) and all survey indices where the Attrition Category is "Baseline Only" (the comparison group in these rows is officers who took both surveys). Most of these differences are insignificant. Second, because we include the group of newly hired officers in our analysis, it is important that this group of new officers who joined the sample does not differ significantly from the group of officers who left the sample. In Table A.4, this comparison can be seen for all demographic covariates where the Attrition Category is "Endline Only" (the comparison group

Table A.4: Attrition Table

Variable	Attrition Category	Estimate	p-value
Male	Endline Only	-0.029	0.30
Male	Both Surveys	0.002	0.97
Age	Endline Only	-3.504	0.08
Age	Both Surveys	-0.317	0.70
College Educated	Endline Only	0.014	0.57
College Educated	Both Surveys	-0.025	0.47
Catholic	Both Surveys	-0.008	0.59
Years Served	Endline Only	-3.030	0.10
Years Served	Both Surveys	-0.310	0.73
Lives in Sorsogon	Endline Only	0.009	0.87
Lives in Sorsogon	Both Surveys	0.097	0.01
NPA Presence 2015	Endline Only	0.016	0.87
NPA Presence 2015	Both Surveys	0.042	0.23
Crime Rate	Endline Only	0.023	0.72
Crime Rate	Both Surveys	-0.029	0.24
Attitude Idx (endline)	Endline Only	-0.055	0.20
Trust Idx (endline)	Endline Only	-0.124	0.26
Empathy Idx (endline)	Endline Only	-0.052	0.31
Account Idx (endline)	Endline Only	0.148	0.00
Corrupt Idx (endline)	Endline Only	-0.226	0.00
Attitude Idx (baseline)	Baseline Only	0.067	0.24
Trust Idx (baseline)	Baseline Only	-0.101	0.28
Empathy Idx (baseline)	Baseline Only	-0.001	0.99
Account Idx (baseline)	Baseline Only	0.057	0.31
Corrupt Idx (baseline)	Baseline Only	0.074	0.31

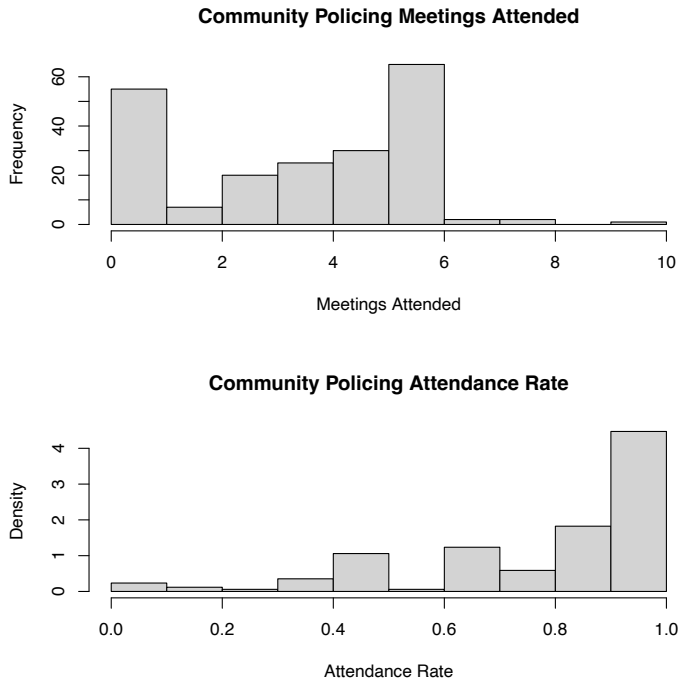
Each row reports the estimates and p-values from regressions that predict covariates based on officers' attrition status. These estimates are calculated without respect to the blocks unlike in Table A.3, since only treatment was assigned with respect to the blocks. Attrition Category is a categorical variable with the following levels: *Baseline Only* (officers who were assigned in Sorsogon at baseline but attritioned before endline), *Endline Only* (officers who were newly assigned in Sorsogon after baseline), and *Both Surveys* (officers who were assigned in Sorsogon through the duration of the experiment). The comparison group for the demographic variables is *Baseline Only* (with the exception of Catholic, which was measured only at endline). The comparison group for all survey indices is *Both Surveys*. Because the endline survey indices (last four rows) are measured post-treatment, these regressions are subset to officers in the control group.

in these rows is officers who only took the baseline survey) and all survey indices where the Attrition Category is "Endline Only" (the comparison group in these rows is officers in the control group who took both surveys).

A.2.4 Treatment Compliance

Figure A.2 displays 1) the number of POP meetings that each treatment officer attended (regardless of whether they later had to drop off a POP team due to reassignment) and 2) the POP meeting attendance rate (subset to the officers who were still active members of a POP team). Both histograms are subset to officers who were randomly selected to participate in the community policing intervention. The first histogram shows that the modal officer attended all six POP meetings, but a number of treatment officers attended zero community policing meetings. This occurred because, right after the intervention began, a number of officers were assigned to a training program in Manila, which also corresponded with a batch of officer reassignments to different stations. Because these officers underwent the community policing training, we consider them "treated" when calculating the ITT. A few officers attended more than six meetings because they temporarily stepped in for a colleague on another barangay's POP team. The second histogram displays the "attendance rate," which is calculated as the number of meetings attended

Figure A.2: Treatment Compliance and Intensity



The top histogram displays the number of POP meetings attended by each treatment offier, regardless of whether they later had to drop off a POP team due to reassignment. The bottom histogram is subset to officers who were still active members of a POP team, excluding reassigned officers.

divided by the number of meetings to which an officer was *assigned*. If, during the intervention, an officer was removed from a POP team, we no longer categorized them as being assigned to attend future meetings.

The above data only allow us to measure compliance with the meetings with barangay leaders, but not interactions with "ordinary" citizens. We do not have systematic data on the number of citizen-facing foot patrols officers participated in between POP meetings, but we note the following relevant facts:

- The intervention concluded with a culminating town hall to which all barangay citizens were invited, and this was officers' last assigned activity before the endline survey. On average, these culminating town halls were attended by nearly 80 citizens and lasted more than three hours. Attendance at the citizen-facing culminating town hall among officers assigned to POP was over 90%.
- Our field officers, who attended each POP meeting, reported that most officers visited their assigned barangay at least once between each POP meeting. These barangay visits typically included a conversation with the barangay leaders, but also included foot patrols targeted at increasing contact with ordinary citizens.
- On our representative endline survey of "ordinary" citizens, we asked respondents if they had interacted with a PNP officer as part of the One Sorsogon program. In treatment villages, 15% of respondents indicated that they had interacted with an officer as part of the program, while only 5% of respondents in control villages reported such an interaction.
- Between the fourth and fifth POP meetings, our field officers conducted an "audit" of whether the assigned officers had interacted with citizens and whether citizens were aware of details of the One Sorsogon program. Field officers were tasked with speaking with one tanod and one barangay counselor who were not on the POP team, plus three additional citizens in the area around the barangay center, using a simple random

walk protocol. We asked citizens to identify the public safety issue focused on by the POP team, to identify actual awareness. In this audit, 40% of ordinary barangay residents correctly identified the issue that the POP team was working on, without any prompting.

A.3 Main Results

In this section, we show the full regression tables underlying the figures in the manuscript. Table A.5 shows the regression results displayed in Figure 1. Table A.6 shows the regression results displayed in Figure 2. Table A.7 shows the regression results displayed in Figure 4.

Table A.5: Main Treatment Effects

	Share Concerns		Attitude Idx		Trust		Empathy		Accountability		Corruption	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated	0.21*	0.25**	0.06	−0.01	−0.06	−0.02	−0.12	−0.08	−0.00	0.01	0.12	0.06
	(0.10)	(0.10)	(0.06)	(0.04)	(0.06)	(0.06)	(0.10)	(0.09)	(0.07)	(0.07)	(0.08)	(0.09)
Base Outcome	0.27***		0.26***		0.34***		0.28***		0.43***		0.26***	
	(0.03)		(0.06)		(0.05)		(0.04)		(0.04)		(0.06)	
Model	Panel	Cross	Panel	Cross	Panel	Cross	Panel	Cross	Panel	Cross	Panel	Cross
R^2	0.13	0.07	0.10	0.04	0.21	0.13	0.10	0.04	0.16	0.05	0.10	0.05
Observations	627	632	460	534	656	671	652	672	536	587	589	621
Clusters	18	18	18	18	18	18	18	18	18	18	18	18

* $p < .10$, ** $p < .05$, *** $p < .01$
Panel models include a dummy variable for missing baseline.
All models include station (municipality) and rank fixed effects for the blocking variables.
Standard errors are clustered at the station-level.

Table A.6: Heterogeneous Treatment Effects - Embeddedness

	Share Concerns	Attitude Index	Trust	Empathy	Account	Corrupt	Attitude Idx (alt)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.28*	0.31**	0.29	0.28	0.08	0.12	0.21
	(0.16)	(0.13)	(0.26)	(0.20)	(0.17)	(0.30)	(0.14)
Lives in Province	0.19*	0.17***	0.20***	0.23	0.11	−0.05	0.11***
	(0.09)	(0.03)	(0.07)	(0.17)	(0.09)	(0.14)	(0.02)
Treat*InProvince	−0.09	−0.28**	−0.38	−0.43*	−0.09	−0.00	−0.21
	(0.16)	(0.12)	(0.28)	(0.21)	(0.18)	(0.31)	(0.14)
Base Outcome	0.27***	0.26***	0.34***	0.27***	0.43***	0.26***	0.24***
	(0.03)	(0.06)	(0.05)	(0.04)	(0.04)	(0.06)	(0.05)
R^2	0.13	0.10	0.21	0.11	0.16	0.10	0.08
Observations	627	460	656	652	536	589	679
Clusters	18	18	18	18	18	18	18

* $p < .10$, ** $p < .05$, *** $p < .01$
Models in this table use the panel specification denoted in equation 1 and include a dummy variable for missing baseline. All models also include station (municipality) and rank fixed effects for the blocking variables. Standard errors are clustered at the station-level.

Table A.7: Heterogeneous Treatment Effects - Rebel Presence (Trust Outcomes)

	Trust Index	Trust (Safe)	Trust (Intent)	Trust (Info)	Perceived NPA Sympathy
	(1)	(2)	(3)	(4)	(5)
Treated	0.17	4.86*	6.24**	2.26	−6.86**
	(0.11)	(2.75)	(2.90)	(1.96)	(2.83)
NPA Presence 2015	0.23	2.95	8.72**	4.32*	−5.11*
	(0.14)	(4.54)	(3.70)	(2.29)	(2.77)
Treated*NPA	−0.57***	−17.10***	−19.14***	−5.04*	12.50
	(0.15)	(4.49)	(4.42)	(2.69)	(7.82)
Base Outcome	0.40***	0.33***	0.35***	0.28***	0.41***
	(0.04)	(0.05)	(0.06)	(0.03)	(0.04)
Base Outcome	0.40***				
	(0.04)				
R^2	0.17	0.12	0.13	0.11	0.16
Observations	612	635	628	628	636
Clusters	18	18	18	18	18

* $p < .10$, ** $p < .05$, *** $p < .01$
Models in this table use the panel specification denoted in equation 1 and include a dummy variable for missing baseline. All models also include rank fixed effects for the blocking variables, though they do not include station fixed effects because NPA Presence is measured at the municipality (station) level. Standard errors are clustered at the station-level.

