Establishing the Rule of Law in Weak and War-Torn States: Evidence from a Field Experiment with the Liberian National Police

Online Appendix

A.1 MAP OF LIBERIA AND SAMPLE COMMUNITIES

Figure A.1 displays a map of Liberia's 15 counties (top left panel) alongside the distribution of treatment and control communities in Lofa (top right), Nimba (bottom left), and Bong (bottom right). Closed circles denote treatment communities; open circles denote control. Eligibility was limited to communities with at least 500 residents located near a usable road.

A.2 IMPLEMENTATION TIMELINE

Figure A.2 displays our implementation timeline. Almost 90% of treatment communities were patrolled four times between July 2014 and September 2015.

A.3 SAMPLE SELECTION

Endline survey respondents were sampled using the random walk method. Enumerators began by identifying all the neighborhoods, or "quarters," within each community with the assistance of a local leader. They then selected four quarters at random. Working with the local leader, they next identified the most central location within each quarter, typically the spot from which all paths feeding the rest of the quarter originate. Enumerators randomly selected one path and walked the length of it, selecting every fifth household. Finally, they created a roster of all adults living in the household, from which they selected one respondent at random.

A.4 MEASUREMENT

We use our survey to measure most of the outcomes in our analysis. To measure **knowledge of Liberian law**, we asked respondents about their and the government's legal obligations in each of seven hypothetical scenarios.[42]) To measure **knowledge of the police**, we asked respondents

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[42]For example: "If a rogue commits a crime in your community, Liberian law says the Community Watch Forum is allowed to beat the person so they cannot escape before the police arrive. True or false?" (False.) Or: "If the police

whether they knew the location of the nearest police station, whether they knew the phone number of any police officer, whether they had heard about the Gbarnga Hub, whether they knew where it was located, and whether they could name its functions.

We measured the **incidence of crime** using a modified version of the U.S. National Crime Victimization Survey. We focused on five categories of crime in particular: armed robbery, theft and burglary,[43] simple assault, aggravated assault,[44] and domestic violence.[45] We asked respondents whether they were victims of any of these crimes in the past twelve months, and—because these are rare events—whether they had witnessed or heard about any similar incidents in their communities in the past 12 months. (For crimes in the community, we added rape as well.)

For each affirmative answer we also asked to which authority, if any, the case was reported, and whether or not the respondent was satisfied with the way that authority handled the case. We use respondents' answers to these latter questions to measure **reporting to the statutory sector**, as well as **reporting to the customary sector**. Again, because these are rare events, we also posed six hypothetical scenarios of crime and violence, ranging in severity from burglary to murder, and asked respondents to which authority, if any, they would prefer to report the case.

To measure **perceptions of the police**, **perceptions of the courts**, and **perceptions of the government**, we asked respondents to describe both their general impressions of these institutions (e.g. whether they believed the government is biased against particular ethnic or religious groups) and their more specific assessments of the LNP (e.g. whether they believed they would have to pay a fee for the LNP to investigate a crime, or whether they thought suspects were likely to be verbally or physically abused while in police custody).

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put someone in jail and no one comes to carry a case against that person, Liberian law says the police got to let him go free. True or false?" (True.)

[43]Theft and burglary are technically distinct categories of crime in Liberia. The latter is a felony, the former a misdemeanor. The legal distinction between the two is ambiguous in Liberia's penal codes, however, and we believe most respondents who reported thefts or burglaries were likely referring to misdemeanors (e.g. pick-pocketing, or theft of livestock).

[44]Under Liberia's penal codes, simple assault involves causing bodily injury without the use of a weapon, and is a misdemeanor. Aggravated assault involves causing bodily injury *with* a weapon, and is a felony.

[45]Domestic violence does not actually appear in Liberia's penal codes; a bill to include it has been stalled for years in the Liberian legislature over a controversy surrounding penalties for female genital mutilation. To the best of our knowledge, when domestic violence is prosecuted at all, it is likely to be prosecuted as a misdemeanor akin to simple assault.

We measured **support for trial by ordeal** using three hypothetical scenarios in which the practice is especially likely to be used—an unsolved burglary, a missing person, and a mysterious death (Blair 2018*a*). For each scenario, we asked respondents whether their community would be likely to use trial by ordeal, and whether they personally would support its use. Finally, we measured **security of property rights** by asking respondents whether they were worried about encroachment on their "house spot" or farmland; whether they had made major improvements to their house spot or farmland in the last year, or planned to do so in the coming year; whether they left their farmland fallow in the past year, or planned to do so in the coming year; and whether they were involved in a dispute over their house spot or farmland in the past year.

A.5   DESCRIPTIVE STATISTICS

Table A.1 summarizes the component dependent variables for each of our outcome clusters across both treatment and control communities. 90% of respondents knew the location of the nearest police station and 12% knew the phone number of a police officer, though a greater proportion could likely access this information in the event of a crime. In contrast, only 16% of respondents knew about the Hub, despite efforts by the government to raise awareness through radio, media, and the Confidence Patrols program.

Knowledge of Liberian law varied by question. Respondents were almost unanimous in their understanding that Liberian law does not allow citizens to beat perpetrators, but were more divided on whether or not it requires the LNP to investigate witnesses as suspects (it does not). Nearly one-quarter of respondents (23%) believed trial by ordeal is legal (it is not), and over one-third (37%) believed they have no recourse to the courts if they suspect the town chief of wrongdoing (they do). In contrast, 83% of respondents knew they have a legal right to habeas corpus if they are suspects in a criminal case.

Perceptions of the police were mixed, but generally unfavorable. Half of all respondents believed they would have to pay for the police to investigate a crime, and only 26% believed the

police would take their case seriously. More than half (56%) described the LNP as corrupt, though a large majority (80%) believed the LNP treats all tribes equally, and only a small minority believed the LNP would verbally or physically abuse them if they were ever held in police custody (9% and 10%, respectively). Perceptions of the government were similarly mixed, with 54% describing the government as corrupt and only 40% describing the government's decisions as transparent, but 90% agreeing that the government treats all tribes the same.

Respondents preferred to rely on the police for most hypothetical scenarios of crime and violence. Preferences for the police were strongest for armed robbery (67%) and murder (69%), and weakest for domestic violence (20%). Support for trial by ordeal varied between 18-26%, depending on the question. Whether respondents referred actual cases to the police also varied with the severity of the crime. But reporting rates were generally low, even for severe crimes involving bodily harm (e.g. aggravated assault).

17% of respondents reported being a victim of at least one crime in the past year. (3% were a victim of two crimes and just under 1% were victims of three.) Burglaries were most common (16% of respondents), followed by simple assault (3%), aggravated assault (2%), and armed robbery (2%). Unsurprisingly, reports of witnessing or hearing about crimes committed against other community members were generally more common, with 30% of respondents reporting at least one crime in the community in the past year. Although respondents reported low rates of domestic violence in their own households, they reported much higher rates (36%) in the community as a whole. Self-reports of domestic violence may be low due to fears of reprisal, but the high prevalence of community reporting confirms that this remains a pervasive problem in Liberia. Reports of burglary were common as well (26%); reports of assault were less so (6%).

Finally, although most respondents felt secure about their house spots and farmland (80% in both cases), disputes were not uncommon. In the past year alone, 5% of respondents reported a dispute over their house spot and 11% reported a dispute over farmland. Moreover, 45% of disputes entailed violence, and 12% resulted in destruction of property.

A.6 CORRELATION MATRIX FOR DEPENDENT VARIABLES

To illustrate how our outcomes relate to one another, we estimate the pairwise correlation between composite indices constructed for each of our outcome clusters. We construct each composite index by taking the average of the component variables within each cluster, then rescaling so the index has a mean of zero and a standard deviation of one. The results are displayed in Table A.2.

A.7 BALANCE TESTS

Table A.3 reports balance tests across a range of community-level variables gleaned from the 2008 census. We find no evidence of imbalance on any of these variables.

A.8 RESULTS WITH MULTIPLE COMPARISONS ADJUSTMENTS

Tables A.4 and A.5 report the results from Figures 1 and 2 in the paper, respectively, including p-values adjusted for multiple comparisons. We use the Average Effect Size estimator to control Type II errors within clusters of outcomes, and therefore only adjust p-values across clusters. As we discuss in the paper, it is not obvious that such an adjustment is necessary in this case: adjusting across clusters only makes sense if we wish to assess whether the Confidence Patrols program had any effect on any outcome at all, which is not a hypothesis we intended to test. Nonetheless, for completeness we report Holm and Benjamini and Hochberg corrections for the 10 outcome clusters in Figure 1, and also for the six outcomes in Figure 2. After adjusting our p-values, the positive AES on knowledge of Liberian law is no longer statistically significant at conventional levels (though it is just shy of statistical significance at the 10% level using the Benjamini and Hochberg correction). Our other results are unchanged.

A.9   RESULTS WITH AND WITHOUT CONTROLS

Table A.6 presents the AES for each of our outcome clusters, estimated with and without controls. As expected, effects vary little across specifications.

A.10   AVERAGE TREATMENT EFFECTS ON COMPONENT DEPENDENT VARIABLES

Figures A.3 and A.4 report the Average Effect Size (AES) for each cluster of outcomes alongside the average treatment effect (ATE) on each component dependent variable within each cluster. The AES is interpreted in terms of standard deviations from the control group mean; because all of our component dependent variables are binary, the ATEs are interpreted in terms of percentage point differences between the treatment and control groups. (In other words, while we plot both the AES and the ATEs on the same figure for compactness, their magnitudes cannot be directly compared.)

