Policing and the Rule of Law in Weak States: Evidence from Liberia

by

Benjamin Sherman Morse

B.A., Colby College (2009)

Submitted to the Department of Political Science
in partial fulfillment of the requirements for the degree of

Doctor of Philosophy in Political Science

at the

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

June 2019

Signature redacted

Author . . . . . .
Department of Political Science
May 31, 2019

Signature redacted

Certified by . . . . . .
Lily L. Tsai
Professor of Political Science
Thesis Supervisor

Signature redacted

Accepted by . . . . . .
M. Taylor Fravel
Chair, Graduate Program Committee

Policing and the Rule of Law in Weak States: Evidence from Liberia

by

Benjamin Sherman Morse

Submitted to the Department of Political Science
on May 31, 2019, in partial fulfillment of the
requirements for the degree of
Doctor of Philosophy in Political Science

Abstract

How can states with limited resources build citizens' trust in the police? How can they ensure the primacy of the police and courts over customary alternatives in peripheral regions long accustomed to autonomy? In urban areas plagued by high levels of crime and insecurity, how can they reduce reliance on vigilantism and extrajudicial justice? My dissertation explores these questions through a series of three essays on policing in Liberia. The first reports results from a large-scale, randomized control trial evaluation of the Liberian National Police's "Confidence Patrols" community policing program in rural Liberia. I find that the program was successful at increasing knowledge of the police and courts, enhancing security of property rights, and increasing crime reporting, but that it also led to backlash from customary chiefs and members of Liberia's traditional society (who are privileged under customary law), possibly because they felt their interests would be threatened by greater access to the state. My second paper evaluates the effectiveness of community policing in the urban setting, with a particular focus on whether community policing combined with the opportunity to form "Watch Forums" can redirect communities away from vigilantism towards lawful activities that complement the efforts of police. I find that the intervention improved police-community relations, reduced support for vigilantism, and mobilized communities to participate in the Watch Forum initiative. I further find that these improvements were accompanied by a roughly 40 percentage point reduction in the incidence of mob violence. I conclude that integrating local communities into formal policing practices is a potentially promising strategy for reducing vigilantism and promoting compliance with the rule of law in countries like Liberia. The third and final paper tests whether citizens expect the police to discriminate against victims of crime on the basis of their class, religion, or (lack of) personal connections to powerful government officials, and whether this in turn discourages crime reporting. I find that citizens expect discrimination on the basis of class and political connections, but that these expectations do not appear to influence the likelihood of crime reporting among actual victims of crime.

Thesis Supervisor: Lily L. Tsai
Title: Professor of Political Science

Acknowledgments

I owe an enormous debt of gratitude to my dissertation committee: Lily Tsai, Evan Lieberman, and Danny Hidalgo. First, for the countless hours they set aside to attend presentations, read drafts, comment on proposals, and provide advice. And second, for their contributions to my professional and scholarly development. Lily, this dissertation would not have been possible without your guidance, encouragement, and patience. You have supported me at every stage of my graduate career, from my decision to join the department, to my choice of a dissertation topic, to my choice of a second dissertation topic, to my circumnavigation of the job market. Along the way, you have encouraged me to pursue projects that are relevant not only to academia but also to the broader world, and you have bent over backwards to make these projects a reality. For all of these things, I am grateful.

Evan, thank you for pushing me to focus on the big-picture implications of my work, for being a source of honest, direct, and helpful feedback, and for tempering this feedback with encouragement and support. Danny, you were instrumental to my training in statistics and causal inference, but your contribution to this project extends well beyond my empirics, as I have also benefited greatly from the many helpful comments, suggestions and insights you've provided on issues of theory and framing. Thank you.

MIT has been an extremely warm and welcoming community, and I am grateful to the many friends and colleagues who've supported me while here, among them: Parrish Bergquist, Paige Bollen, Fiona Cunningham, Dan de Kadt, Dean Knox, Phil Martin, Nina McMurry, Blair Read, Tesalia Rizzo, Leah Rosenzweig, Guillermo Toral, and Stephen Wittels. I would also like to thank Robert Blair and Alexandra Hartman, both of whom have been a friend and mentor to me since before I started graduate school.

This project would not have been possible without the support of friends, colleagues, acquaintances, and countless strangers in Liberia. For help with data collection and project management, I thank Gregory Kitt, Morlu Kpallie, Miatta Konah, Zayzay Kolubah, Boye Pewu, Prince Williams, and the entire staff of Parley Liberia. From the Liberian National Police, I thank Gregory Coleman, Samuel Ford, Blanyon Himmie, Tarnue Kerkula, and all the rank-and-file officers involved in implementing community policing programs for

this study. And lastly, I owe an enormous debt of gratitude to the ordinary citizens who so generously shared with me their stories and experiences.

I am grateful, as ever, to my parents for all of their support over the years. My brothers, Ted and Chris, and my sisters-in-law, Tessa and Kara, have also supported me along the way, most especially by bringing four joyous little kiddos into this world and into my life. Emerson, Sidney, Peter, and Teddy — you are a constant reminder of what is (and what is not) important in life. Lastly, I thank Laura Graham. Laura, thank you for keeping me grounded, for teaching me to embrace myself for who I am and as I am, and for your endless caring, support and encouragement.

Contents

List of Figures

List of Tables

Chapter 1

Introduction

In his account of state formation in sub-Saharan Africa, Jeffrey Herbst (2014) argues that the origins of state weakness in the region date to the pre-colonial era, when the combination of low population density and land abundance discouraged rulers from conquering new territory or investing in state capacity. When European powers divided the continent into colonial states whose borders bore little resemblance to the diverse and highly localized politics that prevailed at the time, they set the region down a tumultuous and often unsuccessful path of state formation that persists to present. Because their borders have been protected by the international system since their inception, states in sub-Saharan Africa have been able to survive without having to engage in the type of state-building that occurred in Europe.

It is perhaps no surprise then, that the region is home to some of the world's weakest states, the likes of which include Liberia, Sierra Leone, South Sudan, Somalia, Guinea, and the Central African Republic. Because these states were never forced to build effective fiscal institutions to prepare for war, their tax bases are small and their bureaucracies often lack adequate resources. Because they were never forced to consolidate their rule and quell domestic opposition, they continue to compete for citizens' loyalty with localized, non-state sources of authority. And because they never had to make concessions to citizens in exchange for 'quasi-voluntary' compliance (Levi, 1989), their political institutions remain unconsolidated and plagued by patrimonialism.

It is against this backdrop that my dissertation explores the politics of policing in

Liberia, one of sub-Saharan Africa's weakest states. The provision of security through effective policing is among the most fundamental functions of government and a key prerequisite for stability and economic growth, yet the challenges that weak states face in creating effective, reliable, and legitimate police forces often prove insurmountable. Because these states lack the resources to adequately train, equip, and monitor police personnel, many citizens are (rightly) skeptical of the value of reporting crimes, providing evidence, giving information, or otherwise cooperating with police. Yet without cooperation from citizens, police find it difficult to do their job effectively. The result is an equilibrium in which non-cooperation between police and citizens allows crime and insecurity to thrive, further suppressing economic activity and making it ever more difficult for states to raise the revenue they need to transform their police forces. This equilibrium may be exacerbated by the corruption within the police force itself, as well as by the absence of effective channels of accountability through which citizens may advocate for improved performance.

Under these circumstances, many communities choose to rely on non-state actors such as customary chiefs, local strongmen, or tribal elders. These actors can help provide a semblance of order in the shadow of the state, but they also have a tendency to discriminate against historically marginalized groups, and they may struggle to handle more serious types of crime, such as those involving violence. In urban settings, where customary institutions are less prevalent, citizens may respond to crime by forming vigilante groups. These actors may also be effective at deterring crime and providing a semblance of order, but they are notorious for committing human rights abuses. Overtime, reliance on these groups can perpetuate a culture of violence that condones violent, retributive forms of justice and militates against the rule of law.

How can states with limited resources build citizens' trust in the police? How can they ensure the primacy of the police and courts over customary alternatives in peripheral regions long accustomed to autonomy? In urban areas plagued by crime and insecurity, how can they reduce reliance on vigilantism and extrajudicial justice? My dissertation explores these questions through a series of three essays on policing in Liberia, drawing on results from two large-scale field experiments with the Liberian National Police (LNP), original data from three large-N surveys, administrative data from the LNP, and field interviews

conducted over the course of more than 12 months of fieldwork.

My first essay evaluates the government of Liberia's effort to expand access to the police and courts in rural counties where the state has historically been weak and absent. Recognizing that it could not afford to adequately train, equip, and monitor police throughout the entire countryside, Liberia elected to concentrate its resources in a small number of well-trained, well-equipped police units stationed at strategic locations around the country. These units are deployed to respond to serious incidents of crime and violence, as well as to engage with citizens on a face-to-face basis through "Confidence Patrols" modeled on community-oriented policing programs in the US and Europe. Meanwhile rank-and-file units deployed more widely across the country remain responsible for handling less serious incidents, despite their inadequacies.

Liberia's approach to expanding its authority into rural counties is founded on 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 (if optimistic) strategy for governments of weak and fragile states, where resource constraints provide few attractive alternatives, but there is as yet little evidence on its effectiveness.

To address this gap, I along with my colleagues from Robert Blair of Brown University and Sabrina Karim of Cornell University partnered with the Liberian National Police to experimentally evaluate the impact of the bi-monthly "Confidence Patrols" by elite units from one of Liberia's "Security Hubs" over a period of 14 months. Drawing on endline survey data from 74 communities — 36 of which were randomly assigned to treatment — we found that the program was successful at increasing knowledge of the police and Liberian law, enhancing security of property rights, and increasing reporting of crimes to the LNP. However, we also found that the program provoked backlash from customary chiefs and members of Liberia's traditional society, and that the increase in crime reporting was concentrated entirely among "non-society" members, who face discrimination under customary law. We conclude that concentrating resources in elite units stationed at hubs around the country and deploying these units on Confidence Patrols is a promising strategy

for improving security and reducing inequities in access to justice in rural communities where the state lacks the capacity to maintain an effective presence, but that the long term effects of this approach remain uncertain in light of the findings on backlash from the customary sector.

My second essay evaluates the impact of the LNP's community policing program in Monrovia, the country's capital city and home to more than half of its four million residents, the majority of whom reside in densely populated, informal settlements. Customary institutions are much weaker in Monrovia than in the countryside, but the crime rate is much higher, and many communities have elected to rely on vigilante groups to keep their communities safe from crime. Vigilantism in this setting occurs along a spectrum (Kantor and Persson, 2010). At one end of the spectrum are self-organized groups of residents that patrol their communities at night for suspected criminals, sometimes reporting them to the police, sometimes meting out justice on the spot. At the other end are seemingly spontaneous acts of mob justice perpetrated by bystanders when someone is accused of committing a crime, often in the light of day. Though systematic data do not exist, anecdotal evidence suggests that both forms of vigilantism are common in urban Liberia.

The LNP's model of community policing in Monrovia is designed to provide communities with an alternative to vigilantism while at the same time harnessing the collective action potential of communities to help address police capacity constraints. In concrete terms, the approach involves encouraging communities to form local security groups and then incorporating these groups into the police's Watch Forum initiative, which provides a set of rules and guidelines governing the conduct of these groups as well as training to members on how they may support police work in their communities.

To evaluate the effectiveness of this approach, I worked with officers in each of Monrovia's ten police zones to identify a sample of 93 communities that would benefit from the program — roughly double the number the police could actually manage to implement the program in. To allow for the rigorous, experimental evaluation, half of these communities were selected to receive the program for a period of 10 months, from February to November 2018, with the remaining set of "control" communities eligible for the program after the

study had concluded.[1] Combining data from two large-scale surveys conducted before and after the program with administrative data on crime reporting and a key informant survey to measure incidents of mob violence, I find that the intervention was successful at building confidence in the police, reducing support for vigilantism, and mobilizing communities to participate in the Watch Forum initiative. I further find that these improvements were accompanied by a roughly 40 percentage point reduction in the incidence of mob violence in treatment communities relative to control communities.

The results reported in the first and second essays of my dissertation make several contributions to the literature on policing and the rule of law in weak and fragile states. First, contrary to the idea that peripheral communities accustomed to autonomous rule will necessarily resist the efforts of aggrandizing states (Migdal, 1988; Scott, 1985), my findings suggests that, in fact, there exists considerable demand for state security institutions among residents of peripheral communities in both the urban and rural setting, with both experiments eliciting greater cooperation from citizens, albeit in slightly different forms.

At the same time, my findings suggest that the sources of this demand are complex and varied. While traditional perspectives typically link demand for state security institutions to Hobbesian security concerns, I argue that this explanation cannot fully account for my findings given the fact that the police remained plagued by persistent problems of corruption and ineffectiveness in both studies, and that the evidence on whether greater reliance induced by the interventions effected improvements in security over the status quo ante is decidedly mixed. Instead, my findings point to two alternative sources of demand for the state, each related to what citizens may view as the limitations of reliance on non-state actors.

In the first experiment, I find that increasing access to the police and courts increases crime reporting, but only among 'non-society' members who face discrimination under customary law. Examining this pattern in further detail, I show that this shift was driven by

[1] While the LNP was encouraged to focus its efforts on treatment communities, they were permitted and indeed encouraged to violate this protocol at their discretion, conducting community policing activities in any community at any time, whether in the treatment group, control group, our out of sample, if they deemed it necessary for crime prevention or other police priorities. The study was thus an "encouragement" experiment, with levels of community policing higher in treatment communities than control communities on average, but with some level of community policing occurring even in control communities.

a reduction in the number of crimes settled privately or not at all, rather than a reduction in crimes reported to customary forums. This result suggests that that prior to the program, discrimination under customary law discouraged disadvantaged residents from reporting crimes and led to a substantial degree of unmet demand for fair and impartial forms of justice. By improving knowledge of the law and knowledge of how to access the police and courts, the program appears to have provided an "exit option" for individuals who otherwise would have been deterred from reporting at all. Thus, demand for the state appears to be born of discrimination under customary law.

By contrast, in the second experiment, I find that residents are quick to take advantage of the opportunity to self-provide security in a manner that respects the rule of law and complements police authority, abandoning vigilantism and adopting more stringent social norms against violent, extrajudicial punishment in process. Moreover, they will continue to rely on lawful alternatives to vigilantism even in the absence of measurable improvements in security, at least for as long as the timespan of my study. Similar to how discrimination by customary institutions appears to underlie demand for the state security institutions among disadvantaged residents in rural Liberia, these results suggests that the violent practices associated with vigilantism are seen as illegitimate and offensive by residents and that this aversion to extra-legal violence underlies their demand for lawful, state-led alternatives.

While my conclusion that demand for state security institutions is driven at least in part by what citizens perceive to be the undesirable aspects of reliance on non-state actors, this does not imply that non-state actors do not in other ways inhibit states' efforts to consolidate their rule of law. To the contrary, the finding of backlash from the customary sector in the first experiment demonstrates how the process of projecting power into communities long accustomed to autonomy is an inherently contentions process, as Migdal (1988) and others have long contended.

The third and final essay of this dissertation is motivated by the absence well-functioning institutions of accountability in many weak states, as well as by the corresponding prevalence of patrimonialism in state bureaucracies. Recognizing that the police bureaucracy is not immune to these pathologies, I test whether citizens expect the police to discriminate against victims of crime on the basis of their class, religious identity, or (lack of) personal

connections to powerful government officials, and whether this in turn discourages crime reporting. Combining data from a conjoint survey experiment with fine-grained data on more than 2600 incidents of crime, I find that citizens do indeed expect discrimination on the basis of political connections and class, but that these expectations do not appear to be an entirely accurate reflection of reality. In particular, while citizens appear to accurately anticipate discrimination against those without connections to elites in government, they appear to overestimate the prevalence of discrimination against the poor, a finding I interpret as related to the conflation of disparate *impact* with disparate *treatment* in regards to the police's "fee for service" model of service delivery. While these patterns have important implications for perceptions of police legitimacy, they do not appear to materially affect citizens' reliance, as reporting by victims of crime is more or less balanced across class, religion, and political connections.

