

Establishing the Rule of Law in Weak and War-torn States:^{*} Evidence from a Field Experiment with the Liberian National Police

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Abstract

How to restore citizens' trust and cooperation with the police in the wake of civil war? We report results from an experimental evaluation of the Liberian National Police's (LNP) "Confidence Patrols" program, which deployed teams of newly-retrained, better-equipped police officers on recurring patrols to rural communities across three Liberian counties over a period of 14 months. We find that the program increased knowledge of the police and Liberian law; enhanced security of property rights; reduced the incidence of some types of crime, notably simple assault and domestic violence; and increased reporting of felony offenses to the LNP. The program did not, however, increase trust in the police, courts, or government more generally. The effects on crime reporting are concentrated almost entirely among residents who are disadvantaged under customary mechanisms of dispute resolution. We consider implications of these findings for post-conflict policing in Liberia and weak and war-torn states more generally.

1 INTRODUCTION

Effective, legitimate police forces are widely viewed as necessary for sustained peace, economic growth, and the rule of law. Because citizens are more likely to interact with police officers than with most other civil servants (Mazerolle et al. 2013), building trust in the police may also help build trust in the state more generally. These intuitions have catalyzed millions of dollars of investments in state security institutions by donors, aid agencies, and the UN—a trend that has accelerated in recent years with the proliferation of organizations and initiatives dedicated to promoting the rule of law and security sector reform (Carothers 2009).

Establishing police effectiveness and legitimacy is especially challenging in the world’s weakest and most war-torn states—arguably the settings in which effective and legitimate police forces are most urgently needed. Citizens in these countries often do not know how to contact the police, and many assume that seeking redress through state security institutions will prove prohibitively expensive. Many also (rightly) fear police corruption and abuse. As a result, victims and witnesses often refuse to report to the police, relying instead on customary mechanisms for dispute resolution. These can be efficient, effective, and easily accessible (Isser 2011), but they also tend to be biased against historically marginalized groups, entrenching inequities in areas beyond the state’s purview (Aldashev et al. 2012). Some also rely on modes of adjudication that are illegal under state law, and that violate norms of due process (e.g. trial by ordeal).

The literature on fostering citizens’ trust and compliance with state security institutions is surprisingly thin, especially in political science, and especially in the developing world. Despite influential early contributions by political scientists (e.g. Wilson 1978), for many years the study of policing was dominated by criminologists, psychologists, and, to a lesser extent, economists (e.g. Levitt 1997; Tyler and Huo 2002). As informative as these studies have been, the majority focus on the US and UK, and may not generalize to developing countries, much less to weak and war-torn states. While a number of scholars have explored “best practices” in post-conflict security sector reform (e.g. Bryden, N’Diaye and Olonisakin 2008), few have addressed whether or how these

reforms affect the relationship between civilians and state institutions. Moreover, these studies are almost all observational, and many rely on anecdotal evidence alone. As such, they are susceptible to selection and other biases.

This situation is beginning to change, with several recent randomized controlled trials evaluating particular approaches to policing in the developing world (Banerjee et al. 2014; Blattman et al. 2017; Cooper 2017; Magaloni, Franco and Melo 2015).¹ We advance this literature by experimentally evaluating the effects of recurring “Confidence Patrols” by elite police officers on crime, property rights, and police-community relations in Liberia. Importantly, and unlike most other studies of policing to date (experimental or otherwise), our evaluation occurs in a broader context of state penetration into communities governed first and foremost by customary authorities—chiefs, elders, and sodalities known as “secret societies.”

Processes of state penetration can sometimes result in mutually advantageous arrangements between statutory and customary providers of security and other public goods (Baldwin 2015). In many cases, however, the projection of state power provokes conflict between competing authorities, each angling to make “*their* rules, whether state law or some other implicit code, become the routine basis upon which people act” (Migdal and Schlichte 2005, 15, emphasis ours). These cases are characterized not by the “mutual empowerment” of statutory and customary institutions, but rather by contestation over “mutually exclusive goals” (Migdal 1994, 24). Our aim in this paper is not only to contribute to the academic literatures on policing, statebuilding, and security sector reform, but also to inform policymaking in Liberia at an especially crucial moment, as UN peacekeepers withdraw and as the government extends its presence into rural areas long accustomed to state absence or abuse.

Our evaluation leverages a combination of original survey and behavioral data, Liberian National Police (LNP) crime records, and qualitative field reports compiled by a Liberian research assistant hired to shadow the LNP for the duration of the experiment. Together these data yield several sets of results. First, we find that the Confidence Patrols program improved security in

¹An important initiative by the Evidence in Governance and Politics (EGAP) network will test the effects of community policing across six different countries; see <http://egap.org/metaketa/metaketa-iv-community-policing>.

treatment communities, strengthening property rights and reducing the incidence of some types of crime, notably simple assault and domestic violence. These results are particularly striking given the initial skepticism with which patrolling officers were often greeted, and given that most patrols were conducted shortly after the end of the Ebola epidemic, when relations between citizens and the state were especially strained (Blair, Morse and Tsai 2017). The program's impact on domestic violence and land disputes is particularly salient given the continued prevalence of these incidents in rural Liberia, and given the threat of violence that land disputes in particular continue to pose (Blattman, Hartman and Blair 2014).

Second, we find that the program reduced the costs of crime reporting by increasing citizens' knowledge of Liberian law and improving their understanding of the mechanisms available to them for accessing the LNP. Relatedly, we find that the program increased the proportion of felony offenses reported to the police and courts by approximately 50% relative to the control group—a substantively large and highly statistically significant effect. Notably, this effect was driven by a reduction in the proportion of felonies that were not reported at all, rather than a shift away from reporting to customary forums. This effect was *not* accompanied by changes in reporting of misdemeanors—an important finding in and of itself, given the severe capacity constraints under which the Liberian police and courts continue to operate.

Third and more surprisingly, we find no evidence that the program improved citizens' perceptions of the police, courts, or government more generally. Most citizens remain skeptical of the efficacy and transparency of these institutions—skepticism that the program did little or nothing to change. On the one hand, this null result provides some reassurance that our positive findings are not artifacts of social desirability bias: if residents of treatment communities were simply parroting back messages they heard during the program, they would not have expressed such negative views of the Liberian government. On the other hand, null effects on perceptions are inconsistent with our expectations, and with studies suggesting that citizens' willingness to cooperate with the state in general (Levi, Sacks and Tyler 2009) and the police in particular (Tyler and Huo 2002) depends on their belief that state institutions are competent and procedurally fair. Contrary to the intuitions

in these studies, residents of our treatment communities became increasingly reliant on institutions that they nonetheless perceived as generally unreliable.

A potential solution to this puzzle lies in the program's differential impact on those with the most to gain from access to a statutory alternative to prevailing customary mechanisms for adjudicating crimes and resolving disputes. We show that the shift from under-reporting to reliance on the police and courts was concentrated almost entirely among residents who were not members of one of Liberia's powerful secret societies. Society membership is an important source of privilege in rural Liberia, and non-members are at a distinct disadvantage when crimes are committed or disputes occur (Lubkemann, Isser and Banks III 2011). Our results suggest that the Confidence Patrols program provided an "exit option" for these individuals, potentially helping to mitigate some of the inequities in access to justice that otherwise characterize customary regimes.

Finally and relatedly, we find some evidence that the program provoked backlash from the customary sector, with residents of treatment communities reporting more frequent appearances of the "bush devil"—a controversial ceremonial figure that secret societies use to maintain social order, often at the expense of non-members (Isser, Lubkemann and N'Tow 2009)—and more stringent social norms against cooperating with the LNP. These results, combined with those above, suggest that the increase in reporting may have come at the cost of inflamed social tensions. Nonetheless, on balance the program appears to have benefited treatment communities, with nearly *all* subsets of the population reporting less crime and greater security of property rights.

Taken together, our results suggest that expanding police presence into rural communities can improve security and help equalize access to third party adjudication, even in areas where distrust of state security institutions is pervasive. Our results also suggest, however, that state penetration is contentious, and is liable to provoke backlash from those who benefit from their status under customary law. Whether the program's impact on crime reporting proves sustainable over time remains an open question, though we suspect much will depend on the performance of rank-and-file police officers and court officials. If the police and courts prove capable of resolving crimes effectively and equitably, then greater reporting now may lead to even greater reporting in

the future. Conversely, if these institutions prove corrupt or ineffective, then the program's positive impact on reporting may prove short-lived. Assessing these and other potential long-run dynamics is beyond the scope of our evaluation. In the short term, however, we view the program as a qualified success.

Our study is not without limitations. First, because our sample size is modest, it is possible that we failed to detect small effects, particularly for outcomes that are relatively rare (e.g. armed robbery). Second, some of our dependent variables are difficult to measure in a survey (e.g. willingness to cooperate with the police), and our proxies may be susceptible to social desirability bias and other forms of measurement error, potentially correlated with treatment assignment. This limitation afflicts all studies that use surveys to measure outcomes, and ours is no exception. We mitigate these limitations by complementing our survey with administrative, qualitative, and behavioral data, and by showing that our particular combination of results (e.g. positive effects on crime reporting but nulls on perceptions of the LNP) are inconsistent with social desirability bias. Nonetheless, our results should be interpreted with these caveats in mind.

Finally, as with any study focused on a single case, we cannot be sure whether our results generalize to other developing countries. The dynamics of state penetration in Liberia are similar to those in other African settings, and problems of police corruption and abuse are endemic to the continent, and to much of the developing (and, indeed, developed) world. We suspect that a similar program would have similar effects in other rural African contexts, and possibly beyond. Nonetheless, we can only speculate as to the generalizability of our findings, and we leave this question for future research to explore.

2 THEORY

2.1 PATHOLOGIES OF POLICING AFTER CIVIL WAR

The challenges of creating effective, legitimate police forces often prove insurmountable in countries recovering from civil conflict. Police effectiveness depends in part on citizens' cooperation,

and on their willingness to report to the police when crimes are committed or violence occurs (Tyler and Huo 2002). This, in turn, depends on citizens' expectations that the benefits of cooperation will exceed the costs. In countries overcoming legacies of civil war, however, citizens may hold strong priors that state security institutions are biased, predatory, or incompetent, and may therefore expect the benefits of cooperation to be low.

This problem is compounded by the often-prohibitive costs of reporting to the police or cooperating in criminal investigations, especially in places where civil war is accompanied by state collapse. In these settings, citizens may not know how to contact the police in the first place, and may have to incur search costs to find someone who does. They may be uncertain about whether certain behaviors (e.g. domestic violence or corporal punishment) are or are not illegal under state law, further increasing search costs and diminishing expected benefits if victims or witnesses fear their cases will be dismissed out of hand. Post-conflict police forces also tend to operate under severe resource constraints, and often have limited physical presence in most areas, forcing victims to pay the additional financial cost of traveling to the nearest police station, or of transporting the nearest police officer to them. And in communities long resistant to state rule, citizens may incur social costs for engaging with the police at all.

The result is an equilibrium in which uncooperative citizens undermine police effectiveness, and ineffective police forces undermine citizen cooperation. Under these circumstances, victims may opt to seek redress through customary (informal) institutions instead,² which many view as quicker, less expensive, more accessible, and more effective than their statutory (formal) counterparts. If the former are perfect substitutes for the latter, then this may not be problematic; indeed, in some settings informal institutions facilitate the provision of security and other public goods by compensating for the lack of state capacity at the local level (Baldwin 2015).

Yet customary institutions are often beset by biases of their own, favoring a “conservative social order” characterized by “patriarchal hierarchy and social inequalities” (Isser 2011, 334). In

²We use the terms “informal,” “traditional,” and “customary” interchangeably to refer to any mechanism of adjudication or dispute resolution that operates in parallel to the police and courts, and that relies on norms and rules rooted in a particular region or community (Isser 2011).