A.11   EFFECTS ON CRIME REPORTING USING LNP DATA

In the paper we estimate differences in crime reporting between the treatment and control groups using our survey-based measures. Here we replicate that analysis using LNP crime records instead. As discussed in the paper, because only a relatively small proportion of crimes are ever reported to the LNP, we believe these records are more appropriately interpreted as measures of crime reporting, rather than of crime per se. Ultimately, however, we cannot distinguish between these two outcomes using LNP data alone. It is because of this ambiguity that we choose to focus on survey-based measures in the paper.

When assessing the program's impact on crime reporting as measured by the LNP, we use a difference-in-differences estimator to control for any pre-treatment differences in reporting, given by

$$y_{vsm} = \alpha + \beta_1 T_{vsm} + \beta_2 post_{sm} + \beta_3 T_{vsm} \times post_{sm} + \gamma_s + \boldsymbol{X}_{vs}\theta + e_{vsm}$$

where y_{civs} indicates the total number of crimes reported from community v of block s in month m. T_{vs} again denotes community-level treatment assignment, $post_{ms}$ denotes the number of months since the first patrol in block m, $\boldsymbol{X}_{vsm}$ denotes our community-level controls, and γ_s denotes block fixed effects. Because our outcomes are counts, and to adjust for potential over-dispersion, we use negative binomial models (NB2) for this analysis. Standard errors are again clustered at the community level.

Figure A.5 displays our results. We report point estimates and 95% confidence intervals for the difference in the number of reported crimes between treatment and control communities in every month over a two year period, starting 8 months prior to program implementation. We observe some imbalance in reporting prior to implementation, with treatment communities statistically significantly less likely to report crimes to the police, though the imbalance disappears in the months immediately before the program began, suggesting that pre-treatment imbalance was likely incidental, especially since treatment and control communities were balanced on other observable characteristics. While the difference between treatment and control communities becomes more positive near the end of the panel (after all patrols were complete), it is statistically indistinguishable from zero in all but one month. These nulls may in fact be consistent with our survey-based results: if the program reduced crime but increased crime *reporting*, then these two effects may offset one another in the LNP data, resulting in a net null. Again, given this ambiguity, we interpret these results with caution.

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A.12 CONDITIONING ON CRIME OCCURRENCE WHEN ESTI-

MATING DIFFERENCES IN CRIME REPORTING

In our pre-analysis plan we hypothesized that the Confidence Patrols program would increase willingness to report crimes to the statutory sector. We test this hypothesis by estimating

$$\mathbb{E}[Y_i|C_i = 1, T_i = 1] - \mathbb{E}[Y_i|C_i = 1, T_i = 0]$$

where Y_i denotes whether crime i was reported to the statutory sector; C_i denotes whether the crime occurred in the first place; and T_i denotes the treatment status of the respondent reporting crime i. This analysis requires conditioning on a post-treatment variable, since the program may have affected both C_i and Y_i, and since Y_i is only identified if $C_i = 1$. In this section we explore the bias that this approach may induce, and consider an alternative specification that is less informative but also less susceptible to bias. Our conclusions remain unchanged regardless.

Letting $Y_i(1)$ denote potential reporting for crime i under treatment and $Y_i(0)$ denote potential reporting under control, and defining $C_i(1)$ and $C_i(0)$ analogously, we have

$$\mathbb{E}[Y_i|C_i = 1, T_i = 1] - \mathbb{E}[Y_i|C_i = 1, T_i = 0]$$
$$= \mathbb{E}[Y_i(1)|C_i(1) = 1, T_i = 1] - \mathbb{E}[Y_i(0)|C_i(0) = 1, T_i = 0]$$

which, by virtue of the fact that T_i is independent of $\{Y_i(1), Y_i(0), C_i(1), C_i(0)\}$, is equal to:

$$\mathbb{E}[Y_i(1)|C_i(1) = 1] - \mathbb{E}[Y_i(0)|C_i(0) = 1]$$

Subtracting and then adding $\mathbb{E}[Y_i(0)|C_i(1) = 1]$ to this expression illustrates the potential for bias:

$$\mathbb{E}[Y_i(1)|C_i(1) = 1] - \mathbb{E}[Y_i(0)|C_i(0) = 1]$$

$$= \mathbb{E}[Y_i(1)|C_i(1) = 1] - \mathbb{E}[Y_i(0)|C_i(1) = 1] + \mathbb{E}[Y_i(0)|C_i(1) = 1] - \mathbb{E}[Y_i(0)|C_i(0) = 1]$$

$$= \underbrace{\mathbb{E}[Y_i(1) - Y_i(0)|C_i(1) = 1]}_{\text{Causal effect on crime reporting}} + \underbrace{\mathbb{E}[Y_i(0)|C_i(1) = 1] - \mathbb{E}[Y_i(0)|C_i(0) = 1]}_{\text{Bias}}$$

The difference in crime reporting between treatment and control communities is thus equal to the causal effect of the program on crime reporting plus a bias term.

This bias will be zero whenever $\mathbb{E}[Y_i(0)|C_i(1) = 1] = \mathbb{E}[Y_i(0)|C_i(0) = 1]$. While it is not possible to verify this assumption empirically, we can test whether $\mathbb{E}[X_i|C_i(1) = 1] = \mathbb{E}[X_i|C_i(0) = 1]$—that is, whether crimes in treatment and control communities are similar to one another along observable characteristics, such as the age, ethnicity, level of education, and gender of the victim, and the distance of the victim to the nearest police station. If crimes in treatment and control communities are similar along these dimensions, then it is likely they are also comparable in terms of their potential reporting outcomes. Table A.7 shows that crimes in treatment and control communities are indeed similar, lending some credence to the assumption that $\mathbb{E}[Y_i(0)|C_i(1) = 1] = \mathbb{E}[Y_i(0)|C_i(0) = 1]$.

As an additional robustness check, we rerun our analysis using an approach that avoids conditioning on crime occurrence. This approach is drawn from the pre-analysis plan for the Evidence in Governance and Politics (EGAP) network's Metaketa IV initiative.[46] For each incident of crime, we construct three variables, each defined for the entire sample: an indicator for whether the crime occurred, an indicator for whether the crime occurred and was reported to a formal authority (with zero indicating that the crime either did not occur, occurred and was reported to an informal authority, or occurred and was not reported at all), and an indicator for whether the crime occurred and was *not* reported to a formal authority (with zero indicating either that the crime did not occur, or occurred and was reported to a formal authority). We then calculate the Average

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[46]See http://egap.org/registration/5154.

Effect Size across each of the three sets of variables, following the procedure outlined in the paper. This approach is not perfect, because individually these effects are of ambiguous interpretation— e.g. a decrease in the the number of crimes that occurred and were reported could indicate fewer crimes, fewer reported crimes, or a combination of both—but when analyzed together, they are potentially informative.

Table A.8 reports our results. We find that the program had a negative but not statistically significant effect on the likelihood that a crime occurred; a positive but not statistically significant effect on the likelihood that a crime occurred and was reported; and a negative and not statistically significant effect on the likelihood that a crime occurred and was not reported. Focusing on misdemeanors alone, we find that the program had a negative and statistically significant effect on the likelihood that a crime occurred; a negative but not statistically significant effect on the likelihood that a crime occurred and was reported; and a negative and not statistically significant effect on the likelihood that a crime occurred and was not reported. Focusing on felonies instead, we find that the program had a positive but not statistically significant effect on the likelihood that a crime occurred; a positive and statistically significant effect on the likelihood that a crime occurred and was reported; and a negative but not statistically significant effect on the likelihood that a crime occurred and was not reported. While these results are difficult to interpret in isolation, together they are consistent with the conclusion we draw in the paper: the Confidence Patrols program increased crime reporting, and it did so primarily by reducing the number of crimes that were not reported at all.

A.13   HETEROGENEITY IN CRIME REPORTING

Tables A.9, A.10, and A.11 report heterogeneity in our survey-based measure of crime reporting by gender, ethnicity, and age, respectively, comparing treatment and control communities. In general we do not find any evidence of heterogeneity along these dimensions.

A.14   ADDITIONAL PRE-SPECIFIED HETEROGENEOUS TREAT-
MENT EFFECTS ANALYSES

In our pre-analysis plan we hypothesized that the Confidence Patrols program would boost tax morale and reduce the incidence of Ebola. We excluded the former analysis from the paper because tax compliance proved to be unrelated to the themes of the program, and we excluded the latter because only three patrols were actually conducted before or during the Ebola epidemic. For completeness, we report results for these hypotheses in Table A.12. We find no evidence that the program improved tax morale. Treatment communities did report fewer cases of Ebola relative to control, but this difference is imprecisely estimated and not statistically significant at conventional levels.

Our pre-analysis plan also specified that we would test for heterogeneous treatment effects along the following dimensions that were not included in the paper:

1. Index of exposure to government violence during the war

2. Index of exposure to rebel violence during the war

3. Whether the respondent is a leader in the community

4. Whether the community has a police depot (a small outpost usually designed to house 1-2 rank-and-file LNP officers)

5. Total number of patrols

6. Number of months since the last patrol

We do not have sufficient statistical power to conduct this last analysis due to a lack of variation: each treatment community was last patrolled in either August or September 2015, two and three months before the start of our endline, respectively. Table A.13 reports results for the remaining analyses. In general we do not find any evidence of heterogeneity along these dimensions.