Chapter 2

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

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

Robert A. Blair - Brown University
Sabrina M. Karim - Cornell University
Benjamin S. Morse - Massachusetts Institute of Technology

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, and reduced the incidence of some types of crime, notably simple assault and domestic violence. The program did not, however, improve trust in the police, courts, or government more generally. We also observe higher rates of crime reporting in treatment communities, concentrated almost entirely among those who were disadvantaged under prevailing 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.

2.1 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 rule of law promotion 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 of these countries often do not know how to contact the police, and many assume that seeking redress through state security institutions will be 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 of dispute resolution. These can be efficient, effective, and easily accessible (Isser, 2011), but are often 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. As informative as these studies have been, the vast majority focus on the US, UK, and Australia, and may not generalize to developing countries, much less to weak and war-torn states (Braga et al., 2014; Sahin et al., 2017). While a number of scholars have explored "best practices" in post-conflict security sector reform (e.g. Bryden et al., 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 exploring particular approaches to policing in the developing world.[1] We advance this

[1]See especially Banerjee et al. (2014); Blair et al. (2018); Cooper (2018); Karim et al. (2018); Karim (2018); Sahin et al. (2017). An 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.

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 (experimental or otherwise), our evaluation occurs in a context of ongoing state penetration into communities governed first and foremost by customary authorities—in particular, chiefs, elders, and sodalities known as "secret societies."

State penetration can sometimes result in mutually advantageous arrangements between statutory and customary providers of security and other public goods (Baldwin, 2015). Often, however, the projection of state power sparks 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, 2005a, 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 delicate moment, as UN peacekeepers withdraw and 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 six sets of results. First, we find that the Confidence Patrols program improved security in treatment communities, strengthening property rights and reducing the incidence of some types of crime as measured in our survey, notably simple assault and domestic violence. 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. Third and relatedly, we find that residents of treatment communities were approximately 50% more likely to report felony

offenses to the police and courts relative to the control group. This difference was not accompanied by a corresponding change in reporting of misdemeanors—an important finding in itself, given the severe capacity constraints under which the Liberian police and courts continue to operate.

Fourth and more surprisingly, we find no evidence that the program improved citizens' (generally negative) perceptions of the police, courts, or Liberian government. These null effects are inconsistent with our expectations, and with studies suggesting that citizens' willingness to cooperate with the state in general (Levi et al., 2009) and the police in particular (Tyler and Huo, 2002; Tyler, 2004) depends on their belief that state institutions are competent and procedurally fair. A potential solution to this puzzle lies in our fifth result: we find that the shift from under-reporting to reliance on the police and courts was concentrated almost entirely among residents who were not members 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 et al., 2011). Our results suggest that the Confidence Patrols program provided an "exit option" for these individuals.

Finally and also 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 et al., 2009)—and more stringent social norms against cooperation with the LNP. Taken together, our results suggest that expanding police presence into rural communities can improve security and help equalize access to justice, even 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 customary law.

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, and our proxies may be susceptible to social desirability bias and other forms of measurement error. This problem afflicts all studies that use surveys to measure outcomes, and ours is no exception. We attempt to overcome these limitations by complementing our survey with administrative, qualitative, and behavioral data, and by showing that our findings are inconsistent with social desirability bias. Nonetheless, our results should be interpreted with these caveats in mind.

Third, the Confidence Patrols program featured multiple components—town hall meetings, Q&A sessions, foot patrols, distribution of pamphlets, and soccer games with local youths—and we are unable to disentangle their relative effectiveness. We view this limitation as minor, both conceptually and practically, since all components of the program were designed with the same purpose in mind—providing information and creating opportunities for positive contact between police officers and civilians—and since the marginal cost of each additional component was low.[2] Finally, as with any study focused on a single case, we cannot be sure whether our results generalize. The dynamics of state penetration in Liberia are similar to those in other African countries, and problems of police corruption and abuse are endemic to the continent, and to much of the developing (and, indeed, developed) world. We believe a similar program would have similar effects in other rural African contexts, and possibly beyond. Nonetheless, we can only speculate about generalizability, and leave this question for future research to explore.

[2]The most substantial expense was fuel for the officers' vehicles. Once the officers reached a community, the marginal cost of conducting a foot patrol or organizing a soccer game was generally insignificant.

2.2 Theory

2.2.1 Pathologies of policing after civil war

The challenges of creating effective, legitimate police forces are especially daunting 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 (Braga, 2008; Tyler and Fagan, 2006; Tyler and Huo, 2002). This, in turn, depends on citizens' belief that the benefits of cooperation will exceed the costs. In countries overcoming legacies of civil war, 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. Previous studies have shown that negative police/citizen interactions are more powerful in damaging citizen views of the police than positive interactions are in improving them (Li et al., 2016)—an especially salient concern in post-conflict settings.

These problems are compounded by the often-prohibitive costs of reporting to the police or cooperating in criminal investigations, especially where civil war is accompanied by state collapse. 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 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, 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 circum-

stances, victims may opt to seek redress through customary (informal) institutions instead,[3] which many view as quicker, less expensive, more accessible, and more legitimate than their statutory (formal) counterparts (Blair, 2018b). If the former are perfect substitutes for the latter, then this may not be a problem; 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 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 use 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).

2.2.2 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-

[3] We 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).

trained, well-equipped police units, which are then deployed to respond to the most serious incidents of crime and violence, and, equally important, to engage with civilians on a face-to-face, day-to-day basis through community-oriented policing tactics borrowed from the US and Europe (Gill et al., 2014a). This approach is increasingly popular in post-conflict countries, several of which are implementing or considering similar strategies.[4] Despite their inadequacies, rank-and-file units remain responsible for less serious incidents.

In theory, this approach can decrease the costs of cooperation, increase the benefits, or both. In their interactions with citizens, 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 jurisdictions, thereby curtailing forum shopping. And they can mitigate social costs by persuading citizens to rely on formal rather than informal mechanisms of adjudication, potentially shifting social norms that discourage cooperation with the police.

As citizens interact with elite units, they may update their priors about the competence and professionalism of the police force as a whole. Some criminologists have found that citizens' attitudes towards the police are malleable, and that mutually respectful, "procedurally just" contact with individual officers can depolarize relations, even where distrust is deeply entrenched (Nix et al., 2015; Sunshine and Tyler, 2003; Tyler and Huo, 2002; Tyler, 2004; Wolfe et al., 2016). Some have argued that even a single encounter during a routine traffic stop can have this effect, provided officers abide 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 not deter crime if

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

elite units are too few or far removed to respond in a timely manner. Whether community-oriented policing can (or should even be expected to) reduce crime remains something of an open question (Gill et al., 2014a); whether positive but brief encounters with individual officers can improve citizens' attitudes remains unsettled as well.[5] Even the notion that procedural justice can improve citizens' trust and cooperation—one of the most influential findings from criminology in recent decades—is increasingly contested (Nagin and Telep, 2017). Conversely, exposure to elite units may 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 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 dispute resolution, and individuals who benefit from these 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 about which the existing literature on police/community relations in the West is largely silent.

[5]Mazerolle et al. (2013, 55) argue that "a little bit of nice goes a long way" in police/citizen interactions. But Sahin et al. (2017, 164) conclude that "a 'quick fix' is not possible when it comes to the issue of people's broader views about the police." Skogan (2006) similarly finds that positive encounters with individual officers have no effect on general confidence in the police. See also Nagin and Telep (2017).

2.3 Setting

2.3.1 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 citizens still 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 et al., 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). 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 state. Of these, the most powerful are sodalities known as "secret societies" (or "Poro" and "Sande" in our study regions).[6] 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 20th century, they enjoyed a "distinct revival" during the civil war (Ellis, 2006, 270), and continue to play a

[6]The term "secret society" is in some respects a misnomer. All adults know about the existence and purpose of secret societies, and most know who is and is not a member (Little, 1965). 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.

central role in conflict resolution in many communities today, where they are expected to be the first and in some cases ultimate arbiters of disputes—an expectation that can extend even to "significant crimes that technically should be referred to the formal court system" (Lubkemann et al., 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 et al., 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.[7]

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, 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 et al., 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 social sanctions in their communities (Lubkemann et al., 2011). 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).

[7]The uses and logistics of trial by ordeal vary. In one common variation, suspected criminals ingest a poison derived from the bark of the sassywood tree. If the suspect vomits the poison, he is deemed innocent; if he does not, or if he dies, he is pronounced guilty (Lubkemann et al., 2011).

2.3.2 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 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.[8] The Hubs are the cornerstone of the government's strategy for decentralizing security and justice provision into rural areas, and for consolidating state authority before 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 reforms, and conduct foot patrols to interact with citizens—forms of engagement often associated with community policing (Gill et al., 2014a). Each patrol typically lasts several hours; in more distant communities, 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.)

Officers participating in the Confidence Patrols program received a day-long training on community policing from the LNP's community services division, supervised and sup-

[8]The PSU is a standby force that provides backup for the rank-and-file, and serves as the LNP's crowd control unit during riots and protests. It was created in 2005 ahead of Liberia's first post-war election, but initially played only a limited role. The PSU's importance began to grow in 2009 as the US, UNMIL, and other donor countries invested more heavily in recruitment, training, and logistical support. Rank-and-file LNP officers were invited to apply to the PSU and, once selected, received up to a year of additional tactical training from UN Police and US-contracted trainers. By 2015, the PSU comprised nearly 1,000 officers, up from 200 in 2009 (Caparini, 2014).

ported by the UN. The training covered topics such as the role of "proactive policing" in identifying potential drivers of crime, the importance of citizen trust and cooperation for police effectiveness, and the roles and responsibilities of police officers, local leaders, civil society organizations, and citizens. In Liberia, the notion that police officers would be willing to proactively visit communities to meet civilians and address their concerns constitutes a radical departure from the status quo, in which officers wait passively in depots for criminal complaints to arrive.

2.4 Research design

2.4.1 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 our 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.

2.4.2 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. These criteria yielded a sample of 74 communities, ranging in population from approximately 500 to 4,000 residents, and located between 1/2 hour and 3 hours from the Gbarnga Hub. Communities were then grouped into nine geographic blocks, each consisting of seven to 10 communities that could be patrolled

in a one-to-two day period.[9] Within each block, four communities were randomly assigned to treatment and the remainder to control. The resulting sample consists of 38 control communities and 36 treatment communities.

2.4.3 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).[10] All patrols originated from the Gbarnga Hub. Typically the patrolling officers would visit two to three proximate communities per day, sometimes spending the night. 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, field reports from our Liberian research assistant suggest the intervention was much stronger than it may appear. The program aimed to affect communities not just through increased police presence, but also through the provision of information that would persist and diffuse 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."[11]

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

[9] Communities were grouped such that none was more than a two hours drive from any other within the same block. Importantly, communities were not so close as to risk spillover, as we discuss in greater detail below.

[10] See the appendix for a timeline of the intervention relative to our endline survey.

[11] Kpayaquelleh 2/19/2015.

elders panicked.... Now that they have known the purpose of the visit, they welcome the PSU and will not be afraid."[12] Similar sentiments were expressed in other communities, and are reflected in our endline survey: 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. The program was also much more intensive than others that have been subjected to experimental evaluation, many of which involved a single short, scripted interaction between police officers and citizens (see Nagin and Telep 2017 for a review).

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 by acknowledging all local leaders, then gave two short presentations, the content of which remained more or less constant across communities. The first presentation 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.

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.) While the program was not designed in terms of procedural justice per se, officers did follow some of its basic tenets during these interactions—e.g. treating citizens with respect, and giving them voice to ask questions and lodge complaints (Tyler and Huo, 2002; Tyler, 2004). Presentations were followed by Q&A, which led to lively and sometimes lengthy discussions about a range of topics, including domestic violence, crime reporting, police misconduct, and the appropriate role of Community Watch Forums—youth groups organized with help from

[12]Zolowee 4/6/2015.

the police to facilitate crime reporting.[13] At the end of each meeting, officers provided attendees with contact information for the Hub and the PSU commander, then walked the community in smaller 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, tapering off to an hour and a half in later stages as residents became familiar with the themes of the visits.

There was inevitably some variation in the way treatment was administered across communities and over time. This is true of virtually all field experiments involving human implementers, and ours is no exception. Based on field reports by our Liberian research assistant, however, implementation was much more homogeneous than one might expect from a program of this sort. The format of the visits remained constant throughout—town hall meetings followed by Q&A followed by foot patrols and distribution of pamphlets—and the progression of topics during the town hall meetings always followed the same standardized template. We do observe some variation in the nature of the questions asked during Q&A; whether or not officers played soccer with local youths (for example, during the rainy season); whether or not officers spent the night in the community; and whether or not officers discussed Ebola (a topic that was eliminated as the epidemic waned). Otherwise, our field reports indicate the intervention was generally homogeneous. This homogeneity was also observed by two of the authors who accompanied the PSU on patrols in non-study communities. While there was some turnover in police personnel over the course of the project, officers rotated relatively infrequently (every six months), and all received the same pre-deployment training in community policing.

[13]Community Watch Forums date back to the civil war, when communities self-organized in the wake of the government's collapse. Since 2005, the LNP has sought to use these groups as a "force multiplier" to compensate for personnel and resource constraints. The LNP has established rules and guidelines to govern the conduct of the Forums, and has developed a formal application process to ensure that members are properly vetted. Community Watch Forums assist the police by conducting nighttime patrols, performing citizens' arrests, and providing crime tips and testimony. Members are not permitted to carry weapons or use force, and all suspects must be promptly turned over to the police, though adherence to these regulations is far from universal (Zanker, 2017).

2.5 Empirical strategy

2.5.1 Hypotheses

We pre-registered 11 hypotheses, nine of which we test here.[14] 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 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 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). 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 not statistically significant, 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 field reports from our Liberian research assis-

[14] We 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. Our pre-analysis plan is available at http://egap.org/registration/1609.

tant, which revealed that patrolling officers repeatedly emphasized disputants' rights to a fair and impartial trial in a court of law.

2.5.2 Data

We leverage 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,[15] the other administered to five purposively-selected local leaders—typically the town chief, two elders, a women's group leader, and a youth group leader. Outcomes from the survey are organized into clusters of three to eight dependent variables, each corresponding to one of our 11 hypotheses. At the end of the survey, we also offered respondents a small "sitting fee" (70 Liberian dollars, 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 officers repeatedly emphasized the importance of Community Watch Forums during patrols, we interpret these donations as a measure of citizens' willingness to incur a personal cost to improve coordination between their community and the police. Summary statistics and further details on measurement are provided in the appendix.

We further complement our survey with administrative data on all crimes reported to the LNP by any community in our sample between June 2014 and June 2016. Because most of the LNP's files are not digitized, collecting this data involved visiting each LNP depot in the area to copy and transcribe their records. While these records are informative, they are also limited in several ways. Incidents are generally only documented 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 reported to the police. This, in turn, captures only

[15]Residents were sampled using the random walk method, described in the appendix.

a fraction of the crimes that occur, most of which are never reported at all. We therefore interpret LNP crime records as proxies for crime *reporting*, rather than 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 data 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.[16] We use these qualitative field reports to contextualize and inform our quantitative results.

2.5.3 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 in Clingingsmith et al. (2009).[17] AES coefficients are interpreted in terms of standard deviations from the control group mean.[18]

[16]It 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 the 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 acclimated quickly to the research assistant's presence, describing him as "one of their own."

[17]The AES across J related dependent variables is given by $\tau = \frac{1}{J}\sum_{j=1}^{J}\frac{\pi_j}{\sigma_j}$, 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 et al. (2009).