Ghana, for example, Goldstein and Udry (2008) find that property rights tend to be stronger among those who occupy a privileged position within the “traditional” power structure. Conversely, in Liberia, Sandefur and Siddiqi (2013, 4) find that plaintiffs who are disadvantaged under customary regimes (e.g. women suing men) tend to prefer, and to be more satisfied by, the treatment they receive in the formal sector, and that the poor in particular tend to benefit from access to the “progressive features” of formal law. Customary institutions may also employ illegal or extrajudicial mechanisms for resolving disputes, or may refuse to abide by legal limits on their authority, encouraging forum shopping and exacerbating uncertainty among complainants who “cannot be sure in advance which legal regime will be applied to their situation” (Tamanaha 2008, 375).

2.2 RESOLVING THE PATHOLOGIES

If police forces could properly train, equip, and monitor the behavior of their own personnel, they could potentially improve performance and increase citizen cooperation, initiating a “virtuous circle” of legitimacy and effectiveness. Yet weak and war-torn states seldom enjoy the resources to achieve a transformation of this sort. In this paper we evaluate an alternative approach that involves concentrating resources in a small number of well-trained, well-equipped police units, which are then deployed to respond to the most serious incidents of crime and violence, and, equally importantly, to engage with civilians on a face-to-face, day-to-day basis through community-oriented policing strategies borrowed from the US and Europe (Gill et al. 2014). Rank-and-file units remain responsible for less serious incidents, despite their inadequacies.

In theory, this approach can decrease the costs of cooperation, increase the benefits, or both. In their interactions with civilians, elite units can reduce search costs by providing information about how to contact the police, and by clarifying what is and is not illegal under state law. They can lower financial costs by increasing police presence and delineating the often-misunderstood boundaries between statutory and customary institutions, thereby curtailing forum shopping. And they can mitigate social costs by persuading civilians to rely on formal rather than informal mechanisms of adjudication, potentially shifting social norms that discourage cooperation with the police.

As civilians interact with elite units, they may update their priors about the competence and professionalism of the police force as a whole. Criminologists have found that citizens' attitudes towards the police are malleable, and that mutually respectful contact with individual officers can depolarize relations, even in places where distrust is deeply entrenched (Tyler and Huo 2002). Some have argued that even a single encounter during a routine traffic stop suffices to generate this effect, provided the officer involved abides by norms of procedural fairness (Mazerolle et al. 2013). If citizens care as much about police intentions as performance, as some criminologists suggest (Tyler and Huo 2002), then concentrating resources in elite units may promote cooperation even before broader security sector reforms are enacted, as the police demonstrate their intention to break with a troubled past.

But the strategy is not without risks. Concentrating resources may fail to deter crime if elite units prove too few or far removed to respond to incidents in a timely manner. Exposure to elite units may also backfire by raising citizens' expectations beyond the capacity of rank-and-file officers to meet them. And even if contact with elite units increases reliance on the police, this may actually exacerbate insecurity if statutory institutions prove imperfect substitutes for customary ones (Isser 2011).

State penetration into areas long accustomed to autonomy may also provoke backlash from those who occupy privileged positions under the status quo (Migdal 1994). Civil war often entrenches informal mechanisms of adjudication and dispute resolution, and citizens and local leaders who benefit from those mechanisms may have no reason to support "increased administrative intrusiveness by central authorities, even if it is justified in terms of an external threat" (Leander 2002, 9). Customary institutions can pursue a variety of strategies to prevent the penetration of statutory ones (Aldashev et al. 2012), reasserting their influence in ways that undermine police effectiveness and discourage citizen cooperation—dynamics on which the existing literature on police-community relations in the West is largely silent.

3 SETTING

3.1 POLICING AFTER CIVIL WAR IN LIBERIA

Liberia has struggled with these challenges since the end of the civil wars that devastated the country from 1989 to 2003. Today, despite over a decade of reform under the auspices of the UN Mission in Liberia (UNMIL), many Liberians continue to perceive the LNP as ill-equipped, inaccessible, and ineffective. Relations between civilians and the LNP were further strained by the 2014-15 Ebola epidemic, which exacerbated, and was exacerbated by, Liberians' distrust of state institutions (Blair, Morse and Tsai 2017). Liberia, in this sense, is an especially "least likely" case for generating trust and cooperation with the police, and our evaluation occurred at an especially "least likely" moment.

During the civil war, Liberians took recourse in customary institutions, which either survived the conflict or were quickly restored thereafter (Sawyer 2005, 59). A variety of customary institutions exist in Liberia, some of which (e.g. chiefs) are legally authorized to resolve petty crimes and non-violent domestic disputes. Others are not recognized under Liberian law, and are entirely unregulated by the Liberian state. Of these, the most powerful are sodalities known as "secret societies" (or "Poro" and "Sande" in our study regions).³ Secret societies have existed in Liberia since before the 17th century, and for most of Liberian history they served as the "most important political institution" (Ellis 1995, 188) and the "dominant social force" in rural communities (Little 1965, 349).

Secret societies are generally considered the peak of the customary hierarchy, with chiefs and elders operating as "purely civic authorities" subject to the "real control" of the societies (Ellis 1995, 188). While their influence waned over the course of the 20th century, they enjoyed a "distinct revival" during the civil war (Ellis 2006, 270), and continue to play a lead role in

³The term "secret society" is in some respects a misnomer (Ellis 1995, 188). All adults know about the existence and purpose of secret societies, and most know who is and is not a member (Little 1965, 349). As Ellis (1995, 188) explains, secrecy is "less an attempt to keep knowledge restricted than to transmit certain messages to members in an esoteric form"—e.g. through rituals and symbols from which non-members are excluded.

conflict resolution in many communities today, where they are expected to be the first and in some cases ultimate arbiter of disputes—an expectation that can extend even to “significant crimes that technically should be referred to the formal court system” (Lubkemann, Isser and Banks III 2011, 213).

While many Liberians perceive informal institutions as viable alternatives to the police and courts, others—especially those without connections to local political power—view the customary sector as “inherently biased against them” (Lubkemann, Isser and Banks III 2011, 219). Moreover, while customary institutions may be effective at resolving petty disputes, they often struggle to address more serious crimes. Some also rely on trial by ordeal, an illegal but still commonly-practiced method for investigating and adjudicating criminal cases.⁴

Aware that biases pervade both the formal and informal sectors, most crime victims opt not to report to either. According to a 2009 survey, out of a total of 3,181 civil cases, only 3% were reported to the formal sector, compared to 38% reported to the informal sector. More striking still, 59% were not reported at all. Similarly, of 1,877 criminal cases, only 2% were reported to a formal venue, 45% to an informal one, and 53% to neither (Isser, Lubkemann and N’Tow 2009). Under-reporting is especially pronounced for sexual violence and domestic abuse—crimes for which state law often conflicts with local norms—and citizens who report these crimes may face serious social sanctions in their communities (Lubkemann, Isser and Banks III 2011, 223). Previous studies have also found that crimes are much more likely to be resolved informally, and much less likely to be reported to the police, when a “powerful” person is involved (Siddiqi and Sandefur 2009, 97).

3.2 REGIONAL JUSTICE AND SECURITY HUBS AND THE CONFIDENCE PATROLS PROGRAM

The Liberian government recently introduced two related policy innovations in an attempt to mitigate these problems. The first is the construction of five Regional Justice and Security Hubs at

⁴The uses and logistics of trial by ordeal vary. In one common variation, suspected criminals are forced to ingest a poison derived from the bark of the sassywood tree. If the suspect vomits the poison before sunrise the following morning, he is deemed innocent; if he does not, or if he dies, he is pronounced guilty (Leeson and Coyne 2012).

strategic locations around the country. Each Hub hosts joint deployments from the courts, the Bureau of Immigration and Naturalization, and—most important for our purposes—the Police Support Unit (PSU), an elite unit of the LNP whose members are better trained and equipped than their rank-and-file counterparts. The Hubs are the cornerstone of the government’s strategy for decentralizing security and justice provision into rural areas, and for consolidating state authority in anticipation of UNMIL’s withdrawal (Cheng-Hopkins and Tah 2013).

Second and related is the “Confidence Patrols” program, which deploys teams of 10-12 PSU officers on recurring visits to towns and villages throughout rural Liberia. During each visit, patrolling officers hold town hall meetings, distribute informational posters about the Hubs and other security sector reforms, and conduct foot patrols to interact with citizens. Each patrol typically lasts several hours; in more distant communities, the officers sometimes spend the night. During the Ebola epidemic the visits also included lessons in Ebola prevention, jointly delivered by government health workers and PSU officers themselves. (We describe the program in further detail below.)

The Confidence Patrols program is grounded in the hope that small deployments of elite officers can improve citizens’ perceptions of the police, increase crime reporting, and enhance security at the local level, even as elite officers continue to be stationed several hours away, and even as the rank-and-file continue to under-perform. This is an increasingly popular approach in post-conflict countries, several of which are implementing or considering similar strategies.⁵ But it is also a gamble, with all of the attending risks and challenges described in Section 2 above.

⁵Similar initiatives include the creation of the Field Force Unit in Uganda, the Task Force in East Timor, and the Crisis Response Unit in Afghanistan.

4 RESEARCH DESIGN

4.1 STUDY SITES

The Confidence Patrols program began shortly after the inauguration of the Regional Justice and Security Hub in Gbarnga, Bong County in 2013, but implementation was sporadic. Our evaluation began in June of the following year. We focused on three counties in particular—Bong, Lofa, and Nimba—which together comprise the jurisdiction of the Gbarnga Hub. (The Gbarnga Hub was the only one of the five to be considered fully operational at the time of the evaluation.) These three counties were especially hard hit during the Liberian civil war (Ellis 2006), and have become focal points for peacebuilding and state consolidation in the post-conflict period.

4.2 SAMPLE & RANDOMIZATION

Eligibility was limited to communities (1) with at least 500 residents, (2) near a usable road, and (3) with limited prior exposure to the program.⁶ These criteria yielded a sample of 74 communities, ranging in population from approximately 500 to 4,000 residents, and located between roughly 1/2 hour and 3 hours from the Gbarnga Hub. Communities were then grouped into nine geographic blocks, with half of the communities in each block randomly assigned to treatment and the other half to control. This resulted in 38 control communities and 36 treatment communities grouped into nine blocks that could be patrolled in a one-to-two day period.⁷

Given that Liberia's roads are unreliable and often impassable in the rain, officers were given the flexibility to patrol within each block in whatever order made the most sense logistically, which varied from month to month. Officers were also free to respond to incident reports in any community in the sample when necessary, treatment or control.⁸ Balance tests are reported in

⁶27 of the 74 communities in our sample (including 15 communities in the control group) had some limited prior exposure to the program.

⁷The randomization procedure assigned half of the communities in each stratum to treatment, rounded down to the nearest whole number. Because two of the strata had an odd number of communities, sample sizes vary across treatment and control.

⁸This occurred only once, in a control community, which remains a control community for purposes of our analysis.

the appendix, and show that treatment and control communities are statistically indistinguishable across a range of socio-demographic indicators from the 2008 Liberian census.

4.3 IMPLEMENTATION

Most treatment communities were visited four or five times over the 14 months of the evaluation. In some cases patrols were delayed or cancelled due to impassable roads; as a result, one community was never visited, and another was visited only twice. (Both of these communities were surveyed at endline and are included as treatment communities in our analysis).⁹ All patrols originated from the Gbarnga Hub. Typically the patrolling officers would visit two to three proximate communities a day, sometimes spending the night. Officers generally stuck to the communities assigned to a given patrol, though in some cases patrols were either cut short due to hazardous road conditions or extended to cover communities that were previously missed. The program was suspended between September 2014 and February 2015 due to the Ebola epidemic, and continued thereafter until September 2015.