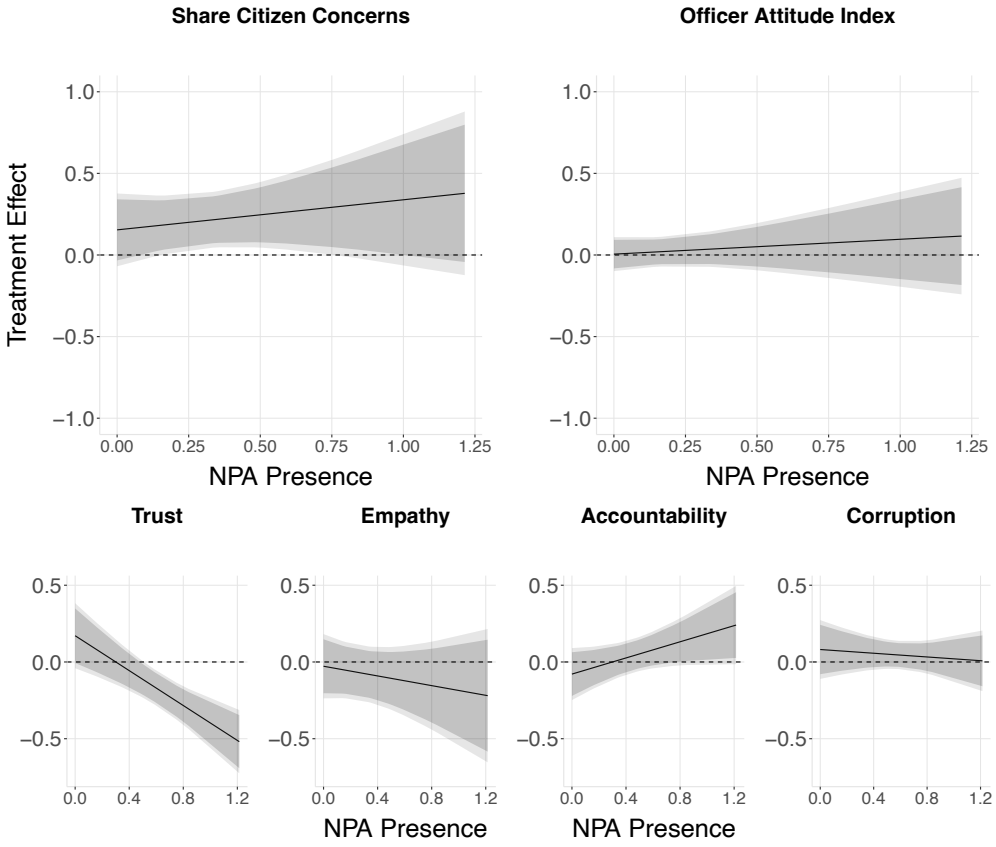
Table A.8: Heterogeneous Treatment Effects - Rebel Presence

	Share Concerns	Attitude Index	Trust	Empathy	Account	Corrupt	Trust Idx (alt)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.15	0.01	0.17	−0.03	−0.08	0.08	0.17
	(0.11)	(0.05)	(0.11)	(0.11)	(0.09)	(0.10)	(0.10)
NPA Presence 2015	0.09	−0.20**	0.23	0.09	−0.21**	−0.21***	0.25*
	(0.11)	(0.09)	(0.14)	(0.10)	(0.09)	(0.06)	(0.14)
Treated*NPA	0.18	0.09	−0.57***	−0.16	0.26*	−0.06	−0.58***
	(0.26)	(0.17)	(0.15)	(0.21)	(0.15)	(0.14)	(0.14)
Base Outcome	0.28***	0.26***	0.40***	0.27***	0.46***	0.25***	0.41***
	(0.03)	(0.07)	(0.04)	(0.04)	(0.04)	(0.06)	(0.05)
Base Outcome			0.40***				0.41***
			(0.04)				(0.05)
R^2	0.09	0.07	0.17	0.08	0.14	0.08	0.17
Observations	627	431	612	610	500	554	625
Clusters	18	18	18	18	18	18	18

* $p < .10$, ** $p < .05$, *** $p < .01$
Models in this table use the panel specification denoted in equation 1 and include a dummy variable for missing baseline. All models also include rank fixed effects for the blocking variables, though they do not include station fixed effects because NPA Presence is measured at the municipality (station) level. Standard errors are clustered at the station-level.

The heterogeneous effects relating to NPA presence that we present in Figure 4 and Table A.7 zoom in on the Trust index and the additional *Perceived NPA Sympathy* outcome. Figure A.3, which displays the marginal effects of regressions in Table A.8, displays the heterogeneous effect of NPA presence on all our main outcomes. The only other CATE of note is a marginally significant increase in officers' perception of accountability in areas with high NPA presence. The coefficient is only marginally significant ($p = 0.099$), we did not have qualitative evidence that pointed us to look for this heterogeneous effect, and the result is insignificant when including station and officer controls. We thus advise caution when interpreting the result. From a purely exploratory standpoint, we think it is possible that the treatment had a positive impact on officers' perceptions of accountability in high-NPA areas because police leaders in these areas are more sensitive to improving citizen-officer relationships as part of the effort to win "hearts and minds" away from the insurgents.

Figure A.3: Heterogeneous Treatment Effects - Rebel Presence (Main Outcomes)



All plots display marginal effects of treatment by NPA (rebel) presence in the municipality, using the panel specification (equation 1). The top panel displays results for shared concerns (knowledge) and the umbrella officer attitude index. The bottom panel displays results for the other main outcome indices. Confidence intervals are displayed at the 95% level (light gray) and 90% level (dark gray). Full regression results are in Table A.8.

A.4 Robustness

A.4.1 Heterogeneous Treatment Effects with Controls

In this section, we show the results of the robustness tests described in the manuscript.Table A.9 probes whether the embeddedness findings are robust to including controls for potential confounders. We include different combinations of control variables when predicting the umbrella Attitude Index in Models 1-3, and then apply the specification in the full model to the sub-index outcomes in Models 4-7. Model 1 retains the station fixed effects and includes controls for officer-level characteristics, including their rank, education, religion, age, gender, years served in the municipality, average daily commute to their assigned police station, and their public service motivation (PSM). Officers' PSM score is based on a standard battery of questions described in Perry and Wise (1990). Model 2 drops the station fixed effects and replaces them with municipality-level control variables from the 2010 census. Models 3-7 includes the full battery of controls, including their interactions with the treatment indicator. All coefficients and standard errors on the interaction term of interest remain consistent across these different specifications.

Table A.10 follows a similar format for the Rebel Presence findings. As a reminder, these models do not include station fixed effects because the NPA Presence moderator is measured at the municipality-level. Models 1-3 display the results on the Trust Index using station-level, officer-level, and interacted controls. The results are highly consistent when including non-interacted controls, including for the alternative (imputed) version of the trust index and all the individual survey items that compose the trust index (Models 4-7). The one coefficient that loses significance using the full battery of controls is the base term on the *Perceived NPA Sympathy* outcome (Model 8), though Model 9 shows that this remains significant when including only the municipality-level controls. Because the moderator is measured at the station-level, the highly-saturated model with interactive controls (Model 3) substantially inflates the standard errors in this case. Though the coefficient on the *Treated*NPA* interaction term remains consistent in this model, it is barely insignificant at conventional levels (p=0.12).

The models with controls (as well as the station fixed effects in the original models) helps address the concern that officers from inside and outside of Sorsogon are systematically assigned to police stations with different observable characteristics. That said, we explore whether these assignments appear to be balanced on municipality characteristics in Table A.11, and we do not find significant differences of municipality characteristics by officer embeddedness.

Table A.9: Heterogeneous Treatment Effects - Embeddedness (with Controls)

	Attitude Index			Trust	Empathy	Account	Corrupt
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.30**	0.29**	14.38	16.23*	7.70	9.56	−7.60
	(0.13)	(0.13)	(11.75)	(9.07)	(10.27)	(10.31)	(12.26)
Lives in Province	0.18***	0.16**	0.16**	0.11*	0.17	0.15	−0.01
	(0.02)	(0.06)	(0.06)	(0.06)	(0.13)	(0.12)	(0.14)
Treated*InProvince	−0.27*	−0.26*	−0.28**	−0.27	−0.38*	−0.06	−0.13
	(0.13)	(0.13)	(0.12)	(0.24)	(0.21)	(0.20)	(0.31)
Base Outcome	0.26***	0.25***	0.26***	0.32***	0.21***	0.43***	0.21***
	(0.07)	(0.07)	(0.07)	(0.06)	(0.04)	(0.05)	(0.06)
Rank (officer)	0.01	−0.00	0.05	−0.01	0.13	−0.02	−0.15
	(0.09)	(0.07)	(0.14)	(0.12)	(0.12)	(0.10)	(0.09)
Education (officer)	−0.01	−0.02	−0.04	−0.02	−0.09	0.01	0.11**
	(0.06)	(0.05)	(0.09)	(0.08)	(0.08)	(0.07)	(0.05)
Catholic (officer)	0.05	0.06	0.11*	0.13	0.30***	−0.05	0.12**
	(0.08)	(0.08)	(0.06)	(0.18)	(0.09)	(0.07)	(0.05)
Age (officer)	−0.01	−0.00	−0.01	0.02**	0.01	−0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Gender (officer)	−0.05	−0.03	−0.04	−0.02	−0.12*	−0.07	0.10
	(0.05)	(0.05)	(0.07)	(0.07)	(0.06)	(0.08)	(0.13)
Years Served (officer)	−0.00	−0.00	−0.00	0.01	−0.00	−0.01	−0.01
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Commute (officer)	−0.02	−0.01	−0.02	−0.01	0.02	−0.02	0.01
	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.05)
PSM (officer)	0.02	0.06	−0.01	0.19*	0.31***	0.02	−0.42***
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.08)	(0.14)
Education (mun)		0.13	0.65	1.55	0.97	−0.33	−1.70**
		(0.73)	(0.79)	(0.91)	(1.06)	(1.07)	(0.77)
Population (mun)		0.00	−0.00	−0.00*	−0.00	0.00	0.00**
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Household Size (mun)		−0.13	0.30	2.34***	0.51	−0.11	−0.66
		(0.41)	(0.32)	(0.55)	(0.39)	(0.32)	(0.60)
Percent Catholic (mun)		−0.18	0.28	5.11	−3.97	1.25	9.74**
		(3.92)	(4.68)	(5.52)	(7.62)	(8.86)	(3.85)
Crime Rate (mun)		0.34*	0.71***	−0.19	0.66***	0.26	0.47
		(0.18)	(0.21)	(0.34)	(0.20)	(0.26)	(0.35)
NPA Presence 2015 (mun)		0.05	0.11	−0.44*	0.20	0.06	0.32
		(0.17)	(0.21)	(0.21)	(0.20)	(0.15)	(0.22)
Treat*Rank			−0.06	0.29	−0.05	0.02	0.03
			(0.21)	(0.22)	(0.26)	(0.17)	(0.18)
Treat*Education			0.09	−0.01	0.21	0.00	0.02
			(0.18)	(0.18)	(0.14)	(0.23)	(0.19)
Treat*Catholic			−0.15	−0.39	−0.32	−0.11	−0.04
			(0.20)	(0.24)	(0.28)	(0.15)	(0.24)
Treat*Age			0.00	−0.03	−0.01	−0.01	0.01
			(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
Treat*Gender			0.06	−0.04	−0.01	0.26	−0.23
			(0.13)	(0.13)	(0.18)	(0.15)	(0.16)
Treat*YearsServed			0.00	0.01	0.00	0.02	−0.02
			(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
Treat*Commute			0.03	0.04	−0.07	0.04	0.05
			(0.06)	(0.05)	(0.05)	(0.04)	(0.07)
Treat*PSM			0.08	0.01	0.14	0.12	0.18
			(0.09)	(0.14)	(0.13)	(0.12)	(0.24)
R^2	0.12	0.10	0.13	0.24	0.16	0.18	0.14
Observations	455	427	427	603	601	496	546
Clusters	18	18	18	18	18	18	18

* $p < .10$, ** $p < .05$, *** $p < .01$

Models in this table use the panel specification denoted in equation 1 and include a dummy variable for missing baseline. Standard errors are clustered at the station-level.