[18]Following our pre-analysis plan, we use the AES estimator to control Type II errors within each outcome cluster, but we do not adjust p-values *across* outcome clusters. It is not clear that such an adjustment is necessary in this case, as we do not wish to test the hypothesis that the program had *any* effect on *any*

All specifications include block fixed effects and individual- and community-level controls. At the individual level, we control for gender, age, household size, tribe, religion, education, and literacy.[19] At the community level we also control for population, mobile phone coverage, an 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 census, and from our endline survey of local leaders. Since the latter was conducted after the intervention, we use it only to control for variables that were unlikely to be affected by treatment. Standard errors are clustered at the community level throughout. In addition, because the probability of treatment assignment varied slightly across blocks, we weight each observation by the inverse probability of being assigned to treatment (for treated units) or control (for control units) within each block, following Gerber and Green (2012, 117).[20]

When estimating differences in crime reporting between treatment and control communities, we fit a crime-level regression given by

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

where y_{civs} indicates whether crime c reported by individual i in community v of block s was referred to a statutory forum, customary forum, both, or neither,[21] T_{vs} denotes community-level treatment assignment, X_{ivs} denotes the individual- and community-level controls listed above, and γ_s denotes block fixed effects. Standard errors are again clustered at the community level.

outcome cluster. Nonetheless, we report results with multiple comparisons adjustments in the appendix. With one relatively minor exception, our results are unchanged regardless.

[19] In our pre-analysis plan we proposed controlling for employment and wealth as well, but since these may have been affected by the program, we believe they are better conceptualized as dependent variables rather than controls.

[20] The probability of being assigned to treatment varied between 0.4 to 0.6 because a fixed number of communities in each block (four) was assigned to treatment, while blocks ranged in size from seven to ten communities.

[21] This regression is only identified for crimes that occurred, and so is potentially susceptible to post-treatment bias. In the appendix we explore the potential magnitude of this bias, and consider an alternative unbiased (though also less informative) specification.

2.5.4 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 assumes the absence of spillover between treatment and control communities. Criminologists distinguish between two types of spillover: displacement (whereby increased police presence displaces crime from one location to another nearby) and diffusion (whereby increased police presence in one location reduces crime in nearby locations as well). The literature on these possibilities is extensive; while results are mixed, the most recent research (including several meta-analyses) suggests that displacement tends to be minimal and is usually offset by the effects of treatment itself. Diffusion is more common (Bowers et al., 2011; Guerette and Bowers, 2009; Telep et al., 2014).

We believe the risk of spillover of either kind is minor in our case, as our communities are rural, and most are located far from one another. (Most criminological studies of displacement and diffusion focus on hot spots policing in cities.) While we did not apply buffers between units, with just one exception, all treatment and control communities are separated by at least one additional village that was not included in the study. Moreover, the average distance between each treatment community and the nearest control is 6.8 kilometers. This is a long way in rural Liberia, where roads are rough and often impassable, and where few citizens have access to a vehicle. (These distances are also measured "as the crow flies"—the driving or walking distance is longer.)

Our qualitative data further suggests that most crimes in our sample originate from within rather than outside communities. As we will see, this is especially true of the types of incidents on 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). Diffusion is more likely than displacement in our context, as residents of control communities may have learned some of the information

provided to treatment communities secondhand. This, however, would bias our estimates towards the null, causing us to *under*estimate the effects of the program.

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, insofar as the program changed respondents' definitions of particular crimes, it likely *expanded* them (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 related, 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. While we cannot eliminate this possibility, we believe it is unlikely to explain our results. 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 officers (e.g. the decision to fallow one's farmland). Moreover, the program appears *not* to have affected outcomes that we would expect to be most susceptible to social desirability bias. In particular, the program did not improve perceptions of the police, and respondents in both treatment and control communities expressed generally negative views of the LNP and the Liberian government. These nulls suggest that respondents were not simply parroting back messages they heard during the program, and provide some reassurance that the positive effects we observe on other outcomes (e.g. crime reporting) are not merely artifacts of social desirability bias.

2.6 Results

2.6.1 Increased knowledge of the police and Liberian law, reduced crime, and improved security of property of rights

Figure 2-1 plots the AES for each of our outcome clusters. The program had small and not statistically significant 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 not statistically significant negative effect on self-reported reliance on trial by ordeal, and an even larger but imprecisely estimated positive effect on donations to Community Watch Forums. The program did not reduce the overall incidence of crime as measured in the survey, 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, and by improving citizens' understanding of how to contact the LNP. The program also modestly but statistically significantly improved knowledge of Liberian law. Most notably, 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, and four percentage points more likely to know that trial by ordeal is illegal. These amount to increases of 5% and 6%, respectively, relative to their control group means.

More striking, the program significantly improved security of property rights. Residents of treatment communities were five percentage points more likely to feel secure about their farmlands (an increase of 7% relative to the control group mean); five percentage points (6%) less likely to be involved in an ongoing dispute over their farmlands; six percentage points (29%) more likely to report making costly improvements to their homes in the past

year; and four percentage points (5%) more likely to report plans to fallow their farmland in the next year—an important proxy for security of property rights in Africa, where many farmers neglect to fallow out of fear that others will appropriate their land (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 et al., 2014; Hartman et al., 2018). Because patrolling officers never mentioned outcomes like fallowing or household improvements in their presentations, it is unlikely that these results are artifacts of social desirability bias.

The null AES on crime in general also masks important variation across specific categories of crime. Figure 2-2 reports average treatment effects on the incidence of armed robbery, theft and burglary, aggravated assault, simple assault,[22] 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 four percentage points, and domestic violence by seven percentage points. Relative to the control group, these amount to substantively large reductions of 37% and 16%, respectively. We view these effects as especially important given the pervasiveness of domestic violence in particular in rural Liberia, and given that community policing-style interventions like the one we evaluate often fail (and may not be expected) to reduce crime at all, especially in the short term (Gill et al., 2014a). The null effects on aggravated assault, armed robbery, and rape may reflect the low (reported) incidence of these crimes in control communities, raising the possibility of floor effects.

2.6.2 Increased crime reporting

For crimes that did occur, Table 2.1 displays differences in reporting to the formal sector, informal sector, both, or neither between treatment and control communities. The likelihood that a crime would go unreported was five percentage points lower in treatment

[22] Simple 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.

communities relative to control, and the likelihood of reporting to the police or courts was a corresponding six percentage points higher. Both of these differences are driven by felonies rather than misdemeanors:[23] felonies in treatment communities were 12 percentage points less likely to go unreported, and 16 percentage points more likely to be reported to the police or courts. There is no difference in reporting to the informal sector for either category of crime.

These are substantively large and practically meaningful results. 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.[24] Notably, this decline 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.

2.6.3 Heterogeneity 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 Liberia's customary sector: gender, age, ethnicity, and membership in a secret society.[25] Of these four, we interpret society membership as the most direct proxy for disadvantage, since society members tend to be favored over

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

[24]On 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 proportion of these incidents that went unreported was about 12 percentage points lower in treatment communities relative to control, which equates to roughly 11 fewer unreported incidents.

[25]This 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-members *regardless* of their other demographic characteristics (Isser et al., 2009).

Figure 2-3 displays heterogeneous treatment effects for each of our outcome clusters; Table 2.2 replicates this analysis for crime reporting, focusing on secret society membership alone. (Analogous crime reporting results 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. Non-society members are the only sub-group for which we find a large and significant increase in donations to Community Watch Forums; they are also the only sub-group for which we observe a significant improvement in perceptions of the police (though this effect is not statistically different from the null among society members).

More importantly, and perhaps not coincidentally, we find that the difference in crime reporting between treatment and control communities is concentrated almost entirely among non-society members. Substantively, the likelihood that a non-society crime victim would report to the police or courts was 16 percentage points higher in treatment communities relative to control. This difference is especially pronounced for felonies—non-society victims of felonies were a full 63 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. There is no difference in the likelihood of reporting between society members in treatment and control communities, regardless of the severity of the crime.

2.7 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. Felony offenses were also significantly more likely to be reported to the police and courts in treatment communities relative to control. The program did not, however, improve citizens' rather bleak perceptions of the police, courts, or gov-

ernment 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. We explore additional themes from the qualitative data in the appendix.

2.7.1 Decreased expected costs of reporting

Our theory characterizes the decision to cooperate with the police as a function of expected costs and benefits. We interpret our results as suggesting that the Confidence Patrols program lowered the expected costs of reporting (e.g. by improving knowledge of Liberian law), but that it did not necessarily increase the expected benefits (e.g. by improving perceptions of the LNP). 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);[26] whether members of Community Watch Forums are legally authorized to "tie criminals with rope" (they are not)[27] or "judge cases and charge people to pay money" (they are not);[28] or whether sexual and gender-based violence (SGBV) can legally be "compromised" outside of court (it cannot).[29] 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.

[26]Wainsue 2/6/2015; Gbenequelle 3/3/2015; Wainsue 3/19/15; Jinnepeleta 3/19/15; Konia 4/11/2015.
[27]Tomato Camp 2/6/15; Jinnepeleta 3/19/15; Wainsue 3/19/15; Gbedin 4/6/15.
[28]Loyee 8/1/15.
[29]Wainsue 2/6/2015; Beyan 4/10/15; Konia 4/11/2015; Ganglota 4/13/15; Gbenequelleh 4/20/15; Flumpa 8/1/2015; Bunadin 8/2/15; Kpayaquellah 8/10/15.

2.7.2 Decreased crime and increased security of property rights

Lower 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 a recurring concern in treatment communities. Citizens requested guidance on resolving land disputes[30] and preventing them from escalating into violence or property destruction.[31] Citizens also asked about theft of livestock—a common crime in rural Liberia.[32] 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 SGBV were even more common. Town hall meetings almost always included some discussion of these topics, typically led 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,[33] 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 usually be identified, making them easier to solve, even for the severely resource-constrained LNP.

[30] Zarsonnoh 4/5/15; Zuluyee 4/7/15.
[31] Doe Town 2/7/15
[32] Tomato Camp 3/18/15; Flumpa 4/4/15; Gbahn 8/2/15; Tassah 3/18/15.
[33] Kpaiyea 8/8/2015; Salayea 8/8/15.

2.7.3 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 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;[34] perhaps civilians simply internalized this message. Or perhaps perceptions are just sticky and hard to change.

Alternatively, perhaps exposure to an elite sub-unit 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.

[34]Dean Town 2/5/2015; Galai Town 2/5/2015; Zowienta 4/21/2015.

2.7.4 Access to the statutory sector for non-society members

One of our most intriguing findings is that the difference in crime reporting between treatment and control communities is driven by 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 "incapable of providing justice;" as one individual cited in Isser et al. (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.[35] This interpretation is consistent with our qualitative data. In many communities, disputants complained of customary authorities "playing" with their cases;[36] 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.[37] 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 "addi-

[35] A potential alternative explanation lies 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.

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

[37] Gbarlorkpala 3/3/15.

tional review and recourse from a more neutral, and usually higher, authority" (Isser et al., 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, heterogeneity by society membership holds even after we control for these other demographic characteristics (and for their interaction with treatment assignment). Our finding that women in the treatment group were no more likely to report may suggest that the program failed to provide them with a satisfactory exit option, or that women are simply less willing to report crimes in general, even with increased police presence. Regardless, society membership appears to be an important driver of the differences we observe between treatment and control communities, even controlling for other potential sources of status.

2.7.5 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.[38] 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 residents whether the "bush devil" had appeared in their community

[38]These analyses were not pre-specified.

in the past year. The bush devil 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 some do), conflict is often the result (Isser et al., 2009, 33). Patrolling officers explicitly discouraged this practice,[39] and we interpret its persistence as a proxy for social tension, and for attempts by secret societies to reassert their authority.

Table 2.3 reports average treatment effects on these outcomes. Residents of treatment communities were seven percentage points more likely to expect social sanctions for reporting to the police in cases of burglary, and eight percentage points more likely to expect social sanctions in cases of domestic violence (increases of 13% and 18%, respectively, relative to the control group). 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.[40] Residents of treatment communities were also five percentage points more likely to report appearances of the bush devil in the past year (a 50% 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.[41]

2.8 Conclusion

Weak and war-torn states often struggle to restore citizens' willingness to rely on the police and courts when crimes are committed or disputes arise. Resistance to police presence is

[39] Gbarlorkpala 3/3/15.

[40] Wainsue 2/6/2015; Ganglota 4/13/15; Gbenequelleh 4/20/15; Flumpa 8/1/15; Bunadin 8/2/15; Kpayaquellah 8/10/15.

[41] Zowienta 8/9/14.

often especially marked in rural areas long accustomed to state absence or abuse. Fearing that the costs of reporting will be high and the benefits low, many victims and witnesses opt not to report at all, or to rely on customary authorities instead. These authorities can be effective, accessible, and inexpensive, but they also tend to discriminate against historically marginalized groups. Customary authorities may also attempt to actively 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 improved knowledge of the police and Liberian law, enhanced security of property rights, and reduced the incidence of some types of crime. We also find that felonies were more likely to be reported to the statutory sector in treatment communities relative to control, and that this difference was especially marked among residents who were disadvantaged under customary regimes. These results are particularly striking given the initial skepticism with which patrolling officers were often greeted; given that many community policing-style interventions fail to deter crime (Gill et al., 2014a); 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 et al., 2017).

We also find, however, that the program did not mitigate citizens' distrust of the police, courts, or government more generally. This is inconsistent with much of the literature on procedural justice, which suggests that citizen cooperation depends on prior trust and perceptions of fairness (Tyler, 2004; Tyler and Huo, 2002). It is, however, consistent with research showing that citizens are unlikely to update their perceptions of the police (much less the government as a whole) in response to a limited number of encounters over a relatively short amount of time (Nagin and Telep, 2017; Sahin et al., 2017; Skogan, 2006). Our results suggest that citizens may be willing to change their behavior without a correspond-

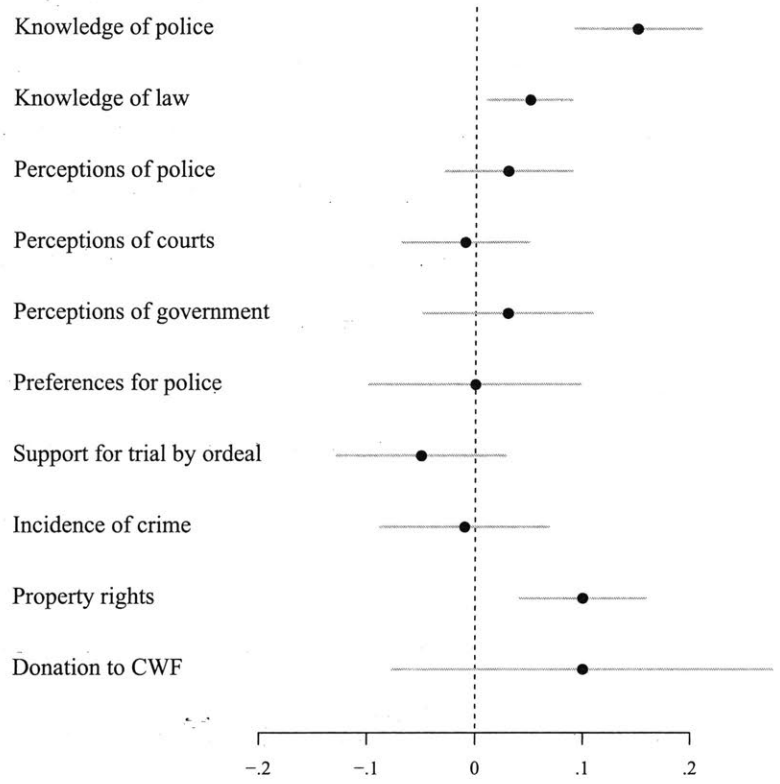
ing change in perceptions (on this point, see also Blair 2018a). The program also appears to have provoked some backlash from the customary sector, and to have aggravated social norms against cooperation with the police.

Whether the change in crime reporting that we observe will persist over time remains an open question, though we expect much will depend on the performance of rank-and-file 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 increased reporting may be 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.

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 under severe resource constraints. Increased police presence may 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, which 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 remains to be seen. We leave this question for future research to address.