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Finally, our pre-analysis plan specified that we would conduct a bounding exercise in which we estimate the potential range of treatment effects on individuals who reside in treatment communities but do not report exposure to the Confidence Patrols program. We hoped that this exercise would help us estimate the proportion of the program's impact that is likely a result of spillover from treated to untreated residents. Unfortunately, this analysis proved unfeasible. The program comprised multiple components—town hall meetings, foot patrols, soccer games, etc.—and different residents were treated in different combinations of ways, complicating the bounding exercise. Moreover, the strong exogeneity assumptions required for the exercise likely vary across treated residents, and are probably not plausible. For these reasons we exclude this analysis altogether.

Table A.14 describes these and all other deviations from our pre-analysis plan.

A.15   Additional themes from qualitative field reports

In the paper we use qualitative field reports from our Liberian research assistant to help us inform and substantiate our quantitative results. Here we use excerpts from the field reports to illustrate additional recurring themes from the town hall meetings and Q&A.

A.15.1   Increasing trust in the police

One of the primary purposes of the Confidence Patrols program was to increase citizens' trust in the police and assuage their fears of increased police presence in and around their communities. The officers typically began their presentation by acknowledging the troubled and often violent history of policing in Liberia, especially during the civil war. They (rather inaccurately) described the years preceding the conflict as a period of trust and cooperation between civilians and the LNP. A town hall meeting in Yila in February of 2015 was typical:

[The officer] explained that before the civil war in Liberia, the citizens and the police

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had a cordial relationship and were friendly. The citizens were not afraid of the officers. They used to report crimes and help the officers fight against crimes. The citizens had trust and confidence in the officers. The citizens used to point out criminals within their communities. But when the war came, the citizens lost trust and confidence in the officers due to their actions and behaviors. Instead of the officers protecting the lives and properties of the citizens, they were seen ill-treating the citizens and making them fearful. As a result of these actions and behaviors, the citizens had fear in the officers, and when they see the officers coming, they run away.[47]

The officer then described security sector reforms implemented since the end of the conflict in 2003, stressing the difference between the "old LNP" and the newly-reformed PSU. Presentations often focused in particular on the role that UNMIL played in retraining the LNP: "UNMIL transformed them that they may be friends of the citizens and to work in partnership with the citizens, so as to regain the lost trust and confidence."[48]

The officers' presentations often involved direct and personal appeals to attendees. In his opening remarks to residents in Bunadin, Nimba County, for example, one of the patrolling officers explained that he and his colleagues "had come to extend an arm of friendship so that you may have trust and confidence in us and work together with us as UNMIL plans to leave."[49] The officers framed the Confidence Patrols program itself as a mechanism to "build a harmonious working relationship" with civilians, emphasizing the importance of cooperation to the LNP's effectiveness: "Without the citizens' help, the officers cannot function properly."[50]

The officers also allowed citizens to air grievances against the LNP and PSU,[51] and to provide the officers with feedback, including on their own performance during the patrols.[52] Typical of community policing, they addressed a wide variety of questions and concerns during the Q&A,

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[47]Yila Town, 2/4/15.
[48]Yila Town, 2/4/15
[49]Bunadin 4/5/2015.
[50]Yila Town, 2/4/15.
[51]Zolowee 4/6/2015; Jinnepeleta 8/20/2015
[52]Kpayaquelleh 2/19/2015; Gbenequelle 3/3/2015.

many of them unrelated to policing per se. For example, patrolling officers offered advice to a fa-
ther whose two young daughters were "stubborn and causing [him] many problems;"[53] to a mother
whose daughter was "in love with a man [she didn't] like," and who had developed the habit of
"[leaving] the house for days in order to spend time with [him];"[54] to a husband whose wife "can't
respect him, and has the constant habit of refusing him in bed;"[55] and to a wife whose husband
"does not want to support the children and [her]."[56] The officers offered advice in all of these cases,
often stepping out of their role as police officers to do so.[57]

Finally, and more controversially, the officers attempted to re-frame grievances against the
LNP in ways that might elicit sympathy for the challenges facing an underfunded and understaffed
police force. Perhaps the most common of these grievances, both in our sample and in Liberia as
a whole, related to the fees that police officers routinely charge to investigate crimes. The PSU
attempted to re-frame these not as bribes, but rather as fees-for-service. While the officers clarified
that civilians are not legally required to pay the LNP to investigate crimes—a point on which there
was much confusion, given the ubiquity of the practice[58]—they also warned that some fees may
simply be unavoidable if the police are to do their jobs properly.

More pointedly, they explained that because "the government is not supplying us regularly
and timely," most LNP depots "have some constraints with regards to fuel and stationery."[59] More-
over, while "it is the police bind duty to respond when called upon," in most places LNP officers
"don't have vehicle and bike to quickly respond to situations," which is causing them to "work

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[53]Zowienta 4/21/2015.

[54]Tassah 2/6/2015.

[55]Salayea 8/8/2015; see also Tukpah 3/4/2015; Kpaiyea 8/8/2015.

[56]Tassah 2/6/2015.

[57]For example, to the mother whose daughter had fallen in love with a man she didn't like, the officer offered the
following advice: "The first thing is that your daughter is not a child anymore. She is above 18 years. She has reached
a stage under the law that she can make her own decisions. You cannot decide for her what she really wants in life.
What you need to do is to invite the boyfriend or the man and get to know him better. Chat with him and ask him if
he truly loves your daughter. Ask him about his parents, education, and what he does for living. Tell him that you are
really interested in your daughter's education and need his support in that direction. If you try to impose your will on
your daughter, she will make you shame. She might even end up bad. So, take time as to how you go about it. Girls at
that age are difficult to deal with" (Tassah 2/6/2015).

[58]Yila Town 2/4/2015; Galai Town 2/5/2015; Kollie-Ta 2/6/2015.

[59]Galai Town 2/5/2015.

ineffectively."[60] The PSU thus explained that "if you want the police to go along with you to put your situation under control or carry on arrest, and [the officer] is not mobile, you can facilitate his movement in order to perform his duty."[61] If citizens "have problems and want the police to help them out," they should "improvise" now and seek reimbursement later: "If the case goes to court, and the complainer was right, his expenses shall be paid."[62]

Another common grievance related to the apparently premature release of suspects without trial or bail—a practice that many citizens interpret as evidence of collusion between criminals and the LNP. Complaints by residents of Turkpah and Dean Town are illustrative:

> "Sometimes when we arrest a thief in our town for stealing our cattle and turn him over to the police, we see the criminal back in the community after two or three days boasting that we can't do anything to him. Are the police helping to fight crime or helping to promote crime?"[63]

> "We have serious problems with some people who are doing drugs business in this town. Some hardcore youths that are involved in taking in these drugs are also causing problems for us. On numerous occasions, we have reported the case to the police at Botota. The police will come and arrest the people and carry them. After two days, we see these people back in the town doing the same business. How can we solve this problem?"[64]

In some cases, suspects are prematurely in exchange for bribes. But as the PSU officers repeatedly explained, in many other cases, premature release results from a misunderstanding of the habeas corpus provisions of Liberian law, which require that detainees be released after 48 hours if no formal complaint is registered against them.

The officers were sometimes defensive on this point: "We are faced with these kinds of problems every day, and you people shift the blame on the police. The problem is not with the

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[60]Zowienta 4/21/2015.
[61]Dean Town 2/5/2015.
[62]Zowienta 4/21/2015.
[63]Turkpah Town 2/7/2015
[64]Dean Town 2/5/2015.

police. The police can only keep a perpetrator in jail for 48 hours. If the complainer does not follow up, the police will definitely release the inmate."[65] On at least one occasion the officers blamed this situation on "the human rights people," who "are checking on every inmate at the police station, finding out when and why they were brought to the station."[66] Whatever the source of the misunderstanding, the officers urged victims not to assume that "when the police make the arrest and put the person in jail, [the complainant's] problems have been solved."[67]

A.15.2   ENCOURAGING CRIME REPORTING

These messages were part of a more general effort to encourage citizens to report crimes to the police, despite disappointing experiences in the past. In addition to simply instructing citizens to report, the officers attempted to clarify the categories of crime that must and may not be reported to the police under Liberian law. Equally important, the officers suggested strategies for reporting crime without risking social sanction by other members of the community, sometimes using elaborate hypothetical scenarios to elicit additional ideas from citizens.[68]

Concerns about social sanctions were especially acute for cases of domestic abuse and sexual and gender-based violence (SGBV). The following two exchanges illustrate:

Question: "Let say a man rapes a child. The family and the community leaders decide to solve it at the community or family level. Is it right for me to report the case to the police if I see this?"

Answer: "Yes, you can report this case. But it should be done secretly without the family and community leaders knowing about it. You can find a private place to call on the police and then give the information. You can even tell the police that the information is confidential and you don't want to be known. But, if you openly tell the

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[65]Turkpah Town 2/7/2015.
[66]Gbarlorkpala 3/3/2015.
[67]Turkpah Town 2/7/2015.
[68]Gbanway 4/11/2015.

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family and the community leaders that you are against it and you will report the case

to the police, you might face some problems with them in the future."[69]

And:

> [Citizen] asked if a female child is raped and both families want to settle it amongst
> themselves, how can she report this case without being blamed? The commander an-
> swered and said that she could take the contact numbers given to them and call to
> inform the police secretly. The police will work on that information and have those
> people arrested, investigated, charged, detained and sent to court for prosecution. Rape
> is a non-bailable crime that must not be compromised or settled at a family or com-
> munity level.[70]

Advice of this sort may have encouraged citizens to report despite fears of ostracism, and may have

indirectly reduced the incidence of domestic abuse and SGBV by leading potential perpetrators to

believe they would be reported and arrested.

