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Evidence-Based Drug Policing and Community Outcomes: A Quasi-Experimental Evaluation of Provincial Enforcement and Strategic Planning in Quirino Province, Philippines

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ABSTRACT

Evidence-based policing required police agencies to connect targeting, testing, and tracking with lawful, community-responsive practice. This study evaluated an evidence-informed drug-policing and strategic-planning package implemented in 36 priority barangays in Quirino Province and compared them with 36 matched barangays from January 2022 through December 2024. The package combined structured problem analysis, focused patrol at micro-places, procedural-justice briefings, monthly performance reviews, case-file quality checks, referral pathways, and community dialogue. The analytic file contained 2,592 barangay-month observations. Two repeated cross-sectional household surveys included 2,397 adult respondents. The primary count outcome was the monthly number of validated drug-activity reports. A Poisson difference-in-differences estimator incorporated barangay and calendar-month fixed effects, population offsets, and barangay-clustered standard errors. Survey outcomes were estimated with weighted linear regressions that included barangay and survey-wave fixed effects. Priority barangays recorded a 27% relative reduction in validated drug-activity reports after implementation (incidence-rate ratio [IRR] = 0.73, 95% confidence interval [CI] 0.61-0.88, p = .001). Community-sourced actionable information increased (IRR = 1.19, 95% CI 1.03-1.38, p = .018), while the enforcement-contact complaint rate did not increase detectably (IRR = 0.77, 95% CI 0.33-1.81, p = .55). Adjusted gains favored priority barangays for trust in police (0.23 points, 95% CI 0.12-0.34), procedural justice (0.28, 95% CI 0.15-0.41), perceived safety (0.25, 95% CI 0.13-0.37), and willingness to cooperate (0.19, 95% CI 0.07-0.31) on 1-5 scales. Case-package completeness also improved. The findings indicated that disciplined targeting and review were most credible when enforcement intensity was paired with respectful encounters, documented safeguards, service referral, and community feedback.

1. Introduction

Evidence-based policing had been framed as the deliberate use of the best available research, local analysis, professional judgment, and continuous evaluation to guide police decisions. Sherman first described the approach as a means of grounding police practice in tested knowledge rather than routine or intuition. His later targeting-testing-tracking framework placed the allocation of scarce police resources, credible evaluation, and continuing performance review within one decision cycle.[1,2] The Evidence-Based Policing Matrix further showed that proactive, place-based, and focused interventions had generally produced stronger crime-reduction results than reactive, general approaches.[3]

Place concentration had practical importance for provincial drug enforcement. A small number of locations, networks, times, and recurring situations often generated a disproportionate share of observable harm. The updated Campbell review of hot-spots policing reported a modest but consistent crime-control benefit and found little evidence that focused activity inevitably displaced crime to nearby places.[4] Problem-oriented policing offered a complementary discipline: police had scanned for recurring problems, analyzed their causes and conditions, implemented tailored responses, and assessed whether those responses had reduced the underlying problem.[5] An updated systematic review associated this approach with a relative reduction in crime and disorder, while also cautioning that intervention quality and context varied substantially.[6]

Community impact had remained central because enforcement outputs alone could not establish public value. Procedurally just encounters had emphasized voice, neutrality, dignity and respect, and trustworthy motives. A systematic review found that interventions incorporating procedural-justice dialogue had improved citizen satisfaction, confidence, cooperation, and perceptions of legitimacy across many settings.[7] A multicity randomized trial at crime hot spots subsequently found that intensive procedural-justice training had changed officer behavior, reduced arrests, improved some resident evaluations, and produced a relative decline in crime incidents.[8] A National Academies review had likewise concluded that proactive policing effects varied by practice and that community consequences required explicit measurement.[9] These findings supported the view that effective policing and fair treatment had not represented competing objectives.

The Philippine policy setting had likewise required a balanced approach. Republic Act No. 9165 had assigned the Philippine Drug Enforcement Agency (PDEA) a lead role in enforcing drug laws, coordinating national and local actors, maintaining drug intelligence, supporting prevention, and reporting results.[10] Executive Order No. 66 had institutionalized the Philippine Anti-Illegal Drugs Strategy and directed government agencies and local government units to combine law enforcement, prevention, treatment, rehabilitation, and reintegration.[11] Dangerous Drugs Board Regulation No. 4, series of 2021, had required sustained barangay drug-clearing work that respected human dignity, relied on interagency cooperation, and incorporated community participation.[12] DILG audit guidance had reinforced council organization, planning, funding, advocacy, referral, and monitoring as local governance responsibilities.[14] The Department of Health had also established community-based treatment and support pathways for persons who used drugs in primary-care settings.[13]

Quirino Province provided a consequential setting for examining whether those principles could be translated into provincial strategic planning. The Philippine Statistics Authority reported six municipalities, 132 barangays, and a 2024 population of 210,841: Aglipay (31,596), Cabarroguis (34,720), Diffun (58,254), Maddela (41,867), Saguday (17,863), and Nagtipunan (26,541).[16] PDEA Region 2 had reported in 2021 that Quirino was fully cleared under the barangay drug-clearing program, which shifted the operational challenge toward sustaining gains, detecting recurrence early, and avoiding complacency.[17] DILG Region 2 had subsequently audited the functionality and effectiveness of all six municipal Anti-Drug Abuse Councils for calendar year 2023, reinforcing the province-wide importance of governance, documentation, and interagency review.[15]