While four or five visits over 14 months may seem like a rather weak treatment, qualitative field reports from our Liberian research assistant suggest that the intervention was in fact much stronger than it may appear. The program aimed to affect treatment communities not just through increased police presence, but also through the provision of information that would persist and diffuse throughout the community even in the officers' absence. Moreover, most of the communities in the sample had little exposure to the police before the start of the evaluation. A field report from the community of Kpayaquelleh is typical: "the participants said that it is unusual to see PSU officers visiting them and educating them about what they need to know about their rights."¹⁰

PSU presence was sufficiently unusual that in some places residents expressed fear of the patrolling officers, at least initially. As one town chief explained, "he was first afraid when he was informed that the PSU was in the town. He and some of the town elders panicked.... Now

⁹See the appendix for a timeline of the intervention relative to our endline survey.

¹⁰Kpayaquelleh 2/19/2015.

that they have known the purpose of the visit, they welcome the PSU and will not be afraid.”¹¹ A similar sentiment was expressed in other communities, and is reflected in our endline survey data: roughly half of all treatment group respondents reported feeling scared of the PSU on the first patrol, compared to just 4% who reported feeling scared by endline.

Town hall meetings were usually held in a public place at the center of the community (e.g. a market or school). Attendance ranged from a low of 15 residents to a high of 125. Representatives from the local leadership were always present at the meetings, including the town chief, a youth group leader, a women’s group leader, and/or elders. The PSU began each meeting with two short presentations, the content of which remained more or less constant across communities. The first focused on the Hub and its role in decentralizing access to justice and security, and described how residents could use the Hub to report crimes, file court cases, and register complaints of police misconduct. The second focused on the roles, responsibilities, and capacities of the various units of the LNP. The presentations were usually delivered in English, with one of the patrolling officers translating into the local dialect when necessary.

Officers often made direct, personal appeals during these presentations, urging citizens to trust them, and stressing the difference between the “old” LNP and its newly-reformed counterpart. (We provide illustrative excerpts in the appendix.) Presentations were followed by Q&A, which lead to lively and sometimes lengthy discussions about a range of topics, including legal and procedural issues related to domestic violence, crime reporting, police misconduct, and the appropriate role of Community Watch Forums—youth groups organized with help from the police to facilitate crime reporting. At the end of each meeting, the PSU provided attendees with contact information for the Hub and the PSU commander, then walked the community in small groups to speak with residents in private and solicit questions or concerns. In many cases officers concluded the visit by organizing an informal soccer match with local youths. Each visit lasted two to three hours on average in the early stages of the evaluation, eventually tapering off to an hour and a half in the later stages, as residents became increasingly familiar with the themes of the visits.

¹¹Zolowee 4/6/2015.

5 EMPIRICAL STRATEGY

5.1 HYPOTHESES

We pre-registered 11 hypotheses, nine of which we test here.¹² We hypothesized that the Confidence Patrols program would reduce the cost of cooperating with the police by increasing citizens' knowledge of the LNP (H1) and of Liberian law (H2), and would heighten the benefits of cooperation by improving citizens' perceptions of the LNP (H3). As citizens became more familiar with their legal rights and obligations and more confident in the LNP, we expected crime reporting to increase (H4), reliance on extrajudicial punishment (especially trial by ordeal) to decline (H5), and support for Community Watch Forums to increase (H6). We also hypothesized that the program would reduce the incidence of crime (H7) and enhance security of property rights (H8). To the extent that citizens attributed improvements in the quality of security provision to the Liberian government, we hypothesized that the program would improve perceptions of the state more generally (H9), and potentially boost tax morale (H10). Finally, because the patrolling officers disseminated information about Ebola prevention and treatment, we expected that the program might reduce the incidence of Ebola as well (H11).

We exclude H10 from our analysis here because tax compliance proved to be quite unrelated to the themes of the program, and we exclude H11 because only three patrols were actually conducted before or during the Ebola epidemic. (As we show in the appendix, the program's effects on these latter two outcomes were substantively and statistically insignificant, though treatment communities did report fewer cases of Ebola on average.) We also test one hypothesis that was not pre-specified, namely that the program would improve perceptions of the courts. This hypothesis is motivated by qualitative field reports from our Liberian research assistant, which revealed that patrolling officers repeatedly emphasized disputants' rights to a fair and impartial trial in a court of law.

¹²We pre-registered our hypotheses on the Experiments in Governance and Politics (EGAP) registry shortly after endline data collection had begun, but prior to any data analysis.

5.2 DATA

Our evaluation leverages four sources of data. First, we conducted a survey of all communities in November and December 2015. The survey was implemented by Parley Liberia, a local NGO, and consisted of two instruments, one administered to a sample of 18 randomly-selected adult residents,¹³ and a second administered to five purposively-selected local leaders—typically the town chief, two elders, a women’s group leader, and a youth group leader. We focused on these particular leaders because they were the most frequent attendees at the town hall meetings. Outcomes from the survey are organized into clusters of three to eight dependent variables, each pertaining to one of our 11 hypotheses. Summary statistics and further details on measurement are provided in the appendix.

At the end of the survey, we offered respondents a small “sitting fee” as a token of our appreciation (70 Liberian dollars, or roughly \$1 USD, equivalent to a little under a day’s wage for the average citizen of these three counties). We then gave them the option of donating a portion of their sitting fee to support an existing or potential Watch Forum in their community. Since the PSU repeatedly emphasized the importance of Community Watch Forums during the town hall meetings and Q&A, we interpret these donations as a measure of citizens’ willingness to incur a personal cost to improve coordination between their community and the police.

We further complement our survey with administrative data on all crimes reported to the LNP from any community in our sample between June 2014 and June 2016. Because most of the LNP’s files are not digitized, this involved visiting each LNP depot in the area to collect and transcribe their records. While these records serve as an important complement to our survey, they are also limited in several ways. Incidents are generally only recorded if the complainant or responding officer anticipates that the resulting case will merit court action; the data thus capture only a fraction of the crimes that are actually reported to the police. This, in turn, captures only a fraction of the crimes that occur in these three counties, most of which are never reported to the police at all. We therefore interpret LNP crime records as proxies for crime *reporting*, rather than

¹³Residents were sampled using the random walk method, described in detail in the appendix.

crime per se, though ultimately we cannot disentangle the two. Because survey-based measures allow us to distinguish between these two outcomes, we focus on them here, and report results using LNP crime records in the appendix.

Finally, we hired a Liberian research assistant to shadow the PSU for the duration of the experiment, accompanying the officers on all patrols and keeping a written log of the proceedings, including topics discussed by the officers and questions asked by residents, as well as more general observations about each visit. The research assistant’s presence was unobtrusive: his goal was to produce a record of each patrol with as much detail as possible, but without interfering in the interactions between officers and residents.¹⁴ We use these qualitative field reports to contextualize and inform our quantitative results.

5.3 ESTIMATION

Apart from support for Community Watch Forums, which we operationalize using a single behavioral measure, each of our outcomes comprises a cluster of three to eight dependent variables. To mitigate the possibility of both Type I and Type II errors, and in accordance with our pre-analysis plan, we estimate the Average Effect Size (AES) across all dependent variables within each cluster using the estimator proposed in Clingingsmith, Khwaja and Kremer (2009).¹⁵ AES coefficients are interpreted in terms of standard deviations from the control group mean.

All specifications include stratum fixed effects and individual- and community-level controls. At the individual level, we control for gender, age, household size, tribe, religion, education, and literacy.¹⁶ At the community level we also control for population, mobile phone coverage, an

¹⁴It is possible that the research assistant’s presence affected the outcomes we measure, though given the care he took to remain unobtrusive, we view this as unlikely. It is also possible that his presence encouraged professionalism among patrolling officers, who might have behaved differently in his absence. Given the length of time over which the project developed, we view this as unlikely as well; anecdotally, the officers seem to have acclimated quickly to our research assistant’s presence, describing him as “one of their own.”

¹⁵The AES across J related dependent variables is given by $\tau = \sum_{j=1}^J \frac{\pi_j}{\sigma_j}$, where π_j is the average treatment effect on each dependent variable and σ_j is the standard deviation of dependent variable j in the control group. To test the null hypothesis of no average effect, the effects π_j are jointly estimated using seemingly unrelated regression. The J dependent variables are stacked to compute a variance-covariance matrix for testing the statistical significance of τ , the AES. For further details see Clingingsmith, Khwaja and Kremer (2009).

¹⁶In our pre-analysis plan we proposed controlling for employment and wealth as well, but since these may have

indicator for whether or not there is an LNP depot in the community, and an index of social services available in the community (clinics, schools, wells, latrines, and guesthouses). Community-level controls are gleaned from the 2008 Liberian census, and from our own endline survey of local leaders. Since the latter was conducted after the intervention, we use it only to control for variables that were very unlikely to be affected by treatment. Standard errors are clustered at the community level throughout.

When estimating the program’s impact on crime reporting as measured in the survey, we use a crime-level specification given by

$$y_{civs} = \alpha + \beta T_{vs} + \gamma_s + \mathbf{X}_{ivs}\theta + e_{civs}$$

where y_{civs} indicates whether crime c reported by individual i in community v of stratum s was referred to a statutory forum, customary forum, both, or neither. T_{vs} denotes community-level treatment assignment, \mathbf{X}_{ivs} denotes the individual- and community-level controls listed above, and γ_s denotes stratum fixed effects. Standard errors are clustered at the community level.¹⁷

5.4 SPILLOVER, MEASUREMENT ERROR, AND SOCIAL DESIRABILITY BIAS

Three caveats are warranted, one related to spillover, one to measurement error, and one to social desirability bias. First, our empirical strategy does not allow for the possibility of spillover between treatment and control communities. We believe the risk of spillover to be minimal, however, as most of our communities are distant from one another: the average distance between each treatment community and the nearest control community is 6.8 kilometers, a long way in rural Liberia, where roads are rough and few citizens have easy access to a vehicle. Moreover, our qualitative data suggests that most of the crime in our sample originates from within rather than outside the community. As we will see below, this is especially true of the types of incidents on

been affected by the program, we believe they are better conceptualized as dependent variables rather than controls.

¹⁷Because this analysis includes both crimes against the respondent and crimes that the respondent knew about in the community, the same crime may be counted more than once. Clustering adjusts for this dependence between observations (along with all other forms of dependence within communities).

which we observe the strongest negative treatment effects: land disputes (which almost always involve neighbors) and domestic violence (which almost always involves members of the same household).

Second, it is possible that the program affected the definition or perceived severity of certain types of crime, thereby complicating interpretation of any differences we observe between treatment and control communities. While this is possible, we do not believe it is a cause for concern. For one, the wording of our questions was clear and specific, leaving little room for misinterpretation or ambiguity. Moreover, we suspect that insofar as the program changed how respondents defined particular crimes, it is likely to have *expanded* their definitions (e.g. in the case of rape or domestic violence), making any reductions we observe relative to the control group all the more notable.

Third and relatedly, it is possible that the program simply taught respondents to give socially desirable answers to survey questions, and that the treatment effects we observe are an artifact of this bias. Again, we do not believe this is cause for much concern. We find that the program affected a number of outcomes for which there is no socially desirable response (e.g. knowledge of the Gbarnga Hub), or for which the socially desirable response is unclear, and/or was never addressed by the patrolling officers (e.g. the decision to fallow one's farm land). Moreover, the program appears *not* to have affected outcomes that we would expect to be most susceptible to social desirability bias, especially in the context of Confidence Patrols (e.g. perceptions of the police). These nulls provide some reassurance that the positive effects we observe on other outcomes (e.g. crime reporting) are not merely artifacts of social desirability bias.