The officers also emphasized that if citizens were dissatisfied with the state's response to

a case, they would have recourse to the appellate process, itself made more accessible by the

proximity of the Hub. For example, when a resident of Turkpah asked what he should do if he

suspects a magistrate court is "playing with [his] case," one of the officers responded that "you can

take your case to the Hub and the Hub will provide you justice. That is the sole purpose of the

Hub. It is built to bring justice and security close to you."[71] In response to a similar question in Doe

Town, the officers responded that "the magistrate or police is not above the law," that complaints of

malfeasance would be "investigated in accordance with the law," and that the Hub would "provide

you justice despite of who you are and what you have."[72] (Of course, these promises may have

also raised citizens' expectations of the police and courts to unreasonable levels.)

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[69] Wainsue 2/6/2015.

[70] Flumpa 8/1/2015.

[71] Turkpah 2/7/2015.

[72] Doe Town 3/4/2015.

A.15.3   DISCOURAGING RELIANCE ON EXTRAJUDICIAL PUNISHMENT

At the same time they encouraged reporting to the police, the patrolling officers explicitly discouraged the use of illegal or extrajudicial mechanisms of adjudication and dispute resolution, especially vigilantism, mob justice, and trial by ordeal. They urged citizens not to harm or "mock" those suspected of petty crimes,[73] nor to torture, kill, or unlawfully detain those suspected of more serious crimes, lest they themselves be charged with assault.[74] On several occasions they warned residents against resorting to mob violence to protest the actions of companies operating in and around their communities, urging them to "exercise patience," to avoid "taking the law into their hands," and to "learn to channel their grievances through their senators, superintendent, and representatives."[75]

A.15.4   ENCOURAGING SUPPORT FOR COMMUNITY WATCH FORUMS

As an alternative to extrajudicial punishment, the officers encouraged citizens to organize Community Watch Forums. In places where a Community Watch Forum already existed, they encouraged citizens to provide its members with food and supplies. They described these groups as "the eyes of the police in the town,"[76] responsible for being "vigilant of incoming criminals" and keeping "watch over the town to protect and keep the town safe."[77] They also emphasized that members of Community Watch Forums are volunteers: they are doing "voluntary service, and the community should assist them."[78] The officers also discouraged members of Community Watch Forums from engaging in vigilantism themselves.[79]

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[73]Gbahn 8/2/2015.
[74]Tassah 2/6/15.
[75]Neignbain 2/15/2015.
[76]Gbarlorkpala 3/3/2015.
[77]Gbahn 8/2/2015.
[78]Loyee 8/1/2015.
[79]Gbahn 8/2/2015.

A.15.5   INCREASING KNOWLEDGE OF LIBERIAN LAW

The line between vigilantism and self-defense is ambiguous and poorly understood in most Liberian communities, and much of the Q&A was spent answering basic factual questions about Liberian law. In one community, for example, a farmer asked "if someone is in the constant habit of stealing from my farm, do I have the right to shoot him on sight?" (he does not);[80] in another, a member of the local Community Watch Forum asked whether citizens have the right to tie up suspected criminals with rope while they wait for the police to arrive (they do not; as the patrolling officer explained, "those days of tying people with ropes are over").[81]

Citizens also asked whether the police are required by law to detain witnesses as suspects in criminal cases (they are not);[82] whether victims are legally obliged to pay to transport police investigators to and from the scene of a crime (they are not);[83] whether "there is a penalty for sexual assault or harassment" (there is);[84] and whether rape or other cases of SGBV can legally be settled (or "compromised") outside of court (they cannot).[85]

A.15.6   INCREASING KNOWLEDGE OF THE POLICE

Lessons in Liberian law were accompanied by primers on the roles and responsibilities of the LNP. In many cases citizens were unsure about the types of grievances that do and do not fall under police jurisdiction. In one community, for example, a citizen asked whether "if an individual refuses to do town work and doesn't want to listen to the town chief, should we call the police or PSU?"[86] Another said that he "noticed that [his] wife has a boyfriend," and asked whether he could call the police to "intervene."[87]

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[80]Zowienta 2/4/2015.

[81]Tassah 2/6/2015.

[82]Wainsue 2/6/2015; Gbenequelle 3/3/2015; Konia 4/11/2015.

[83]Yila Town 2/4/2015; Galai Town 2/5/2015; Dean Town 2/5/2015; Kollie-Ta 2/6/2015.

[84]Kpaiyea 8/8/2015.

[85]Wainsue, 2/6/2015 Konia 4/11/2015; Flumpa 8/1/2015.

[86]Yila 3/4/2015.

[87]Yila 3/4/2015.

In other cases citizens asked about the division of labor between the police and courts;[88] between the police and the town chief;[89] or between the police and the Community Watch Forums (which are authorized to assist the police but not to make arrests or adjudicate crimes).[90] And in many communities citizens simply sought generic advice about appropriate procedures for responding to crime: "If someone commits a crime in this town, what should we do?";[91] "If we caught a criminal in our town, what should we do to him?";[92] or "If someone is causing serious problems in the town, and the person does not want to change, what do we need to do?"[93]

A.15.7 REDUCING CRIME

Finally, and most obviously, in addition to encouraging trust in, and cooperation with, the LNP, the Confidence Patrols program was designed to reduce the incidence of crime in treatment communities. Of the various categories of crime that the PSU addressed, the two that received the most attention in the presentations and Q&A—and over which the communities in our sample expressed the most frequent concern—were domestic abuse and SGBV on the one hand, and land disputes on the other.

The officers repeatedly emphasized that domestic abuse is a crime for which perpetrators could expect to be punished, and explicitly discouraged husbands from beating their wives in response to perceived slights.[94] They delivered similar messages about rape and other forms of SGBV, which they repeatedly characterized as a non-bailable offense over which the state claims original jurisdiction, and which therefore cannot be "settled" or "compromised" informally, even if the victim or the victim's family would prefer to do so.[95]

The officers also provided phone numbers for reporting incidents of SGBV, and alerted potential victims to the existence of a dedicated SGBV office at the Regional Justice and Security

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[88]Kpayea 4/11/2015; Dean Town 4/21/2015.

[89]Tukpah 3/4/2015.

[90]Loyee 8/1/2015.

[91]Gbalorkpala 2/5/2015.

[92]Jinnepelata 2/6/2015.

[93]Yila 2/4/2015.

[94]Kpaiyea 8/8/2015.

[95]Konia 4/11/2015; Ganglota 4/13/2015; Flumpa 8/1/2015.

Hub in Gbarnga. On several occasions residents specifically mentioned that domestic abuse and SGBV had declined as a result of the program.[96] As the town chief in Gbenequelle explained, "domestic violence has reduced because of the messages and the contact numbers left with them in case of violence and crimes in the town. So people who are involved in committing crimes, causing trouble and getting involved in violence are now aware of the steps that the town leaders and the residents would take against them."[97]

While the PSU is generally not responsible for responding to land disputes, they remain endemic in rural Liberia, and are the most important threat to security of property rights in the country. Not surprisingly, land emerged as a common cause of concern for citizens, and the patrolling officers attempted to provide specific guidance to those involved in ongoing disputes. For example, when a resident of the town of Jinnepeleta complained that another community member had encroached on her land because "he has money and the upper hand," the officers suggested that she first appeal to the town chief for help, and, failing that, to the Land Coordination Center in Gbarnga.[98]

The officers also explicitly discouraged the use of violence to resolve land disputes, and instructed citizens to call them immediately should violence erupt: "We will come to put this situation under control before the case can be taken to court or to Land Commission."[99] The guidance the officers provided may have helped raise awareness of the variety of mechanisms available to resolve non-violent land disputes, and their promises to intervene to diffuse violent ones may have helped reassure property owners that conflicts over land use or boundaries would not be allowed to spiral out of control.

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[96]Gbenequelle 3/3/2015; Tukpah 4/20/2015; Wainsue 4/23/2015.
[97]Gbenequelle 3/3/2015.
[98]Jinnepeleta 2/6/15.
[99]Doe Town 2/7/2015.