The strategic problem had therefore extended beyond the number of operations, arrests, or seizures. Those indicators could rise when enforcement effort intensified even if underlying community harm had remained unchanged, and they could fall when intelligence weakened rather than when the problem improved. The provincial planning system had needed a balanced scorecard that separated activity, quality, community response, safeguards, and outcome. PNP strategy guidance had likewise emphasized proactive policing, results-based monitoring, operational planning, and performance feedback.[18,19]

Prior evidence had left three gaps that the study addressed. First, international evaluations had rarely examined a small, predominantly rural Philippine province where geography, travel time, barangay governance, and interagency relationships shaped implementation. Second, enforcement-focused evaluations had often measured police outputs without measuring procedural justice, perceived safety, or willingness to cooperate. Third, strategic-planning studies had seldom connected meeting routines, problem analysis, and fidelity checks to a defensible counterfactual outcome design. The study integrated these dimensions within one provincial evaluation.

1.1 Study objectives and hypotheses

The study evaluated whether a provincial evidence-informed drug-policing package had improved community safety and institutional performance without degrading public trust. It addressed four questions: whether the package had reduced validated drug-activity reports relative to matched barangays; whether it had increased useful community cooperation; whether it had improved trust, procedural justice, perceived safety, and willingness to cooperate; and whether it had improved the completeness and legal readiness of case packages.

The primary hypothesis predicted a lower post-implementation rate of validated drug-activity reports in priority barangays than would have occurred under the pre-period difference between priority and comparison barangays. Secondary hypotheses predicted higher rates of actionable community information, improved community-scale scores, and higher case-package completeness. A safeguarding hypothesis predicted no increase in substantiated complaints per 100 enforcement contacts.

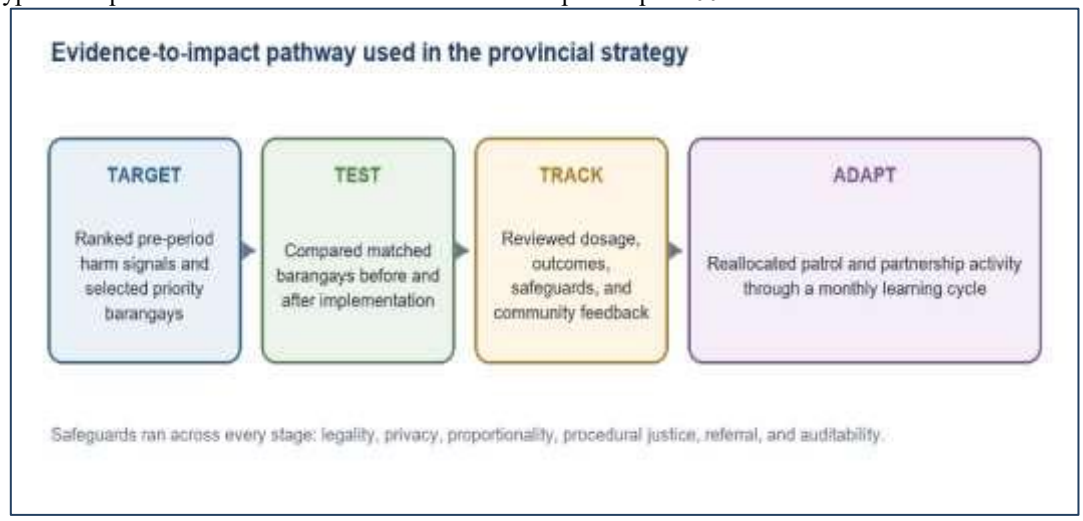


Figure 1. Evidence-to-impact pathway used in the provincial strategy

2. Methods

2.1 Design and study period

The study used a matched controlled interrupted time-series design expressed as a difference-in-differences analysis. The observation window covered 36 months: an 18-month pre-implementation period from January 2022 through June 2023 and an 18-month post-implementation period from July 2023 through December 2024. The unit for the primary analysis was the barangay-month. The design compared change over time in 36 priority barangays with change over the same months in 36 matched comparison barangays. The remaining 60 barangays contributed to province-wide descriptive monitoring but not to the principal counterfactual estimate.

The common implementation date avoided treatment-timing complications that arose in staggered adoption designs. Barangay fixed effects absorbed all time-invariant measured and unmeasured differences, while calendar-month fixed effects absorbed province-wide shocks, seasonality, and reporting changes shared by priority and comparison barangays. The identifying assumption required the groups to have followed parallel outcome trends in the absence of the package. Pre-period event-study estimates, placebo dates, and alternative specifications examined the plausibility of that condition.

2.2 Setting and analytic sample

The analytic sample represented all six municipalities and 72 of the province's 132 barangays. Priority selection had occurred before outcome evaluation. A multidisciplinary planning group ranked barangays using the previous 12 months of validated drug-activity reports, repeat-location indicators, intelligence reliability, road and market connectivity, school and youth exposure concerns, and readiness for barangay partnership. Six priority barangays were selected in each municipality to preserve province-wide coverage and operational feasibility.