6 RESULTS

6.1 INCREASED KNOWLEDGE OF THE POLICE AND LIBERIAN LAW, REDUCED CRIME, AND IMPROVED SECURITY OF PROPERTY OF RIGHTS

Figure 1 plots the AES for each of our outcome clusters. The program had small and substantively insignificant effects on several key outcomes. Most notably, it did not improve Liberians' perceptions of the police, courts, or government, and did not increase preferences for the police in hypothetical scenarios of crime and violence. It had a substantively large but statistically insignificant negative effect on self-reported reliance on trial by ordeal, and an equally substantively large but imprecisely estimated positive effect on donations to the Community Watch Forum. The program did not reduce the incidence of crime overall, though as we discuss below, this null AES masks variation in the average treatment effect on particular categories of crime.

The program did, however, increase knowledge of the police, largely by increasing awareness of the Gbarnga Hub and the services it provides. The program also modestly but statistically significantly improved knowledge of Liberian law. Residents of treatment communities were four percentage points more likely to know that detainees must be released after 48 hours if no criminal complaint is filed; four percentage points more likely to know that trial by ordeal is illegal; and two percentage points more likely to know that Liberian law does not permit citizens to physically harm suspects—an important result given the high incidence of mob violence in rural areas. These improvements amount to increases of 6%, 7%, and 4%, respectively, relative to their control group means.

Most striking, the program significantly improved security of property rights across most of the indicators we measured. Residents of treatment communities were four percentage points and six percentage points more likely to feel secure about their household plots and farmlands, respectively (increases of 3% and 6% relative to their control group means); five percentage points (or 6%) less likely to be involved in an ongoing dispute over their farmlands; six percentage points

(21%) more likely to report making costly improvements to their house plots in the past year; and five percentage points (6%) more likely to report plans to fallow their farmland in the next year—an important proxy for security of property rights in sub-Saharan Africa, as many farmers neglect to fallow out of fear that others will appropriate their land if they do (Goldstein and Udry 2008). We view these effects as especially promising given the continued prevalence of land disputes in rural Liberia, and the persistent threat of violence they pose (Blattman, Hartman and Blair 2014). Because patrolling officers never mentioned outcomes like fallowing or household improvements in their presentations, it is unlikely that these results are an artifact of social desirability bias.

The null AES on crime in general masks important variation across specific categories of crime. Figure 2 reports average treatment effects on the incidence of armed robbery, theft and burglary, aggravated assault, simple assault,¹⁸ domestic violence, and rape. While the program did not reduce the incidence of aggravated assault, armed robbery, rape, or theft and burglary, it did reduce the incidence of simple assault by five percentage points, and domestic violence by seven percentage points. Relative to our control group, these amount to substantively large reductions of 50% and 17%, respectively. We view these effects as especially important given the pervasiveness of domestic violence in particular in rural Liberia. The null effects on aggravated assault, armed robbery, and rape may reflect the low (reported) incidence of these crimes in control communities, suggesting that the nulls may be a function of floor effects.

6.2 INCREASED CRIME REPORTING

For crimes that did occur, Table 1 displays the program’s impact on reporting to the formal sector, informal sector, both, or neither. The program reduced the likelihood that crimes would go unreported by five percentage points, and increased the likelihood of reporting to the police or courts by a corresponding six percentage points. Both of these effects are driven by felonies rather than misdemeanors:¹⁹ felonies in treatment communities were 12 percentage points less likely to go un-

¹⁸Simple assault in Liberia is analogous to battery in the US. It involves causing bodily injury without a weapon, and is a misdemeanor. Aggravated assault involves a weapon, and is a felony.

¹⁹Felonies include aggravated assault, armed robbery, rape, and violence or property destruction related to land conflicts. Misdemeanors include simple assault, burglary, theft, and non-violent land conflicts. While domestic violence

reported, and 16 percentage points more likely to be reported to the police or courts. The program had no effect on reporting to the informal sector for either category of crime.

These are substantively large and practically meaningful effects. Calculations based on the average population of our sample communities (about 1,000) suggest that these changes equate to roughly 11 fewer unreported felonies per community per year.²⁰ Notably, this reduction was *not* accompanied by an increase in reporting of misdemeanors to the statutory sector—an important finding in and of itself, given that the police and courts are already severely over-burdened and under-resourced, and must therefore prioritize the most serious criminal cases.

6.3 HETEROGENEOUS TREATMENT EFFECTS BY SECRET SOCIETY MEMBERSHIP

Improving access to state security and justice institutions should be especially beneficial to those who are disadvantaged under customary regimes (Aldashev et al. 2012; Sandefur and Siddiqi 2013). We explore this possibility by testing for heterogeneous treatment effects along four indicators of status within the customary sector in Liberia: gender, age, ethnicity, and membership in a secret society.²¹ Of these four, we interpret society membership as the most direct proxy for disadvantage, since society members tend to be favored over non-members *regardless* of their other demographic characteristics (Isser, Lubkemann and N'Tow 2009).

Figure 3 displays heterogeneous treatment effects for each of our outcome clusters; Table 2 replicates this analysis for crime reporting, focusing on secret society membership alone. (Analogous results on crime reporting for other sub-groups are in the appendix, and are uniformly null.) In general, we find little evidence of treatment effect heterogeneity, with one important exception.

does not technically appear in Liberia's penal codes—a bill to include it has been stalled in the legislature for years—it is typically prosecuted as a misdemeanor (when it is prosecuted at all). Our results are substantively similar when we instead distinguish between violent and non-violent crime.

²⁰On average, residents were a victim of 0.09 felonies in the past year. This implies there were roughly $0.07 \times 1,000 = 90$ such incidents per community per year. The program reduced the proportion of these incidents that went unreported by about 12 percentage points, which equates to roughly 11 fewer unreported incidents.

²¹This last analysis was not pre-specified. It is, however, consistent with the rest of our pre-analysis plan, which proposed to assess status within the customary sector as a source of treatment effect heterogeneity. We report additional pre-specified heterogeneity analyses in the appendix.

Non-society members are the only sub-group for which we find a statistically significant increase in donations to Community Watch Forums; they are also the only sub-group for which we observe an improvement in perceptions of the police (though this effect is not quite statistically significant at conventional levels— $p = 0.12$ —and is not statistically different from the null among society members).

More importantly, and perhaps not coincidentally, we find that the increase in crime reporting is concentrated almost entirely among non-society members. Substantively, the program increased the likelihood that a non-society crime victim would report to the police or courts by 14 percentage points. This effect is especially pronounced for felonies—non-society victims of felonies are a full 52 percentage points more likely to report to the police or courts—and is driven by a corresponding reduction in the likelihood of refusing to report at all. The program had no impact on the likelihood of reporting among society members, regardless of the severity of the crime. We discuss these results in further detail in Sections 7.4 and 7.5 below.

7 DISCUSSION

Our quantitative results suggest that the Confidence Patrols program improved knowledge of the police and Liberian law, reduced the incidence of some types of crime, and enhanced security of property rights. The program also increased reporting of felony offenses, but did not improve citizens' rather bleak perceptions of the police, courts, or government more generally. In this section we use our qualitative data to inform and substantiate our interpretation of the quantitative results, focusing on five outcomes in particular: (1) crime reporting; (2) domestic violence and security of property rights; (3) perceptions of the police; (4) access to statutory institutions for those disadvantaged under customary regimes; and (5) backlash by the customary sector itself. We address additional themes from the qualitative data in the appendix.

7.1 DECREASED EXPECTED COSTS OF REPORTING

In Section 2 we characterize the decision to cooperate with the police as a function of the expected costs and benefits of cooperation. We interpret our results as suggesting that the Confidence Patrols program lowered these expected costs, without necessarily increasing expected benefits. Our qualitative data corroborate this interpretation. Patrolling officers repeatedly urged citizens to report crimes to the police, and provided contact and logistical information that should have mitigated the obstacles to doing so.

Officers also answered basic factual questions about Liberian law, such as whether the police are legally required to detain witnesses as suspects (they are not);²² whether members of Community Watch Forums are legally authorized to “tie criminals with rope” (they are not)²³ or “judge cases and charge people to pay money” (they are not);²⁴ or whether sexual and gender-based violence (SGBV) can legally be “compromised” outside of court (it cannot).²⁵ Our quantitative results suggest that residents absorbed these lessons, and that increased knowledge of the police and Liberian law helped lower the perceived barriers to reporting.

7.2 DECREASED CRIME AND INCREASED SECURITY OF PROPERTY RIGHTS

Reduced costs of reporting may also help explain the reduction we observe in the incidence of some types of crime, as well as the improvement in security of property rights. Greater knowledge of the police and courts may have provided residents with a sense of safety, while the recognition that other residents also have this knowledge may have deterred potential criminals from committing crimes in the first place (particularly crimes for which the perpetrator can be readily identified, such as domestic violence).

Our qualitative data provide further insight into these results. Property disputes were re-

²²Wainsue 2/6/2015; Gbenequelle 3/3/2015; Wainsue 3/19/15; Jinnepeleta 3/19/15; Konia 4/11/2015.

²³Tomato Camp 2/6/15; Jinnepeleta 3/19/15; Wainsue 3/19/15; Gbedin 4/6/15.

²⁴Loyee 8/1/15.

²⁵Wainsue 2/6/2015; Beyan 4/10/15; Konia 4/11/2015; Ganglota 4/13/15; Gbenequellah 4/20/15; Flumpa 8/1/2015; Bunadin 8/2/15; Kpayaquellah 8/10/15.

curing concerns in our treatment communities. Citizens requested guidance on resolving land disputes²⁶ and on preventing them from escalating into violence or property destruction.²⁷ Citizens also asked about theft of livestock—a common crime in rural Liberia.²⁸ The officers provided often-detailed guidance in these cases, advising against vigilantism, directing disputants to the Land Commission or Land Coordination Center, and reassuring them that the police would intervene in the case of escalation.

Questions about domestic abuse and sexual and gender-based violence (SGBV) were even more common. Town hall meetings almost always included lectures on these topics, typically delivered by one of the female officers, who emphasized that domestic violence is an offense for which perpetrators can expect to be prosecuted. The officers also discouraged husbands from abusing their wives,²⁹ and alerted past or potential victims to the existence of a dedicated SGBV office at the Gbarnga Hub, and of a Women and Children Protection Section at most police stations. While the LNP's capacity to respond to these incidents is limited, domestic violence and land disputes are cases in which the perpetrator can be readily identified—land disputes typically involve members of the same community, and domestic violence usually involves members of the same household—making them easier to solve, even for the severely resource-constrained LNP.

7.3 NULL EFFECT ON PERCEPTIONS

Contrary to our expectations, we find that the Confidence Patrols program had no effect on citizens' perceptions of the police, courts, or Liberian government more generally. There are a number of potential explanations for these null results. Perhaps the program changed perceptions of the PSU but not the LNP as a whole. (Unfortunately we only measure the latter, and so cannot test this possibility.) Or perhaps it improved perceptions of the LNP's capacity to address particular types of (violent) crime, without changing assessments of the force overall. (We cannot test this possibility either, though we find no evidence that the program strengthened citizens' preferences

²⁶Zarsonnoh 4/5/15; Zuluyee 4/7/15.

²⁷Doe Town 2/7/15

²⁸Tomato Camp 3/18/15; Flumpa 4/4/15; Gbahn 8/2/15; Tassah 3/18/15.

²⁹Kpaiyea 8/8/2015; Salayea 8/8/15.

for the LNP in hypothetical scenarios of violent crime.) As we discuss in the appendix, patrolling officers repeatedly emphasized the severe capacity constraints under which the LNP operates;³⁰ perhaps civilians internalized this message. Or perhaps perceptions are simply sticky and hard to change.

Alternatively, perhaps exposure to an elite subunit raised citizens' expectations beyond the capacity of the rank-and-file LNP to meet them, such that perceptions improved after treatment, but regressed to the mean by the time of endline data collection. While we cannot test this interpretation directly, we do observe some patterns that are consistent with it: only about half (56%) of crime victims who reported to the LNP in the past year described themselves as satisfied with the LNP's response, and crime victims in the treatment group were about 10 percentage points less likely to be satisfied than those in the control group (though this difference is not statistically significant). But these results are only suggestive: we do not know whether the decision to report was preceded or followed by a change in perceptions of the LNP, nor whether dissatisfaction deterred future reporting.