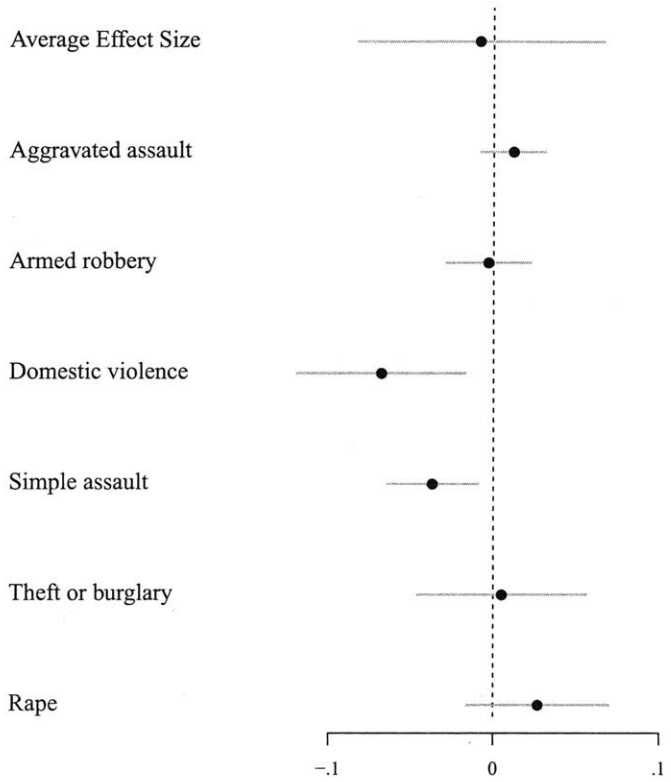
2.9 Tables and Figures

Figure 2-1: 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 2-2: 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 2.1: Heterogeneity in crime reporting

	Nowhere			Statutory only			Customary only			Statutory & customary		
All crimes												
Treatment	-0.04^{+}			0.06*			-0.01			0.00		
	[0.03]			[0.03]			[0.03]			[0.01]		
Felonies												
Treatment		-0.12*			0.16*			-0.04			0.01	
		[0.06]			[0.07]			[0.05]			[0.03]	
Misdemeanors												
Treatment			-0.02			0.03			-0.00			-0.00
			[0.03]			[0.02]			[0.03]			[0.01]
Control group mean	.43	.36	.44	.19	.41	.15	.33	.17	.36	.05	.06	.05
Observations	1840	264	1576	1840	264	1551	1840	264	1576	1840	264	1576

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

Table 2.2: Heterogeneity in crime reporting by secret society membership

	Nowhere	Statutory only	Customary only	Statutory & customary
All crimes				
Treatment	0.04	-0.03	-0.01	0.00
	[0.04]	[0.03]	[0.04]	[0.01]
Treatment × not in society	-0.19*	0.19**	-0.01	0.01
	[0.09]	[0.07]	[0.08]	[0.03]
Not in society	0.05	-0.03	-0.03	0.01
	[0.07]	[0.05]	[0.06]	[0.02]
Observations	935	935	935	935
Felonies				
Treatment	-0.09	-0.02	0.10	0.01
	[0.12]	[0.10]	[0.10]	[0.05]
Treatment × not in society	-0.54**	0.65***	-.15	0.04
	[0.19]	[0.16]	[0.18]	[0.07]
Not in society	0.19	-0.24*	0.10	-0.06
	[0.13]	[0.11]	[0.12]	[0.05]
Observations	125	125	125	125
Misdemeanors				
Treatment	0.04	-0.03	-0.01	-0.00
	[0.04]	[0.03]	[0.05]	[0.02]
Treatment × not in society	-0.12	0.11^{+}	-0.00	0.02
	[0.10]	[0.06]	[0.08]	[0.03]
Not in society	0.04	0.00	-0.05	0.01
	[0.07]	[0.04]	[0.06]	[0.02]
Observations	810	810	810	810

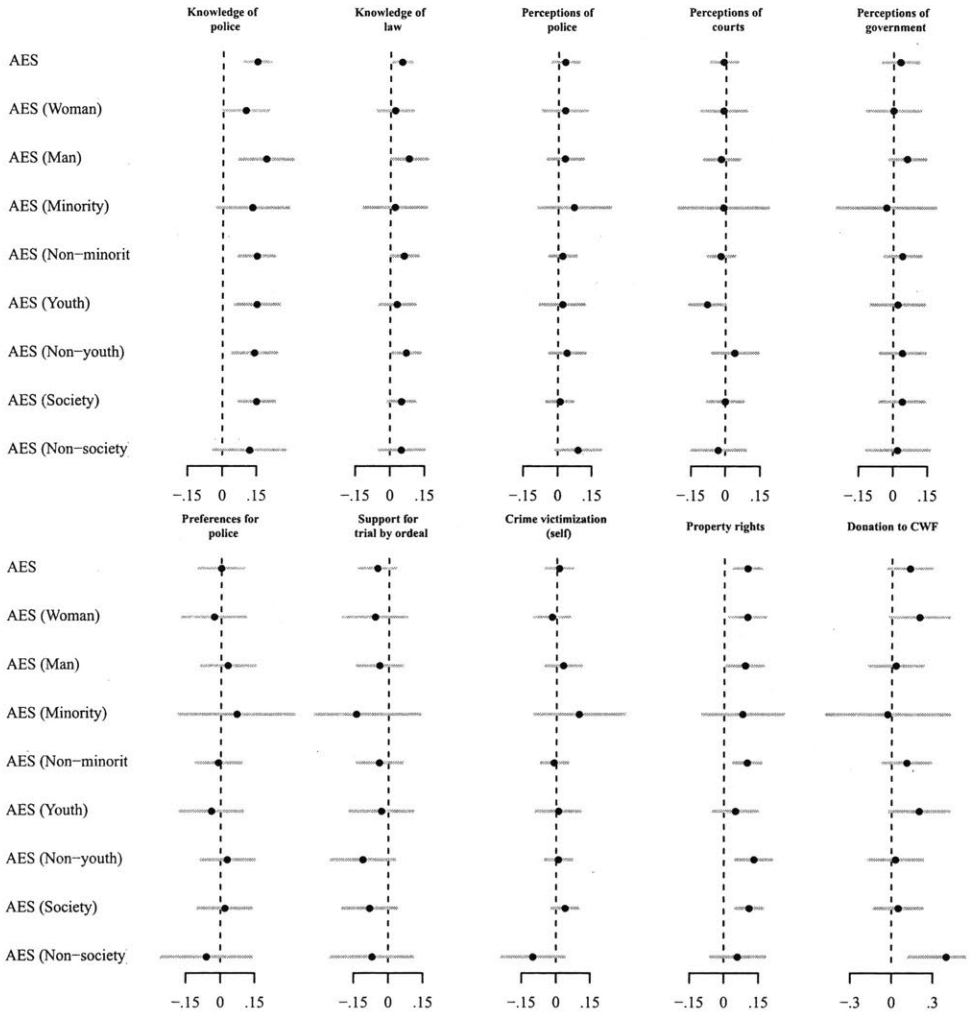
Notes: Standard errors, clustered by community, in brackets. This analysis includes only crimes against the respondent or the respondent's immediate family, and excludes those that occurred in the community at large. $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$.; $^{***}p < 0.001$.

Table 2.3: 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*	0.09*	0.04+
	[0.03]	[0.04]	[0.02]
Control group mean	0.55	0.44	0.10
Observations	1324	1324	1407

Notes: Control group means are covariate-adjusted. Standard errors, clustered by community, in brackets. $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$.; $^{***}p < 0.001$.

Figure 2-3: 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.

Chapter 3

Reducing Vigilantism in Fragile States: Evidence from a Field Experiment with the Liberian National Police

Reducing Vigilantism in Fragile States: Evidence from a Field Experiment with the Liberian National Police

Benjamin S. Morse - Massachusetts Institute of Technology

Abstract

How to increase reliance on the police and reduce reliance on vigilantism in fragile states? Community policing programs have long been at the forefront of policymakers' efforts to address this challenge. These programs tend to be more expansive than those found in developed countries, and often focus not only on building trust through meetings and foot patrols, but also on eliciting coproduction from communities to supplement police capacity and provide alternatives to vigilantism. I partnered with the Liberian National Police (LNP) to experimentally evaluate the effectiveness of this approach in Monrovia, one of sub-Saharan Africa's most crime-ridden cities. Combining data from a large-scale survey of residents with data on crime reporting from the LNP, I find that the program improved relations between police and citizens, strengthened social norms against vigilantism, and mobilized communities to participate in the police's "Watch Forum" initiative by forming and sustaining local security groups designed to facilitate reliance on police. These changes were accompanied by a roughly 40 percent reduction in the incidence of mob violence in treatment communities relative to control communities over a one year period, as measured by responses to the resident survey as well as an independent survey of key informants from each of Monrovia's police stations. Despite these improvements, the program did not reduce crime, improve perceptions of security, or increase crime reporting. These findings suggest that integrating local communities into formal policing practices can strengthen state authority, promote compliance with the rule of law, and help to redirect communities away from extralegal justice. However, such a strategy should not be viewed as a panacea for broader problems of crime and police (in)effectiveness in weak states.

3.1 Introduction

Insecurity due to crime and violence is pervasive in many of the world's weakest states (Mack et al., 2013). To address this problem, donors and aid organizations invest millions of dollars annually in assistance programs designed to strengthen the capacity of state security forces and legitimate their authority in the eyes of citizens. This two-pronged approach has long dominated the practice of security sector reform, and is motivated by the idea that institutional capacity and citizen cooperation are "close complements" in the production

of security and therefore must be built in tandem if countries are to escape fragility and violence (Baranyi et al., 2011).

Yet in recent years a growing number of scholars have begun to question whether this narrow, state-centric approach to security sector reform is optimal, and in particular, whether it adequately addresses short term security needs (Baker, 2009; Chirayath et al., 2005; Gordon, 2014). At the heart of these concerns is a fundamental problem of sequencing: building strong, effective security forces takes decades; in the interim, encouraging citizens to rely on these institutions even as they remain fundamentally untrustworthy and unreliable is unlikely to lead to meaningful improvements in security, and could potentially backfire if engaging with corrupt and incompetent security forces serves to harden citizens' negative perceptions.

Traditional, state-centric models of security reform also struggle to provide a viable alternative to extrajudicial practices such as mob violence, lynching, and vigilantism. Instead, they address these problems with outreach, awareness, and sensitization programs designed to foster norms that support the rule of law and reject extrajudicial practices (Carothers, 2006; Valters et al., 2015). Yet because these interventions do little to address the deficiencies of state security forces, reliance on extrajudicial mechanisms tends to persist.

If building effective, legitimate security institutions takes decades, how can policymakers improve security and reduce reliance on extrajudicial practices in the short term? In this paper, I partner with the Liberian National Police (LNP) to test an approach rooted in "multidimensional" models of security reform, which call for policymakers to recognize that multiple non-state actors often play a role in security provision in countries with severe and deeply-rooted capacity deficits (OECD, 2008; Baker and Scheye, 2007). In these settings, "first-best" solutions founded on the Weberian ideal of the state as the sole legitimate provider of security are not always possible; instead, the best way to "respond to the short-term needs of enhanced security and justice service delivery, while also building the

medium-term needs of state capacity" may be for security forces to develop "productive partnerships" with local communities and non-state security actors (OECD, 2008, 7).

The challenge, of course, is to do so in a way "that builds on local networks and institutions *without* encouraging vigilantism" (Rose-Ackerman, 2004, 187, emphasis mine). Liberia's model of community policing responds to this challenge by expanding the scope of traditional, 'Weberian-style' community policing programs, to focus not only on encouraging reliance on the police, but also on providing communities with a lawful, rights-respecting avenue through which they may 'coproduce' security in coordination with the police.[1] The program, which centers on the LNP's "Community Watch Forum" initiative, is founded on the recognition that communities living in the shadow of the state will invariably seek to self-provide protection, and that, in the absence of lawful alternatives, the risk that these efforts will be directed towards extralegal practices is high. Rather than ignore this reality, the program aims to provide an alternative that will redirect communities away from extralegal practices towards activities which complement police authority and potentially help address the capacity constraints that limit their effectiveness.

In practical terms, the LNP's 'coproduction' model of community policing involves two sets of activities. First, in line with traditional models of community policing, small teams of officers organize town hall meetings, conduct foot patrols, and distribute informational pamphlets in targeted communities on a bi-monthly basis. Through these efforts, the program aims to build trust, engender norms against the use of mob violence, and provide residents with the knowledge, familiarity, and confidence they need to cooperate with the police. Second, officers use the town hall meetings and foot patrols to introduce residents to the "Community Watch Forum" initiative, which calls for communities and their leaders to assist the police by forming (and sustaining) groups whose members i) meet regularly

[1] I use the term 'coproduction' to refer to citizens performing security-enhancing activities which are traditionally performed by police, such as investigating crimes or apprehending suspects. For additional background on coproduction, see Ostrom (1996) on coproduction and its role in the provision of public services in developing countries and Cammett and MacLean (2014) on the relationship between coproduction and non-state welfare provision.

with officers to design proactive, collaborative strategies to combat crime; ii) proactively seek out information about security threats to share with the police; iii) help officers investigate crimes in their communities; and iv) monitor their communities at night on occasion or during periods of peak crime.

Coproductive models of community policing are increasingly common in fragile states, and often receive considerable support from aid agencies and international donors (Wisler and Onwudiwe, 2008; Dinnen and Peake, 2013; Kagoro, 2019). However, many policymakers remain reluctant to endorse this approach because they fear that local security groups could evolve into vigilante groups, become co-opted for political purposes, or reinforce unaccountable power structures within communities. But there is little rigorous experimental evidence to inform whether these fears are well-founded and outweigh the potential benefits.

To address this gap, I partnered with the Liberian National Police to experimentally evaluate the effectiveness of their community policing program in Monrovia, one of sub-Saharan Africa's most crime-ridden cities and a place where authorities have long struggled to combat vigilantism and mob violence (Zanker, 2017; Downie, 2013). Working with officers across Monrovia's ten police zones, I identified a sample of 93 communities that were eligible for the program, and randomly selected 45 of these communities to receive the program for a period of 10 months, from February to November 2018.

Using a combination of baseline and endline survey data, official crime reports, and qualitative field reports from research assistants assigned to shadow the police for the duration of the study, I provide four sets of results. First, I find that the program improved relations between police and citizens, with residents in treatment communities more likely to view the police was capable and well-intentioned, and more likely to personally know individual officers. Second, and more significantly, the program mobilized communities to more actively organize and support local security groups. While such groups were common in control communities as well, groups in treatment communities were much more

likely to be registered with the police's Watch Forum initiative, and were sustained by greater contributions of time and effort from residents, as measured by attendance at security meetings and participation in community patrols. Importantly, these effects were measured more than three months after the program had ended, and thus reflect changes in behavior unrelated to participation in the program itself.

A central concern for policymakers is whether coproduction models of community policing can re-orient communities' efforts to self-provide security towards more rights-respecting practices. My third set of findings strongly suggests this was the case. In addition to being more trusting of police, residents of treatment communities were more knowledgeable of the rules and guidelines governing local security groups (especially with regards to suspects' rights) and less willing to condone mob violence. Crucially, these average improvements are also observed among the subset of respondents who were actively involved in local security groups, suggesting that security providers in treatment communities were on average more trusting, knowledgeable, and opposed to mob violence than security providers in control communities. Finally, and perhaps most significantly, residents of treatment communities reported 40% fewer instances of mob violence in the year preceding the survey, as compared to residents of control communities. This result does not appear to be driven by reporting bias, and is replicated using data on incidents of mob violence from an independent survey of key informants at each of Monrovia's 32 police stations.

I also find little evidence to suggest security groups undermined social accountability or enabled predatory behavior by group members or leaders. To the contrary, members of security groups were broadly representative of the communities they served in terms of religion, age, income, education, ethnicity, and political orientation (though not in terms of gender). In addition, the program led to large and substantively meaningful improvements in perceptions of the fairness, integrity, and transparency of local leaders, who played a leading role in coordinating the intervention and organizing security groups.