Table A.10: Heterogeneous Treatment Effects - Rebel Presence (with Controls)

	Trust Index			Trust (Safe)	Trust (Intent)	Trust (Info)	Trust Idx (alt)	Perceived NPA Sympathy	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated	0.04	0.01	16.23*	0.20	1.38	−0.03	0.03	−4.15	−5.91**
	(0.08)	(0.08)	(9.07)	(1.85)	(1.89)	(1.71)	(0.07)	(2.55)	(2.51)
NPA Presence 2015	−0.32	−0.38*	−0.44*	−17.53**	−6.73	−6.63**	−0.38*	4.03	2.41
	(0.21)	(0.20)	(0.21)	(6.12)	(5.22)	(2.80)	(0.20)	(7.46)	(6.67)
Treated*NPA	−0.37***	−0.33***	−0.35	−9.62***	−11.16***	−1.81	−0.31***	8.15	10.64
	(0.09)	(0.08)	(0.22)	(3.20)	(2.57)	(2.27)	(0.08)	(7.72)	(7.59)
Base Outcome	0.35***	0.31***	0.32***	0.27***	0.29***	0.20***	0.31***	0.38***	0.40***
	(0.05)	(0.05)	(0.06)	(0.05)	(0.07)	(0.04)	(0.06)	(0.04)	(0.04)
Education (mun)	1.27	1.32	1.55	34.23	28.25	36.39**	1.29	−2.99	−13.59
	(0.85)	(0.83)	(0.91)	(27.23)	(21.65)	(12.75)	(0.82)	(35.02)	(34.47)
Population (mun)	−0.00	−0.00	−0.00*	−0.00*	−0.00	−0.00**	−0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Household Size (mun)	1.52***	1.68***	2.34***	46.71***	43.78***	32.92***	1.67***	−10.27	−10.04
	(0.50)	(0.49)	(0.55)	(15.39)	(11.53)	(7.81)	(0.49)	(16.74)	(15.90)
Percent Catholic (mun)	3.11	5.30	5.11	−27.34	262.27**	117.32	5.46	−248.03	−136.22
	(5.39)	(5.11)	(5.52)	(187.11)	(123.18)	(100.84)	(5.14)	(265.16)	(245.20)
Crime Rate (mun)	−0.19	−0.22	−0.19	−7.76	−7.55	−3.66	−0.22	7.68	6.79
	(0.28)	(0.29)	(0.34)	(8.52)	(6.20)	(5.60)	(0.29)	(8.24)	(6.78)
Rank (officer)	0.23**	0.09	−0.01	3.37	0.66	1.80	0.07	3.42	−3.15
	(0.09)	(0.10)	(0.12)	(2.41)	(3.09)	(2.49)	(0.10)	(2.51)	(2.59)
Education (officer)		−0.05	−0.02	−2.99	−1.51	−0.23	−0.04	1.96	
		(0.07)	(0.08)	(2.82)	(2.17)	(1.02)	(0.08)	(1.35)	
Catholic (officer)		0.02	0.13	1.32	−0.78	−0.75	−0.02	0.05	
		(0.15)	(0.18)	(3.45)	(2.56)	(3.44)	(0.13)	(5.55)	
Age (officer)		0.01	0.02**	0.15	0.24	0.25	0.01	−0.49*	
		(0.01)	(0.01)	(0.17)	(0.20)	(0.18)	(0.01)	(0.25)	
Gender (officer)		−0.03	−0.02	−1.97	−1.15	0.62	−0.03	−0.24	
		(0.07)	(0.07)	(2.40)	(1.46)	(1.71)	(0.07)	(2.78)	
Years Served (officer)		0.01	0.01	0.48*	0.26	0.19	0.01	−0.47	
		(0.01)	(0.01)	(0.23)	(0.26)	(0.18)	(0.01)	(0.33)	
Commute (officer)		0.00	−0.01	0.05	−0.44	0.05	0.00	1.42	
		(0.02)	(0.03)	(0.75)	(0.43)	(0.37)	(0.02)	(0.99)	
PSM (officer)		0.23***	0.19*	4.41	3.60**	9.27***	0.21**	−6.70**	
		(0.07)	(0.10)	(2.74)	(1.53)	(2.31)	(0.08)	(2.34)	
Controls Interacted	No	No	Yes	No	No	No	No	No	No
R^2	0.20	0.22	0.24	0.17	0.19	0.17	0.22	0.20	0.17
Observations	612	603	603	625	617	618	623	625	636
Clusters	18	18	18	18	18	18	18	18	18

* $p < .10$, ** $p < .05$, *** $p < .01$
Models in this table use the panel specification denoted in equation 1 and include a dummy variable for missing baseline.
Models do not include station fixed effects because NPA Presence is measured at the municipality (station) level.
Standard errors are clustered at the station-level.

Table A.11: Balance of Municipality Characteristics by Officer Embeddedness

Outcome Variable	Estimate	p-value
Municipality Education	0.031	0.44
Municipality Population	-332.162	0.97
Municipality Home Quality	-0.001	0.56
Municipality Household Size	-0.004	0.87
Municipality Percent Catholic	0.000	0.97
Municipality Crime Rate	-0.075	0.14
Municipality NPA Presence	0.078	0.39

Each row reports the estimates and p-values from regressions that predict municipal characteristics based on officers' embeddedness, which is measured with the dummy variable for whether they live in Sorsogon.

A.4.2 Alternative Operationalization

In this section, we probe whether the results are robust to operationalizing the main concepts in alternative ways. Table A.12 replicates the main average treatment effects (Table A.5) but uses alternative versions of the main attitude indices. In this alternative version, we imputed missing responses for individual items in the indices, using predictions based on non-missing responses to the other questions in the same index. We describe this method in the PAP and SI Section A.2.2 above. Where relevant, results with these alternative indices are also included in heterogeneous effects tables in the Main Results (SI Section A.3).

Table A.12: Treatment Effects - Alternate Indices

	Attitude Idx (Alternative)		Trust (Alternative)		Empathy (Alternative)		Accountability (Alternative)		Corruption (Alternative)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated	0.01	0.01	−0.04	−0.02	−0.07	−0.08	0.03	0.01	0.07	0.06
	(0.06)	(0.05)	(0.05)	(0.05)	(0.10)	(0.09)	(0.08)	(0.07)	(0.09)	(0.09)
Base Outcome	0.24***		0.34***		0.28***		0.44***		0.27***	
	(0.05)		(0.05)		(0.04)		(0.03)		(0.06)	
Model	Panel	Cross	Panel	Cross	Panel	Cross	Panel	Cross	Panel	Cross
R^2	0.08	0.03	0.21	0.13	0.10	0.04	0.16	0.04	0.10	0.05
Observations	679	683	678	684	689	691	582	615	659	671
Clusters	18	18	18	18	18	18	18	18	18	18

* $p < .10$, ** $p < .05$, *** $p < .01$
Panel models include a dummy variable for missing baseline.
All models include station (municipality) and rank fixed effects for the blocking variables.
Standard errors are clustered at the station-level.

Table A.13 displays results on alternative versions of the "Shared Citizen Concerns" outcome. In the main regressions presented in the article (Table A.5), we measure the outcome by counting the number of matches between officers' selected issues and the four most commonly mentioned issues on the same question posed to community leaders (LGU). These results are reproduced in Models 1-2. In Models 3-4, we construct the outcome by counting the number of matches between officers' selected issues and the *three* most commonly mentioned issues on the same question posed to community leaders (LGU). The results remain significant at the .05 level when using this construction.

In Table A.13, Models 5-6, we calculate the matches with community leaders' (LGU) responses, but instead use a version of the question to officers phrased: "Now, what do you think CITIZENS in the municipality where you are assigned consider to be the three most important public safety concerns in their community." We asked the question in this way to try to capture differences between officers' firsthand beliefs and their perceptions of what citizens thought was important. However, because we asked about officers' firsthand beliefs before asking about what they thought citizens believe, officers' answers to the second question may have been colored by their answers to the first question. The correlation between responses to the two versions of the question is $\rho = .83$, leaving us unable to clearly distinguish between these subtly different outcomes. The coefficients remain substantively similar to those in the main models and, in the cross-sectional model, remain significant at the .1 level. In Models 7-8, we instead calculate the matches of officers' responses with the aggregate responses of ordinary citizens (rather than the LGU leaders) in their assigned municipality. The coefficients remain substantively similar in these models, though they are barely insignificant in this case (p=.11 in the cross-sectional model). The correlation between the officer match with 1) LGU and 2) ordinary citizen responses was $\rho = .71$. Once again, this left us unable to clearly distinguish between these subtly different outcomes.

Table A.13: Treatment Effects: Alternative Knowledge Measures

	Top 4 LGU/own		Top 3 LGU/own		Top 4 LGU/their		Top 4 CIV/own	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	0.21*	0.25**	0.16**	0.19**	0.15	0.18*	0.14	0.16
	(0.10)	(0.10)	(0.07)	(0.07)	(0.10)	(0.10)	(0.10)	(0.10)
Base Outcome	0.27***		0.21***		0.25***		0.15***	
	(0.03)		(0.05)		(0.04)		(0.04)	
Model	Panel	Cross	Panel	Cross	Panel	Cross	Panel	Cross
R^2	0.13	0.07	0.12	0.09	0.11	0.07	0.08	0.06
Observations	627	632	627	632	627	632	627	632
Clusters	18	18	18	18	18	18	18	18

* $p < .10$, ** $p < .05$, *** $p < .01$
Panel models include a dummy variable for missing baseline.
All models include station (municipality) and rank fixed effects for the blocking variables
Models labeled with "Top 4" indicate that the outcome was measured as the match of officer responses with the top 4 issues mentioned by LGU/civilian respondents in the same municipality, while models labeled "Top 3" indicate that we matched with only the top 3 issues mentioned. Models labeled "LGU" indicate that we matched officer responses with the aggregate responses of the LGU community leaders, while models labeled "CIV" indicate that we matched with the aggregate responses of ordinary civilians. Models labeled "own" indicate that we matched using the issues that officers themselves thought were important, while models labeled "their" indicate that we matched using the issues that officers thought that LGU/civilian respondents thought were important.