7.4 ACCESS TO THE STATUTORY SECTOR FOR NON-SOCIETY MEMBERS

Perhaps our most intriguing finding is that the program's impact on crime reporting is concentrated almost entirely among those who do not belong to one of Liberia's powerful secret societies. In our view, the most likely explanation for this result lies in the tendency of secret societies (and other customary institutions) to discriminate against non-members. While membership in secret societies is widespread—over 75% in our sample—women, youths, and minority groups (especially Muslims and Pentecostal Christians) tend to be excluded, as do “strangers” who migrate from other communities as adults.

Historically, those who did not belong to secret societies were considered “ceremonially unclean,” and members could “ride roughshod” over them (Little 1965, 358). Similar dynamics persist today, and many non-members believe that customary institutions are biased and thus “in-

³⁰Dean Town 2/5/2015; Galai Town 2/5/2015; Zowienta 4/21/2015.

capable of providing justice;” as one respondent cited in Isser, Lubkemann and N’Tow (2009, 36) categorically explains, “anyone who is not a member of the Poro [secret] society will never be right.” Perhaps as a result, non-members in our control group were eleven percentage points less likely to report felonies to customary authorities, and sixteen percentage points more likely not to report them at all.

Our results suggest that the Confidence Patrols program provided an “exit option” for these individuals.³¹ This interpretation is consistent with our qualitative data. In many communities, disputants complained of customary authorities “playing” with their cases;³² in response, patrolling officers encouraged them to report to the police, courts, or magistrates instead. They also admonished communities not to rely on customary practices that are known to cause tension between secret society members and non-members.³³ Our interpretation is also consistent with anthropological studies showing that where the police and courts are present, disputants who are not members of secret societies often seek “additional review and recourse from a more neutral, and usually higher, authority” (Isser, Lubkemann and N’Tow 2009, 45).

A remaining puzzle is why we observe increased reporting among non-society members, but not among women, youths, and ethnic or religious minorities—groups that are also believed to be disadvantaged under customary law. In our sample, however, youths and minorities are no more or less likely to report crimes to customary authorities, suggesting that whatever discrimination they face is not sufficient to deter them from relying on the customary sector. (Women are less likely to report to *either* sector—a pattern that the program seems not to have changed.) Moreover, our heterogeneous treatment effects by society membership hold even after we control for these other demographic characteristics (and for their interaction with treatment assignment). Our finding that the program had no differential impact on women may suggest that it failed to provide them with a satisfactory exit option, or that women are simply less willing to report crimes in general, even

³¹A potential alternative explanation lies not in the disadvantages faced by non-members, but rather in social norms that discourage members from reporting crimes to statutory rather than customary forums. This explanation, however, cannot account for the reduction we observe in not reporting crimes at all.

³²Yila 3/4/15; Tukpah Town 3/4/15; Galai 3/18/15; Kpein 4/7/15; Beyan 4/10/15; Zarsonnoh 8/2/15.

³³Gbarlorkpala 3/3/15.

with increased police presence. Regardless, society membership appears to be an important driver of treatment effect heterogeneity, even controlling for other potential sources of status.

7.5 BACKLASH FROM THE CUSTOMARY SECTOR

Attempts to project state power into communities governed first and foremost by customary institutions can provoke backlash from those most invested in the status quo (Migdal 1988; 1994). Though we did not anticipate testing for this possibility, our survey did include several questions that allow us to do so.³⁴ First, we asked respondents whether they believed their neighbors would “get vexed” if (hypothetical) burglaries or incidents of domestic violence were reported to the police. We interpret these as proxies for the strength of social norms against reporting.

Second, we asked respondents whether the “bush devil” had appeared in their community in the past year. The bush devil (typically a man in costume) is a ceremonial figure associated with secret societies who emerges to restore social order in times of unrest; when he appears, non-society members are required to retreat indoors, and if they refuse (as many do), conflict is often the result (Isser, Lubkemann and N’Tow 2009, 33). Patrolling officers explicitly discouraged this practice,³⁵ and we interpret its persistence as a proxy for social tension, and for attempts by secret societies to reassert their authority.

Table 3 reports average treatment effects on these outcomes. Residents of treatment communities were eight percentage points more likely to expect social sanctions for reporting to the police in cases of burglary and domestic violence (increases of 18% and 15%, respectively, relative to the control group means). Notably, we do not find evidence of heterogeneous treatment effects on these outcomes by secret society membership, suggesting that the program heightened perceptions of social sanctions among members and non-members alike. Concerns about social sanctions appear repeatedly in our qualitative data as well, especially around cases of domestic violence and sexual assault.³⁶ Residents of treatment communities were also five percentage points

³⁴These analyses were not pre-specified.

³⁵Gbarlorkpala 3/3/15.

³⁶Wainsue 2/6/2015; Ganglota 4/13/15; Gbenequellah 4/20/15; Flumpa 8/1/15; Bunadin 8/2/15; Kpayaquellah

more likely to report appearances of the bush devil in the past year (a 40% increase relative to the control group, though only weakly statistically significant). Indeed, in one community, the bush devil appeared to protest the presence of patrolling officers themselves—an incident that required several hours of mediation to defuse.³⁷

8 CONCLUSION

Weak and war-torn states often struggle to (re)build citizens’ willingness to rely on the police and courts when crimes are committed or disputes arise. Resistance to police presence is often especially marked in rural areas long accustomed to state absence or abuse. Fearing that the costs of accessing the police will be high and the benefits low, many victims and witnesses opt not to report at all, or to rely on customary mechanisms for dispute resolution instead. These mechanisms can be effective, accessible, and inexpensive, but they also tend to discriminate against historically marginalized groups. Customary authorities can also actively attempt to undermine the police, heightening the risk of conflict as the state projects power nationwide (Aldashev et al. 2012).

We experimentally evaluate a central component of the Liberian government’s ongoing efforts to expand the presence of state security and justice institutions in rural areas. We find that recurring “Confidence Patrols” by elite police officers enhanced security of property rights, reduced the incidence of some types of crime, and increased reporting of felonies to the statutory sector, especially among those who are disadvantaged under customary regimes. We also find, however, that the program did not mitigate citizens’ distrust of the police, courts, or government more generally, and that it seems to have provoked some backlash from the customary sector, while aggravating social norms against cooperation with the police.

From a policy perspective, our results suggest that better-trained, better-equipped police officers can effectively deter (some types of) crime and improve security of property rights, even when elite units are based several hours away, and even when rank-and-file officers continue to languish

8/10/15.

³⁷Zowienta 8/9/14.

under severe resource constraints. Increased police presence can also help mitigate inequities under prevailing customary regimes. But our findings also suggest that post-conflict police forces should anticipate resistance to their presence, especially initially. Projecting state authority into communities long accustomed to autonomy is often a contentious process, one that may disrupt existing power dynamics and provoke backlash from those whose interests are threatened by an encroaching state. Whether and how these dynamics will resolve as the Liberian government continues to expand nationwide, and as the UN retreats, remains to be seen. We leave this question for future research to address.

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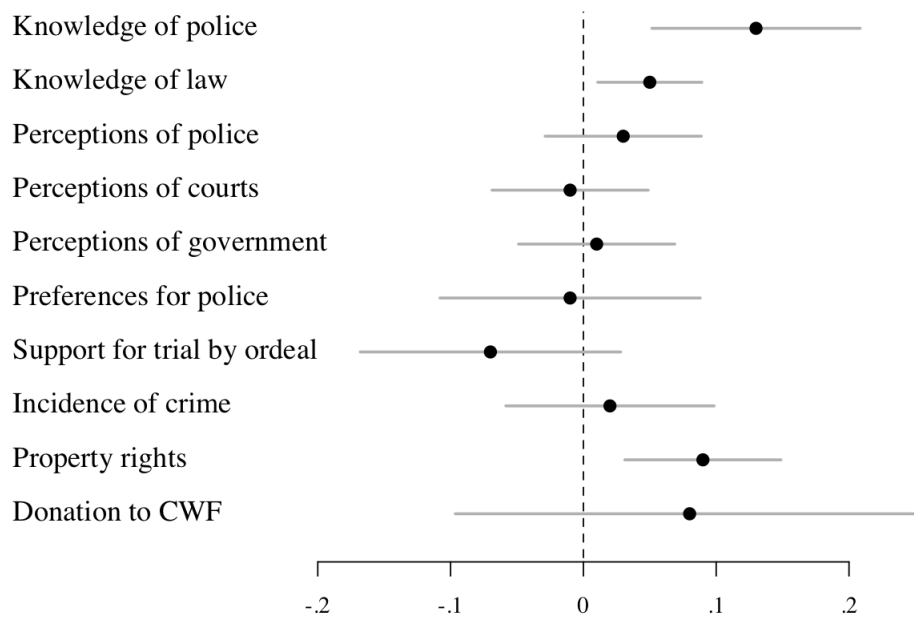
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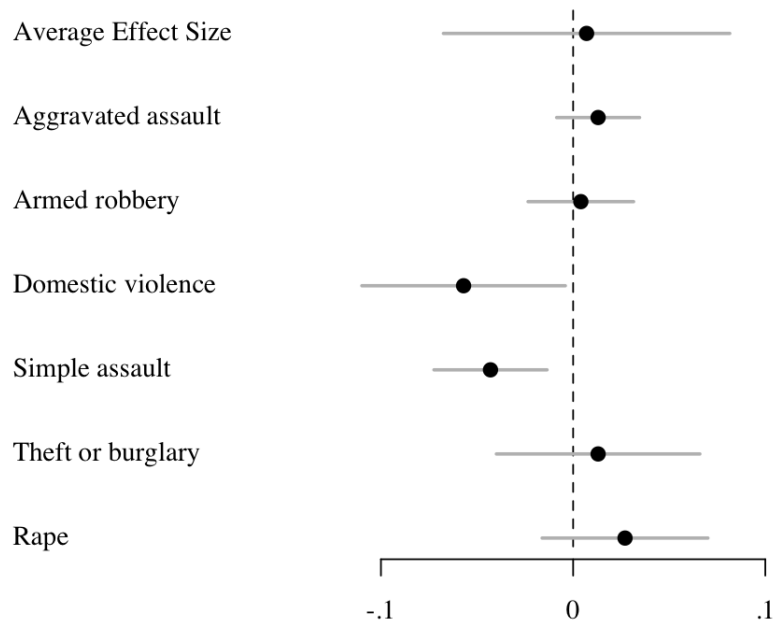
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Figure 1: Average Effect Size by outcome cluster



Notes: Average Effect Sizes (AES) for each cluster of outcomes. AES coefficients are interpreted in terms of standard deviations from the control group mean.

Figure 2: Effects on crime disaggregated by category



Notes: Average Effect Size (AES) on all categories of crime and average treatment effects (ATE) on crime disaggregated by category. AES coefficients are interpreted in terms of standard deviations from the control group mean.