My final set of findings relates to the program's downstream impacts on crime and security. Here, I find that although program made communities more trusting, better organized, and less supportive of mob violence, these improvements did not lead to reductions in the overall incidence of crime or the incidence of any particular category of crime, as measured by both the survey and police administrative data. The program also did not increase crime reporting or individual forms of cooperation with police, such as providing crime tips or assisting with police investigations. And it did not improve residents' perceptions of security or their overall satisfaction with police performance.

These null effects suggest that policymakers should remain clear-eyed about what community policing programs can achieve in countries like Liberia. They are not a panacea for problems of crime, violence and police (in)capacity in fragile states, and they cannot "short-circuit" the long process of security sector reform by mobilizing coproduction from citizens. So long as the police remain incapable of responding effectively to crimes or thoroughly investigating cases, residents are likely to remain insecure, reluctant to report crimes, and unsatisfied with police performance.

But this is not to minimize the significance of the program's successes. Reliance on extrajudicial justice is a stubborn issue in Liberia and in many countries around the world, and the policy literature on security sector reform offers few ways to address this problem. The results reported here suggest that one potential remedy may lie in the adoption of coproduction models of service delivery. While coproduction has traditionally been viewed as a potential remedy for capacity constraints in resource-scarce settings (Ostrom, 1996), the findings reported here suggest it can also serve to divert communities away from harmful or rights-abusing practices. Further research is needed to verify this potential, however, as well as to determine the conditions under which coproduction would be ill-advised in light of the potential risks. In the interim, policymakers should proceed cautiously and incrementally, taking care to incorporate monitoring mechanisms that can identify signs of adverse impact before they materialize.

This study makes two main contributions to the academic literature. First, this study contributes to the state-building literature on governance in "hybrid political orders" by showing that delegation to local communities need not undermine state authority, and may in some circumstances strengthen it by diverting communities away from extralegal practices (Boege et al., 2009).[2] This conclusion runs counter to traditional perspectives on state-building, which have long viewed hybrid spaces as a realm of conflict in which state and non-state actors confront each other in a zero-sum competition for social control (Migdal et al., 1994, 66). This perspective, which has proven influential among practitioners, implies that successful state-building requires ever increasing centralization and state domination over contested spaces. The findings reported here, by contrast, draw attention to the many forms of collaboration and accommodation that occur along the way, and the role that these arrangements can play in strengthening the state and its rule of law.[3]

This study also contributes to the literature on the causes of vigilantism and mob violence. Whereas scholars have historically viewed these practices as the "best response" of communities plagued by high levels of crime and weak or absent security institutions (Abrahams, 1998; Buur and Jensen, 2004; Rotberg, 2004), more recent research contests this view, arguing that violent, extralegal practices in fact do little to improve security, and are instead driven by emotive reactions to crime, such as anger, moral outrage, and desire for retribution (García-Ponce et al., 2018; Darley et al., 2000; Johnson, 2009). Mob violence is irrational, according to this latter perspective, and there is little policymakers can do to combat it. My findings are inconsistent with this view, and instead suggest that coproductive models of community policing can redefine what communities view as their "best response" to crime in settings plagued by weak policing, in line with more traditional

[2]Following (Boege et al., 2009) and much of the literature on multi-layered models of security reform (Dinnen and Peake, 2013), I use the term *hybrid* to refer to security systems in which both state and non-state actors contribute to the production of security.

[3]For other examples of accommodation and collaboration between state and non-state actors, see Baldwin (2016) on the provision of public goods, Tamanaha (2008) on legal pluralism, Goldstein and Udry (2008) on property rights, Ansell and Lindvall (2013) on religious versus secular education, and Qi (2013) and Hillenbrand (2006) on traditional-conventional medicine collaboration in sub-Saharan Africa.

perspectives on the causes of extralegal violence.

3.2 Theory

3.2.1 Background on community policing in fragile states

Community policing programs are premised on the idea that citizen cooperation is essential for effective policing (Moore, 1992; Tyler and Fagan, 2008; Skogan and Frydl, 2004). Because police cannot be everywhere all of the time, they must rely on citizens to learn about crimes that have occurred, identify potential suspects, and gather evidence to bring wrongdoers to justice. Recognizing the central role that citizens play in crime prevention, proponents of community policing have called for a shift in how police work is done, from strategies oriented around the exercise of coercive law enforcement power, to strategies focused on building and sustaining relationships with citizens, community leaders, and civil society organizations (Gill et al., 2014b). The hope is that, as residents' confidence and familiarity with the police improves, they will become more cooperative and more willing to provide information to the police. And as the police become more familiar with the communities they serve, they will tailor their strategies to more effectively address the drivers of crime. Together, greater cooperation from citizens and greater local knowledge among rank and file officers will improve the police's overall ability to prevent, detect, and solve crimes (Ferreira, 1996).

A wide range of interventions have been implemented under the auspices of community policing, from town hall meetings and beat patrols, to door-to-door canvassing and pamphlet distribution, to education programs in schools, churches, and mosques. Despite their varied form, these interventions all share the overarching goal of increasing citizen cooperation with the police. Viewed through the lens of costs and benefits, this may occur either because these programs increase the expected benefits of cooperation (e.g. by building trust in police intentions or confidence in their ability to investigate crimes), lower the expected

costs (e.g. by increasing knowledge of how to contact the police or the functioning of the criminal justice system), or do a combination of both.

Although community policing programs originally came to prominence in the U.S. and UK during the 1970s and 1980s as a response to rising crime, in recent years they have become popular in fragile and conflict-affected states as policymakers search for cost-effective strategies to improve police legitimacy and strengthen the rule of law. Major donors such as the World Bank, DFID, USAID and others now spend millions of dollars annually to promote community-oriented policing programs (Denney and Jenkins, 2013). The UN, for its part, has made community policing a mainstay of its peacekeeping strategy, calling for "community-oriented policing and intelligence-led policing to guide all operational activities of the United Nations police in their support to host state police" (United Nations, 2016, 7).

To a significant degree, support for community policing in fragile states is based on the same premise that undergirds support in developed countries — namely, that greater cooperation from citizens will lead to greater effectiveness on behalf of the police. The UN's manual on community policing, for example, describes the logic behind community policing as follows: "greater public trust and confidence in the police leads to an enhanced flow of quality information from the public, which in turn fosters increased police effectiveness" (United Nations, 2018, 10). And greater police effectiveness, in turn, creates a more positive public perception of the police, precipitating a "virtuous cycle" of trust and police effectiveness (United Nations, 2018, 11).

Yet scholars and policymakers have increasingly begun to question whether this optimistic logic holds in fragile, low-capacity states, where security forces often face shortages of personnel, vehicles, and even basic supplies such as batons, radios and stationary (Downie, 2013; Baker, 2009). From a theoretical perspective, the same premise that motivates community policing — that citizen cooperation and police capacity are 'close complements' in the production of security — also implies that greater cooperation will be

of little benefit when the police cannot reliably respond to incidents, investigate reported crimes, or follow-up on crime tips. Absent broader, more comprehensive reforms addressing police capacity constraints, community policing programs may be bound to fail.

There are other risks to community policing in these settings as well, beyond the risk of failure. The irony of promoting reliance on police who remain fundamentally unreliable is not likely to be lost on ordinary citizens, whose everyday interactions with the police often involve acts of incompetence or corruption | experiences which research suggests tend to have an outsize influence on citizens' perceptions of the police (Sahin et al., 2017; Brunson, 2007). Positive interactions during town hall meetings and foot patrols are unlikely to outweigh these experiences, and may be ignored altogether if citizens view these overtures as "cheap talk" or "bluffing", with potentially lasting damage to police credibility (Steinberg, 2008, 36).

Even if these programs do prove persuasive, building citizens' trust and increasing their reliance on police, emerging research suggests these effects could prove counterproductive in the long-run. Blair et al. (2016), for instance, find that reoccurring "Confidence Patrols" by the police in rural Liberia increased crime reporting but decreased victims' satisfaction with how reported crimes were handled, a finding they interpret as evidence that the intervention raised expectations beyond the police's capacity to meet them, with ambiguous implications for reliance in the long run. Officials in the UN's Peacekeeping Office reached a similar conclusion after reviewing a decade of UN support for community policing in host-states, warning that:

> Newly introduced community-oriented policing initiatives are all too frequently too quick to promise much and deliver little of tangible benefit to the recipient communities; which with time and repetitiveness are likely to undermine confidence, trust and respect in the police and can leave police-community relations in a worse position than before. (United Nations, 2018, 9)

In addition to concerns about the ability of the police to follow-through on their promises, observers have expressed concerns about the fact that traditional models of community policing tend to view security in the Weberian sense, as "a non-negotiable state monopoly"

(Wisler and Onwudiwe, 2008, 435), when in reality communities pursue security through a variety of avenues, ranging from private security firms, to neighborhood watch forums, to vigilante groups, to mob violence (Hills, 1999; Rose-Ackerman, 2004). Though these actors often operate outside the law, they are often viewed as legitimate in the eyes of ordinary citizens, and they often serve as important providers of security, deterring crimes when the police are too weak or ineffective to do so themselves (Baker, 2009; OECD, 2008). In such settings, encouraging reliance on the police at the expense of local sources of security could exacerbate problems of crime and violence if the police prove to be 'imperfect substitutes' for non-state security providers, with potentially negative consequences for long-term efforts to promote reliance on formal-sector justice and security institutions.

3.2.2 Community policing and 'multi-layered' models of security reform

In many ways, concerns about the viability of community policing programs in fragile states are emblematic of what Andrews et al. (2017) refer to as the problem of "premature load bearing" in the field of development, which occurs when "institutions and organizations are required to perform tasks before they are actually capable of doing them" (Andrews et al., 2017, 54). The result, they argue, is akin to "putting too much weight on a structure before it is able to support it ... not only does this not accomplish the task at hand, it also sets progress back" (54).

Premature load bearing is common in many sectors of states undergoing reform, but it may be especially common in the justice and security sectors because of the premium placed on the state's role as the *sole* legitimate provider of these services. In line with Weberian notions of state authority, efforts to improve security and strengthen the rule of law typically focus exclusively on *state* security institutions, and very much depend on citizen cooperation and reliance for success. Yet because these reforms take decades to materialize, policymakers often find themselves in the uncomfortable position of promoting

reliance on security institutions which are still very much in the process of reform.

This contradiction is not lost on policymakers, however, and in recent years many have begun to advocate for a more "multi-layered" approach to security sector reform that builds on local networks and institutions in order to supplement police capacity and ease the demands placed on under-resourced police (OECD, 2008; Baker and Scheye, 2007; Chirayath et al., 2005; Denney, 2014). This approach calls for policymakers to recognize that "first-best" solutions involving state delivery of security and justice services may be unrealistic when the state faces significant capacity deficits and lacks legitimacy in the eyes of large segments of its population (OECD, 2008, 7). Rather than focus exclusively on state institutions, reforms should focus on developing partnerships between police and local leaders and communities, and empowering these actors to contribute to the provision of their own security within the confines of established rules and guidelines. In this way, the multi-layered approach to security reform aims to direct the collective action potential of communities towards activities which "complement rather than undermine the state's ability to provide security," and thereby help "respond to the short-term needs of enhanced security and justice service delivery, while also building the medium-term needs of state capacity" (OECD, 2008, 11).

In practice, multi-layered security reforms require a degree of the delegation on behalf of the police, as well as a corresponding amount of 'coproduction' by communities (Wisler and Onwudiwe, 2008; Ostrom, 1996). For example, police may delegate authority over misdemeanor crimes to local leaders or chiefs, encouraging them to adjudicate these cases while referring more serious ones to the police solving crimes. Similarly, police may delegate responsibility for nighttime security patrols to community watch groups, freeing officers to focus on responding to crimes and incidents. In both of these arrangements, inputs from government are combined with complementary inputs from "citizen producers," resulting in an improvement over government production alone (Ostrom, 1996, 1082).

For those involved in tailoring traditional community policing programs to meet the

needs of fragile states, the concept of coproduced policing has become increasingly influential (Baker, 2009; United Nations, 2018). Increasingly, these programs focus not only on building trust and compliance with the police, but also on encouraging communities to take ownership over their security and form coproductive partnerships with police. In urban settings, one of the principal ways that this approach has been put into practice is through community watch programs, wherein communities elect volunteers who, after being trained and vetted by the police, are tasked with keeping watch over their communities at night, assisting with police investigations, and providing information about potential criminal activity. More generally, members of these groups may serve as advocates for the police in their communities, encouraging their fellow citizens to remain vigilant, to contribute to community-wide efforts to combat crime, and to rely on and cooperate with the police.

This 'coproductive' approach to community policing holds the potential to address the risks and pitfalls of more traditional models of community policing in several important ways. First, by harnessing the collective action potential of communities to serve as a "force multiplier" for the police, the coproduction approach to community policing may help address the manpower constraints that so often prevent police from responding effectively to crime, and thereby may help reduce the risk that greater reliance on police alone will prove either ineffective or counterproductive in combatting crime. Second, coproduction stands to be a more effective way to manage expectations in the process of building trust and confidence: rather than focus exclusively on improving citizens' expectations of police competence and trustworthiness, only to risk being unable to meet them, the coproduction model requires that police openly acknowledge their constraints in their bid to elicit coproduction from citizens. And finally, the coproduction model recognizes that communities living in the shadow of the state will invariably seek to self-provide protection, and that, in the absence of lawful alternatives, these risk that these efforts will be directed towards practices that serve to *substitute* for the state, such as vigilantism and mob violence,

is high. Whereas traditional models largely ignore local forms of security, and thus do little to remedy their more problematic aspects, the coproduction model seeks to address extra-legal practices through diversion — i.e. by directing communities towards lawful, rights-respecting activities which complement, rather than substitute for, the police.

The coproduction model of community policing holds the potential to address the risks and pitfalls of traditional community policing programs, but the strategy is not without risks of its own. Delegating security is inherently risky, and could potentially increase rights abuses if watch groups do not abide by established rules and guidelines. Even when accompanied by careful vetting and training to ensure that members obey the law and respect suspects' rights, these risks cannot be ruled out entirely. Alternatively, the level of collective action from communities required to sustain these groups may prove difficult to achieve in practice. Prior research shows that efforts to empower communities through "community-driven development" programs are often unsuccessful, particularly in countries recovering from conflict (King and Samii, 2014). Communities may view watch groups as nonviable and therefore not worth their time and effort, or they may lack the collective action capacity to sustain them overtime. Police, for their part, may lack the organizational capacity to mobilize, track, and manage these groups; they may find them to be difficult to manage or 'illegible', with constantly shifting memberships and unclear leadership structures; or they may view them as a threat to their authority. In light of these barriers, the coproduction approach to community policing may well fail to engender meaningful levels of coproduction or collective action from citizens.

While coproductive models of community policing have become increasingly common in fragile states — and increasingly popular among the donors that support them — support for this approach is based almost entirely on before/after comparisons or qualitative assessments, and there remains little rigorous evidence to inform whether the potential benefits of this approach indeed outweigh the risks.

3.3 Setting

Liberia is a small West African nation of approximately 4 million people. Between 1989 and 2003, the country experienced two devastating civil wars that killed over two hundred thousand civilians, displaced a large majority of the population, and left the country's government in a state of collapse. Efforts to reform the security sector began in 2004 under the direction of the UN peacekeeping mission (UNMIL). Since then, the government and its international partners have invested hundreds of millions of dollars to train and equip Liberia's police force. Yet after more than a decade and a half of intensive reform, Liberia's police force still lacks effective presence in most neighborhoods and much of the country remains plagued by rampant crime and insecurity. This is especially true in Monrovia, Liberia's capital city and home to roughly 70% of the country's residents, many of whom live in densely-populated informal settlements. According to Afrobarometer data collected in 2016, 65 percent of the city's residents reported that they or someone they knew was a victim of theft in the past year, 35 percent reported that they or someone they knew was physically assaulted, and 78 percent said they felt unsafe in their neighborhood "several times", "many times", or "always" in the past year. Across all three sets of questions, these figures are the highest of any of the 36 capital cities covered by Afrobarometer's Round 6 Survey.