In Table A.14, we use an alternative measure of the embeddedness moderator. Instead of an indicator for whether the officer lived in Sorsogon province, we use an indicator for whether the officer lived in the same *municipality* where they were assigned (see Table A.14). For reference, 87% of the officers in our sample lived in Sorsogon Province, but only 53% of officer lived in their assigned municipality. The same patterns we picked up in the original test also show up using this alternative measure of embeddedness. The heterogeneous effect on the main attitude index is insignificant in this specification, but we find significant heterogeneous effects on *Trust* and *Empathy*, the same sub-indices that were driving the effects using the original operationalization. Moreover, the results are significant when using the alternate version of the attitude index that imputed missing values for individual survey items.

Table A.14: Heterogeneous Treatment Effects - Embeddedness (Lives In Municipality)

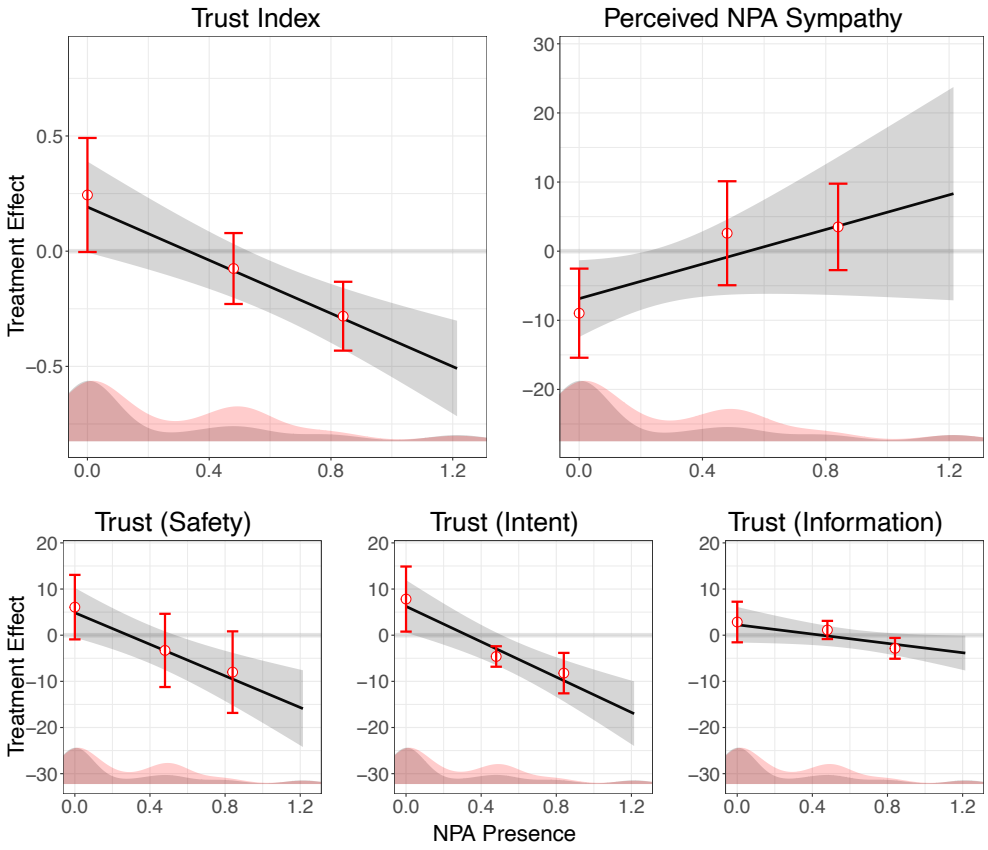
	Share Concerns	Attitude Index	Trust	Empathy	Account	Corrupt	Attitude Idx (alt)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.34***	0.10	0.05	0.03	0.03	0.05	0.09
	(0.11)	(0.09)	(0.07)	(0.12)	(0.15)	(0.13)	(0.09)
Lives in Municipality	0.11**	0.05	0.15**	0.08	0.00	0.04	0.05
	(0.04)	(0.06)	(0.07)	(0.05)	(0.09)	(0.07)	(0.04)
Treated*InMunicipality	−0.19	−0.08	−0.19*	−0.27*	−0.07	0.11	−0.14*
	(0.11)	(0.10)	(0.10)	(0.13)	(0.14)	(0.18)	(0.08)
Base Outcome	0.26***	0.26***	0.29***	0.22***	0.42***	0.21***	0.23***
	(0.03)	(0.07)	(0.06)	(0.04)	(0.05)	(0.06)	(0.05)
Rank (officer)	−0.17	0.01	0.06	0.09	0.01	−0.11	0.03
	(0.11)	(0.09)	(0.08)	(0.08)	(0.12)	(0.09)	(0.06)
Education (officer)	−0.00	−0.01	−0.12*	−0.07	−0.03	0.16*	0.02
	(0.08)	(0.06)	(0.06)	(0.06)	(0.07)	(0.08)	(0.03)
Catholic (officer)	0.12	0.06	−0.03	0.20*	−0.06	0.06	0.10
	(0.09)	(0.08)	(0.13)	(0.11)	(0.09)	(0.07)	(0.07)
Age (officer)	0.01**	−0.01	0.01	−0.00	−0.01*	0.00	−0.01
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Gender (officer)	0.06	−0.04	0.01	−0.07	0.04	0.01	0.01
	(0.10)	(0.05)	(0.07)	(0.06)	(0.06)	(0.14)	(0.04)
Years Served (officer)	−0.02***	−0.00	0.01	0.00	−0.00	−0.01*	−0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Commute (officer)	0.00	−0.02	0.01	0.01	−0.02	0.02	−0.00
	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.04)	(0.02)
PSM (officer)	0.06	0.01	0.24***	0.34***	0.06	−0.39***	0.10
	(0.11)	(0.10)	(0.07)	(0.09)	(0.08)	(0.10)	(0.09)
R^2	0.15	0.11	0.23	0.13	0.17	0.13	0.09
Observations	616	455	645	641	531	579	667
Clusters	18	18	18	18	18	18	18

* $p < .10$, ** $p < .05$, *** $p < .01$
Models in this table use the panel specification denoted in equation 1 and include a dummy variable for missing baseline. Models include station and rank fixed effects. Standard errors are clustered at the station-level.

A.4.3 Marginal Effects with Binning Estimator

Figure A.4 replicates Figure 4 in the article using the binning estimator described in Hainmueller et al. (2019), implemented with the *Interflex* package in R. The binning estimator helps establish whether there is substantial non-linearity in the interaction effects, as well as whether there is sufficient data across the range of the moderator to avoid excessive extrapolation. We rely on this estimator to provide additional support for two main types of claims related to the interaction terms. First, we use the binning estimator to identify whether the intervention had a significant effect on officer outcomes at specific levels of the moderating variable. For this purpose, we look at whether the conditional average treatment effect (CATE) of the intervention is significantly different from zero at "high," "medium," and "low" levels of the moderator, which are defined as the median values of each tercile of the running variable (the default in the *Interflex* package). These CATEs are indicated by the red point estimates and confidence intervals. Second, we use the binning estimator to verify whether the intervention had significantly *different* effects on officer outcomes at high vs. low levels of the moderator. For this purpose, the relevant comparison is whether the confidence intervals of the marginal effects at the "high" level of the moderator overlap with the point estimate of the marginal effects at the "low" level of the moderator (and vice versa).

Figure A.4: Heterogeneous Effect of Community Policing on Officer Trust, by NPA Presence



The displayed marginal effects are produced by a model interacting the baseline crime victimization rate in an officer's municipality with the treatment status, using the panel model specification described in the manuscript. The distribution of the running variable is represented at the bottom of the chart with density plots for treatment officers (red) and control officers (gray). The bin locations were specified using the default settings in the *Interflex* R package, which uses the median value within each tercile of the running variable.

Consistent with the interpretation in the paper, the CATE on officers' trust levels is negative and significant in areas with a high level of NPA presence (Figure A.4). Moreover, the CATE in high-NPA areas is significantly different from the CATE in low-NPA areas. There does not appear to be substantial non-linearity in the heterogeneous effects: the CATE estimates produced by the binning estimator are all close to the predicted marginal effects produced by the linear interaction model. A similar pattern presents for the sub-index items of the trust index. Consistent with the proposed mechanism, officers assigned to the intervention in low-NPA areas perceived that citizens were less sympathetic to the NPA, relative to their control group counterparts. Again, in this model, the CATE among officers assigned to low-NPA areas was negative and significantly different from zero, as well as significantly different from the CATE among officers assigned to high-NPA areas.

A.4.4 Block Bootstrapped Standard Errors

Tables A.15, A.16, A.17, and A.18 replicate the four main results tables in SI Section A.3 using block-bootstrapped standard errors, to account for the relatively small number of clusters (in this case, stations). The robustness tests suggest that, if anything, the standard errors in our pre-registered specification were biased upwards due to the small number of clusters. Using the block-bootstrapped standard errors, the p-values on the coefficients of interests tend to be slightly smaller compared to the original models.

Table A.15: Main Treatment Effects with Block Bootstrapped Standard Errors

	Share Concerns		Attitude Idx		Trust		Empathy		Accountability		Corruption	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated	0.21*	0.25**	0.06	−0.01	−0.06	−0.02	−0.12	−0.08	−0.00	0.00	0.12*	0.07
	(0.11)	(0.10)	(0.05)	(0.06)	(0.06)	(0.05)	(0.11)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Base Outcome	0.27***		0.26***		0.34***		0.28***		0.43***		0.26***	
	(0.04)		(0.07)		(0.06)		(0.04)		(0.04)		(0.06)	
Model	Panel	Cross	Panel	Cross	Panel	Cross	Panel	Cross	Panel	Cross	Panel	Cross
R^2	0.13	0.07	0.10	0.04	0.21	0.13	0.10	0.04	0.16	0.05	0.10	0.05
Observations	627	632	460	534	656	671	652	672	536	587	589	621
Clusters	18	18	18	18	18	18	18	18	18	18	18	18

* $p < .10$, ** $p < .05$, *** $p < .01$
The results in this table correspond to those shown in Table A.5. Panel models include a dummy variable for missing baseline.
All models include station (municipality) and rank fixed effects for the blocking variables.
Standard errors are clustered and block-bootstrapped at the station-level.

Table A.16: Heterogeneous Treatment Effects - Embeddedness with Block Bootstrapped Standard Errors

	Share Concerns	Attitude Index	Trust	Empathy	Account	Corrupt
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.28*	0.31**	0.29	0.28*	0.08	0.12
	(0.17)	(0.14)	(0.25)	(0.17)	(0.21)	(0.36)
Lives in Province	0.19*	0.17***	0.20***	0.23**	0.11	−0.05
	(0.10)	(0.02)	(0.06)	(0.11)	(0.08)	(0.09)
Treat*InProvince	−0.09	−0.28*	−0.38	−0.43**	−0.09	−0.00
	(0.20)	(0.14)	(0.26)	(0.17)	(0.21)	(0.34)
Base Outcome	0.27***	0.26***	0.34***	0.27***	0.43***	0.26***
	(0.04)	(0.07)	(0.04)	(0.04)	(0.03)	(0.06)
R^2	0.13	0.10	0.21	0.11	0.16	0.10
Observations	627	460	656	652	536	589
Clusters	18	18	18	18	18	18

* $p < .10$, ** $p < .05$, *** $p < .01$
The results in this table correspond to those shown in Table A.6. Models in this table use the panel specification denoted in equation 1 and include a dummy variable for missing baseline. All models also include station (municipality) and rank fixed effects for the blocking variables. Standard errors are clustered and block-bootstrapped at the station-level.