Table 1: Effects on reporting to statutory vs. customary authorities

	Nowhere			Statutory only			Customary only			Statutory & customary		
All crimes												
Treatment	-0.05*			0.06**			-0.01			0.00		
	[0.03]			[0.03]			[0.03]			[0.01]		
Felonies												
Treatment	-0.12**			0.16**			-0.05			0.01		
	[0.06]			[0.07]			[0.05]			[0.03]		
Misdemeanors												
Treatment	-0.03			0.03			-0.00			-0.00		
	[0.03]			[0.02]			[0.03]			[0.01]		
Control group mean	.45	.45	.45	.16	.32	.16	.33	.18	.35	.05	.04	.05
Observations	1791	260	1531	1791	260	1531	1791	260	1531	1791	260	1531

Notes: Standard errors, clustered by community, in brackets. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 2: Heterogeneous treatment effects on crime reporting by secret society membership

	Nowhere	Statutory only	Customary only	Statutory & customary
All crimes				
Treatment	0.03 [0.04]	-0.03 [0.03]	-0.00 [0.05]	0.00 [0.01]
Treatment × not in society	-0.17* [0.09]	0.17** [0.07]	-0.02 [0.08]	0.01 [0.03]
Not in society	0.06 [0.07]	-0.02 [0.05]	-0.04 [0.06]	0.00 [0.01]
Observations	926	926	926	926
Felonies				
Treatment	-0.07 [0.11]	0.04 [0.09]	0.01 [0.10]	0.02 [0.05]
Treatment × not in society	-0.46*** [0.16]	0.48*** [0.18]	-0.02 [0.18]	0.00 [0.05]
Not in society	0.13 [0.13]	-0.09 [0.13]	0.02 [0.11]	-0.05 [0.04]
Observations	125	125	125	125
Misdemeanors				
Treatment	0.04 [0.04]	-0.03 [0.03]	-0.01 [0.05]	-0.00 [0.02]
Treatment × not in society	-0.10 [0.11]	0.10+ [0.06]	-0.02 [0.09]	0.02 [0.03]
Not in society	0.04 [0.08]	0.00 [0.04]	-0.05 [0.07]	0.01 [0.02]
Observations	801	801	801	801

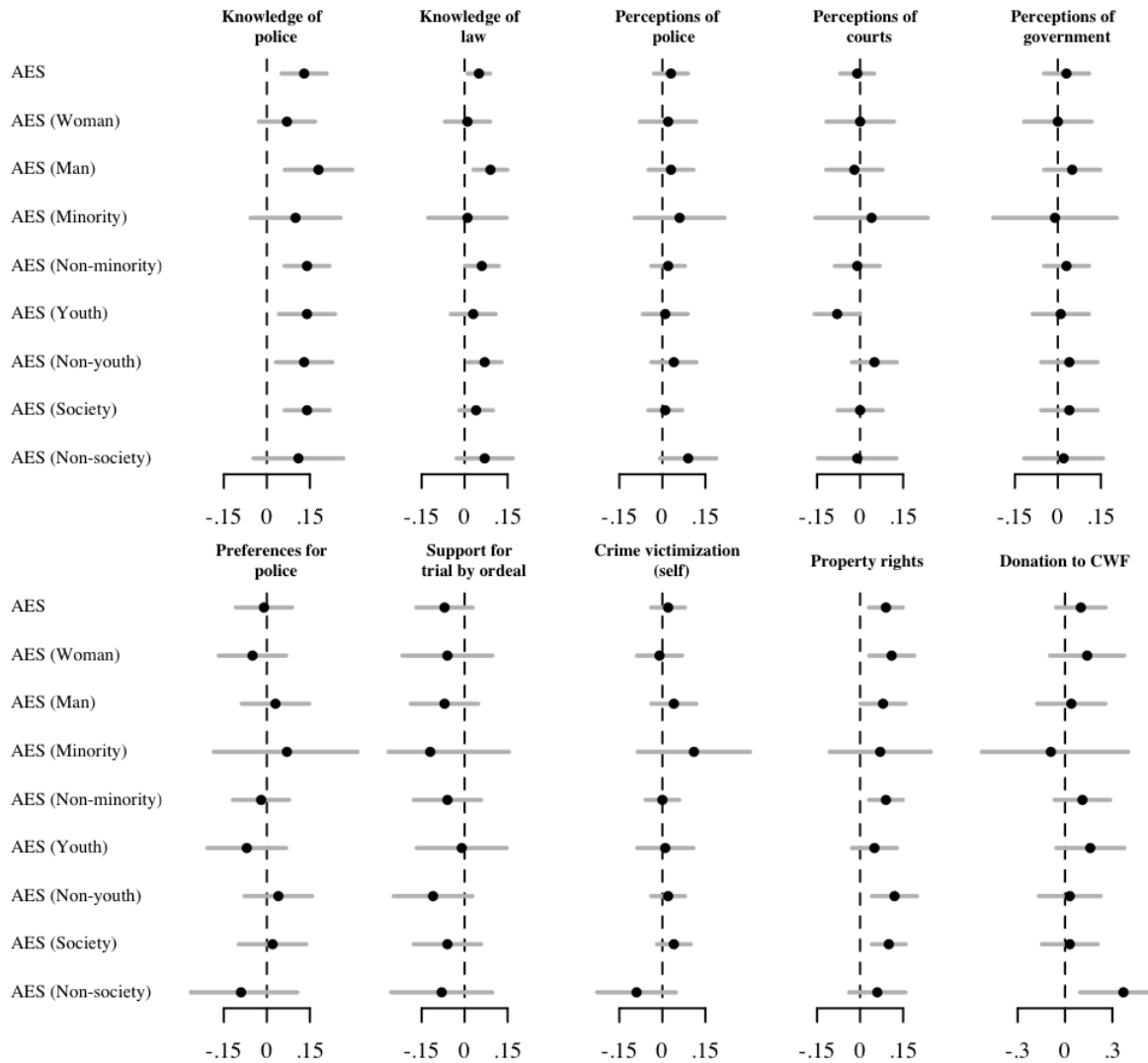
Notes: Standard errors, clustered by community, in brackets. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3: Effects on social sanctions & appearances of bush devil

	Reporting domestic abuse is controversial	Reporting burglary is controversial	Bush devil appeared in past year
Treatment	0.08** [0.03]	0.08** [0.04]	0.04* [0.02]
Control group mean	0.55	0.44	0.10
Observations	1,284	1,284	1331

Notes: Control group means are covariate-adjusted. Standard errors, clustered by community, in brackets. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Figure 3: Heterogeneous treatment effects



Notes: Average Effect Sizes (AES) for each cluster of outcomes disaggregated by subgroup. Each pair of coefficients is derived from a separate regression. AES coefficients are interpreted in terms of standard deviations from the control group mean.

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

Online Appendix

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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 (1) with at least 500 residents, (2) near a usable road, and (3) with limited prior exposure to the program.

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. Around 30% were patrolled a fifth time. Five communities (about 14% of the treatment group) were patrolled only three times.

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 originated. 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 endline survey to measure eight families of outcomes. To measure **knowledge of Liberian law**, we asked respondents about their and the government's legal obligations in each

of seven hypothetical scenarios.³⁸) To measure **knowledge of the police**, we asked respondents 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,³⁹ simple assault, aggravated assault,⁴⁰ and domestic violence.⁴¹ 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

³⁸For 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 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.)

³⁹Theft 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).

⁴⁰Under 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.

⁴¹Domestic 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 typically prosecuted as a misdemeanor akin to simple assault.

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 the LNP’s custody).

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 2017). 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

Tables A.1 and A.2 summarize 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 25% 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 robbery (67%) and murder (69%), and weakest for domestic violence (20%). Support for illegal or extrajudicial mechanisms of dispute resolution varied between 18-26%, depending on the question. Whether respondents referred actual cases to the police also varied. Only 5% reported taking a case to the police, whereas 15% reported taking a case to the town chief. But 29% of cases eventually went to court, suggesting that local leaders often referred victims' claims to formal venues.

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% and 76%, respectively), 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 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.7 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.8 EFFECTS ON CRIME REPORTING USING LNP DATA

In the paper we estimate ATEs on crime reporting 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

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

where y_{vsm} indicates the total number of crimes reported from community v of stratum s in month m . T_{vs} again denotes community-level treatment assignment, $post_{ms}$ denotes the number of months since the first patrol in stratum m , \mathbf{X}_{vsm} denotes our community-level controls, and γ_s denotes stratum fixed effects. Because our outcomes are counts, and to adjust for potential overdispersion, 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 the 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.

A.9 HETEROGENEOUS TREATMENT EFFECTS ON CRIME REPORTING

Tables A.4, A.5, and A.6 report heterogeneous treatment effects on our survey-based measure of crime reporting by gender, ethnicity, and age, respectively. In general we do not find any evidence of heterogeneity along these dimensions.

A.10 ADDITIONAL PRE-SPECIFIED HETEROGENEOUS TREATMENT 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.7. 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. Tables A.8 through A.11 report results for the remaining analyses. In general we do not find any evidence of heterogeneity along these dimensions.

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.

A.11 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.11.1 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 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.⁴²

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.”⁴³

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

⁴²Yila Town, 2/4/15.

⁴³Yila Town, 2/4/15

have trust and confidence in us and work together with us as UNMIL plans to leave.”⁴⁴ 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.”⁴⁵

The officers also allowed citizens to air grievances against the LNP and PSU,⁴⁶ and to provide the officers with feedback, including on their own performance during the patrols.⁴⁷ Typical of community policing, they addressed a wide variety of questions and concerns during the Q&A, many of them unrelated to policing per se. For example, patrolling officers offered advice to a father whose two young daughters were “stubborn and causing [him] many problems;”⁴⁸ 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];”⁴⁹ to a husband whose wife “can’t respect him, and has the constant habit of refusing him in bed;”⁵⁰ and to a wife whose husband “does not want to support the children and [her].”⁵¹ The officers offered advice in all of these cases, often stepping out of their role as police officers to do so.⁵²

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

⁴⁴Bunadin 4/5/2015.

⁴⁵Yila Town, 2/4/15.

⁴⁶Zolowee 4/6/2015; Jinnepeleta 8/20/2015

⁴⁷Kpayaquelleh 2/19/2015; Gbenequelle 3/3/2015.

⁴⁸Zowienta 4/21/2015.

⁴⁹Tassah 2/6/2015.

⁵⁰Salayea 8/8/2015; see also Tukpah 3/4/2015; Kpaiyea 8/8/2015.

⁵¹Tassah 2/6/2015.

⁵²For 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).

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 practice⁵³—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.”⁵⁴ Moreover, 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 ineffectively.”⁵⁵ 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.”⁵⁶ 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.”⁵⁷

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?”⁵⁸

“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,

⁵³Yila Town 2/4/2015; Galai Town 2/5/2015; Kollie-Ta 2/6/2015.

⁵⁴Galai Town 2/5/2015.

⁵⁵Zowienta 4/21/2015.

⁵⁶Dean Town 2/5/2015.

⁵⁷Zowienta 4/21/2015.

⁵⁸Turkpah Town 2/7/2015

we see these people back in the town doing the same business. How can we solve this problem?”⁵⁹

In some cases, suspects 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 the victim (or the victim’s lawyer) fails to register a formal complaint against them.

The officers were sometimes quite 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 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.”⁶⁰ 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.”⁶¹ 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.”⁶²

A.11.2 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.⁶³

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

⁵⁹Dean Town 2/5/2015.

⁶⁰Turkpah Town 2/7/2015.

⁶¹Gbarlorkpala 3/3/2015.

⁶²Turkpah Town 2/7/2015.

⁶³Gbanway 4/11/2015.

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 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.”⁶⁴

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 answered 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 community level.⁶⁵

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

⁶⁴Wainsue 2/6/2015.

⁶⁵Flumpa 8/1/2015.

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.”⁶⁶ 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.”⁶⁷ (Of course, these promises may have also raised citizens’ expectations of the police and courts to unreasonable levels.)

A.11.3 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,⁶⁸ nor to torture, kill, or unlawfully detain those suspected of more serious crimes, lest they themselves be charged with assault.⁶⁹ 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.”⁷⁰

A.11.4 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,”⁷¹ responsible for being “vigilant of incoming criminals” and keeping

⁶⁶Turkpah 2/7/2015.

⁶⁷Doe Town 3/4/2015.

⁶⁸Gbahn 8/2/2015.

⁶⁹Tassah 2/6/15.