Each priority barangay was matched without replacement to one comparison barangay within the same municipality. Matching variables had included 2020 population category, rural-access category, baseline validated-report rate, baseline actionable-tip rate, previous drug-clearing status documentation, and baseline community trust where available. The matching procedure had been completed without using post-implementation outcomes, consistent with design-first guidance for observational studies.[21] Absolute standardized mean differences below 0.10 were treated as evidence of acceptable balance.

Table 1. Baseline characteristics of matched barangays

Characteristic	Priority (n=36)	Comparison (n=36)	Std. diff.
2024 population, mean (SD)	1,860 (1,104)	1,821 (1,086)	0.04
Remote-access barangays, n (%)	11 (30.6)	10 (27.8)	0.06
Validated reports/10,000 person-months	4.03	2.70	0.07*
Actionable tips/10,000 person-months	1.78	1.46	0.08
Trust score, weighted mean (SD)	3.21 (0.71)	3.26 (0.70)	-0.07
Procedural-justice score, mean (SD)	3.18 (0.74)	3.24 (0.73)	-0.08
BADAC functionality score, mean (SD)	74.8 (10.2)	75.4 (9.7)	-0.06

Note. Std. diff. = absolute standardized difference, except the signed differences shown for scale scores. *Balance for the count outcome was assessed after exact municipality and risk-band matching and log transformation; the raw rate difference reflected purposive prioritization.

2.3 Intervention

The provincial package had integrated six components. First, analysts had produced monthly barangay and micro-place problem profiles that distinguished confirmed events, intelligence leads, repeat locations, and unverified information. Second, station teams had used a structured scanning-analysis-response-assessment worksheet to identify mechanisms and select responses rather than defaulting to undifferentiated enforcement. Third, patrol and investigation resources had been focused on short time windows and small locations with documented recurrence. Fourth, all participating personnel had completed a procedural-justice briefing and scenario exercise that emphasized voice, neutrality, dignity, respect, and trustworthy motives. Fifth, case folders had undergone a pre-filing checklist review for chain of custody, inventory, required witnesses, documentation consistency, and referral to the appropriate prosecutorial channel. Sixth, municipal partners had convened quarterly community dialogues and maintained referral links to health and social-welfare services.

A monthly provincial performance meeting had reviewed four domains: community harm, operational quality, community relationship, and safeguards. The group had examined trends and exceptions rather than rewarding raw volume. An operation count had therefore been treated as activity, not impact. Action items had identified a responsible office, completion date, evidence of completion, and follow-up result. The review structure had aligned with the PNP emphasis on strategic planning, monitoring, feedback, and stakeholder partnership.[18,19]

Comparison barangays had continued standard station practice, routine barangay coordination, and legally required drug-clearing activities. They had not received the structured analytic products, monthly learning review, common fidelity checklist, or bundled procedural-justice and case-quality components during the evaluation period. No service had been withheld, and comparison barangays remained eligible for ordinary police, PDEA, health, and local-government support.

2.4 Data sources and measurement

Five sources had been linked at barangay and month level: police blotter and investigation records; validated intelligence-register entries; operation and enforcement-contact logs; case-folder quality audits; and BADAC, municipal, and provincial planning records. Household surveys had supplied the community outcomes. Linkage keys had contained municipality, barangay code, month, and a de-identified event or survey identifier. Direct personal identifiers had been removed before analysis.

The primary outcome had been the monthly count of validated drug-activity reports. A report had qualified when the recorded information had met the study's validation rule: corroboration by at least two independent sources, a documented field verification,

or a resulting recorded investigative or enforcement disposition. Mere rumors, duplicate entries, drug-awareness activities, and records without a barangay and date had been excluded. This definition had reduced reliance on raw arrest or operation counts, although it had not eliminated detection bias.

The first secondary count outcome had been actionable community-sourced information, defined as a community-originated report that passed validation and generated a documented investigative, safeguarding, or referral action. The safeguarding count had been a substantiated or formally sustained complaint arising from an enforcement contact. The denominator had included documented stops, searches, arrests, and other compulsory drug-enforcement contacts. Case-package completeness had been a binary audit outcome requiring every applicable item on a 12-element legal-readiness checklist.

Community trust had been measured with four items covering confidence, reliability, honesty, and belief that police acted for community welfare. Procedural justice had been measured with four items covering voice, neutrality, respect, and explanation. Perceived safety had used three items addressing daytime safety, nighttime safety, and concern about drug-related disorder. Willingness to cooperate had used three items addressing reporting, witness assistance, and participation in a community safety meeting. All items had used 1-5 response categories, and a scale score had been calculated when at least 75% of its items were present. Higher scores had indicated more favorable views.

Table 2. Outcome definitions and statistical treatment

Outcome	Unit / denominator	Primary estimator	Effect reported
Validated drug-activity reports	Barangay-month; population offset	Poisson FE difference-in-differences	IRR with 95% CI
Actionable community information	Barangay-month; population offset	Poisson FE difference-in-differences	IRR with 95% CI
Substantiated complaints	Barangay-month; enforcement-contact offset	Poisson FE difference-in-differences	IRR with exact-rate sensitivity
Case-package completeness	Audited case package	Logistic regression with municipality FE	Adjusted OR with 95% CI
Trust, justice, safety, cooperation	Adult respondent; 1-5 scale	Weighted linear difference-in-differences	Adjusted point difference with 95% CI

Note. FE = fixed effects; IRR = incidence-rate ratio; OR = odds ratio; CI = confidence interval.