Table A.17: Heterogeneous Treatment Effects - Rebel Presence (Trust Outcomes) with Block Bootstrapped Standard Errors

	Trust Index	Trust (Safe)	Trust (Intent)	Trust (Info)	Perceived NPA Sympathy
	(1)	(2)	(3)	(4)	(5)
Treated	0.17	4.86	6.24***	2.26	−6.86**
	(0.10)	(3.01)	(2.37)	(1.37)	(2.71)
NPA Presence 2015	0.23**	2.95	8.72***	4.32**	−5.11**
	(0.10)	(2.90)	(2.77)	(1.94)	(2.50)
Treated*NPA	−0.57***	−17.10***	−19.14***	−5.04***	12.50
	(0.13)	(5.59)	(3.95)	(1.94)	(7.65)
Base Outcome	0.40***	0.33***	0.35***	0.28***	0.41***
	(0.04)	(0.06)	(0.07)	(0.03)	(0.03)
R^2	0.17	0.12	0.13	0.11	0.16
Observations	612	635	628	628	636
Clusters	18	18	18	18	18

* $p < .10$, ** $p < .05$, *** $p < .01$

The results in this table correspond to those shown in Table A.7. Models in this table use the panel specification denoted in equation 1 and include a dummy variable for missing baseline. All models also include rank fixed effects for the blocking variables, though they do not include station fixed effects because NPA Presence is measured at the municipality (station) level. Standard errors are clustered and block-bootstrapped at the station-level.

Table A.18: Heterogeneous Treatment Effects - Rebel Presence with Block Bootstrapped Standard Errors

	Share Concerns	Attitude Index	Trust	Empathy	Account	Corrupt	Trust Idx (alt)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.15*	0.01	0.17*	−0.03	−0.08	0.08	0.19**
	(0.08)	(0.04)	(0.09)	(0.10)	(0.06)	(0.08)	(0.08)
NPA Presence 2015	0.09	−0.20**	0.23**	0.09	−0.21***	−0.21***	0.25**
	(0.09)	(0.08)	(0.10)	(0.09)	(0.08)	(0.06)	(0.10)
Treated*NPA	0.18	0.09	−0.57***	−0.16	0.26***	−0.06	−0.58***
	(0.20)	(0.17)	(0.11)	(0.23)	(0.10)	(0.10)	(0.15)
Base Outcome	0.28***	0.26***	0.40***	0.27***	0.46***	0.25***	0.40***
	(0.03)	(0.07)	(0.06)	(0.03)	(0.03)	(0.06)	(0.05)
R^2	0.09	0.07	0.17	0.08	0.14	0.08	0.17
Observations	627	431	612	610	500	554	634
Clusters	18	18	18	18	18	18	18

* $p < .10$, ** $p < .05$, *** $p < .01$

The results in this table correspond to those shown in Table A.8. Models in this table use the panel specification denoted in equation 1 and include a dummy variable for missing baseline. All models also include rank fixed effects for the blocking variables, though they do not include station fixed effects because NPA Presence is measured at the municipality (station) level. Standard errors are clustered and block-bootstrapped at the station-level.

A.4.5 Weighted Regression

Tables A.19, A.20, A.21, and A.22 replicate the four main results tables in SI Section A.3 using weighted regression, to account for officers' different probability of being assigned to treatment. Weights were calculated based on officers' station and rank, as well as the number of treatment barangays in the municipality. For example, the Prieto Diaz station had eleven eligible junior officers, nine eligible senior officers, and four barangays assigned to the One Sorsogon intervention. Because each of the four barangays was assigned one junior officer and one senior officer, this translated to a 36% probability of assignment for junior officers and a 44% probability of assignment for senior officers employed by that station. These weights were incorporated into the models using inverse-probability weighting. The results are similar to the main specifications. The ATE on *Shared Concerns* remains marginally significant in the cross-sectional model, though barely misses significance in the panel model (Table A.19). The heterogeneous effects of embeddedness appear stronger in the weighted models, with the base terms and interaction terms all being significant at conventional levels for the *Attitude Index*, *Trust*, and *Empathy* outcomes (Table A.20). The heterogeneous effects of NPA presence on the *Trust* index and its constituent survey items also appear stronger in the weighted models, except for *Trust (Info)*. The heterogeneous effect of NPA presence on the *Perceived NPA Sympathy* outcome is statistically significant at conventional levels using this specification, whereas they were barely insignificant in the main specification (Table A.21). Finally, we show that the results remain significant for the main outcome of interest in the heterogeneity analyses when incorporating the probability of being assigned as a replacement for one of the original officers assigned to treatment, an additional 19% likelihood. The results using this alternative weighting scheme are displayed in Model 7 of Table A.20 and Model 6 of Table A.21.

Table A.19: Main Treatment Effects Using Weighted Regression

	Share Concerns		Attitude Idx		Trust		Empathy		Accountability		Corruption	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated	0.18	0.21*	0.07	0.02	−0.10	−0.06	−0.08	−0.05	−0.04	−0.03	0.16	0.10
	(0.12)	(0.12)	(0.05)	(0.05)	(0.08)	(0.08)	(0.10)	(0.09)	(0.13)	(0.12)	(0.14)	(0.15)
Base Outcome	0.27***		0.26***		0.34***		0.28***		0.43***		0.26***	
	(0.03)		(0.06)		(0.05)		(0.04)		(0.04)		(0.06)	
Model	Panel	Cross	Panel	Cross	Panel	Cross	Panel	Cross	Panel	Cross	Panel	Cross
R^2	0.10	0.07	0.08	0.03	0.14	0.11	0.06	0.02	0.09	0.05	0.07	0.04
Observations	627	632	460	534	656	671	652	672	536	587	589	621
Clusters	18	18	18	18	18	18	18	18	18	18	18	18

* $p < .10$, ** $p < .05$, *** $p < .01$
Panel models include a dummy variable for missing baseline.
All models include station (municipality) and rank fixed effects for the blocking variables.
Standard errors are clustered at the station-level.

Table A.20: Heterogeneous Treatment Effects - Embeddedness Using Weighted Regression

	Share Concerns	Attitude Index	Trust	Empathy	Account	Corrupt	Attitude Index (Alt)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.09	0.16***	0.43*	0.50***	0.05	−0.23	0.17***
	(0.19)	(0.05)	(0.21)	(0.12)	(0.15)	(0.15)	(0.05)
Lives in Province	0.13**	0.14***	0.25***	0.36***	0.07**	−0.20***	0.14***
	(0.06)	(0.01)	(0.03)	(0.09)	(0.03)	(0.07)	(0.01)
Treat*InProvince	0.09	−0.10**	−0.59**	−0.66***	−0.10	0.43*	−0.10**
	(0.18)	(0.04)	(0.24)	(0.12)	(0.23)	(0.21)	(0.04)
Base Outcome	0.27***	0.26***	0.34***	0.27***	0.43***	0.26***	0.26***
	(0.03)	(0.06)	(0.05)	(0.04)	(0.04)	(0.06)	(0.06)
R^2	0.10	0.08	0.16	0.09	0.09	0.08	0.08
Observations	627	460	656	652	536	589	460
Clusters	18	18	18	18	18	18	18

* $p < .10$, ** $p < .05$, *** $p < .01$
Models in this table use the panel specification denoted in equation 1 and include a dummy variable for missing baseline. All models also include station (municipality) and rank fixed effects for the blocking variables. Standard errors are clustered at the station-level.

Table A.21: Heterogeneous Treatment Effects - Rebel Presence (Trust Outcomes) Using Weighted Regression

	Trust Index	Trust (Safe)	Trust (Intent)	Trust (Info)	Perceived NPA Sympathy	Trust Index (Alt)
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.16*	5.50**	6.28***	1.83	−7.19***	0.15
	(0.09)	(2.48)	(1.93)	(1.95)	(2.07)	(0.09)
NPA Presence 2015	0.27***	7.20**	8.98***	4.02***	−5.39***	0.26***
	(0.06)	(2.49)	(1.93)	(1.06)	(1.23)	(0.07)
Treated*NPA	−0.68***	−24.61***	−22.38***	−5.14	16.13**	−0.67***
	(0.15)	(4.71)	(5.20)	(3.00)	(7.53)	(0.15)
Base Outcome	0.40***	0.33***	0.35***	0.28***	0.41***	0.40***
	(0.04)	(0.05)	(0.06)	(0.03)	(0.04)	(0.04)
R^2	0.14	0.11	0.09	0.09	0.13	0.14
Observations	612	635	628	628	636	612
Clusters	18	18	18	18	18	18

* $p < .10$, ** $p < .05$, *** $p < .01$
Models in this table use the panel specification denoted in equation 1 and include a dummy variable for missing baseline. All models also include rank fixed effects for the blocking variables, though they do not include station fixed effects because NPA Presence is measured at the municipality (station) level. Standard errors are clustered at the station-level.

Table A.22: Heterogeneous Treatment Effects - Rebel Presence Using Weighted Regression

	Share Concerns	Attitude Index	Trust	Empathy	Account	Corrupt	Trust Idx (alt)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.16	0.04	0.16*	−0.02	−0.13	0.16	0.19**
	(0.13)	(0.05)	(0.09)	(0.09)	(0.13)	(0.15)	(0.08)
NPA Presence 2015	0.17	−0.28***	0.27***	0.07*	−0.35***	−0.17***	0.30***
	(0.12)	(0.05)	(0.06)	(0.04)	(0.07)	(0.03)	(0.06)
Treated*NPA	0.05	0.09	−0.68***	−0.16	0.36*	−0.19	−0.72***
	(0.32)	(0.15)	(0.15)	(0.16)	(0.19)	(0.21)	(0.15)
Base Outcome	0.28***	0.26***	0.40***	0.27***	0.46***	0.25***	0.40***
	(0.03)	(0.07)	(0.04)	(0.04)	(0.04)	(0.06)	(0.05)
R^2	0.07	0.07	0.14	0.06	0.08	0.07	0.13
Observations	627	431	612	610	500	554	634
Clusters	18	18	18	18	18	18	18

* $p < .10$, ** $p < .05$, *** $p < .01$
Models in this table use the panel specification denoted in equation 1 and include a dummy variable for missing baseline. All models also include rank fixed effects for the blocking variables, though they do not include station fixed effects because NPA Presence is measured at the municipality (station) level. Standard errors are clustered at the station-level.

A.4.6 Spillovers

In this section, we consider the implications of spillovers for the interpretation of our results, and estimate the presence of spillovers shaped by officers' family ties and beat patrol partner assignments. The most plausible type of spillovers in the context of our study are "positive" spillovers that would bias the treatment effects towards null, driven by two mechanisms: 1) officers sharing experiences and opinions with each other and 2) increased commitment to community policing by police leadership. First, treatment officers may relay their experiences with citizens to control officers, leading control officers to learn about citizens' public safety concerns. Interactions between officers may also create a pathway for the diffusion of trust or empathy towards citizens. Second, it is possible that the "top down" mechanism we theorized – the effect of police leadership's commitment to improve citizen interactions – may have also affected the attitudes of control officers.