While there are many factors that contribute to Monrovia's crime rate, capacity constraints and management issues within the Liberian National Police (LNP) are widely seen as playing a central role. In 2009, the International Crisis Group described the LNP as "an institution that has serious management deficiencies, few working vehicles and scant communications equipment, often lacks even handcuffs or torchlights and still suffers from a widespread perception of malpractice" (International Crisis Group, 2009, 17). Nearly a decade later, this remains an accurate description of the state of the force. Despite improvements within elite units (Caparini, 2014), the rank and file officers with whom resi-

dents interact most often continue to lack essential equipment such as handcuffs, batons, rain gear, and even stationary (Downie, 2013). This lack of support, combined with low salaries and limited oversight by commanders, has created a "culture of impunity" within the force that permits absenteeism, petty corruption, and the practice of "hustling" at ad-hoc checkpoints — practices which not only erode trust, but also draw resources away from crime prevention (Human Rights Watch, 2013).

Among citizens, the police are widely perceived as "insufficiently motivated to adequately respond to crime," and "lacking the resources to patrol or be proactive in crime prevention" (Reeve and Speare, 2012, 8). This lack of confidence has made many citizens reluctant to cooperate with the police through activities like crime reporting, information sharing, and evidence provision, further hindering their ability to combat crime. According to survey data collected in 2012, fewer than half of crimes that occur are ever reported to the police (Afrobarometer 2012). Among those that are reported, attending officers often complain of difficulty persuading citizens to come forward with information or evidence. As a result, only a small fraction of reported cases are ever solved or prosecuted in court (Human Rights Watch, 2013).

In addition to lacking confidence in the capacity of the police, many residents are unfamiliar with the law, the criminal justice system, and the procedures and costs associated with reporting crimes to the police. For victims of crime, this lack of awareness means they must spend considerable time and effort to learn about police procedures before reporting crimes, potentially deterring them from reporting at all.

Local Security Groups

Exasperated by a lack of security and frustrated by poor police performance, many communities have elected to self-provide security by organizing local, community-based security groups. The origins of these groups date back to the period of crime and lawlessness that followed the end of the civil war, when communities formed vigilante groups to protect

themselves from criminal gangs and armed robberies. Initially, these efforts were encouraged by the government as it sought to deliver on its campaign promise to crackdown on lawlessness (Rennie, 2006), though it never went so far as to provide material support or formal regulation.

Local security groups continued to operate independently until 2009, when the government sought to establish greater control over their activities as part of a larger effort to crackdown on mob violence and strengthen community/police partnerships (Zanker, 2017). With support from the UNDP, the Community Services Section of the LNP established the "Community Watch Forum" initiative, which outlined a set of rules and guidelines to govern the conduct of local security groups, developed a formal application process to ensure that members are properly trained and vetted, and assigned responsibility to local commanders for managing and overseeing group activities. In addition, the campaign sought to broaden the focus of these groups to also encompass the role that members play in facilitating police work in their communities and encouraging cooperation and reliance among their fellow citizens.

This initiative has not proven to be sustainable overtime, however, and today most Community Watch Forums are no longer active. In their place, local security groups with little or no connection to the LNP have again emerged. Supported by donations of food, tea, and sometimes money from community members, these groups tend to be very loosely organized and to activate only in response to specific incidents or during periods of peak criminal activity (e.g. during the rainy season).

Despite their lack of affiliation, these groups do not necessarily function as vigilante groups prone to violence, as in years past. To the contrary, many officers view them as an important source of support for the police — and an important alternative to extra-legal actions such as mob violence. Officers credit this shift in part to the Watch Forum Initiative, which although unsustainable, succeeded in widely publicizing that local security groups — whether affiliated with the police or not — must never engage in violence and always

immediately report suspects to police.

Community policing in urban Liberia

Recognizing the need to build trust, educate citizens about the criminal justice system, and provide an alternative to vigilantism, the LNP with support from its international partners initiated a significant expansion of its community policing program in 2014. In Monrovia, two activities have been central this expansion. First, in each of Monrovia's ten administrative police zones, the LNP created outreach offices staffed by Community Policing Officers (CPOs) with special training in community outreach. In addition to their regular duties as patrol officers, the CPOs are responsible for organizing town hall meetings in communities on a semi-regular basis. During these meetings, officers educate residents about the criminal justice system, solicit information about security threats and brainstorm strategies to address them, and provide residents with the opportunity to ask questions or express concerns. Alongside these meetings, officers conduct foot patrols in which they interact with residents in small groups, solicit additional information about security concerns, and distribute informational pamphlets that reinforce the content communicated during the town hall meetings.

In the second component of the program, the CPOs use the town hall meetings as an opportunity to (re)introduce community leaders and residents to the Watch Forum initiative. The CPOs explain that Watch Forums are composed of groups of concerned citizens who assist the police by sharing information about security threats; meeting regularly with the police to design proactive, collaborative strategies to combat crime; facilitating police investigations in their communities; and conducting nighttime security patrols during periods of peak crime.

The LNP's support for this two-pronged approach to community policing is founded on the hope that town hall meetings, foot patrols and educational pamphlets will provide residents with the knowledge, familiarity, and confidence they need to rely on the police, while

encouraging communities to form Watch Forums will help to direct them towards lawful, coproductive forms of security provision that strengthen rather than undermine state authority and help address police capacity constraints. This model remains untested, however, and comes with several potential limitations and risks, as discussed in Section 4.2.

3.4 Research Design

3.4.1 Sample & randomization

Monrovia is divided into ten administrative police zones, which are akin to police precincts in major U.S. cities and typically composed of between 15 and 40 communities or neighborhoods. Within each zone, local research assistants worked with the CPOs to identify any 'high priority' communities to be nominated for the intervention based on assessments of crime rates, police-community relations, or other factors. This process identified 35 'high priority' communities. Because this sample size was smaller than anticipated and would have resulted in an under-powered study, an additional 65 communities were randomly sampled from the remaining population of communities to yield a target sample size of 100 communities. Half of the communities within each zone were then randomly assigned to treatment via block randomization.[4] During the baseline survey, two communities were found to be duplicates of other communities and were dropped; during implementation, staffing constraints within the research team required that the smallest police zone be dropped. These two changes resulted in a final sample size of 93 communities, 45 of which were assigned to treatment. Appendix B.3 shows that treatment and control communities are well-balanced on baseline outcome indices and covariates: differences for only two out of twenty variables tested have a $p < 0.05$.

[4] In zones with an odd number of communities, I randomly assigned $(N_b - 1)/2$ communities to treatment, where N_b denotes the number of communities in block b, resulting in a slightly less or slightly higher than .5 probability of assignment to treatment, depending on rounding. I account for this in the analysis by weighting observations by the inverse of the probability of assignment to treatment/control, following Gerber and Green (2012, p. 117), and as described in Section 3.5.3, below.

Communities in Monrovia are sub-divided into anywhere from three to six 'blocks', which are akin to small neighborhoods or street blocks in the United States. Because many communities as a whole are very large, with populations of up to roughly 5000 residents, the intervention targeted the most central block in each community plus the largest two adjacent blocks. Within each community, respondents for the baseline and endline surveys were randomly sampled from the selected blocks following a random walk procedure.

3.4.2 Intervention & implementation

Implementation began in February 2018 and continued for a period of ten months. In each of the 45 treatment communities, town hall meetings were held approximately every other month, usually on weekend afternoons, and were followed by foot patrols during the week in which officers distributed informational pamphlets. In total, each community hosted between 5 and 6 town hall meetings and foot patrols.

The town hall meetings covered a variety of topics, including: the Watch Forum initiative; the 'concept' of community policing and the importance of police/community partnerships; the procedures for reporting crimes to the police; the Professional Standards Division of the LNP and its role in handling incidents of police misconduct; the Women and Children Protection Services division of the LNP and its role in handling domestic disputes and child endangerment; and the names and phone numbers of 'key contacts' at the local police station. (An example of one of pamphlets used to summarize the messages communicated during the meetings can be found in Appendix B.1.) Attendance at the meetings ranged from as little as 10 residents to as many as 60, but most meetings were attended by between 20 and 30 residents.

Implementation of the program was monitored by research assistants from a local NGO who worked in close collaboration with the CPOs and accompanied them on all meetings and foot patrols, taking detailed notes on the proceedings, the topics covered during the meetings, and the questions raised during the Q&A sessions. Research assistants also col-

lected information on the number of residents and officers attending each meeting.

Most meetings featured a lecture the Watch Forum initiative. In the beginning, the focus was on introducing residents to the initiative and explaining how communities should go about organizing a Watch Forum. As a research assistant wrote in his Observation Notes:

> [The Zone 9 Commander] lectured on organizing a Watch Forum. He discussed the definition of a Watch Forum and the roles and responsibilities of the Forum. The Watch Team should be composed of minimum 15 members based on the size of the community and the willingness of the citizens.
>
> ... After the recruitment process, candidates will be vetted by Crime Services Division to ensure they are free of criminal charges. The leader of the Forum shall complete the Application form obtained from CPOs with attached passport size photos. Once approved they will be directed as to where they can obtain ID cards.
>
> ... He educated the residents that the Watch Forum should report all suspected criminal activities to the police. Members may apprehend suspected criminals where necessary but should report said criminals immediately to the police. They should desist from Mob Action against any violator or suspected criminals. The should not apply mob justice. They are only the eyes and ears of the police. He concluded that the community and the police need to build an active partnership in the fight against crime.[5]

Despite the emphasis on the formal application and vetting process, these procedures were seldom followed. Communities struggled to raise funds for passport photos and ID cards, while the LNP was never able to produce the official application form and almost certainly would not have been able to check candidates' criminal records.[6] In practice, vetting was the responsibility of the Town Chairman/woman and whomever s/he assigned to lead the Watch Forum. In many communities, members were drawn from pre-existing security groups that had been operating independently of the police. Training was minimal, and consisted mainly of lectures delivered either as part of the intervention or through separate security meetings organized between CPOs and Forum leaders.

But if there was one thing that officers *were* consistent about, it was in warning residents

[5]Ballah Creek Community, 4/14/2018.

[6]To the best of my knowledge, the LNP does not maintain a database that would allow for this.

to refrain from mob violence or any form of force. Members of the watch team "should be persuasive and use good manner of approach when interacting with people," an officer told attendees in Neckly Town, "they may not and should not carry weapons because they are not vigilantes."[7] In another community, the Commander made clear that instances of force by Watch Forums were against the law and would be prosecuted: "Remember, in as much as we are encouraging you to form the Watch Forum, you will answer to the law for any act of violence you commit."[8] The message was much the same in Lajoy Community: "If you bring a rogue to the police station and he is hurt and bloody, we will detain you and hold you responsible."[9]

But the threat of sanction was not the only argument that officers made to dissuade residents from mob violence; they also appealed to principles of human rights, the protections enshrined in Liberian law, and the importance of due process. The following exchanges are illustrative:

> Question: Participant Solomon asked why criminal should not be beaten when he is rightfully caught committing a crime?
>
> Answer: Patrolman Henry Flomo responded that beating the criminal is *prejudging* and torturing him. The law of Liberia is against torture. The law also says that all suspects are innocent until proven guilty in a court of law. Therefore, the suspect must be turned over to the police for investigation and eventual prosecution.

And:

> Question: Participant Ramcy asked, "suppose a criminal enters my house, and in the process of tussling he gets killed, will I be right?"
>
> Answer: Patrolman Henry Flomo responded that Murder is a crime and it is punishable by law. Report all suspected criminals to the police for investigation and allow the law to take its course.

[7]Neckly Town, 5/6/2018.
[8]Jamaica Road, 6/16/2018.
[9]Lajoy Community, 9/9/2018.

3.4.3 Hypotheses

My framework for generating hypotheses centered on two broad pathways through which I expected the program to reduce crime and insecurity. First, I considered the program's potential impact on citizens' expectations of the costs and benefits of cooperation, and how changes in cooperation would in turn influence police effectiveness and crime. Second, I considered the program's potential impact on community coordination and collective action in coproductive forms of self-protection, and how these changes would in turn influence police effectiveness and crime.

Within this framework, I pre-specified 17 hypotheses, 15 of which I test here.[10] I organize these hypotheses into three mechanism categories — costs, benefits, and coproduction — as well as one set of primary hypotheses. First, I hypothesized that the program would reduce the *costs of cooperation* by improving familiarity with the police (M1a), increasing knowledge of the criminal justice system (M1b), reducing mistrust of police intentions (M1c), and reducing social norms discouraging reliance on police (M1d). Second, I hypothesized that the program would increase the expected *benefits of cooperation* by improving perceptions of police capacity (M2a) and police responsiveness to citizen concerns (M2b). And third, I expected the program to improve community coordination and collective action in coproductive forms of security provision by reducing support for extra-legal practices such as mob violence (M3a), improving knowledge of the rules governing local security provision (M3b), and increasing participation in local security groups, particularly those affiliated with Community Watch Forum initiative (M3c).

These three sets of mediating hypotheses led to the following primary hypotheses. First, as a result of lower costs and greater benefits to cooperation, I expected individual-level forms of cooperation to increase, as measured by information sharing (H1), crime reporting

[10] Apart from the coproduction hypotheses, the hypotheses tested is this study were harmonized with the Metaketa initiative (Blair et al., 2018). The two that are excluded from this paper relate to the Metaketa initiative but are not directly relevant to the topics covered here. Concretely, they pertain to secondary impacts on attitudes toward government, as well as officer-level changes of behavior, neither of which were expected in this study. See Appendix B.8 for results from the tests of the two excluded hypotheses.

(H2), and willingness to report acts of police abuse (H3). Due to these changes as well as greater community coproduction, I expected the police to become more effective, leading to lower crime (H4), improved perceptions of security (H5), and greater satisfaction with police performance (H6).

In addition to the hypotheses listed above, this study tests whether the program reduced the incidence of mob violence (H7). This hypothesis was not pre-registered, but was central to the theoretical discussion in the pre-analysis plan.[11] A concise summary of the hypotheses tested in this study can be found in Table 3.1.

3.5 Empirical Analysis

3.5.1 Data

This study draws on three sources of data. First, I draw on data from baseline and endline surveys administered to a random sample of 20 adults in July 2017 and January 2019, respectively, in each of the 93 study communities. In addition to background information on demographics, the surveys measured outcomes pertaining to each of my fifteen hypotheses, as detailed below. Second, I draw on the LNP's administrative data on crimes reported between July 2017 — eight months prior to the start of the intervention — and February 2019 — three months after the intervention's conclusion. However, because these data capture only a fraction of crimes that are actually reported to the police,[12] and because they do not allow us to distinguish between crimes that occur and crimes that were reported, the focus of my analysis is on measures of crime and crime reporting from the large-N survey.

I also draw on data from a survey of rank-and-file officers and logbooks at each of

[11] My pre-analysis plan is available at: http://egap.org/registration/5472.

[12] Fieldwork conducted prior to the baseline survey revealed that incidents reported to the police are usually only recorded in a station's Occurrence Book if the complainant or attending officer anticipates that the case might merit court action. Furthermore, while official protocol calls for stations to forward a summary of each incident in the Occurrence Book to the LNP Statistics Division at the end of each month for inclusion in the national database, field interviews suggest Commanders usually only forward summaries for the most notable and high-profile incidents.

Monrovia's 32 police stations designed to capture information on incidents of mob violence. These data were collected were collected in March 2019 for purposes of informing and validating the results from the large-N survey. Further details on these are provided in Section 3.6.3.