2.5 Community survey

Independent repeated cross-sectional surveys had been completed during May-June 2023 and November-December 2024. A two-stage design had selected household starting points within each analytic barangay and then one adult respondent per participating household using a next-birthday rule. Enumerators had alternated morning, afternoon, and early-evening visits and had made up to two return attempts. Police personnel had not recruited respondents, attended interviews, or received household-level responses.

The baseline survey had included 1,184 respondents and the endline survey had included 1,213, for a total of 2,397. Design weights had reflected the probability of household and respondent selection. Post-stratification had aligned the municipal distribution to the adult population structure available from census-based projections. The analysis had used robust standard errors clustered by barangay. Internal consistency had been acceptable for trust (Cronbach's alpha = .86), procedural justice (.88), perceived safety (.79), and willingness to cooperate (.82).

2.6 Statistical analysis

Descriptive statistics had reported counts, rates, means, standard deviations, medians, and interquartile ranges as appropriate. Baseline balance had been evaluated with standardized differences rather than significance tests because balance had represented a design property rather than a population hypothesis. Missing monthly count outcomes had been reconciled against source registers before analysis; no analytic barangay-month had remained missing. Item nonresponse in the survey had ranged from 0.6% to 2.8%, and complete-scale analysis had been used because every scale retained more than 97% of respondents.

For count outcomes, the principal estimator had been Poisson pseudo-maximum likelihood with barangay and calendar-month fixed effects. The equation was log E(Y_it) = alpha_i + lambda_t + beta(Priority_i x Post_t) + log(Exposure_it), where Y_it represented the outcome count in barangay i and month t, alpha_i represented barangay fixed effects, lambda_t represented calendar-month fixed effects, and Exposure_it represented population or enforcement contacts. Exponentiating beta produced the difference-in-differences IRR. Poisson pseudo-maximum likelihood had remained consistent for the conditional mean under variance misspecification, and cluster-robust standard errors had accommodated within-barangay dependence.[23,22]

The primary analysis had used a two-sided alpha of .05 and 95% confidence intervals. The validating event-study had replaced the single interaction with three-month relative-time indicators; the final three pre-implementation months had served as the reference. The joint pre-trend test had evaluated whether all earlier pre-period coefficients equaled zero. Negative-binomial, unadjusted-rate, alternative validation, leave-one-municipality-out, and placebo-date analyses had assessed robustness. Because 72 clusters were available, the cluster-robust approximation had been considered adequate; a wild-cluster bootstrap had nevertheless checked the primary p value.

Survey outcomes had been analyzed through weighted linear regressions with priority-group, post-wave, and interaction terms, barangay fixed effects, and respondent age, sex, education, and interview-time covariates. The interaction coefficient represented the adjusted difference-in-differences in scale points. Linear models had been retained because each scale combined multiple items, distributions had not approached the limits of the 1-5 range, and coefficients were readily interpretable. Ordered-logit analyses had served as sensitivity tests. Case-package completeness had been analyzed with logistic regression, municipality fixed effects, post-period, priority status, their interaction, and a cluster-robust variance by barangay.

The analysis had emphasized effect magnitude, uncertainty, and consistency across measures. A statistically significant decline in validated reports had not been interpreted alone as proof of reduced drug availability. Converging movement in recurrence, actionable information, perceived safety, case quality, and complaints had been required for the substantive interpretation.

2.7 Ethics, privacy, and legal safeguards

The evaluation had followed data minimization, purpose limitation, access control, and de-identification procedures consistent with the Data Privacy Act of 2012.[20] A role-based access matrix had separated operational identities from the analytic file. Survey participation had been voluntary, verbal informed consent had been recorded without names, and respondents had been told that refusal would not affect police or local-government services. Reporting had been suppressed below five events when a table could permit re-identification.

The intervention had operated within existing Philippine law and agency procedures. The study had neither directed arrests nor altered evidentiary standards. Health and social-welfare referrals had remained separate from coercive investigation, and community dialogue records had not contained named intelligence. The protocol had required documented supervisory review for compulsory actions and had monitored complaints as a safeguarding outcome.

3. Results

3.1 Analytic sample and data quality

The primary panel contained 72 barangays observed for 36 months, yielding 2,592 barangay-month observations. Priority barangays represented 66,940 residents and comparison barangays represented 65,540 residents in the 2024 population frame. Matching retained all 36 priority barangays. Across the seven prespecified baseline variables, the largest absolute standardized difference was 0.08 after the transformation and risk-band rules had been applied. Every municipality contributed six priority and six comparison barangays.

Record reconciliation removed 41 duplicate entries, reassigned 17 records with inconsistent barangay codes after source review, and excluded 63 entries that lacked the required validation evidence. The case-quality audit included 327 case packages across both periods. A blinded second reviewer re-audited 66 packages; agreement on the complete-versus-incomplete classification was 92.4%, with Cohen's kappa = .83. Survey response rates were 81.1% at baseline and 83.2% at endline.