Figure A.1: Map of Liberia and sample communities
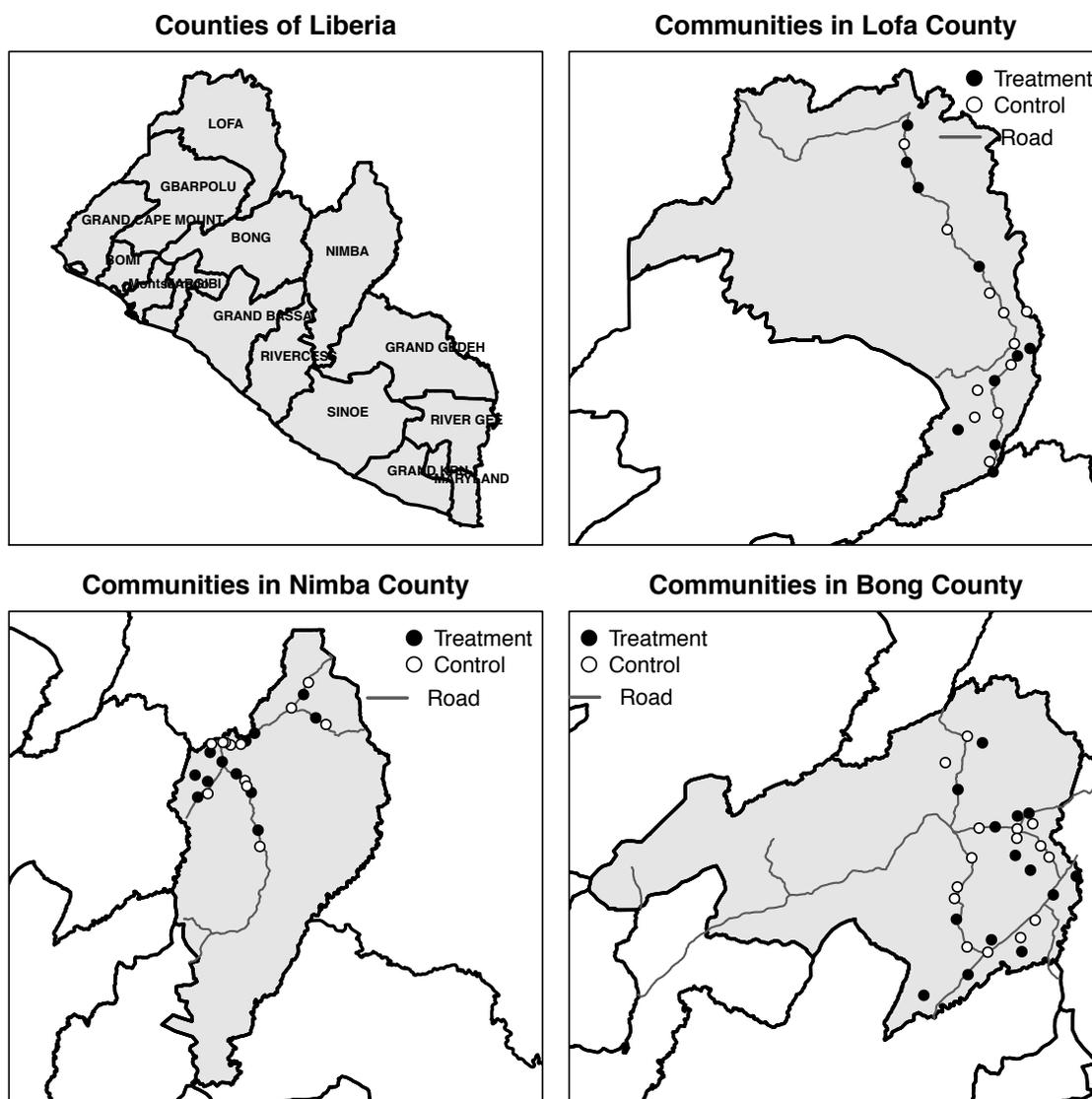
Figure A.2: Implementation timeline, June 2014-December 2015
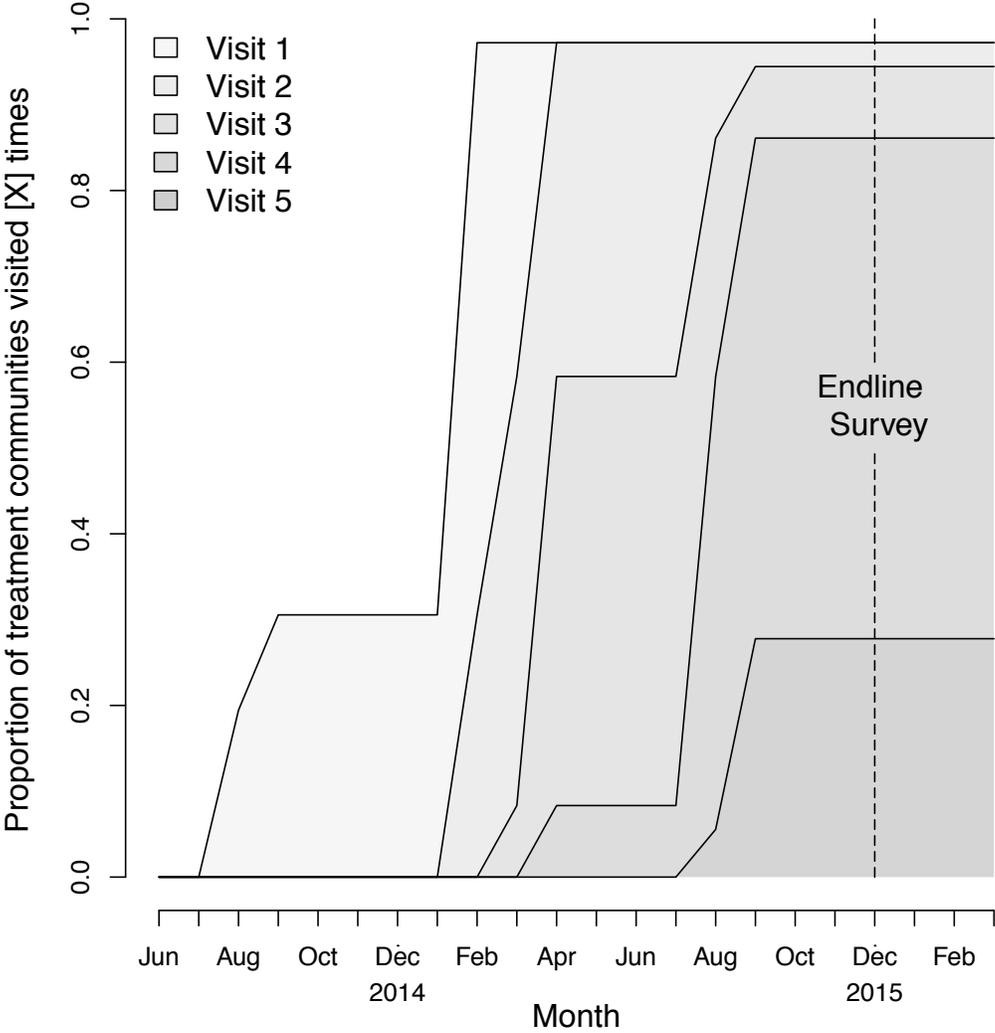
Table A.1: Descriptive statistics

	N	Mean
Knowledge of police		
Know where nearest police station is?	1,316	0.90
Know phone number of any police officer?	1,316	0.12
Know about the Hub?	1,316	0.16
Know where Hub is located?	1,316	0.12
Know what Hub does?	1,316	0.11
Knowledge of Liberian law		
Law allows citizens to beat suspects?	1,315	0.09
Law requires LNP to investigate witnesses as suspects?	1,315	0.68
Law requires habeas corpus?	1,315	0.83
Law proscribes investigating town or village chief?	1,314	0.37
Law allows trial by ordeal?	1,315	0.23
Perceptions of police		
Police will make victim pay a bribe to investigate?	1,315	0.50
Police will take victim's case seriously?	1,315	0.26
Police will free suspect without trial?	1,315	0.21
Police will verbally abuse suspect?	1,315	0.09
Police will physically abuse suspect?	1,315	0.10
Police will free suspect for a bribe?	1,314	0.34
Police are corrupt?	1,315	0.56
Police treat all tribes equally?	1,315	0.80
Police treat women and men equally?	1,315	0.70
Perceptions of government		
Government is corrupt?	1,413	0.54
Government treats all tribes equally?	1,413	0.90
Government makes decisions transparently?	1,413	0.40
Reliance on police (hypothetical)		
Prefer police to respond to burglary?	1,413	0.42
Prefer police to respond to domestic violence?	1,413	0.20
Prefer police to respond to armed robbery?	1,413	0.67
Prefer police to respond to murder?	1,413	0.69
Prefer police to respond to mob violence?	1,413	0.44
Prefer police to respond to inter-ethnic riot?	1,413	0.59

Table A.1: Descriptive statistics (cont.)

	N	Mean
Reliance on trial by ordeal (hypothetical)		
Community supports trial by ordeal for mysterious death?	1,324	0.18
Community supports trial by ordeal for missing person?	1,324	0.19
Community supports trial by ordeal for burglary?	1,324	0.26
You yourself support trial by ordeal for mysterious death?	1,324	0.17
You yourself support trial by ordeal for missing person?	1,323	0.18
You yourself support trial by ordeal for burglary?	1,322	0.23
Incidence of crime (individual)		
Victim of armed robbery in past year?	1,310	0.02
Victim of burglary in past year?	1,311	0.16
Victim of aggravated assault in past year?	1,308	0.02
Victim of simple assault in past year?	1,308	0.03
Victim of domestic abuse (physical) in year?	1,310	0.02
Victim of domestic abuse (verbal) in past year?	1,310	0.05
Victim of domestic abuse (threats) in past year?	1,311	0.02
Incidence of crime (neighborhood)		
Any armed robbery in town in past year?	1,413	0.01
Any burglary in town in past year?	1,309	0.21
Any aggravated assault in town in past year?	1,307	0.02
Any simple assault in town in past year?	1,308	0.06
Any domestic violence in town in past year?	1,309	0.36
Any rape in town in past year?	1,310	0.04
Reporting of crimes that occurred		
Armed robbery reported to police?	41	0.58
Burglary reported to police?	658	0.23
Aggravated assault reported to police?	70	0.18
Simple assault reported to police?	148	0.11
Domestic violence reported to police?	668	0.16
Rape reported to police?	64	0.78
Security of property rights		
House property is secure?	1,413	0.80
Made improvements to house property in past 12 months?	1,317	0.25
Farm property is secure?	1,413	0.80
Made improvements to farm property in past 12 months?	1,043	0.76
Fallowed land in 2015?	1,041	0.85
Plan to fallow land in 2016?	1,043	0.84
Dispute over house property in past 12 months?	1,317	0.05
Dispute over farm property in past 12 months?	1,044	0.11

Table A.2: Correlation matrix for dependent variables

	1	2	3	4	5	6	7	8	9
1. Knowledge of police	1.00								
2. Knowledge of Liberian law	0.19 (0.00)	1.00							
3. Perceptions of police	-0.03 (0.21)	0.07 (0.01)	1.00						
4. Perceptions of courts	-0.05 (0.08)	0.06 (0.03)	0.45 (0.00)	1.00					
5. Perceptions of government	-0.02 (0.42)	0.02 (0.47)	0.38 (0.00)	0.41 (0.00)	1.00				
6. Reliance on police (hypothetical)	0.16 (0.00)	0.11 (0.00)	0.02 (0.45)	-0.05 (0.07)	-0.04 (0.13)	1.00			
7. Reliance on trial by ordeal (hypothetical)	-0.03 (0.32)	-0.22 (0.00)	-0.10 (0.00)	-0.06 (0.01)	-0.12 (0.00)	0.01 (0.81)	1.00		
8. Incidence of crime (individual)	0.15 (0.00)	0.06 (0.03)	-0.10 (0.00)	-0.06 (0.02)	-0.15 (0.00)	0.18 (0.00)	0.10 (0.00)	1.00	
9. Security of property rights	0.06 (0.03)	0.01 (0.61)	0.09 (0.00)	0.07 (0.02)	0.07 (0.01)	-0.01 (0.71)	-0.05 (0.05)	-0.07 (0.01)	1.00

Notes: All indices standardized. P-values in parentheses.