⁷⁰Neignbain 2/15/2015.

⁷¹Gbarlorkpala 3/3/2015.

“watch over the town to protect and keep the town safe.”⁷² They also emphasized that members of Community Watch Forums are volunteers: they are doing “voluntary service, and the community should assist them.”⁷³ The officers also discouraged members of Community Watch Forums from engaging in vigilantism themselves.⁷⁴

A.11.5 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);⁷⁵ 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”).⁷⁶

Citizens also asked whether the police are required by law to detain witnesses as suspects in criminal cases (they are not);⁷⁷ whether victims are legally obliged to pay to transport police investigators to and from the scene of a crime (they are not);⁷⁸ whether “there is a penalty for sexual assault or harassment” (there is);⁷⁹ and whether rape or other cases of SGBV can legally be settled (or “compromised”) outside of court (they cannot).⁸⁰

A.11.6 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

⁷²Gbahn 8/2/2015.

⁷³Loyee 8/1/2015.

⁷⁴Gbahn 8/2/2015.

⁷⁵Zowienta 2/4/2015.

⁷⁶Tassah 2/6/2015.

⁷⁷Wainsue 2/6/2015; Gbenequelle 3/3/2015; Konia 4/11/2015.

⁷⁸Yila Town 2/4/2015; Galai Town 2/5/2015; Dean Town 2/5/2015; Kollie-Ta 2/6/2015.

⁷⁹Kpaiyee 8/8/2015.

⁸⁰Wainsue, 2/6/2015 Konia 4/11/2015; Flumpa 8/1/2015.

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?”⁸¹ Another said that he “noticed that [his] wife has a boyfriend,” and asked whether he could call the police to “intervene.”⁸²

In other cases citizens asked about the division of labor between the police and courts;⁸³ between the police and the town chief;⁸⁴ or between the police and the Community Watch Forums (which are authorized to assist the police but not to make arrests or adjudicate crimes).⁸⁵ 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?”;⁸⁶ “If we caught a criminal in our town, what should we do to him?”;⁸⁷ or “If someone is causing serious problems in the town, and the person does not want to change, what do we need to do?”⁸⁸

A.11.7 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.⁸⁹ They delivered similar messages about rape and other forms of

⁸¹Yila 3/4/2015.

⁸²Yila 3/4/2015.

⁸³Kpayea 4/11/2015; Dean Town 4/21/2015.

⁸⁴Tukpah 3/4/2015.

⁸⁵Loyee 8/1/2015.

⁸⁶Gbalorkpala 2/5/2015.

⁸⁷Jinnepelata 2/6/2015.

⁸⁸Yila 2/4/2015.

⁸⁹Kpaiyea 8/8/2015.

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.⁹⁰

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 Hub in Gbarnga. On several occasions residents specifically mentioned that domestic abuse and SGBV had declined as a result of the program.⁹¹ 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.”⁹²

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.⁹³

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.”⁹⁴ 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

⁹⁰Konia 4/11/2015; Ganglota 4/13/2015; Flumpa 8/1/2015.

⁹¹Gbenequelle 3/3/2015; Tukpah 4/20/2015; Wainsue 4/23/2015.

⁹²Gbenequelle 3/3/2015.

⁹³Jinnepeleta 2/6/15.

⁹⁴Doe Town 2/7/2015.

ones may have helped reassure property owners that conflicts over land use or boundaries would not be allowed to spiral out of control.

Figure A.1: Map of Liberia and sample communities

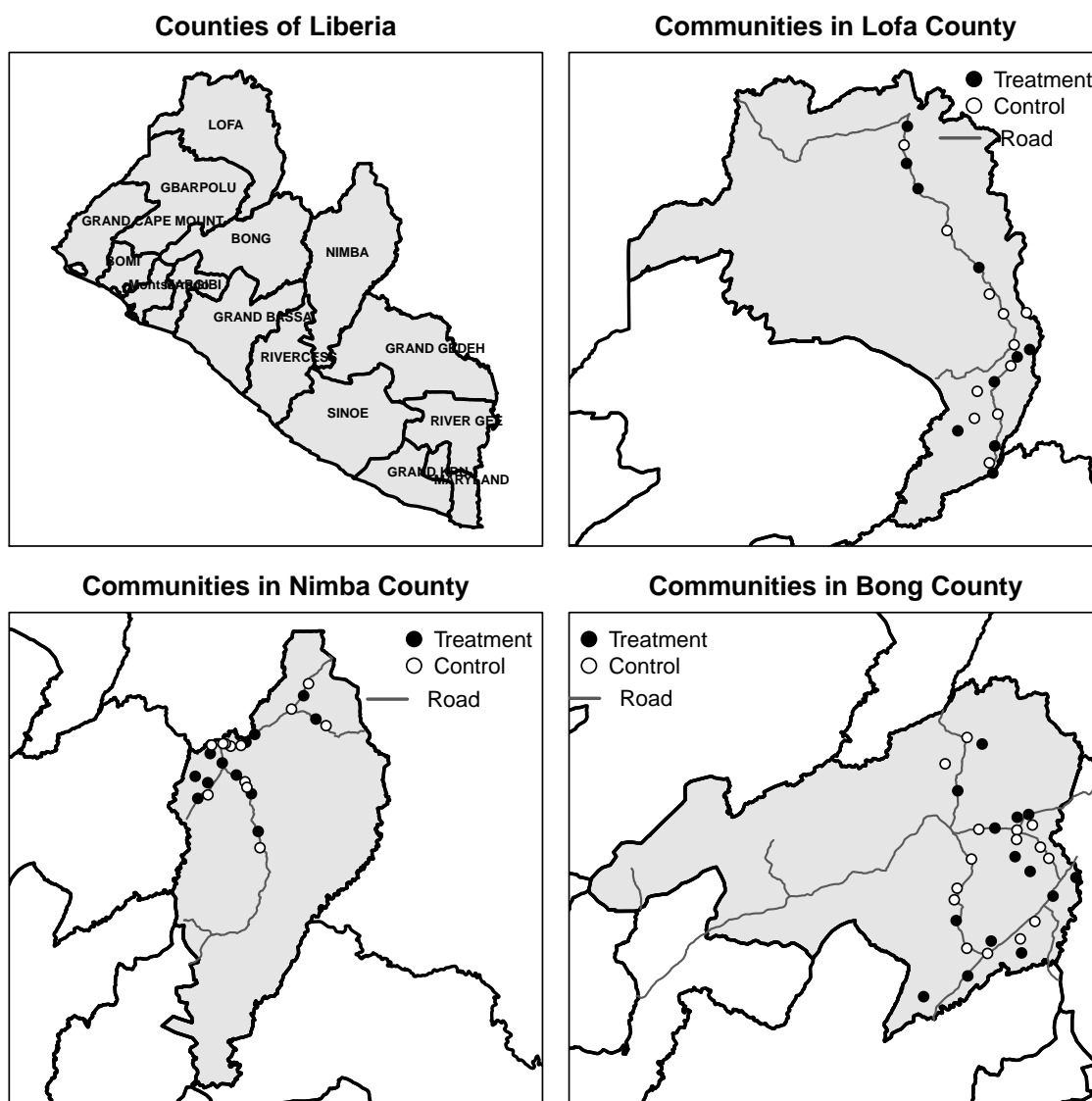


Figure A.2: Implementation timeline, June 2014-December 2015

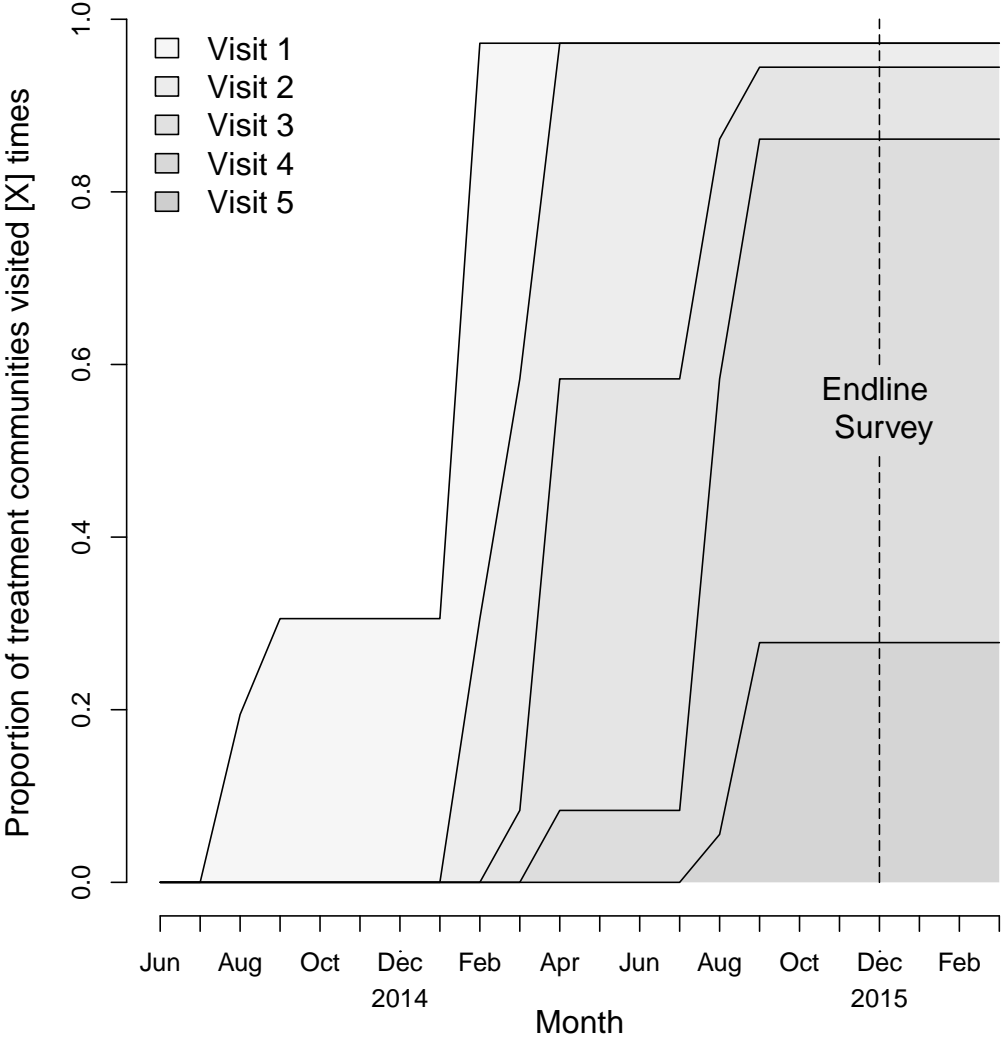


Table A.1: Descriptive statistics

	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.2: Descriptive statistics (cont.)