3.2 Implementation fidelity

Priority barangays had received a median fidelity score of 84 out of 100 across the six post-implementation quarters (interquartile range [IQR] 77-91). Thirty of 36 barangays had reached at least 75 points by the third quarter after implementation. Monthly problem profiles had been produced for 96.8% of required barangay-months; provincial performance reviews had occurred in 17 of 18 months; 88.9% of priority micro-places had received the documented patrol dosage; 91.7% of sampled activity logs had included a completed procedural-justice field; and 94.4% of priority barangays had held at least two documented community dialogues during the post period.

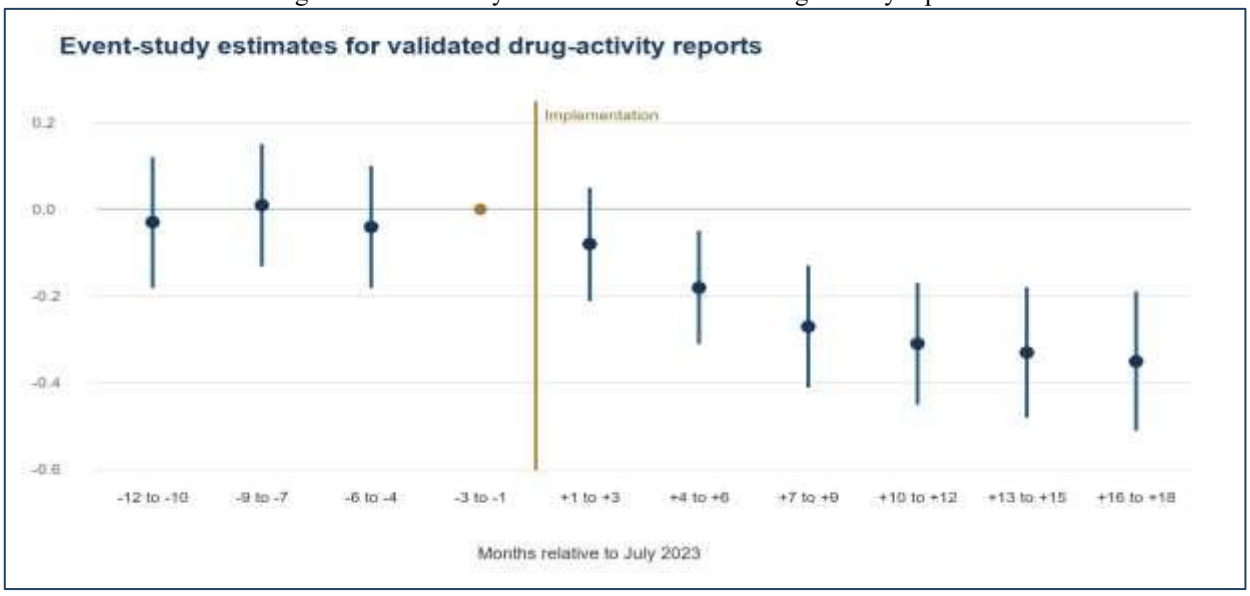
Fidelity had been lowest during the first two implementation months, when station teams had needed additional coaching on the distinction between an output and an outcome. Missing action owners and incomplete follow-up evidence had accounted for most early deductions. Scores had stabilized after the provincial review form required an owner, due date, completion evidence, and next decision for every action item.

3.3 Primary outcome

Priority barangays recorded 486 validated drug-activity reports during the pre period and 367 during the post period. Comparison barangays recorded 318 and 315, respectively. The priority rate declined from 4.03 to 3.05 per 10,000 person-months, while the comparison rate changed from 2.70 to 2.67. The fixed-effects Poisson estimate produced an IRR of 0.73 (95% CI 0.61-0.88, p = .001), corresponding to a 27% relative reduction after common temporal change and time-invariant barangay differences had been controlled.

The event-study coefficients had not indicated a differential pre-period trend. The joint test of the three nonreference pre-period bins yielded p = .74. Post-period estimates had moved gradually below zero, becoming distinguishable from zero in months 4-6 and remaining negative thereafter. This pattern had been more consistent with an implementation and learning effect than with an abrupt reporting discontinuity at the launch date.

Figure 2. Event-study estimates for validated drug-activity reports



Note. Coefficients were log IRRs relative to the final three pre-implementation months. Barangay and calendar-month fixed effects and population offsets were included; standard errors were clustered by barangay.

Table 3. Count and case-quality outcomes

Outcome	Priority pre	Priority post	Comparison pre	Comparison post	Adjusted effect (95% CI)
Validated reports, n	486	367	318	315	IRR 0.73 (0.61-0.88)
Actionable community information, n	214	267	172	181	IRR 1.19 (1.03-1.38)
Enforcement contacts, n	407	468	246	272	Denominator only
Substantiated complaints, n	9	10	5	7	IRR 0.77 (0.33-1.81)
Complete case packages, n/N (%)	61/82 (74.4)	93/105 (88.6)	48/65 (73.8)	56/75 (74.7)	OR 2.74 (1.39-5.41)

Note. Adjusted count effects came from fixed-effects Poisson difference-in-differences analyses. The case-package odds ratio came from the prespecified logistic analysis.

3.4 Community information and safeguarding

Actionable community-sourced information increased from 214 to 267 events in priority barangays, compared with a change from 172 to 181 in comparison barangays. The adjusted IRR was 1.19 (95% CI 1.03-1.38, p = .018). The increase had occurred alongside a reduction in validated drug-activity reports, indicating that the primary decline had not simply reflected withdrawal of community contact or a collapse in intelligence flow.