In an ideal world, we would have designed the experiment to include several "pure control" stations, where none of the officers engaged in community policing. This design would have allowed us to compare attitudes of control officers in control stations to control officers in treatment stations. However, we determined that this was not a feasible approach given power considerations associated with the number of police stations in Sorsogon. In the absence of a design-based approach to spillovers, we conducted several individual-level and station-level analyses to investigate the likelihood that significant spillovers took place.

We attempt to capture control officers "exposure" to treatment by measuring two aspects of officers' social networks that shape which colleagues officers are most likely to interact with: 1) family ties between officers and 2) officers' beat patrol assignments. We coded family ties using matches between officers' surnames within each station. In the Philippines (especially in non-urban areas), kinship networks have been shown to be a highly salient pathway for information transmission and opinion formation (Cruz et al., 2017; Haim, 2022). Moreover, the nature of surnames in the Philippines provides an opportunity to measure family networks. In the mid-19th century, the Spanish colonial regime assigned unique Hispanic surnames to each family within each province, with the goal of increasing legibility of the population. Surnames are passed down from both the mother's and father's side of the family using Spanish naming conventions. The legacy of this policy is that two individuals within a locality that

share at least one surname are very likely to be related to each other, even nearly two centuries later (Cruz et al., 2017).

The second way we measured officers' ties was using data on daily beat patrol schedules. There is no standardized format through which stations record their beat schedules, but eight of the 17 stations in our sample recorded data on daily beat assignments. In these stations, the average size of beat teams was just under three officers, ranging from two to five officers per team. The remaining stations either did not record beat patrol schedules or did so using a level of aggregation that did not allow us to decipher which officers went on beat patrols with each other. Beat data were provided to us for the first two months of the POP intervention, as well as the year prior to the intervention. We denoted a tie between all officers who went on at least one patrol together during the treatment period. Most stations tended to keep the same beat partners together until an officer was transferred to another station, with the average officer-pair patrolling together for 30 days within the first two months of the intervention. Most of the officers in our sample had the same beat partners during the treatment period that they had a year prior to the intervention.

In our context, approximately 20% of control officers shared a surname with at least one officer assigned to treatment in their station and 30% of control officers (in the stations with beat data) were assigned to patrols with treatment officers during the period of the intervention. Although there certainly exist other relevant ties between officers that we were unable to measure, family relationships and beat partnerships are a highly salient pathway of information transmission between officers, and if spillovers existed, we would expect to see that they are structured to some extent by these ties.

Table A.23: Spillover Analysis - Individual-Level

	Shared Concerns			Attitude Index			Trust	Empathy	Account	Corrupt
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exposure to treated (Any)	−0.07			−0.05			−0.05	−0.06	0.11	−0.26**
	(0.10)			(0.09)			(0.09)	(0.14)	(0.08)	(0.11)
Exposure to treated (Family)		0.04			−0.09					
		(0.15)			(0.09)					
Exposure to treated (Beat)			−0.04			0.03				
			(0.18)			(0.09)				
R^2	0.09	0.09	0.05	0.08	0.08	0.07	0.21	0.06	0.06	0.05
Observations	452	452	140	343	343	109	431	433	377	400
Clusters	17	17	8	17	17	8	17	17	17	17

* $p < .10$, ** $p < .05$, *** $p < .01$
All models include station (municipality) and rank fixed effects for the blocking variables
Regressions are subset to officers in the control group. The independent variables indicate whether the officer had a family member in the treatm
(Family), a beat patrol partner in the treatment group (Beat), or either a family member or beat patrol partner in the treatment group (Any).

We conducted the first set analyses at the individual-level. For each officer in our sample, we create several dummy variables for their exposure to treatment. We code 1) whether officers had at least one family member assigned to treatment – *Exposure (Family)*, 2) whether officers had at least one beat patrol partner assigned to treatment – *Exposure (Beat)*, and 3) whether officers had at least one family member or at least one beat partner assigned to treatment – *Exposure (Any)*. Among the subset of officers in the control group, we run OLS regressions to see whether any of these exposure variables predict officers' shared concerns with citizens or their attitudes towards citizens. We find that none of the *Shared Concerns*, *Officer Attitude Index*, *Trust*, *Empathy*, or *Accountability* outcomes are significantly predicted by exposure to treatment using these measures (Table A.23). The only sub-index significantly predicted by control officers' exposure to treatment is *Corruption* – control officers with exposure

to treatment officers were less likely to report that corrupt activities were acceptable, relative to control officers without exposure. We thus advise caution when interpreting a substantive null effect of the intervention on this outcome.

In additional to the individual-level spillover analysis, we explore spillovers using heterogeneous effects at the station-level, coding which stations may be the most conducive to generating spillovers. If spillovers are present, we expect that knowledge and attitudes may be more likely to transfer between officers 1) in smaller stations (where a larger proportion of officers were treated) and 2) in stations with denser overall family networks. In these stations, there likely existed more interactions between treatment and control officers, which would result in smaller treatment effects if spillovers occurred. We measure station-level family network density using a standard network density measure: the number of *observed* family ties between officers divided by the *possible* number of ties in a station, given the total number of officers.

Table A.24: Spillover Analysis - Station-Level

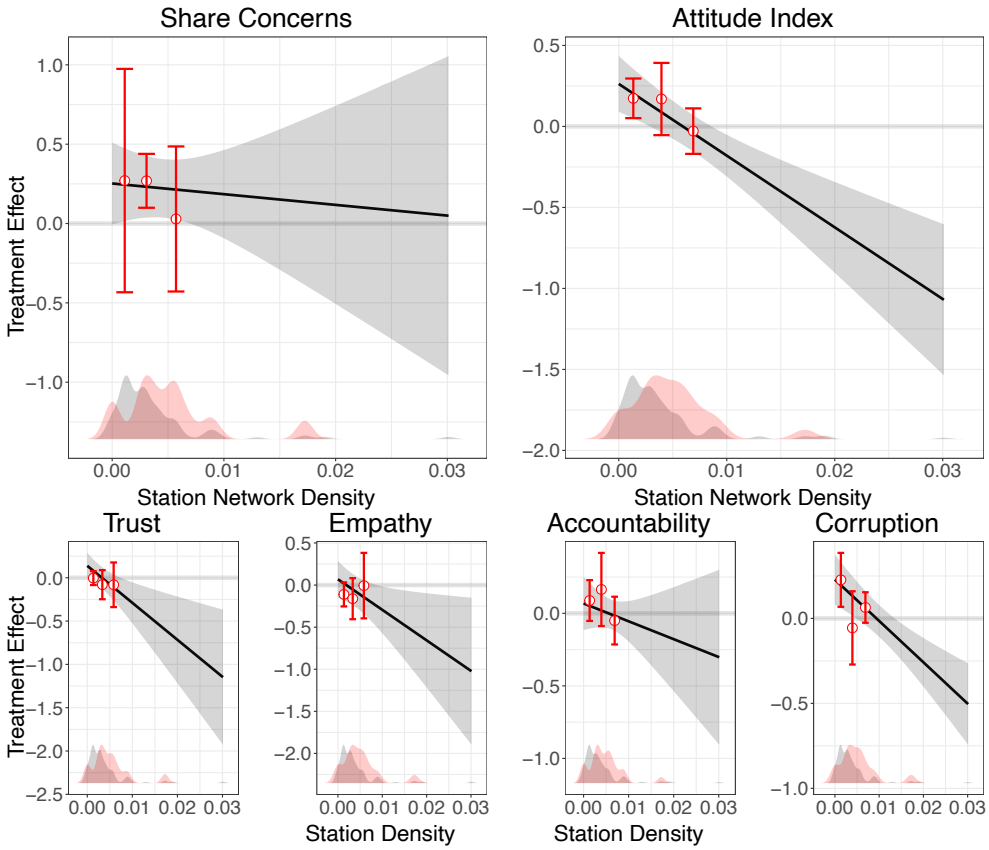
	Share Concerns	Attitude Index	Trust	Empathy	Account	Corrupt	Share Concerns	Attitude Index	Trust	Empathy	Account	Corrupt
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated	0.070	0.096	−0.200*	−0.072	0.097	−0.002	0.253*	0.263***	0.138*	0.068	0.068	0.229***
	(0.151)	(0.082)	(0.107)	(0.120)	(0.075)	(0.076)	(0.131)	(0.087)	(0.076)	(0.111)	(0.093)	(0.075)
StationSize	−0.001	−0.001	−0.003*	−0.001	−0.002**	0.001*	−0.000	−0.001	−0.003*	−0.002	−0.002**	0.001**
	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.000)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Treated*Size	0.003*	−0.001	0.002*	−0.001	−0.001	0.002**						
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)						
StationDensity							21.639*	14.069**	9.167	5.847	13.046	7.850*
							(11.043)	(5.691)	(14.515)	(8.524)	(8.698)	(4.130)
Treated*Density							−6.750	−44.266***	−42.704***	−36.265**	−12.317	−24.337***
							(19.770)	(9.839)	(14.399)	(16.370)	(12.629)	(5.625)
Base Outcome	0.283***	0.256***	0.353***	0.258***	0.443***	0.254***	0.278***	0.257***	0.357***	0.265***	0.437***	0.257***
	(0.033)	(0.063)	(0.051)	(0.039)	(0.036)	(0.059)	(0.035)	(0.065)	(0.051)	(0.039)	(0.037)	(0.057)
Education	1.044	0.134	1.068	0.709	−0.077	−1.289**	0.602	0.005	1.154	0.757	−0.411	−1.268*
	(1.172)	(0.721)	(0.946)	(0.952)	(0.978)	(0.486)	(1.039)	(0.599)	(1.022)	(0.891)	(1.160)	(0.613)
Population	−0.000	0.000	−0.000	−0.000	0.000	0.000**	−0.000	0.000	−0.000	−0.000	0.000	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
HH Size	−0.436	−0.144	1.479**	0.191	−0.220	−0.540	−0.919*	−0.158	1.576**	0.317	−0.458	−0.504
	(0.492)	(0.371)	(0.518)	(0.435)	(0.319)	(0.338)	(0.509)	(0.288)	(0.654)	(0.428)	(0.476)	(0.323)
Catholic	−5.479	1.173	5.339	−4.083	−0.042	9.059**	−5.268	0.793	5.327	−4.014	0.050	8.878**
	(7.819)	(4.200)	(5.718)	(5.658)	(7.255)	(3.877)	(6.979)	(3.726)	(4.931)	(4.682)	(7.475)	(4.049)
Crime Rate	0.219	0.344*	−0.160	0.234	0.145	0.337*	0.080	0.443**	−0.059	0.339	0.105	0.378*
	(0.295)	(0.175)	(0.298)	(0.250)	(0.182)	(0.177)	(0.303)	(0.166)	(0.254)	(0.201)	(0.197)	(0.196)
R^2	0.113	0.084	0.206	0.089	0.155	0.099	0.114	0.105	0.212	0.097	0.156	0.100
Observations	627	431	612	610	500	554	627	431	612	610	500	554
Clusters	18	18	18	18	18	18	18	18	18	18	18	18

* $p < .10$, ** $p < .05$, *** $p < .01$
Models in this table use the panel specification denoted in equation 1 and include a dummy variable for missing baseline.
Models include rank fixed effects, but not station fixed effects because the moderating variables are measured at the station level.
Standard errors are clustered at the station-level.