Table A.3: Balance

	Treatment	
Wealth index	0.05	0.08
	[0.19]	[0.21]
% literate	-0.05	0.29
	[1.44]	[1.79]
% with no schooling	0.39	0.60
	[1.21]	[1.59]
Mean years of education	0.03	-0.00
	[0.18]	[0.25]
% unemployed	-0.05	0.09
	[0.54]	[0.63]
% under 18	1.68	2.68
	[2.61]	[3.81]
Community population	0.00	0.00
	[0.00]	[0.00]
Ethnic diversity (ELF)	0.01	-0.02
	[0.41]	[0.52]
Religious diversity	-0.57	-0.62
	[0.50]	[0.63]
% displaced during the war	-0.10	-0.32
	[0.37]	[0.70]
Constant	-0.68	-1.36
	[2.08]	[3.06]
Block FE	N	Y
Observations	74	74
R-squared	0.04	0.06

Notes: OLS regressions of treatment assignment on community-level control variables. Standard errors, clustered by community, in brackets. $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$.; $^{***}p < 0.001$.

Table A.4: Average Effect Sizes with and without controls

	Knowledge of police	Knowledge of law	Perceptions of police	Perceptions of courts	Perceptions of government
Treatment	0.14***	0.06*	0.03	-0.01	0.03
	(0.03)	(0.02)	(0.03)	(0.03)	(0.04)
Observations	1,316	1,407	1,314	1,673	1,674
P-value (standard)	0.000	0.031	0.283	0.598	0.343
P-value Holm (1979)	0.000	0.248	1.000	1.000	1.000
P-value Benjamini & Hochberg (1995)	0.000	0.104	0.481	0.748	0.490

	Preferences for police	Support for trial by ordeal	Incidence of crime	Property rights	Donation to CWF
Treatment	-0.00	-0.05	-0.01	0.10***	0.09
	(0.05)	(0.04)	(0.04)	(0.03)	(0.09)
Observations	1,790	1,679	1,489	1,041	1,283
P-value (standard)	0.988	0.286	0.819	0.000	0.288
P-value Holm (1979)	1.000	1.000	1.000	0.003	1.000
P-value Benjamini & Hochberg (1995)	0.988	0.480	0.912	0.002	0.481

Notes: Average Effect Sizes (AES) for each cluster of outcomes displayed above standard errors, clustered by community, in parentheses. Standard p-values reported, along with those proposed by Holm (1979) and Benjamini and Hochberg (1995). $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$.; $^{***}p < 0.001$. Sample sizes vary due to missing data from "do not know" responses and because not all questions administered to leaders.

Table A.5: Average treatment effects on crime with multiple comparisons adjustments

	Aggravated assault	Armed robbery	Domestic violence	Simple assault	Theft or burglary	Rape
Treatment	0.01	-0.00	-0.07*	-0.04**	0.00	0.03
	(0.01)	(0.01)	(0.03)	(0.01)	(0.03)	(0.02)
Observations	1,663	1,663	1,663	1,663	1,663	1,662
P-value (standard)	0.278	0.797	0.011	0.009	0.878	0.229
P-value Holm (1979)	0.914	1.000	0.056	0.056	1.000	0.914
P-value Benjamini & Hochberg (1995)	0.417	0.878	0.032	0.032	0.878	0.417

Notes: Average Treatment Effects for each outcome displayed above standard errors, clustered by community, in parentheses. Standard p-values reported, along with those proposed by Holm (1979) and Benjamini and Hochberg (1995). $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$. Sample sizes vary due to missing data from "do not know" responses.

Table A.6: Average Effect Sizes with and without controls

	Knowledge of police		Knowledge of law		Perceptions of police		Perceptions of courts		Perceptions of government	
Treatment	0.11*	0.15***	0.04	0.05*	0.02	0.03	-0.02	-0.01	0.03	0.03
	(0.05)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
Controls	N	Y	N	Y	N	Y	N	Y	N	Y
Observations	1,316	1,316	1,331	1,331	1,307	1,307	1,666	1,666	1,667	1,667

	Preferences for police		Support for trial by ordeal		Incidence of crime		Property rights		Donation to CWF	
Treatment	-0.01	-0.00	-0.03	-0.05	-0.00	-0.01	0.09**	0.10***	0.07	0.10
	(0.06)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.09)	(0.09)
Controls	N	Y	N	Y	N	Y	N	Y	N	Y
Observations	1,331	1,331	1,657	1,657	1,662	1,662	1,041	1,041	1,269	1,269

Average Effect Sizes (AES) for each cluster of outcomes. AES coefficients are interpreted in terms of standard deviations from the control group mean. Standard errors in parentheses, clustered by community. $* p < 0.05$, $** p < 0.01$, $*** p < 0.001$. Not all outcomes were measured for local leaders, and some are missing for some residents. Sample sizes vary accordingly.

Figure A.3: Average Effects Sizes with average treatment effects on component dependent variables

Knowledge of Police and Hubs

Average effect (std)
Knows location of nearest police station
Knows number of any police officer
Knows about the Hub
Knows where the Hub is located
Knows what the Hub does

Knowledge of Liberian Law

Average effect (std)
Knows Town Watch Team cannot beat suspects
Knows law protects informants
Knows about Habeas Corpus
Knows that formal law trumps customary law
Knows trial by ordeal is illegal

Perceptions of police

Average effect (std)
Police will make you pay a bribe? Disagree
Police take cases seriously? Agree
Police will free a criminal for a bribe? Disagree
Victims who report will be satisfied? Agree
Police verbally abuse suspects? Disagree
Police physically abuse suspects? Disagree
Police make victims pay bribes? Disagree
Police are corrupt? Disagree
Police treat all tribes equally? Agree
Police treat women and men the same? Agree

Perceptions of courts

Average effect (std)
Courts are transparent? Agree
Courts treat men and women the same? Agree
Courts treat all tribes equally? Agree
Courts are corrupt? Disagree

Perceptions of government

Average effect (std)
Govt is corrupt? Disagree
Government treats all tribes equally? Agree
Government open and transparent? Agree

-.1   0   .1   .2

Notes: Average Effect Sizes (AES) for each cluster of outcomes displayed above the average treatment effect (ATE) for each component variable. Note that because the AES and ATE are measured in different units, their magnitudes cannot be directly compared.

Figure A.4: Average Effects Sizes with average treatment effects on component dependent variables (cont.)

Preferences for police
- Average effect (std)
- Prefer police respond to burglary?
- Prefer police respond to domestic violence?
- Prefer police respond to armed robbery?
- Prefer police respond to murder?
- Prefer police respond to mob violence?
- Prefer police respond to inter-ethnic violence?

Support for Trial by Ordeal
- Average effect (std)
- Support trial by ordeal for unsolved murder?
- Support trial by ordeal for missing person?
- Support trial by ordeal for unsolved burglary?
- Town supports trial by ordeal for unsolved murder?
- Town supports trial by ordeal for missing person?
- Town supports trial by ordeal for unsolved burglary?

Incidence of crime
- Average effect (std)
- Aggravated assault
- Armed robbery
- Domestic violence
- Simple assault
- Theft or burglary
- Rape

Security of property rights
- Average effect (std)
- Confident house land is secure
- Made improvements to house in past year
- Confident farm land is secure
- Made improvements to farm in past year
- Left farm fallow last year
- Plans to leave farm fallow next year
- No disputes over house land in past year
- No disputes over farm land in past year

Donation to Community Watch Team
- Average effect (std)

-.2   -.1   0   .1   .2

Notes: Average Effect Sizes (AES) for each cluster of outcomes displayed above the average treatment effect (ATE) for each component variable. Note that because the AES and ATE are measured in different units, their magnitudes cannot be directly compared.

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Table A.7: Balance across crimes in treatment and control communities

|	Control mean	Treatment mean	Difference	SE	t-statistic	N
Female	0.34	0.36	0.02	0.03	0.72	1,813
Age (26-35)	0.26	0.28	0.02	0.03	0.60	1,813
Age (36-45)	0.25	0.24	-0.01	0.02	-0.60	1,813
Age (46-55)	0.16	0.17	0.01	0.02	0.55	1,813
Age (56-65)	0.09	0.10	-0.00	0.02	-0.04	1,813
Age (65+)	0.11	0.11	-0.00	0.02	-0.12	1,813
Primary education	0.19	0.22	0.04	0.03	1.15	1,813
Secondary education	0.25	0.23	-0.02	0.03	-0.80	1,813
Post-secondary education	0.32	0.31	-0.01	0.03	-0.34	1,813
Log of town population	6.77	6.90	0.10	0.15	0.68	1,813
Police depot in town	0.13	0.12	-0.02	0.08	-0.31	1,813
Society member	0.76	0.77	0.01	0.05	0.30	1,326
Minority	0.12	0.14	0.02	0.04	0.49	1,781

Differences in characteristics of crime victims across treatment and control communities. N=1,813 crimes.