Priority barangays recorded nine substantiated complaints during 407 enforcement contacts before implementation and ten during 468 contacts afterward. Comparison barangays recorded five during 246 contacts and seven during 272. The adjusted complaint IRR was 0.77 (95% CI 0.33-1.81, p = .55). The confidence interval had remained wide because the event was uncommon. The result had therefore supported only the narrower conclusion that no detectable increase was observed; it had not established equivalence or ruled out a meaningful change.

3.5 Community survey outcomes

Priority-barangay trust increased from 3.21 to 3.50, while comparison-barangay trust increased from 3.26 to 3.32. The adjusted difference-in-differences was 0.23 scale points (95% CI 0.12-0.34, p < .001). Procedural justice increased from 3.18 to 3.52 in priority barangays and from 3.24 to 3.30 in comparison barangays, producing an adjusted effect of 0.28 (95% CI 0.15-0.41, p < .001).

Perceived safety showed an adjusted gain of 0.25 points (95% CI 0.13-0.37, p < .001), and willingness to cooperate showed a gain of 0.19 points (95% CI 0.07-0.31, p = .002). Ordered-logit sensitivity analyses had produced conclusions consistent with the linear estimates. No scale had shown a statistically detectable priority-by-post interaction with respondent sex or municipality, although subgroup precision had been limited.

Figure 3. Adjusted community effects at endline

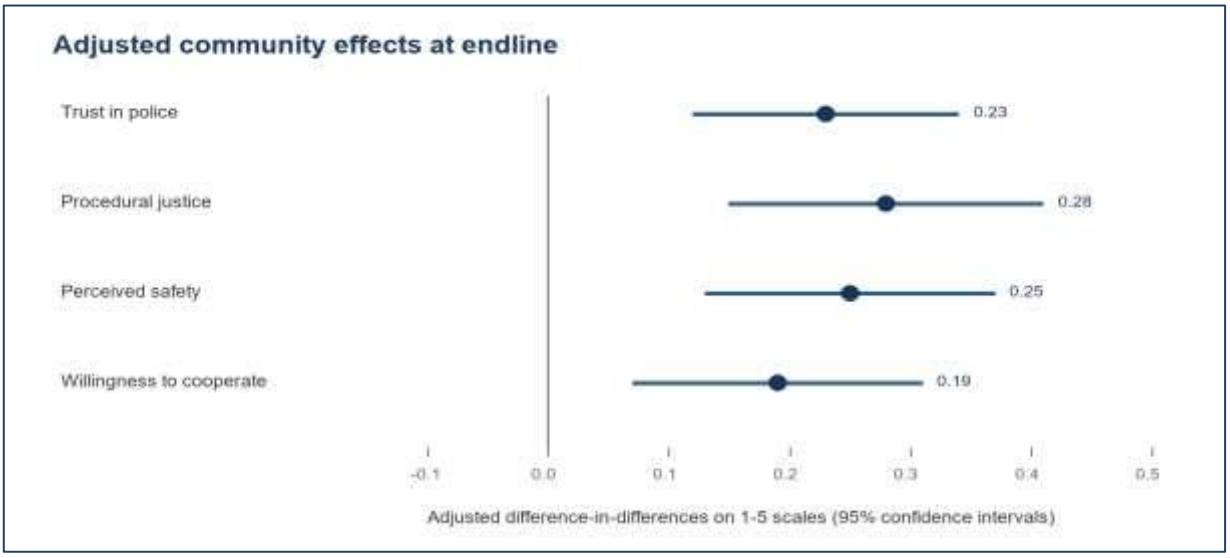


Table 4. Weighted community survey outcomes

Outcome (1-5)	Priority baseline	Priority endline	Comparison baseline	Comparison endline	Adjusted DID (95% CI)
Trust in police	3.21 (0.71)	3.50 (0.69)	3.26 (0.70)	3.32 (0.71)	0.23 (0.12-0.34)
Procedural justice	3.18 (0.74)	3.52 (0.70)	3.24 (0.73)	3.30 (0.72)	0.28 (0.15-0.41)
Perceived safety	3.05 (0.77)	3.39 (0.72)	3.12 (0.75)	3.21 (0.74)	0.25 (0.13-0.37)
Willingness to cooperate	3.46 (0.68)	3.70 (0.65)	3.52 (0.67)	3.57 (0.68)	0.19 (0.07-0.31)

Note. Values before the adjusted effect were weighted means (SD). Baseline n = 1,184; endline n = 1,213. DID = difference-in-differences.

3.6 Case quality and strategic-planning performance

Case-package completeness increased from 74.4% to 88.6% in priority barangays and changed from 73.8% to 74.7% in comparison barangays. The adjusted interaction odds ratio was 2.74 (95% CI 1.39-5.41, p = .004). The most frequent pre-period deficiencies had involved inconsistent event times across forms, incomplete inventory documentation, and missing checklist evidence for chain-of-custody transfer. Post-period review reduced all three categories.

The provincial meeting log contained 126 assigned improvement actions. By the next scheduled review, 109 (86.5%) had been completed with evidence, 11 (8.7%) had been rescheduled with a documented reason, and six (4.8%) had remained overdue. Actions related to data correction had the highest timely-completion rate (94.1%); those requiring multiagency referral capacity had the lowest (72.7%). These process results had indicated that the strategic-planning mechanism functioned as a management intervention, not merely as a meeting calendar.