Table A.24 displays results of regressions where we interact officers' treatment status with the size and density of their station, controlling for the other station-level characteristics. Looking at these results, it initially appears as if there were significant positive spillovers – treatment effects on a number of the outcomes were significantly more positive in larger and less densely connected stations, where spillovers are of less concern. But upon further investigation, these results are driven entirely by outliers in the running variables. This pattern is visible in Figure A.5, which uses the Hainmueller et al. (2019) binning estimator for the models that interact treatment with station

density. For every outcome, the models are excessively extrapolating the results to a very small number of officers in stations that exhibit extraordinarily high network density. Within the range where there is a sufficient number of officers to support the substantive interpretation of marginal effects, none of the interaction terms are significant. Although the station size moderator has a bimodal distribution rather than a long tail, the binning estimator for these models also fails to reveal a consistent pattern that would indicate spillovers are at play (Figure A.6).

Figure A.5: Spillover Marginal Effects - Station Level Network Density

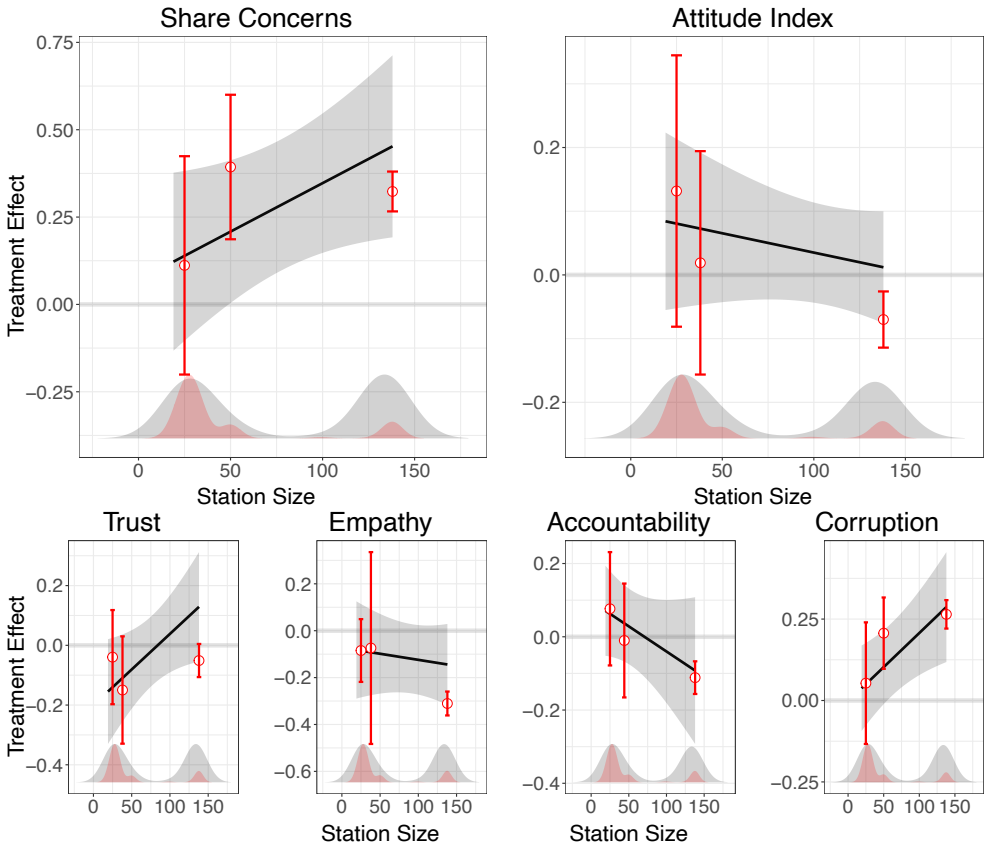


In summary, the above analyses mitigate the concern that spillovers explain why we found null effects of community policing on officers' attitudes towards citizens. That said, in the absence of a design-based approach to spillovers, we advise some caution in interpreting the results. Beyond the tests presented in this section, we note that if spillovers took place, the heterogeneous effects presented in the inductive theory section would also be biased towards null. For example, if treatment officers developed worse attitudes towards citizens as a result of community policing in high-insurgency areas and proceeded to kickstart a process of diffusion of these attitudes to other officers in their station, the heterogeneous effects would be biased towards null.

A.5 Qualitative Methodology

To develop our inductive theory and interpret our quantitative results, we drew on a rich trove of qualitative information gathered before, during, and after our intervention. For the most part, we viewed this qualitative information-gathering exercise as an effort to uncover nuances and mechanisms that we may or may not have been aware of previously and which could help make sense of seemingly unexpected or counter-intuitive results. We also designed our qualitative investigation to identify whether the intervention was being implemented as we expected,

Figure A.6: Spillover Marginal Effects - Station Level Size



and if there were deviations, to determine why they occurred and how they impacted our results. The two main types of qualitative evidence collection were author interviews and field staff observations, which we describe below.

A.5.1 Author Interviews

First, the authors conducted semi-structured key informant interviews for over a year leading up to the start of the intervention. Given that the idea for this project came about while we were already in the field working on an unrelated project, we were well-positioned to interview mid- and high-ranking police officers, community leaders, bureaucrats, and ordinary citizens. In most cases the author(s) took hand-written notes during these interviews, though the primary emphasis was on contextualizing the policing a public safety situation in the region to understand local circumstances and design an appropriate intervention, rather than on recording direct quotes to use as evidence. A priority was also placed on using these interviews to ensure that the intervention could be carried out safely and ethically, as we discuss in Section A.6.

Next, the authors had hundreds of informal conversations with citizens, community leaders, government officials, and police officers of all ranks before, during, and after the intervention, which contributed to our understanding of the policing and public safety contexts in the region. Oftentimes what began as an informal chat with a taxi driver, bureaucrat, or police desk sergeant ended up yielding relevant information that helped contextualize our systematic quantitative results, even though we did not intend for the exchange to be an "interview" when it began. Because the results of these conversations are neither systematic nor reproducible, we think of them less as

evidence in their own right and more as important background information that helps us ask the right questions of our quantitative data, better interpret the results, and serve as the basis for inductive theory development.

A.5.2 Field Staff Observations

Individuals from our team of approximately 20 local field researchers were assigned to thoroughly monitor the intervention. All of our field staff came from the study area, and most had been involved in evaluations of previous randomized interventions. A member of our field staff was in attendance at every POP meeting and culminating town hall during the intervention, and traveled to the treatment barangays to observe the implementation of solutions developed by the POP teams. In addition, field officers conducted a "barangay audit" after the fourth POP meeting, in order to gather perspectives on the program from individuals who were not on the POP team. In total, our field staff attended nearly two thousand meetings and events associated with the intervention. We view these activities as embedded research, as the field researchers were present for a large portion of the intervention but did not actively participate in it. Only the local field researchers, and not the authors, were present during intervention activities because we did not want our presence as foreigners to influence the intervention.

Our field researchers – particularly the research manager and senior field officers (supervisors) – also conducted hundreds of informal conversations about the public safety context in Sorsogon similar to those we conducted ourselves and describe above. For example, field researchers often arrived early to POP meetings and struck up informal conversations with barangay officials while waiting for other meeting participants to arrive, or discussed the intervention with curious friends or family members who became aware of the PNP's initiatives. Our more senior field researchers also frequently met with mid-ranking police leadership and the officers involved in the intervention to coordinate intervention logistics, providing numerous opportunities to learn about their reactions to and perspectives on the intervention.

We instructed our field staff to take notes at all the activities they attended, and report back to us on the intervention's implementation, progress, and any potential deviations. We also tasked them to think critically and take notes on the factors they were enhancing or inhibiting the success of the program. As with our own conversations, since the focus was primarily on monitoring the implementation of the intervention and understanding contextual nuances, we did not ask the field researchers to capture direct quotations. The field staff were grouped into three teams, each headed by a senior field officer, and overseen by an experienced field manager. Each week, each team of field officers met with the field manager to discuss their notes and elaborate on any insights they had about the intervention. The authors then had weekly meetings with the field manager to discuss the main points raised by the field staff. In addition, at least one of the authors was present at the field site during most times during the intervention, and we often discussed the intervention directly with the field staff.

A.5.3 Interpretation of Qualitative Evidence

Given the informal nature of most of this qualitative information collection, learning from it was primarily an exercise in triangulating big-picture context from thousands of distinct interactions. Most of these interactions painted complementary, if incomplete, pieces of a unified picture. As qualitative information accumulated to paint a more complete picture of the safety and policing context in Sorsogon, and later our intervention's effects, we used it in the following ways:

1. Understand local threats to safety that could affect research on community policing, both in terms of threats to the intervention itself and threats to data collection, for the purposes of mitigating ethical concerns.
2. Design a locally-appropriate community policing intervention that could be realistically implemented. This task relied heavily on conversations with PNP leadership, who had strong opinions about activities they

expected to be effect and resources they could deploy, but also with community leaders who had a useful perspective on public safety needs that were not being served by the existing policing model.

3. Understand and contextualize the quantitative results of our main pre-registered tests. In particularly, we used qualitative information to confirm that the intervention was implemented as expected and null results were not caused by non-compliance. We also gained insights about the challenges that inhibited certain mechanisms from taking hold, such as factors that inhibited police leadership's ability to improve accountability.
4. Identify mediating factors which could have obscured effects in our primary tests, with the goal of motivating inductive theory building and revealing useful additional quantitative tests. Insights from our field officers revealed that officers' home locality, as well as the presence of nearby NPA activity, were two particularly important potential mediators. We also gathered information on a number of other, less developed, mediators that might be shaping program effectiveness (such as the role of mid-level station leadership).

A.6 Ethics and Permissions

All procedures were approved by the University of California San Diego (UCSD) IRB under protocol numbers 170974 and 170415.

Partnering with the Sorsogon PNP on the One Sorsogon community policing initiative required careful consideration of several important ethical issues. In the manuscript, we discuss two concerns that were particularly challenging in the context of our study: the broader Philippine Drug War and local insurgent activity. Here, we further describe our process for assessing these issues and discuss several other ethical issues that were particularly pressing in the context of our study.

An important piece of context is that One Sorsogon was a home-grown initiative of the Provincial Police Chief, who wished to de-emphasize the aggressive tactics of the Drug War and instead focus on non-drug related public safety issues. This does not mean that we, as researchers, absolve ourselves of the risks involved in implementing the program. We went through an extended process of due diligence to assess the risks of the intervention. That said, the homegrown nature of the program does reduce the risk that we as outsiders would introduce harms to the community by introducing a policy that was not locally appropriate. In addition, because the policy would have been implemented with or without our involvement, it was unlikely that we would introduce any additional harms that would not have already been experienced. If anything, the system we put in place to monitor abuses should reduce harms to citizens, in expectation.

Given this context, we saw substantial ethical benefits stemming from the potential for catalyzing broader reform within the PNP, paired with conditions that allowed us to implement the intervention safely. Policing in the Philippines is highly decentralized; regional and provincial police chiefs have a great degree of autonomy over the tactics used by their units. As a result, the Sorsogon Police Chief's community policing initiative was likely to have a large impact on day-to-day policing practices across the province. The chief hoped to overhaul the Sorsogon PNP's approach to focus on community policing before his potential promotion to the regional office so that he could use it as a model for more widespread reform. When he learned of our interest to partner on an evaluation of One Sorsogon, he was adamant that, if the program was successful, a credible external evaluation could help sell this "softer" approach to policing to higher ups in the PNP. Indeed, the model of One Sorsogon helped spur a much larger region-wide community outreach initiative.