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Table A.8: Effects on crime reporting without conditioning on crime occurrence

	Crime occurred	Crime occurred & reported	Crime occurred & not reported
All crimes			
Treatment	-0.02	0.02	-0.03
	(0.03)	(0.03)	(0.02)
Felonies			
Treatment	0.02	0.07*	-0.02
	(0.04)	(0.04)	(0.03)
Misdemeanors			
Treatment	-0.06*	-0.03	-0.05
	(0.03)	(0.03)	(0.03)
Observations	1790	1790	1790

Average effect sizes (AES) for crime occurrence and reporting. AES coefficients are interpreted in terms of standard deviations from the control group mean. Standard errors in parentheses, clustered by community. $^+$ $p < 0.5$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure A.5: Difference in crime reporting over time using LNP crime records

Total # of reported crimes
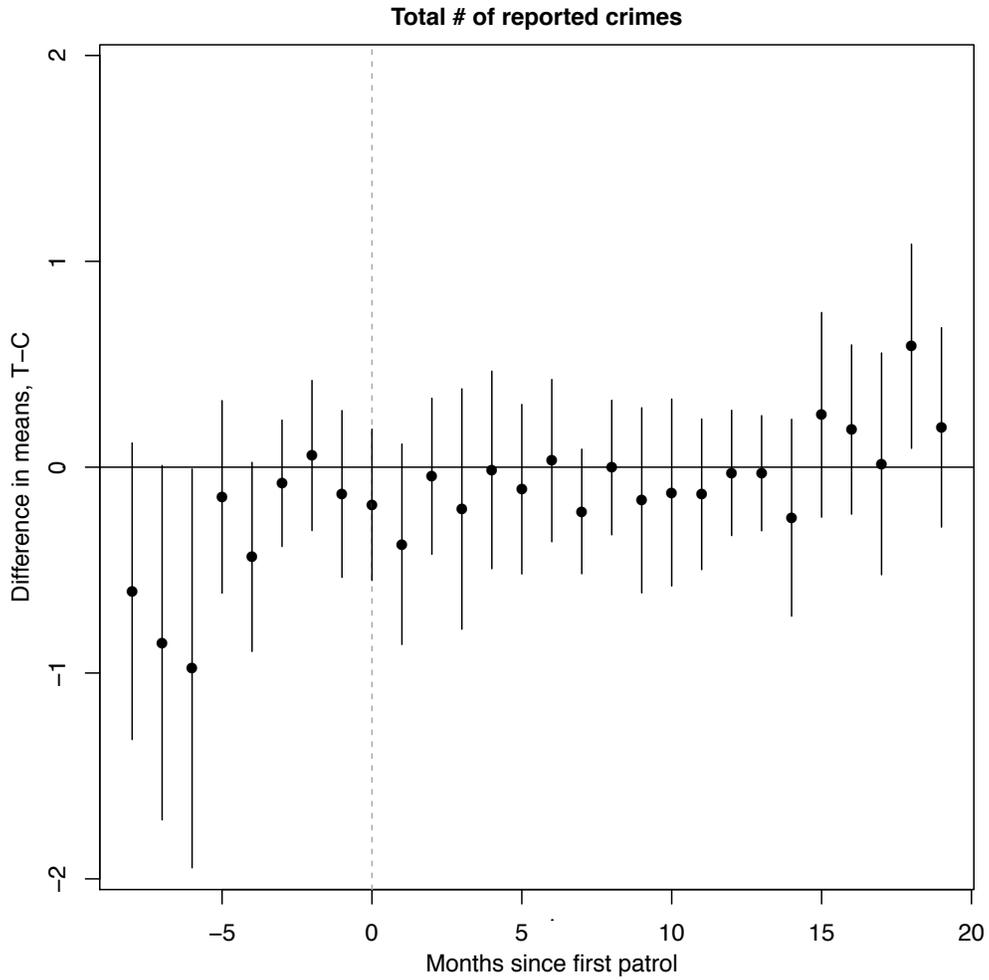
Notes: Overtime differences in crime reporting between treatment and control communities with 95% confidence intervals. Standard errors are clustered by community.

Table A.9: Heterogeneous treatment effects on crime reporting by gender

	Nowhere	Statutory only	Customary only	Statutory & customary
All crimes				
Treatment	-0.03	0.03	-0.01	0.02
	[0.03]	[0.04]	[0.04]	[0.02]
Treatment × female	0.03	-0.03	0.04	-0.03
	[0.06]	[0.05]	[0.06]	[0.03]
Female	0.12***	-0.03	-0.09**	-0.01
	[0.04]	[0.04]	[0.04]	[0.02]
Observations	1145	1145	1145	1145
Felonies				
Treatment	-0.21^{+}	0.08	0.05	0.07
	[0.12]	[0.13]	[0.10]	[0.06]
Treatment × female	0.06	0.23	-0.16	-0.13
	[0.22]	[0.20]	[0.17]	[0.09]
Female	-0.08	-0.14	0.13	0.08
	[0.19]	[0.16]	[0.11]	[0.09]
Observations	126	126	126	126
Misdemeanors				
Treatment	-0.01	0.02	-0.02	0.01
	[0.03]	[0.04]	[0.04]	[0.02]
Treatment × female	0.02	-0.05	0.05	-0.02
	[0.07]	[0.05]	[0.06]	[0.03]
Female	0.14**	-0.03	-0.10*	-0.02
	[0.04]	[0.03]	[0.04]	[0.02]
Observations	1019	1019	1019	1019

Notes: Standard errors, clustered by community, in brackets. $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01.$; $^{***}p < 0.001$.

Table A.10: Heterogeneous treatment effects on crime reporting by ethnicity

	Nowhere	Statutory only	Customary only	Statutory & customary
All crimes				
Treatment	-0.03	0.01	0.02	0.00
	[0.03]	[0.03]	[0.04]	[0.01]
Treatment $\times$ minority	0.11	0.02	-0.13	0.00
	[0.10]	[0.09]	[0.12]	[0.04]
Minority	-0.08	0.07	0.03	-0.02
	[0.06]	[0.06]	[0.08]	[0.02]
Observations	1136	1136	1136	1136
Felonies				
Treatment	-0.16$^+$	0.13	-0.02	0.04
	[0.09]	[0.09]	[0.09]	[0.05]
Treatment $\times$ minority	-0.25	0.19	0.14	-0.08
	[0.31]	[0.31]	[0.25]	[0.08]
Minority	0.31	-0.21	-0.09	-0.01
	[0.23]	[0.21]	[0.18]	[0.04]
Observations	126	126	126	126
Misdemeanors				
Treatment	-0.02	-0.01	0.02	0.00
	[0.03]	[0.03]	[0.04]	[0.02]
Treatment $\times$ minority	0.15	-0.00	-0.17	0.00
	[0.11]	[0.07]	[0.13]	[0.04]
Minority	-0.10	0.09$^+$	0.02	-0.00
	[0.07]	[0.05]	[0.08]	[0.02]
Observations	1010	1010	1010	1010

Notes: Standard errors, clustered by community, in brackets. $^+p < 0.1$; $^*p < 0.05$; $^{**}p < 0.01.$; $^{***}p < 0.001$.

Table A.11: Heterogeneous treatment effects on crime reporting by age

	Nowhere	Statutory only	Customary only	Statutory & customary
All crimes				
Treatment	0.01	-0.03	0.02	-0.00
	[0.05]	[0.05]	[0.05]	[0.02]
Treatment $\times$ youth	-0.04	0.07	-0.05	0.02
	[0.07]	[0.07]	[0.07]	[0.03]
Youth	0.08	-0.04	-0.04	-0.01
	[0.05]	[0.05]	[0.05]	[0.02]
Observations	967	967	967	967
Felonies				
Treatment	-0.14	0.01	0.06	0.06
	[0.12]	[0.13]	[0.13]	[0.07]
Treatment $\times$ youth	-0.12	0.32	-0.12	-0.08
	[0.18]	[0.20]	[0.19]	[0.07]
Youth	0.23[+]	-0.25[*]	0.03	-0.02
	[0.13]	[0.12]	[0.10]	[0.06]
Observations	126	126	126	126
Misdemeanors				
Treatment	0.04	-0.03	0.00	-0.02
	[0.05]	[0.04]	[0.05]	[0.02]
Treatment $\times$ youth	-0.05	0.04	-0.03	0.04
	[0.08]	[0.06]	[0.08]	[0.03]
Youth	0.07	-0.01	-0.05	-0.01
	[0.06]	[0.05]	[0.05]	[0.02]
Observations	841	841	841	841

Notes: Standard errors, clustered by community, in brackets. $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$.; $^{***}p < 0.001$. Sample excludes leaders, who were not asked about their age

Table A.12: Effects on secondary outcomes not reported in paper

	# in community with Ebola	Gov't has right to tax
Treatment	-0.11	0.00
	[0.17]	[0.02]
Observations	1,638	1,675
R^2	0.06	0.04

Notes: Standard errors, clustered by community, in brackets. $^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Sample size varies due to missing data from "do not know" responses.