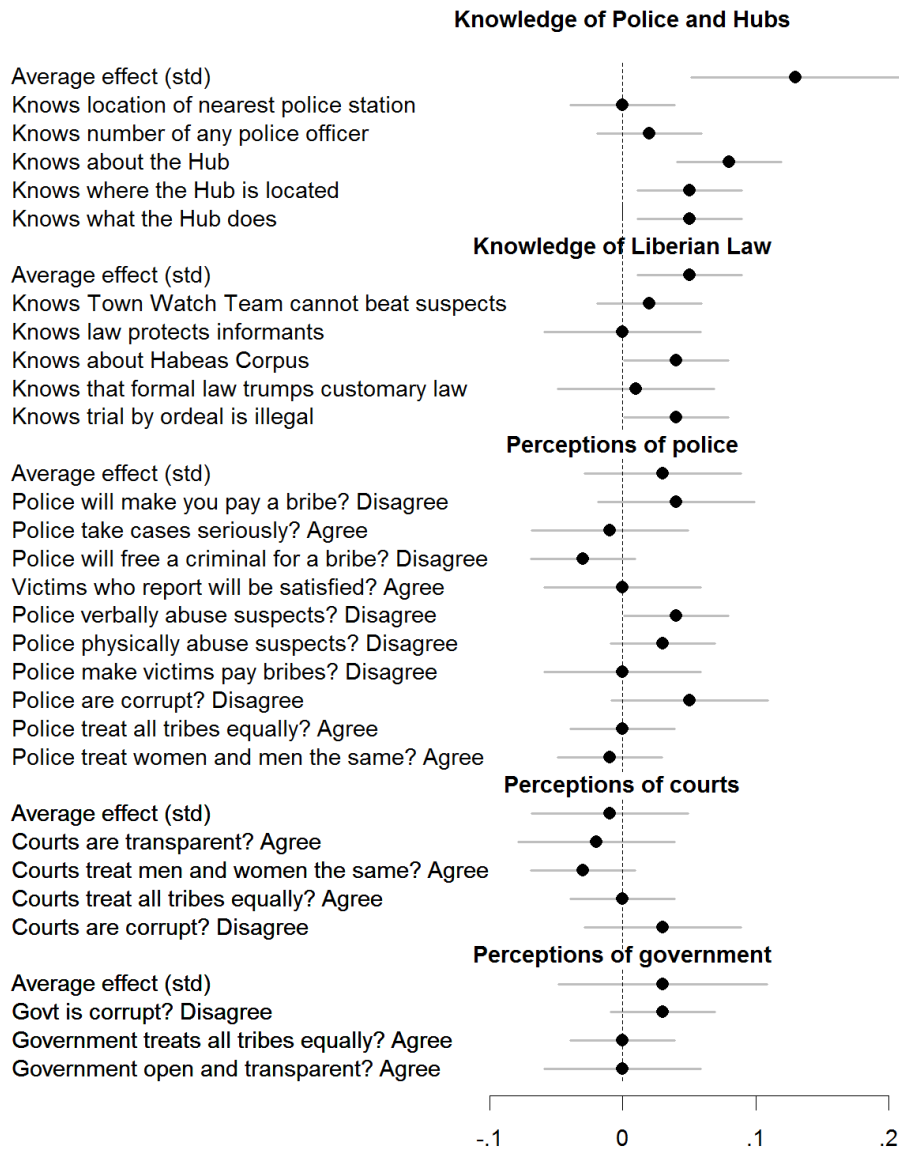
	N	Mean
Reliance on trial by ordeal (hypothetical)		
Community supports trial by ordeal for mysterious death?	1,324	0.18
Community supports trial by ordeal for missing person?	1,324	0.19
Community supports trial by ordeal for burglary?	1,324	0.26
You yourself support trial by ordeal for mysterious death?	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.3: Balance

	Treatment	
Wealth index	0.05 [0.19]	0.08 [0.21]
% literate	-0.05 [1.44]	0.29 [1.79]
% with no schooling	0.39 [1.21]	0.60 [1.59]
Mean years of education	0.03 [0.18]	-0.00 [0.25]
% unemployed	-0.05 [0.54]	0.09 [0.63]
% under 18	1.68 [2.61]	2.68 [3.81]
Community population	0.00 [0.00]	0.00 [0.00]
Ethnic diversity (ELF)	0.01 [0.41]	-0.02 [0.52]
Religious diversity	-0.57 [0.50]	-0.62 [0.63]
% displaced during the war	-0.10 [0.37]	-0.32 [0.70]
Constant	-0.68 [2.08]	-1.36 [3.06]
Stratum 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. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

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



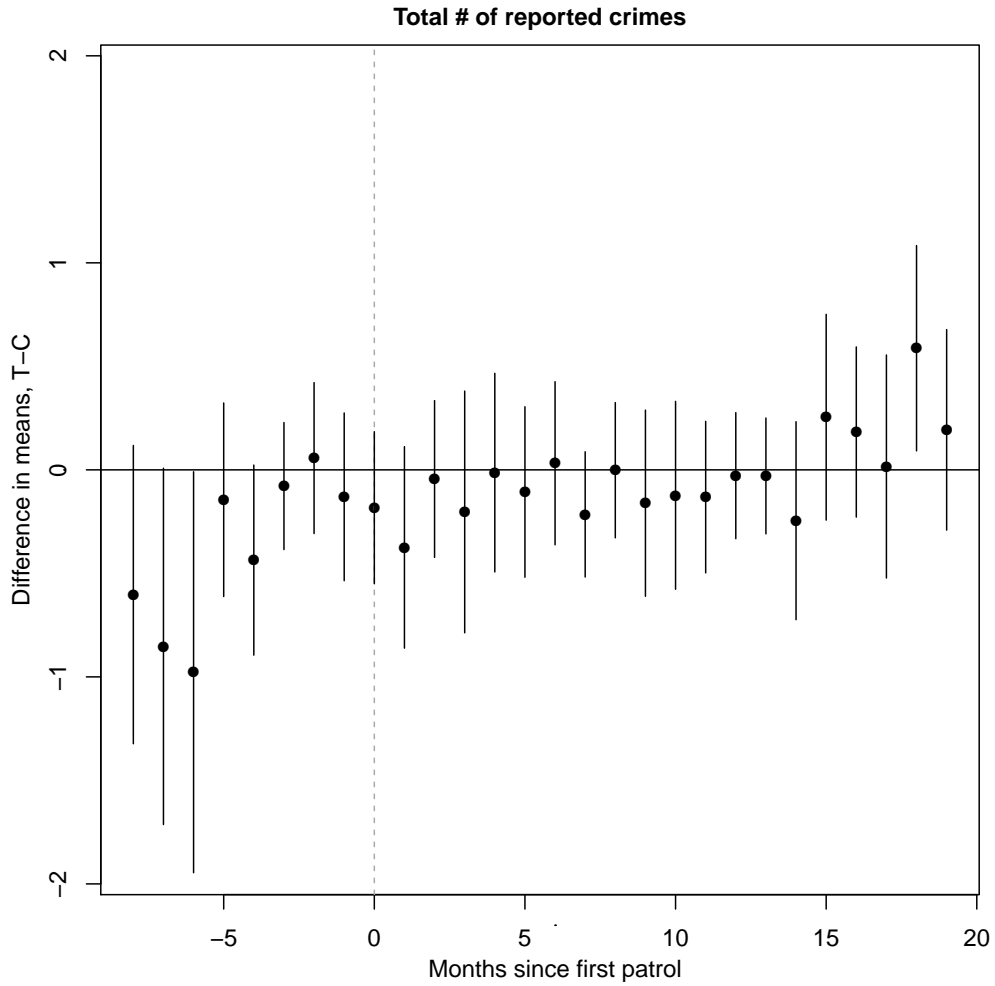
Notes: Average Effect Sizes (AES) for each cluster of outcomes displayed above the average treatment effect (ATE) for each component variable. Note that because the AES and ATE are measured in different units, their magnitudes cannot be directly compared.

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



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



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

Table A.4: Heterogeneous treatment effects on crime reporting by gender

	Nowhere	Statutory only	Customary only	Statutory & customary
All crimes				
Treatment	-0.03 [0.03]	0.03 [0.04]	-0.01 [0.04]	0.02 [0.02]
Treatment × female	0.03 [0.06]	-0.03 [0.05]	0.04 [0.06]	-0.03 [0.03]
Female	0.12*** [0.04]	-0.03 [0.04]	-0.09** [0.04]	-0.01 [0.02]
Observations	926	926	926	926
Felonies				
Treatment	-0.20 ⁺ [0.12]	0.08 [0.13]	0.05 [0.10]	0.07 [0.06]
Treatment × female	0.05 [0.22]	0.21 [0.20]	-0.13 [0.17]	-0.14 ⁺ [0.09]
Female	-0.06 [0.19]	-0.13 [0.16]	0.11 [0.11]	0.08 [0.09]
Observations	125	125	125	125
Misdemeanors				
Treatment	-0.01 [0.03]	0.02 [0.04]	-0.02 [0.04]	0.01 [0.02]
Treatment × female	0.02 [0.07]	-0.05 [0.05]	0.05 [0.06]	-0.02 [0.03]
Female	0.15*** [0.04]	-0.02 [0.03]	-0.11** [0.04]	-0.01 [0.02]
Observations	801	801	801	801

Notes: Standard errors, clustered by community, in brackets. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.5: Heterogeneous treatment effects on crime reporting by ethnicity

	Nowhere	Statutory only	Customary only	Statutory & customary
All crimes				
Treatment	-0.03 [0.03]	0.01 [0.03]	0.01 [0.04]	0.00 [0.01]
Treatment \times minority	0.11 [0.10]	0.01 [0.09]	-0.14 [0.12]	0.02 [0.04]
Minority	-0.08 [0.06]	0.07 [0.06]	0.03 [0.08]	-0.02 [0.02]
Observations	926	926	926	926
Felonies				
Treatment	-0.16* [0.09]	0.14+ [0.09]	-0.02 [0.09]	0.03 [0.05]
Treatment \times minority	-0.22 [0.31]	0.16 [0.31]	0.14 [0.25]	-0.08 [0.08]
Minority	0.27 [0.23]	-0.18 [0.21]	-0.09 [0.18]	-0.01 [0.04]
Observations	125	125	125	125
Misdemeanors				
Treatment	-0.01 [0.03]	-0.00 [0.03]	0.01 [0.04]	0.00 [0.02]
Treatment \times minority	0.15 [0.11]	-0.00 [0.07]	-0.17 [0.13]	0.02 [0.04]
Minority	-0.12* [0.06]	0.09* [0.05]	0.04 [0.08]	-0.01 [0.02]
Observations	801	801	801	801

Notes: Standard errors, clustered by community, in brackets. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.6: Heterogeneous treatment effects on crime reporting by age

	Nowhere	Statutory only	Customary only	Statutory & customary
All crimes				
Treatment	0.01 [0.05]	-0.03 [0.05]	0.02 [0.05]	-0.00 [0.02]
Treatment \times youth	-0.03 [0.07]	0.07 [0.07]	-0.06 [0.07]	0.01 [0.03]
Youth	0.07 [0.05]	-0.03 [0.04]	-0.04 [0.05]	-0.00 [0.02]
Observations	926	926	926	926
Felonies				
Treatment	-0.14 [0.12]	0.02 [0.13]	0.06 [0.13]	0.06 [0.07]
Treatment \times youth	-0.12 [0.18]	0.32 ⁺ [0.20]	-0.12 [0.19]	-0.08 [0.07]
Youth	0.22* [0.13]	-0.23* [0.12]	0.03 [0.10]	-0.01 [0.06]
Observations	125	125	125	125
Misdemeanors				
Treatment	0.04 [0.05]	-0.03 [0.04]	0.01 [0.06]	-0.01 [0.02]
Treatment \times youth	-0.04 [0.08]	0.04 [0.06]	-0.04 [0.08]	0.03 [0.03]
Youth	0.06 [0.06]	-0.01 [0.05]	-0.05 [0.05]	-0.01 [0.02]
Observations	801	801	801	801

Notes: Standard errors, clustered by community, in brackets. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.7: Effects on secondary outcomes not reported in paper

	# in community with Ebola	Gov't has right to tax
Treatment	-0.16 [0.19]	0.01 [0.02]
Observations	1,595	1,657
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.8: 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]	0.00 [0.03]	0.04 [0.04]	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	1,331	1,331	1,331	1,331	1,331	1,307	1,307	1,307	1,307	1,307

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

Table A.9: 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 [0.12]	-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	1,307	1,307	1,656	1,656	1,331	1,331	1,331	1,331	1,331

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

Table A.10: 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 [0.10]	0.06 [0.03]	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. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Not all outcomes and independent variables measured among leaders; sample sizes vary accordingly.

Table A.11: 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. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Not all outcomes and independent variables measured among leaders; sample sizes vary accordingly.