Although we saw significant potential benefits to the research, the primary factor in our ethical calculus was whether we could participate in the research without putting subjects or research staff in harm's way. During the project planning period, we held frequent meetings (both with and without the PNP leadership) to discuss how to mitigate risks associated with the intervention. One particularly important consideration was that the level of decentralization in the PNP could backfire. Despite the leadership-level buy-in with community policing practices,

we were concerned that street level officers may not adopt a similar perspective. To guard against the risk that officers may commit abuses as a result of the intervention, we put in place an extensive monitoring system for our research team to observe potential abuses. Multiple staff members from our experienced local research team acted as "community observers" during all town halls conducted as part of the intervention. This allowed us to monitor the vast majority of police-citizen interactions that took place as part of the intervention. A member of the research team was also present at every POP meeting. Finally, we designed the intervention so that LGU officials took the lead in implementing the public safety initiatives decided on during POP in their own communities, with the PNP playing a supporting role. We were prepared to immediately end our involvement in the project if we learned that the intervention created opportunities for individual PNP officers to abuse or intimidate citizens. No instances of abuse were reported.

Several ethical considerations were especially salient in our context. First, there were important ethical challenges that arose from working with the PNP in the midst of President Duterte's War on Drugs. During the period from late 2016 through September 2021, the Drug War resulted in 12,000-30,000 deaths, including at least 6,200 at the hands of the police ("How many people have been killed in Rodrigo Duterte's war on drugs?" *The Economist*, November 2021, https://www.economist.com/graphic-detail/2021/11/22/how-many-people-have-been-killed-in-rodrigo-dutertes-war-on-drugs. We were wary of increasing contact between police officers and citizens due to the risk of police abuse. Our scoping work shed important light on how this risk manifested in Sorsogon. First, the PNP is a very decentralized organization, with provincial chiefs having a great amount of discretion over policing practices. The vast majority of Drug War violence occurred in major urban areas, especially Manila. Before approaching the PNP, we learned from our contacts in the area that the Sorsogon Provincial Police Chief was a well-known reformer who opposed the violent tactics of the Drug War.

A survey of citizens we conducted before approaching the PNP also suggested low rates of police abuse in Sorsogon: 99% of respondents had not heard of any instances of police abuse in their community in the last six months, and 95% of respondents said that the PNP's methods for fighting drugs had not become more violent in their area since Duterte's election. To reduce response bias to these potentially sensitive questions, respondents privately recorded their responses on an iPad that locked after responses were submitted. ACLED's special project on the Philippines Drug War recorded only five drug-related deaths in Sorsogon Province between Duterte's election and the initiation of our project, and none in the nine months preceding the intervention. Still, we were prepared to walk away from the project in response to indications of police abuse. We employed field staff who attended each community policing event, so they could report any instances of coercion or abuse. We received no reports of concerning behavior.

The second main ethical concern stemmed from the threat of NPA presence in Sorsogon, including the risk that the intervention might increase incentives for insurgents to target civilians or the police with violence. To understand the nature of this risk, we engaged in extensive conversations with local experts on the insurgency, including individuals with current and former ties to the NPA in Sorsogon. Among these experts, there was unanimous agreement that the community policing intervention would not increase the risk of violence if implemented in our proposed sample areas with low NPA activity. We relied heavily on our local network in each municipality to identify areas where NPA presence was strong enough to make the intervention unsafe. We also consulted the local AFP command and each municipal PNP station, who wanted to exclude barangays where there was a risk of ambushes. Finally, we required approval from each barangay captain in the already pared-down sample. All captains in our sample consented to the intervention and the study.

A broader concern is whether the goal of improving trust and cooperation with the PNP as an institution was actually a good thing during a time when the institution was the face of abusive human rights violations. We wrestled significantly with this concern, but in the end several factors led us to proceed. First, the majority of citizens in Sorsogon seem to recognize the degree of decentralization in the PNP and the vastly different approaches to policing in major urban areas like Manila (where the Drug War was primarily taking place) and rural provinces

like Sorsogon. In other words, citizens can generally separate between the PNP "institution" at these two levels. Second, we devoted significant attention to mitigating the risk of the Sorsogon PNP capitalizing on increased citizen trust to enable abusive practices over the long term. Based on discussions with key informants, we came to believe that One Sorsogon would empower the community-focused municipal Police Community Relations (PCR) officers to remain influential, even if a changeover in provincial police leadership occurred. This proved correct when the provincial chief was promoted to the regional office early in the POP intervention. Although his successor wanted to implement his own initiatives, the momentum of One Sorsogon convinced him to continue with a community-focused approach. In terms of our own commitment as researchers, we viewed our involvement in One Sorsogon to be the beginning of longer term efforts to help implement "best practices" of policing across the province and the region. Each of us is still involved in ongoing research on policing in the region and at no time did we consider this to be a one-off project.

Finally, the participation of local researchers and stakeholders proved to be a key factor in our ability to accurately assess ethical risks and benefits. As we mentioned in the manuscript and in the preceding paragraphs, we relied on an extensive network of local stakeholders to brainstorm and assess the situations that might arise as a result of the intervention. One of the study authors was born and raised in the region, as was the full-time field research manager. The field research manager has a long-running relationship with the PIs that allows her to freely express ethical or other concerns about things she observed in the field. These researchers' extensive local knowledge and experience proved critical in our ability to make these determinations and carry out the research in a safe, ethical manner.

A.7 Study Timeline

- January 2016: PIs begin conducting observational research on policing in Sorsogon Province, including background interviews and a citizen survey.
- September 2016: PIs initiate discussions with PNP Provincial Director about evaluating his planned community policing initiative.
- February–April 2017: Police hotline rollout and advertisement (evaluated in other research)
- August 2017: Baseline survey of PNP officers
- December 2017–May 2018: Problem Oriented Policing program implementation
- June 2018: Endline survey of PNP officers

A.8 Pre-Analysis Plans

There are two pre-analysis plans relevant to this study, which are available online at https://osf.io/tfpx7/ and https://osf.io/zxefj/. The first is a multi-country PAP for a harmonized study on community policing in six different country sites. The second is a country-specific PAP for the Philippines component of the larger study. [For the purpose of journal review, we append a blinded version of both PAPs below.] The final versions of both PAPs were officially filed in January 2020. The gap between our endline survey and the filing date was driven by the fact that ours was the first country intervention implemented in the larger study referenced in the meta-PAP. The steering committee for the larger study needed to incorporate additional details relevant to the interventions that were rolled out later, and asked that all teams hold off on uploading PAPs until this process was completed. In accordance with this plan, we were restricted from downloading any data until the PAPs were filed.

Because these PAPs include pre-registered analyses for multiple papers, we include information below on 1) where the relevant measures and analyses for this paper are located in the PAPs and 2) how the analyses in the PAP were implemented, including some minor deviations.

A.8.1 Hypotheses and Measurement

Guide to PAP

- The main hypotheses for the *Empathy*, *Accountability* and *Corruption* outcomes (as well as the umbrella hypothesis on *Officer Attitudes*) are pre-registered as Hypothesis 3a under the header "Primary Outcome Family 3" on page 12 of the meta-PAP.
- The tests of these hypotheses (associated with the outcome indices) are specified in Table 4 on page 14 of the meta-PAP and in the table on page 17 of the country-specific PAP.
- The survey items that compose the officer-facing indices specified in Table 4 are found in the table on 59-62 of the meta-PAP. Here you can also find the list of survey items included in each outcome index.
- The hypothesis corresponding with the "Shared Concerns" outcome is pre-registered as hypothesis 3c on page 19 of the country-specific PAP. The construction of the measure is labeled *mip_officer_match* on page 27 of the country-specific PAP.
- The pre-registered method for index construction (including details on standardization and imputation of missing items) is located on page 39-40 of the meta-PAP.
- The remaining hypotheses in both PAPs are citizen-facing hypotheses, and many aspects of the research design, measurement, and estimation are only relevant to these citizen-facing aspects of the study.

Implementation and Deviations

- We did not ask a set of questions relating to a hypothetical scenario about officer abuse of citizens (hypothetical scenario 5 in the meta-PAP). After consultation with PNP leadership, it was determined that these questions risked introducing substantial preference falsification among officers, including on remaining survey items, given the political climate at the time. Three of the questions about this hypothetical scenario (regarding whether officers would report the incident and whether leaders would punish them for it) were included in the pre-registered *Accountability* index, but were not included in our measure. Another subset of these questions were pre-registered as the *Abuse* index in the meta-PAP, which we exclude.
- We inadvertently forgot to include the *Trust* index in our country-specific PAP, although we always intended for it to be a part of this design. This intention is clear in the way that we grouped these questions in our officer survey by asking them sequentially in the section labeled "trust," and these three survey items were labeled with the prefix "*trust_*" in the officer questionnaire included in our country-specific PAP. Nevertheless, we recognize this oversight.
- The pre-registered method for index construction involves imputing missing values for survey items prior to combining the items into the index. We present the results for the indices constructed in this way (which we label the alternative indices) in SI Table A.12, as well as in the other tables throughout the SI. The results are all consistent with simple additive versions of the indices (constructed without imputation), which we present in the main body of the paper for ease of interpretation.
- The pre-registered *Shared Concerns* outcome (*mip_officer_match* in the country-specific PAP) matches officer concerns with the *three* most commonly mentioned citizen issues. These results are presented in Table A.13. In the main body of the paper, we use an alternative version of the measure that matches officer concerns with the *four* most commonly mentioned citizen issues. The results are consistent using these two measures.

A.8.2 Research Design and Estimation

Guide to PAP

- The officer-specific estimation equation was not included in the PAP, which was an oversight. However, the estimation strategy for this component of the study follows the same structure as the other pre-registered specifications relating to officer-facing outcomes in the meta-PAP and the other citizen-facing specifications

in the country-specific PAP. The structure of Equation 1 (panel specification) is pre-registered in the country-specific PAP as the third equation in the page header above Hypothesis 3c.

- The clustering strategy is pre-registered on page 15 of the country-specific PAP.
- The process for dealing with missing data in baseline outcomes by replacing missing values with zeros and adding an indicator variable for missingness is pre-registered on page 40 of the meta-PAP.

Implementation and Deviations

- Equation 2 (cross-sectional specification) is not pre-registered. We include it as a robustness check to mitigate concerns regarding attrition.
- Because the officer randomization occurred within the subset of barangays assigned to POP treatment, the alternate treatment arms included in the estimating equations for the citizen-facing outcomes are not relevant to the officer-facing outcomes.
- On page 15 of the country-specific PAP, we note an issue with the barangay-level blocking code that led us to exclude block fixed effects in our models. However, this issue is specific to the barangay-level randomization and not the officer-level randomization. The officer randomization was blocked based on station and rank, as noted in section 7.3 of the country-specific PAP. This was implemented as planned, and so we retain the fixed effects for these variables, consistent with the estimation strategy for officer-facing outcomes in the meta-PAP.
- None of the heterogeneity analyses were pre-registered, and we do not consider these analyses to constitute hypothesis tests. Rather, the heterogeneity analyses were conducted in an exploratory fashion to show whether patterns in the data were consistent with our inductive theory developed using qualitative evidence.