3.5.2 Outcome variables

Table 3.1 summarizes the hypotheses tested in this study. Each hypothesis is linked to a cluster of between two and fourteen variables relating to individual survey questions. To mitigate the risk of false positives, I test each hypothesis using a single composite index constructed by i) recoding each variable within a given cluster so that higher values correspond to 'better' outcomes, ii) standardizing each variable by its baseline mean and standard deviation, and then iii) taking the mean across the individual variables in the cluster for each respondent.[13] One exception to this procedure is the index of crime, which is constructed as the total number of crimes reported across all categories. Appendix B.9 provides further details on the construction of the composite indices and the corresponding variables and survey questions; Appendix B.2 reports summary statistics organized by outcome cluster.

Table 3.1 organizes the hypotheses and corresponding outcomes into two categories — mechanism hypotheses and primary hypotheses. When testing the primary hypotheses, I use the procedure outlined in Benjamini and Hochberg (1995) to adjust my p-values and control the risk of false discovery to 5% (this procedure is not used when testing hypotheses about mechanisms and secondary outcomes, however). I also report results individually for all the variables within each cluster, also using the procedure outlined in Benjamini and Hochberg (1995) to control the within-cluster false discovery rate to 5%.

[13]Some variables are only available at endline. Here, I standardize the variables by the control group mean and standard deviation at endline rather than by the baseline mean and standard deviation pooled across treatment and control communities.

Table 3.1: Hypotheses and Outcome Indices

Mechanisms	Index name	Hypothesis	Pre-specified?
Costs of cooperation			
Increase familiarity with the police	know_pol_idx	M1a	Y
Increase knowledge of the criminal justice system	know_idx	M1b	Y
Improve perceptions of police intentions	intentions_idx	M1c	Y
Reduce social sanctions for reporting	norm_idx	M1d	Y
Benefits of cooperation			
Improve perceptions of police capacity	police_capacity_idx	M2a	Y
Improve perceptions of police responsiveness	pol_responsiveness	M2b	Y
Community coordination & coproduction			
Reduce norms supporting mob violence	sup_mobviol_idx	M3a	Y
Improve knowledge of rules governing coproduction	know_cwt_idx	M3b	N
Increase contributions to community coproduction	ca_sec_idx	M3c	Y
Primary Hypotheses	Index name	Hypothesis	
Cooperation with police			
Increase reporting of crimes to the police	crime_reporting_idx	H1	Y
Increase crime tips & information sharing	tips_idx	H2	Y
Increase willingness to report police abuse	police_abuse_idx	H3	Y
Security			
Reduce the incidence of crime	crime_victim_idx	H4	Y
Improve perceptions of security	future_security_idx	H5	Y
Improve satisfaction with police performance	satis_idx	H6	Y
Reduce the incidence of mob violence	cmob_num	H7	N

Notes: Each index is a composite index constructed from several individual variables. For full details on the construction of the indices, see Appendix B.9.

3.5.3 Estimation

The estimand in this study is the average treatment effect of the intervention on the sample communities (i.e. the SATE). To estimate this, I use weighted least squares (OLS) regression of the outcome on a dummy variable indicating whether or not the community was assigned to the treatment group, with weights constructed as the product of i) the inverse of the probability of assignment to treatment/control, and ii) the inverse of the probability that an individual was selected for the endline survey. The former accounts for the fact that the probability that a community is assigned to treatment or control varies across randomization blocks (i.e. police zones) (see Gerber and Green (2012), Section 3.4.3); the latter accounts for the fact that individuals from relatively large communities have a lower likelihood of being included in the endline sample compared to those from relatively small communities.[14]

Standard errors are clustered at the community level to account for the cluster-randomized design, and all specifications include block fixed effects and individual-level controls for gender, age, household size, religion, education, and literacy. For outcomes measured at both baseline and endline, I also include the community-level average outcome at baseline as a control variable (because the surveys constitute a community-level but not individual-level panel, I cannot control for baseline outcomes at the individual level).

3.5.4 Supplementary analyses

Analysis of administrative crime data

I also collect and analyze administrative crime data from the LNP (see Section B.9 for a full list of administrative crime variables). However, these data are not used to directly

[14]Sampling probabilities will be constructed using community-level population estimates. Recall that communities are subdivided into anywhere from three to six blocks, and that the intervention and survey covered the three most central blocks within each community. I make the simplifying assumption all blocks within a community are of the same size, and calculate the sampling probability for individual i in community c as: $\frac{1}{3 \times \frac{Town\_Pop_c}{Num\_Blocks_c}}$.

test any of the above hypotheses, because in addition to being very noisy, they reflect both the incidence of crime and the rate of crime reporting, making it difficult to interpret the results. Instead, I interpret these impacts cautiously and with reference to effects on crime outcomes in the survey, viewing them as corroborative rather than conclusive.

This analysis is conducted at the community-level using weighted least squares regression to account for variation in the probability of assignment to treatment/control across blocks. Specifically, I estimate:

$$y_{vb} = \beta_0 + \beta_1 T_{vb} + \gamma_b + \epsilon_{vb} \tag{3.1}$$

where y_{vb} indicates the outcome for community v in randomization block (i.e. police zone) b, γ_b denote randomization block fixed effects, and T_{vb} denotes community-level treatment assignment.

Patterns of crime reporting in treatment and control communities

Crime reporting is also measured in the crime victimization module of the survey. For each incident of crime, respondents were asked whether it was reported to the police, local leaders, or nowhere at all. To analyze these data, I employ a crime-level specification given by

$$y_{civb} = \alpha + \beta T_{vb} + \gamma_b + \mathbf{X}_{ivb}\theta + e_{civb} \tag{3.2}$$

where y_{civb} indicates whether crime c reported by individual i in community v of randomization block b was reported to the police or courts. T_{vb} denotes community-level treatment assignment, $\mathbf{X}_{ivb}$ denotes the individual-level controls for age, gender household size, religion, education, and literacy, and γ_b denotes block fixed effects. Standard errors are clustered at the community level.

The use of the crime-victimization module to measure crime-reporting helps address

some of the limitations of the administrative data, but it too is subject to potential threats to valid inference, insofar as it (implicitly) requires conditioning on crime occurrence, which itself may be affected by treatment. Comparing rates of crime reporting in treatment and control communities would therefor be misleading if treatment altered type or nature of crimes that occur, such that crimes that occur in treatment communities differed from those in control communities in terms of their potential reporting outcomes. For this reason, I interpret these effects with the same level of caution as the effects on the administrative data, viewing them as corroborative in light of effects on other outcomes rather than definitive on their own.

$$y_{civb} = \alpha + \beta T_{vb} + \gamma_b + \mathbf{X}_{ivb}\theta + e_{civb} \tag{3.3}$$

where y_{civb} indicates whether crime c reported by individual i in community v of randomization block b was reported to the police or courts. T_{vb} denotes community-level treatment assignment, $\mathbf{X}_{ivb}$ denotes the individual-level controls for age, gender household size, religion, education, and literacy, and γ_b denotes block fixed effects. Standard errors are clustered at the community level.

3.5.5 Threats to inference

Measurement error

Most of the outcomes in this study are self-reported. This introduces the possibility of response bias correlated with treatment. For example, if respondents associate the endline survey with the intervention, this could cause them to underreport certain crimes or incidents, or to report attitudes consistent with messages conveyed during the program (e.g. opposition to mob violence), introducing bias. While I cannot rule out this possibility entirely, I can point out that, taken as a whole, the patterns reported below are inconsistent with social desireability bias: although some effects align with social desirability bias to-

wards program messages, effects on several of the outcomes most closely associated with program messages and the risk of bias are null (including, among others, effects on self-reported willingness to report crimes and self-reported willingness to cooperate with the police).

Spillover

Another potential threat to inference derives from two potential sources of spillover. The first is the risk that treatment spills-over into nearby control communities, partially treating control residents and potentially leading me to underestimate the effect of the intervention. Because the sample size is small, there is not a lot that can be done to diagnose or mitigate this risk. However, I believe this risk to be relatively low, because communities are large, and because no two communities are adjacent.

The second source of spillover derives from the randomization procedure, which randomizes within but not across police zones/jurisdictions. This means that officers who participate in the intervention will also be interacting with residents of control communities in the course of their regular, everyday responsibilities. If the intervention leads to changes in officers' that in turn influence residents' perceptions and behaviors in control communities, then this could lead me to underestimate the program's impact. For instance, the program could in theory make officers more empathetic or caring, which in turn could improve residents perceptions in control communities.

My study design cannot account for these kinds of 'general equilibrium' dynamics. For this reason, my results should be interpreted as the 'partial equilibrium' effects of changing citizens' attitudes of and exposure to the police while holding constant any resulting changes in police behavior, which influence both treatment and control communities equally.

3.6 Results

3.6.1 Impacts on hypothesized mechanisms

Figure 3-1 plots average treatment effects on composite indices for each of my hypothesized mechanisms. (I report effects on the component variables that make up each composite index in the Appendix). The program did not improve residents' overall familiarity with the police, as measured by the composite index, but this null effect masks important variation in effects across components of the index, as reported in the Appendix. On the one hand, the program appears to have had a *negative* effect on knowledge of police units and services. Relative to residents of control communities, residents of treatment communities eight percentage points less likely to know about the Women and Children Protection Unit, three percentage points less likely to know about the LNP's investigative unit, and six percentage points less likely to know about the Professional Standards Division, which is responsible for handling police misconduct. On the other hand, the program appears to have improved residents' familiarity with local police officers. Substantively, residents of treatment communities were five percentage points more likely to know the name of their local commander, four percentage points more likely to know a rank-and-file officer by name, and three percentage points more likely to know their phone number (this latter effect is insignificant, however).

Figure 3-1 also suggests that the program did not improve knowledge of Liberia law, as measured by questions on habeas corpus, the right to legal counsel, the legality of "case registration fees," and laws on statutory rape and child abuse, among other basic legal questions. This null effect along with the program's potentially adverse effect on knowledge of police services are surprising given that these topics were covered extensively during the town hall meetings. One partial explanation, at least for the null effects on knowledge of Liberian law, may lie in the mixed messages conveyed during the town hall meetings

over the discrepancy between *de jure* and *de facto* police procedures. The following excerpt from a police presentation in New Georgia Community, as paraphrased by a research assistant assigned to observe the proceedings, is illustrative:

> The Officer told the citizens that are not required to give money to the police. Giving money to police to register or follow up your case is a crime, as you can see from all the signs and billboards . . . but it's true that police officers in Liberia do not have vehicle to response to cases. So if you the citizen can help the police with transportation to follow up your case, you can do that.[15]

Similar contradictions were communicated about: habeas corpus and what residents perceived as the practice of "catching and releasing" criminals without prosecution; "feeding fees" charged to complainants to feed detainees; and the challenges the government faces in providing legal counsel to defendants. These mixed messages may have led residents to mistakenly believe that certain practices are legal in light of police capacity constraints.

Confusion over the discrepancy between *de jure* and *de facto* practices cannot acccount for the program's lack of impact on knowledge of other dimensions of Liberian law, however; nor can it account for the program's apparent negative effect on knowledge of police services. While these results remain somewhat puzzling, the main takeaway is that program does not appear to have made it easier for residents to engage with the police by improving their knowledge of police services or their knowledge of the law.

Figure 3-1 also suggests the program did not reduce the social costs that citizens face when reporting crimes. Substantively, residents of treatment communities were roughly just as likely to say that community members become angry when you report burglaries, land disputes, or domestic violence to the police, as compared to residents of control communities. They were also roughly just as likely to say that providing the police with information to solve crimes can lead to backlash.

Despite these null effects, the program was successful at improving perceptions of police intentions and capacity. Most notably, residents of treatment communities were six

[15]New Georgia Community, 3/3/2018.

percentage points less likely to view the police as corrupt (a decrease of 12% relative to the control group mean); five percentage points more likely to say the police treat all citizens fairly (12%), six percentage points more likely to say the police care about the well-being of residents (9%), and seven percentage points (12%) more likely view the police as capable of responding to crimes in a timely manner. Improvements on other dimensions of intentions and capacity were modest and not statistically significant, but always positive.

Though it may seem unsurprising that positive interactions during community outreach would improve perceptions, this finding runs counter to prior studies suggesting that community policing is unlikely to improve perceptions in the absence of actual improvements to police capacity and performance (Blair et al., 2019). Indeed, outside the context of the intervention, negative experiences with the police remained common. At the time of the endline survey, for example, 8% of residents had paid a bribe to the police in the past six months, and 6% had witnessed police physically or verbally abusing suspects or residents such as market vendors.[16] Moreover, experiences with poor police performance and misconduct came up repeatedly during the town hall meetings (and during the Q&A in particular, as residents took the opportunity to air their grievances), often undermining the messages the police were trying to communicate.[17]

For policymakers, these findings are encouraging insofar as they suggest that that efforts to build trust and legitimate police authority in countries with weak capacity are not bound to fail, and that officers can have frank exchanges with residents that improve their image while avoiding the trap of 'unrealistic expectations' (United Nations, 2018, 9). This also implies, however, that improvements in perceptions are likely to be modest, and may prove insufficient to improve reliance and cooperation. I return to this discussion in the ensuing section.

[16] Asked to describe these experiences in a free-form response, respondents most often cited harassment of market vendors, presumably for operating without a permit or too close to a roadway.

[17] Among the most frequently-cited complaints: informal fees for case registration, transportation, stationary, and feeding; lack of police follow-up on reported crimes or crime tips; officers moonlighting as enforcers for powerful landlords; and officers seen fraternizing with known criminals in 'ghettos' or 'criminal hideouts'.

The program's largest and most consistent impacts were on the set of outcomes targeted by the Community Watch Forum initiative. Here, I find large and substantively meaningful effects on community collective action and contributions to local security groups. As reported in Figure 3-2, residents of treatment communities were 13 percentage points more likely to attend a security meeting in the past month, four percentage points more likely to participate in a security patrol, six percentage points more likely to report that a community watch forum exists in their community, six percentage points more likely to report the watch forum meets at least monthly, and seven percentage points more likely to report the watch forum was registered or affiliated with the police, as compared to residents of control communities.[18] As a whole, these improvements amount to a .18 standard deviation increase in the community coordination and coproduction index. It is important to note that because the one-month recall period for these actions does not overlap with the intervention, which ended three to four months prior to the endline, these effects are not merely a function of participation in the intervention itself.

Importantly, greater coproduction by communities was accompanied by greater knowledge of the rules governing local security groups. As compared to residents of control communities, residents of treatment communities were: nine percentage points more likely to know security groups are not permitted to carry weapons, five percentage points more likely to know they cannot beat an uncooperative suspect into submission, four percentage points more likely to know they should not enter into dangerous situations, and four percentage points more likely to know they *can* detain a suspect until police arrive provided they do not cause harm.

And finally, the program appears to have reduced support for mob violence, as measured by whether respondents believe mob violence would be "justified" or "somewhat justified"

[18]There is considerable variation across police zones in what it means for a Watch Forum to be "registered" with the police. The survey question on registration was not written on the assumption that respondents would necessarily be well-informed about the registration process or their Forum's registration status; rather it was meant to capture whether residents perceive the Forum to be working in coordination with the police (e.g. because they see police attend Forum meetings, or because they see group members walking or talking with police in their community).

in response to three hypothetical scenarios of crime. While this effect is not statistically significant at conventional levels ($p < 0.16$), it is consistent with the findings reported above and below, lending credence to the assumption that it is not a Type I error.

3.6.2 Impacts on primary outcomes - cooperation, crime, and security

The LNP's community policing program was designed to reduce the costs of cooperation, increase the expected benefits, and catalyze coproduction from citizens. These changes, in turn, were expected to increase cooperation, reduce reliance on extralegal practices, and improve police effectiveness, potentially leading lower crime and greater security.

Thus far, the results suggest the program may have increased the expected the benefits of cooperation and reduced the expected costs by improving perceptions of police capacity and intentions, but that overall changes to individuals' cost-benefit calculations were likely modest due to the null effects on knowledge of Liberian law, familiarity with police services, and perceptions of police responsiveness. Improvements in coproduction, by contrast, were large and substantively meaningful, and could potentially have contributed to greater security either directly (e.g. by deterring crimes), indirectly (e.g. by facilitating police work), or by substituting legal forms of coproduction for violent, illegal practices.