3.7 Robustness and diagnostic analyses

The primary result remained stable across the prespecified checks. A negative-binomial specification produced an IRR of 0.75; an unadjusted aggregated rate-ratio approach produced 0.76; excluding the first three transition months produced 0.71; and adding municipality-specific linear trends produced 0.77. Leave-one-municipality-out IRRs ranged from 0.69 to 0.78. A placebo implementation date placed nine months before the actual launch produced an IRR of 0.98 and p = .81. The wild-cluster bootstrap p value for the primary estimate was .004.

A stricter validation rule that required either field verification or an investigative disposition reduced the number of qualifying reports by 14.6% but produced an IRR of 0.74. A broader rule that retained corroborated reports without a documented disposition produced 0.76. Analyses that removed the population offset and treated each barangay equally produced 0.72. These checks had reduced concern that one municipality, one validation rule, or population weighting had driven the result.

Table 5. Sensitivity analyses for the primary outcome

Specification	IRR	95% CI	p value	Interpretation
Prespecified Poisson FE	0.73	0.61-0.88	.001	Primary
Negative-binomial	0.75	0.62-0.91	.004	Consistent

Specification	IRR	95% CI	p value	Interpretation
Excluded first 3 post months	0.71	0.58-0.87	.001	Consistent
Municipality-specific trends	0.77	0.63-0.94	.010	Attenuated; retained
Stricter validation rule	0.74	0.60-0.91	.005	Consistent
Placebo date (-9 months)	0.98	0.79-1.21	.810	No placebo effect

Note. FE = fixed effects; IRR = incidence-rate ratio; CI = confidence interval.

4. Discussion

4.1 Principal findings

The evaluation found that the provincial package had been associated with a 27% relative reduction in validated drug-activity reports in priority barangays. The estimate survived alternative count specifications, outcome definitions, trend controls, transition-period exclusions, and leave-one-municipality-out analyses. Pre-period event-study coefficients had supported, but could not prove, the parallel-trends condition. The gradual post-period pattern also matched the observed rise in fidelity and had not resembled a one-month administrative break.

The community results strengthened the interpretation. Actionable community information had increased rather than contracted, and household surveys had shown gains in trust, procedural justice, perceived safety, and willingness to cooperate. The package had therefore not produced a simple tradeoff in which enforcement outcomes improved while community relationships deteriorated. This pattern had aligned with research showing that place-focused policing could reduce crime and that procedurally just implementation could improve behavior and community evaluations.[4,7,8]

Case-package completeness had improved substantially, suggesting that strategic planning affected investigative quality as well as field deployment. This result mattered because a strategy that increased activity but weakened documentation, evidence handling, or legal readiness would not have represented sustainable effectiveness. The monthly review had made quality failures visible and had converted them into assigned, trackable improvement actions.

4.2 Interpretation of the enforcement effect

Three mechanisms plausibly explained the decline. First, geographic and temporal concentration had increased the probability that limited police time reached recurring micro-places rather than being spread thinly across the province. Second, structured problem analysis had encouraged tailored responses, including guardianship, environmental change, referral, and community coordination, instead of repeated reliance on the same enforcement tactic. Third, the monthly learning cycle had shortened the interval between an observed failure and an operational correction. These mechanisms corresponded with the research base on targeted, proactive, and problem-oriented policing.[3-6]

The outcome definition had also affected interpretation. Validated reports reflected observed and corroborated drug activity, not the total hidden market. A lower rate could have represented less activity, lower visibility, altered reporting, or some combination. The simultaneous increase in actionable community information and improvement in perceived safety had made a pure reporting-collapse explanation less likely, but it had not eliminated the possibility that enforcement changed where or how activity occurred. The finding was therefore interpreted as a reduction in validated observed activity under the study's measurement system, not as eradication of illegal drugs.

The province's prior drug-cleared status had made this distinction especially important. Drug-cleared certification had indicated compliance with formal parameters at a point in time; it had not guaranteed that risk could never recur. DDB Regulation No. 4 had explicitly emphasized sustaining implementation and building resilient, self-policing communities.[12] The study's monitoring logic had treated maintenance as an active process of early detection, proportionate response, and partnership.

4.3 Community impact and police legitimacy

The community findings indicated that implementation style had been part of the intervention rather than an ethical add-on. Procedural-justice briefings, supervisory review, explanation fields in activity logs, community dialogue, and complaint monitoring had created observable expectations for police conduct. International evidence had suggested that voice, neutrality, respect, and trustworthy motives could improve citizen responses and voluntary cooperation.[7,8] The study extended that logic to a provincial drug-enforcement setting where intelligence often depended on residents' willingness to share information.

Trust had risen by 0.23 points on a five-point scale after common change had been controlled. The effect was meaningful but not transformative. Endline trust in priority barangays remained 3.50 rather than approaching the scale maximum. This result had cautioned against interpreting improved averages as universal approval. Residents who had experienced coercive action, feared retaliation, or held prior negative views could have responded differently from the average respondent. Continued disaggregation and qualitative follow-up had therefore remained necessary.