Table A.13: Additional pre-specified heterogeneous treatment effects analyses

	Knowledge of police						Knowledge of law						Perceptions of police					
Treatment	0.13*** [0.04]	0.14** [0.05]	0.10* [0.04]	0.15*** [0.04]	0.34** [0.12]	0.05* [0.03]	0.04 [0.04]	0.04 [0.03]	0.06* [0.03]	0.07 [0.09]	0.03 [0.03]		0.04 [0.04]	0.00 [0.03]	0.03 [0.03]	0.21* [0.09]		
Treatment x leader																		
Treatment x rebel violence		-0.00 [0.04]						0.01 [0.02]					-0.01 [0.03]					
Treatment x gov't violence			0.08 [0.05]						0.02 [0.03]					0.07* [0.03]				
Treatment x police depot				-0.15 [0.16]						-0.07 [0.09]					-0.02 [0.05]			
Treatment x # patrols					-0.05 [0.03]						-0.01 [0.02]					-0.05* [0.02]		
Observations	1,316	1,316	1,316	1,316	1,316	1,316	1,331	1,331	1,331	1,331	1,331		1,307	1,307	1,307	1,307		

Notes: Standard errors, clustered by community, in brackets. $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$.; $^{***}p < 0.001$. Not all outcomes and independent variables measured among leaders; sample sizes vary accordingly.

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Table A.13: Additional pre-specified heterogeneous treatment effects analyses (cont.)

	Perceptions of courts						Perceptions of government					Preferences for police			
Treatment	-0.01 [0.04]	0.00 [0.05]	-0.01 [0.04]	-0.02 [0.03]	-0.13 [0.09]	0.03 [0.04]	0.06 [0.05]	0.03 [0.04]	0.02 [0.04]	0.02 [0.12]	-0.01 [0.05]	0.01 [0.06]	-0.02 [0.05]	0.04 [0.05]	-0.13 [0.19]
Treatment x leader	-0.02 [0.09]						-0.01 [0.09]								
Treatment x rebel violence		-0.01 [0.03]						-0.03 [0.03]				-0.01 [0.03]			
Treatment x gov't violence			-0.00 [0.04]						-0.02 [0.05]				0.02 [0.05]		
Treatment x police depot				0.04 [0.06]						-0.00 [0.12]				-0.40* [0.16]	
Treatment x # patrols					0.03 [0.02]						0.00 [0.03]				0.03 [0.04]
Observations	1,655	1,306	1,306	1,655	1,655	1,656	1,307	1,307	1,656	1,656	1,331	1,331	1,331	1,331	1,331

Notes: Standard errors, clustered by community, in brackets. $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$. Not all outcomes and independent variables measured among leaders; sample sizes vary accordingly.

Table A.13: Additional pre-specified heterogeneous treatment effects analyses (cont.)

	Reliance on trial by ordeal					Crime victimization					Property rights				
Treatment	-0.06 [0.06]	-0.04 [0.07]	-0.05 [0.06]	-0.04 [0.05]	0.13 [0.13]	0.01 [0.03]	0.02 [0.04]	0.01 [0.03]	0.02 [0.03]	0.06 [0.10]	0.09*** [0.03]	0.11** [0.04]	0.07* [0.03]	0.08** [0.03]	0.19* [0.08]
Treatment x leader	0.10 [0.10]														
Treatment x rebel violence		-0.03 [0.03]					-0.00 [0.02]					-0.02 [0.02]			
Treatment x gov't violence			-0.02 [0.05]					0.00 [0.03]					0.04 [0.03]		
Treatment x police depot				0.04 [0.13]					-0.03 [0.09]					0.13 [0.13]	
Treatment x # patrols					-0.04 [0.03]					-0.01 [0.02]					-0.02 [0.02]
Observations	1,645	1,299	1,299	1,645	1,645	1,273	1,273	1,273	1,273	1,273	1,041	1,041	1,041	1,041	1,041

Notes: Standard errors, clustered by community, in brackets. $^+p < 0.1$; $^*p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$. Not all outcomes and independent variables measured among leaders; sample sizes vary accordingly.

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Table A.13: Additional pre-specified heterogeneous treatment effects analyses (cont.)

	Donations to CWF				
Treatment	0.08 [0.09]	0.07 [0.11]	0.07 [0.09]	0.06 [0.09]	-0.12 [0.22]
Treatment x leader	-0.01 [0.15]				
Treatment x rebel violence		0.02 [0.05]			
Treatment x gov't violence			0.03 [0.08]		
Treatment x police depot				0.29 [0.19]	
Treatment x # patrols					0.05 [0.05]
Observations	1,255	977	977	1,255	1,255

Notes: Standard errors, clustered by community, in brackets. $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$.; $^{***}p < 0.001$. Not all outcomes and independent variables measured among leaders; sample sizes vary accordingly.

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Table A.14: Deviations from pre-analysis plan

	PAP	Deviations from PAP
Hypotheses	We pre-specified 11 hypotheses:	• We break H1 into two hypotheses, one on knowledge of police, one on knowledge of Liberian law
	1. Increased knowledge of police and Liberian law	
	2. Improved perceptions of police	• We add a hypothesis on improved perceptions of the courts
	3. Reduced incidence of crime and violence	
	4. Increased willingness to report to police	• We drop H6
	5. Reduced reliance on illegal mechanisms of dispute resolution	• We report results for H8 and H10 in the appendix
	6. Increased perceived efficacy of registering complaints against police	
	7. Increased support for community watch forums	
	8. Reduced incidence of Ebola	
	9. Increased security of property rights	
	10. Increased tax compliance	
	11. Improved perceptions of Liberian government	

Table A.14: Deviations from pre-analysis plan (cont.)

	PAP	Deviations from PAP
Heterogeneous treatment effects	We proposed to test for heterogeneous treatment effects (HTEs) along: 1. Number of months since last patrol 2. Number of patrols 3. Gender 4. Ethnicity 5. Age 6. Exposure to wartime violence perpetrated by government 7. Exposure to wartime violence perpetrated by rebels 8. Existence of an LNP depot in the community 9. Leadership role in the community	• We drop HTE1 due to insufficient variation. All communities were last patrolled in either August or September 2015, two and three months before the start of our endline, respectively • We report results for HTE6 through HTE9 in the appendix
Data sources	We proposed to draw on four data sources: 1. Survey of 18 randomly-selected residents 2. Survey of five purposively-selected local leaders (chief, two elders, women's group leader, youth group leader) 3. LNP crime data 4. Contributions to community watch forums	None

Table A.14: Deviations from pre-analysis plan (cont.)

	PAP	Deviations from PAP
Independent variables	We proposed to operationalize treatment as a dummy indicating assignment to treatment. We also proposed to measure number of months since last patrol, and number of patrols, for our HTE analyses	We drop HTE1, and so do not use our measure of number of months since last patrol
Econometric specification	• We proposed to cluster outcomes by theme and compute the intent-to-treat effect within each cluster using the Average Effect Size (AES) estimator • We proposed to cluster standard errors at the community level • We proposed to estimate the AES for residents and local leaders both separately and together • We proposed to report treatment effects with and without controls	• To adjust for variation in the probability of assignment to treatment across blocks (block), we include block fixed effects and inverse probability weights, following Gerber & Green (2012, 117) • In the paper, we report the AES for each outcome for residents and local leaders together. In the appendix, we report AES heterogeneity by leader status. We do not analyze leaders separately due to a lack of statistical power
Data cleaning and preprocessing	• We proposed to cap outliers at the 99th percentile • We proposed to exclude any observations with df(beta) greater than $2/\sqrt{N}$	None

Table A.14: Deviations from pre-analysis plan (cont.)

	PAP	Deviations from PAP
Individual-level control variables	We proposed including nine individual-level controls: 1. Gender 2. Age 3. Household size 4. Tribe 5. Religion 6. Education 7. Literacy 8. Employment 9. Wealth When pooling residents and local leaders or analyzing local leaders separately, we proposed controlling for leadership position in the community as well	Due to an oversight, questions on household size, ethnicity, and religion were not included in the leader survey. To avoid having to exclude leaders from our analysis, we omit these variables from our list of controls. Because treatment was randomly assigned, omission of these controls may reduce the precision of our estimates, but will not induce bias
Community-level control variables	We proposed including 6 community-level controls, measured in the survey of local leaders: 1. Population 2. Mobile phone coverage 3. Distance to nearest road 4. Distance to nearest police station 5. Presence of an LNP depot 6. Index of public services	Because we do not have the geo-coordinates of police stations in our study region, we are unable to calculate distance to the nearest police station, and so omit this variable from our list of community-level controls

Table A.14: Deviations from pre-analysis plan (cont.)

	PAP	Deviations from PAP
Extensions	We proposed to conduct a bounding exercise to test spillover onto residents of treatment communities who did not report interacting with patrolling officers	We drop this exercise. The program comprised multiple components, and different residents were treated in different combinations of ways, making the exercise infeasible. Moreover, the strong exogeneity assumptions required for the exercise likely vary across treated residents
Analyses included in paper but not pre-specified		Our PAP did not specify how we would estimate effects on crime reporting. This analysis differs slightly from the other procedures outlined in the PAP because the observation is measured at the crime level, rather than the respondent level, and because outcomes are measured singularly (e.g. crime reported to police or not; crime reported to local leaders or not; etc.), rather than as clusters of component variables. We therefore drop the AES approach, and estimate the ATE at the crime level on each reporting outcome separately. For this analysis we include the same set of pre-specified controls, block (i.e. block) fixed effects, and inverse probability weights