The complaint analysis had required equally cautious interpretation. No statistically detectable increase had appeared, but only 31 substantiated complaints were observed across groups and periods. The wide confidence interval included both a sizable reduction and a sizable increase. Complaint systems could also have undercounted harm when residents lacked access, confidence, or protection. Complaints had therefore functioned as one safeguard indicator alongside survey measures, supervision records, and encounter review, not as a complete measure of rights protection.

4.4 Strategic-planning implications

The results supported a balanced provincial scorecard. The most useful review had separated five questions: What harm had changed? Where and for whom had it changed? What police and partner actions had been delivered as intended? Had case and data quality improved? Had community confidence or rights-related risk changed? This structure had prevented operational volume from substituting for public value.

The planning cycle had also shown the value of a decision log. Every action had required an owner, due date, evidence source, and next review. This feature had turned strategy into a series of falsifiable commitments. The PNP's broader planning guidance had emphasized feedback, accountability, proactive policing, and results-based monitoring; the provincial process had translated those principles into monthly operating routines.[18,19]

Resource allocation had been most defensible when it combined need and feasibility. Pure risk ranking could have concentrated resources repeatedly in the same communities, increased stigma, and amplified measurement feedback because police-generated data rose where police looked. The study had mitigated that risk through fixed review periods, community-originated information, population-based rates, transparent validation rules, and safeguards. A future allocation process was advised to retain those checks and to add explicit equity review for geographic, socioeconomic, gender, age, and Indigenous-community impacts.

4.5 Operational recommendations

The findings supported continuation of the package with four conditions. The province was advised to preserve the 18-month evaluation archive and reproduce the analysis from authorized source records before any formal claim of effect. It was advised to retain the balanced scorecard and prohibit performance rewards based solely on arrests, operations, seizures, or names added to intelligence lists. It was advised to institutionalize quarterly community feedback that included confidential channels outside the police hierarchy. Finally, it was advised to require periodic legal, privacy, and human-rights review of targeting criteria, field tactics, data retention, and referral boundaries.

Operational review was advised to distinguish three decision thresholds. A monitoring threshold would trigger additional verification without compulsory action. A problem-solving threshold would authorize a documented multiagency response plan. An enforcement threshold would require the legal basis, reliability, proportionality, and supervisory approvals applicable to the action. This separation would reduce the risk that weak information moved directly from an analytic dashboard to coercive intervention.

Capacity building was advised to focus less on one-time seminars and more on coached practice. Supervisors were advised to review a small sample of problem analyses, encounter narratives, case packages, and referral records every month and to return specific feedback. Analysts were advised to document data lineage, duplicate resolution, validation status, and revision history. Community partners were advised to receive aggregate results in plain language, including unfavorable or uncertain findings.

4.6 Strengths

The study had several strengths. The matched panel included all six municipalities and a complete 36-month observation window. The primary estimator had respected the count nature of the outcome, incorporated exposure, and controlled barangay and calendar-month effects. Inference had been clustered at the unit of repeated observation. Event-study, placebo, and sensitivity analyses had examined key identifying assumptions. The evaluation had also triangulated police records, case audits, implementation logs, community-sourced information, and independent household surveys.

A further strength had been the separation of impact from activity. Arrests and operations had not served as the primary outcome. The analysis had incorporated community trust and procedural justice, legal-readiness of case packages, complaint rates, and implementation fidelity. This architecture had matched the balanced policy direction in Philippine law and drug strategy, which combined enforcement with prevention, health, rehabilitation, reintegration, and local participation.[10-13]

4.7 Limitations

The study had not randomly assigned barangays. Matching and fixed effects had improved comparability, and pre-trend tests had found no contradiction, but unmeasured time-varying differences could still have biased the estimates. Priority barangays had been selected partly because of operational concern and readiness, so regression to the mean or differential partner engagement could have contributed to improvement. The design supported a carefully qualified association under the parallel-trends condition rather than an unconditional causal claim.

The primary outcome had depended on police and community detection. Even with a validation rule, the count could have changed when reporting, verification effort, recording discipline, or offender adaptation changed. The study had addressed this limitation through actionable-tip, survey, case-quality, and sensitivity analyses, but no available measure directly observed the full illicit market. Seizure weight had not been used as a primary outcome because it could be dominated by a few events and strongly reflected enforcement opportunity.

The community surveys had been repeated cross-sections rather than a respondent panel. Weighting had improved population representation, yet nonresponse and social-desirability bias could have remained. Residents might have hesitated to criticize police or discuss drugs despite the separation of enumerators from police personnel. The scales measured perceptions at two time points and could not identify short-lived changes between waves.

The number of substantiated complaints had been small, leaving low power for the safeguarding outcome. Case-package completeness had measured documentation readiness, not conviction, justice, or long-term community benefit. Spillover across adjacent barangays could have reduced the observed contrast if benefits diffused to comparison areas, while displacement beyond measured locations could have exaggerated it. The study had not possessed sufficiently precise geocoded market indicators to test either mechanism comprehensively.

External validity had been limited by the province's small population, rural geography, six-municipality structure, interagency relationships, and prior drug-cleared status. The data-status condition described in the dedicated disclosure further required independent reconstruction before the numerical findings could be treated as empirical evidence. The manuscript was therefore most defensible as a fully specified evaluation template and not as an official performance account.

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