This section tests for such downstream impacts. As above, I report average effects on the composite index for each hypothesis in the main text of the paper and effects on the individual variables that make up each index in the appendix.[19] The results, reported in Figure 3-3, indicate the program did not improve individual forms of cooperation with the police, as measured by i) willingness to report crimes to the police, ii) provision of information or crime tips to police in the past six months, or iii) willingness to report police misconduct. (The program also did not significantly increase the likelihood that victims of crime subsequently reported their case to the police, as I show in the Appendix).

As alluded to above, these null effects may reflect the fact that changes to individuals' assessments of the costs and benefits of cooperation were too modest to influence cooperation or reliance. Indeed, during the town hall meetings, instances of police failing to follow-up on crime tips or adequately investigate crimes came up repeatedly, complaints

[19]Results on the indices for each of my primary hypotheses with multiple comparisons adjustments are also reported in the Appendix. Levels of statistical significance are unchanged from those reported here.

which officers openly acknowledged as issues they struggle with. Given that the intervention did not materially change police capacity or resources, residents' grim assessments of the value of reliance may have (justifiably) remained largely unchanged.

The program also did not meaningfully reduce the overall incidence of crime or most of the particular categories of crime measured in the survey, including: armed robbery, burglary, aggravated assault, simple assault, sexual violence, domestic violence, and violent and non-violent land disputes. Although the program does appear to have reduced verbal forms of domestic abuse as well as the number of non-violent crimes reported in the catch-all "other" category, these effects should be interpreted with caution in light of the absence of impacts on other types of crime and the risk of false positives when testing for effects on so many outcomes at once. In the appendix, I replicate the null overall effect on crime using the LNP's administrative data on reported crimes, lending additional weight to the conclusion that the program did not reduce crime.

Perhaps unsurprisingly given the null effects on crime, the program appears to have had little overall impact on perceptions of security. Substantively, residents of treatment communities were about as likely to fear violent or non-violent crime in the future or to have feared crime or felt unsafe walking in their community in the past six months, as compared to residents of control communities. They were, however, four and five percentage points, respectively, more likely to say they would feel safe leaving their generator or motorbike outside their home at night — modest though statistically significant effects that could potentially reflect greater nighttime activity by local security groups.

Perhaps the most important finding reported in Figure 3-3 is the .16 standard deviation reduction in the incidence of mob violence. Substantively, residents of treatment communities reported an average of .34 fewer instances of mob violence in their communities in the past year, a reduction of 39% relative to the control group mean.[20] Notably, these results do

[20]The wording of the question measuring mob violence was "In the past year, were there any incidents of MOB JUSTICE in your community (i.e. beating of flogging of someone suspected of committing a crime)?", followed by "How many times did this happen in the past year (12 months)?" for affirmative responses.

not appear to be a function of social desirability bias. Differences between treatment and control communities in the incidence of other types of crime that could potentially been seen as socially undesirable by respondents, such as riots, murders or any of the categories of crime measured in the crime index, are small and statistically insignificant.[21] Moreover, we do not observe positive effects on other sets of outcomes potentially seen as socially desirable, such as willingness to report crimes or the provision of information to police in the past 6 months, as reported above.

3.6.3 Robustness of results on mob violence

It is not possible to validate the mob violence finding using administrative crime reports from the LNP because incidents of mob violence are seldom recorded in official crime reports, even when reported to police. Indeed, of the 17,508 incidents included the LNP's database of crimes occurring between July 2017 and January 2019, there is only one report of mob violence, despite the fact that there were at least several high-profile incidents during this timeframe that *did* involve a police response and were reported in the local press.

In light of the limitations to administrative data, I assigned a team of research assistants to visit each of Monrovia's 32 police stations in March 2019 — three weeks after the end-line concluded and two weeks after a preliminary analysis of findings had been completed — to survey rank-and-file officers with firsthand knowledge of incidents of mob violence that occurred in their jurisdictions in the past 8 months (from July 2018 to February 2019). As street-level agents with a close working familiarity of communities, officers are well-positioned to serve as key-informants for incidents of mob violence, including those that never made it into police records.

The resulting dataset includes 41 incidents. All but two were precipitated by an act of

[21] As detailed in the Appendix, for each category of crime, we separately asked i) if it occurred to the respondent or a member of their household, and ii) if it occurred to anyone else they know in their community. The crime index takes the sum across all categories of self and others' victimization.

petty theft, such as stealing a phone or handbag in a market, and about half occurred at or after dusk. None of the incidents resulted in death, but 73% (30/41) required treatment at a hospital. None of these incidents were recorded in the LNP's electronic database, and only 20% (8/41) were documented in police logbooks — an illustration both of the limitations of police data and the virtues of using officers as key informants.

Of the 41 incidents documented by the key informant survey, 15 occurred in communities included in this study's sample – 11 in control communities and 4 in treatment communities. Incidents do not map perfectly to incidents reported by survey respondents, but there is a correlation — respondents from communities with incidents recorded in the mob violence survey were 4 percentage points more likely to report an incident themselves ($p < 0.07$) — consistent with the idea that some, but certainly not all, incidents of mob violence come to the attention of police.

Table 3.2 reports the program's impact on mob violence as measured by the follow-up mob violence survey, following Equation 3.1. The results suggest that treatment reduced the likelihood of mob violence occurring in treatment communities by 14 percentage points ($p < 0.07$), or about 60% relative to the control group mean. The effect is only marginally significant, but it is consistent with the findings from the citizen survey in direct and in magnitude relative to the control group mean.

Table 3.2: Effect on mob violence (officer survey)

	Act of mob violence in community, past 8 months	
treatment	-0.14^{+}	-0.15^{+}
	(0.08)	(0.08)
Ctrl group mean	0.23	0.23
Block fixed effects	N	Y
N	93	93

Notes: Standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < .01$, *** $p < .001$.

3.6.4 Coproduction and the risk of adverse impact

Coproduction plays a central role in helping resource-constrained governments provide essential public services (Post et al., 2017). However, because it requires the state to delegate authority and concede direct control of service provision, coproduction can also compromise accountability and equity in access to services (Cammett and MacLean, 2014). This is especially true of coproduction in the security sector, where the power afforded to non-state actors has the potential to undermine informal sources of accountability and enable predatory behavior (Baranyi et al., 2011, p. 135). In extreme cases, collaboration with non-state security actors can give rise to vigilante groups or militias that become emboldened to acquire political power and resist states' efforts to reassert control (Hidalgo and Lessing, 2015; Krasner and Risse, 2014).

While examples of these risks materializing typically involve the delegation of *coercive power*, similar dynamics could conceivably emerge even within the confines of the Watch Forum initiative. In light of these risks, this section tests for signs of adverse impact along three dimensions. First, I test whether members of local security groups, as measured in the survey by attendance at security meetings or participation in security patrols, are representative of the communities they serve in terms of age, gender, income, religion, ethnicity, and political orientation (and as a corollary, whether the intervention improved or exacerbated (mis)representation in these groups). Second, I test whether the intervention's salutary effects on trust in police, knowledge of rules governing security groups, and opposition to mob violence extend to the subset of respondents who participate in local security groups, thus indicating that security providers in treatment communities are on average more trusting, knowledgeable, and opposed to mob violence than those in control. And finally, I assess the impact of the intervention on perceptions of community leaders, who were actively involved in facilitating the intervention and coordinating security groups, on the assumption that perceptions of leaders would decline if local security groups lacked

accountability or became involved in predatory behavior.

Are local security providers representative of their communities?

To test whether local security groups are representative of their communities, I estimate:

$$\texttt{local\_security\_provider}_{ic} = \alpha_c + \mathbf{X}_{ic}\theta + e_{ic} \tag{3.4}$$

where $\texttt{local\_security\_provider}_{ic}$ denotes whether individual i in community c attended a security meeting or participated in a security patrol in the past month[22]; $\mathbf{X}_{ic}$ is a matrix of socio-demographic measures of representation, and α_c denotes community fixed effects. Because the inclusion of community fixed effects limits the analysis to within-community variation, θ coefficients can be interpreted as the extent to which members reflect their communities in terms of the variables included in $\mathbf{X}_{ic}$. For comparison, I estimate Equation 3.4 separately for treatment and control communities.

The results, reported in Table 3.3, suggest that members of local security groups are broadly representative of their communities in terms of age, education, ethnicity, religion, and political orientation, and that this pattern holds in both treatment and control communities. The exception, however, is gender. In control communities, males are over-represented by about 10 percentage points; in treatment communities, they're over-represented by about 16 percentage points, suggesting that the intervention disproportionately mobilized males. Nevertheless, taken as a whole, these results are inconsistent with the idea that coproduction models of community policing run the risk of marginalizing vulnerable members of society.

[22] 26% of respondents from control communities attended a meeting or participated in a security patrol in the past month, as compared to 37% in treatment communities.

Table 3.3: Are members of local security groups representative of their communities?

	Member of local security group	
	Control	Treatment
male	0.10** (0.03)	0.16*** (0.03)
age1830	0.02 (0.06)	0.02 (0.06)
age3140	0.05 (0.07)	0.04 (0.07)
age4150	0.13 (0.07)	0.08 (0.08)
age5160	0.01 (0.06)	-0.00 (0.08)
rel_Christian	-0.04 (0.05)	0.01 (0.06)
edu_none	0.03 (0.05)	-0.07 (0.09)
edu_abc	0.11 (0.06)	-0.12 (0.09)
edu_jh	0.10 (0.06)	-0.07 (0.06)
edu_hs	0.04 (0.04)	-0.10 (0.06)
literate	0.07* (0.03)	0.03 (0.04)
below_median_income	-0.04 (0.03)	-0.03 (0.03)
vote2017	0.05 (0.04)	0.08 (0.05)
voteOpposition2017	0.01 (0.03)	-0.05 (0.04)
tribe_bassa	0.01 (0.04)	0.04 (0.05)
tribe_grebo	-0.07 (0.05)	0.07 (0.06)
tribe_kpelle	0.00 (0.04)	0.08 (0.05)
tribe_kru	0.07 (0.05)	0.09 (0.08)
tribe_lorma	0.04 (0.06)	0.02 (0.07)
N	956	895

Notes: This table reports the association of the listed variables with membership in local security groups, controlling for community of residence. Robust standard errors in parentheses, clustered by community. * $p < 0.05$, ** $p < .01$, *** $p < .001$

Effects on local security providers' orientation towards the state and the rule of law

To test whether members of security groups differ systematically across treatment and control communities in terms of their orientation towards the police and the rule of law, I re-run the analysis reported in Section 3.6.1 on the subset of respondents that reported participating in local security groups, as measured by attendance at security meetings or participation in patrols. Because participation is post-treatment, the results from this analysis do not have a causal interpretation; rather they should be interpreted descriptively as an indication of whether there are statistically significant differences among security providers

across treatment and control communities.

The results, reported in Table 3.4, indicate that local security providers in treatment communities are more optimistic about police intentions and capacity, more knowledgeable of the rules governing their conduct, (weakly) less supportive of mob violence and (weakly) less opposed to cooperation with police — in short, the opposite of what would be expected if collaboration ran the risk of giving rise to vigilante groups that oppose the state.

Table 3.4: Differences among members of local security groups

	Familiarity w/ police	Knowledge of law	Police intentions_idx	Norms against cooperation
treatment	-0.07	-0.03	0.16*	-0.08
	(0.09)	(0.08)	(0.08)	(0.07)
N	581	581	581	581
	Perceptions Police capacity	Support for mob violence	Knowledge of security rules	
treatment	0.23**	-0.12	0.04*	
	(0.09)	(0.09)	(0.02)	
N	580	580	581	

Robust standard errors in parentheses, clustered by community

* $p < 0.05$, ** $p < .01$, *** $p < .001$

Effects on perceptions of local leaders

Table 3.5 reports the program's impact on perceptions of local leaders, following the estimation procedure used for the main analysis and outlined in Section 3.5.3. Contrary to the idea that mobilizing communities to form local security groups empowers 'local despots', the results suggest the intervention actually *improved* perceptions of local leaders, who played a central role in organizing the intervention and managing the Watch Forums. Substantively, residents of treatment communities were about 6 percent more likely to believe leaders treat all residents the same, 4 percent less likely to view leaders as corrupt, and 4 percent more likely to believe their leaders rule transparently, relative to residents in control communities. Not all of these differences are significant, but they all point in the same direction, and the effect on the composite index (Column 1) *is* statistically significant.

Table 3.5: Effects on perceptions of local leaders

	Perceptions index (std)	Leaders treat all equal?	Leaders corrupt?	Leaders rule transparently?
treatment	0.14*	0.13^{+}	-0.09	0.09
	(0.06)	(0.07)	(0.06)	(0.06)
Ctrl mean	-0.08	2.03	1.62	2.37
N	1851	1851	1851	1851

Robust standard errors in parentheses. Outcomes in columns 2-4 measured on a four-point, agree-disagree scale. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < .01$, *** $p < .001$

3.6.5 Mechanisms underlying reductions in mob violence

By what pathways did the intervention reduce mob violence? While it is impossible to answer this question definitively, the theoretical framework presented in this paper suggests at least three potential pathways. First, the intervention may have altered the *composition* of local security groups, incorporating law-abiding citizens and removing those with a reputation for violence or criminal behavior (e.g. through the vetting process associated

with the Watch Forum initiative).[23] Second, rather than altering the 'type' of resident who becomes a member, the intervention may have altered members' knowledge, attitudes, and norms, such that they became on average more integrated into formal policing practices and more supportive of the rule of law relative to members in control communities. These changes, in turn, may have made these groups less likely to perpetrate mob violence and more likely to intervene to prevent such incidents from happening in their communities. Third, and relatedly, the intervention may have altered knowledge, attitudes and norms among residents as a whole, making them more aware of how they can respond to crimes in a lawful manner by supporting or calling upon Watch Forums, more supportive of the rule of law, and less likely to resort to vigilantism.

The first of these explanations seems unlikely given the results presented in Table 3.3, which indicate that members of groups in treatment communities are comparable to those in control communities in terms of their observable characteristics (with the possible exception of gender). If the intervention had altered the types of individuals who join these groups, then we would expect to see greater differences across indicators like age, religion, political orientation, education, and ethnicity than those reported in Table 3.3.

The second explanation is consistent with the results reported in Table 3.4, which show that the intervention integrated local security groups into formal policing practices and strengthened support for the rule of law among group members. Further support for this explanation is found in the Observation Notes recorded by local research assistants during the town hall meetings, which document several instances in which residents spoke of local security groups diffusing situations that otherwise could have escalated into mob violence. In one community, the Chairman began the meeting with an account of a recent incident

[23]The vetting process varied across communities, but generally fell short of the police's goal to conduct a formal background check on every member. Nevertheless, the idea that members should be law-abiding citizens and upstanding members of the community was central to the intervention and emphasized repeatedly in the town hall meetings. More common forms of vetting included: i) members presenting themselves at town hall meetings, affording residents the opportunity to contest their membership on the basis of past criminal behavior, or ii) the Town Chairman/woman herself selecting or disqualifying candidates on the basis of their reputation.

in which members of the newly-initiated watch team performed a citizens' arrest of a suspicious person in the middle of the night and reported him to the nearest police station.[24] In another, a member of the Watch Forum recounted how he and his colleagues directly intervened to prevent mob violence:

> The member of the Watch Forum said that he was grateful to the police for the knowledge they have gained from the Watch Forum initiative. He said that recently the Watch Forum was able to save an alien from mob violence in their community. He said there was a strange man who could not speak to the common understanding of the local residents. Community people had surrounded him to beat him because they suspected him to be a missing criminal.
>
> According to him, because of their knowledge on mob violence and their role in the Watch Forum, they were able to intervene and asked the man what he wanted to do in their community? The man told them that he went to see somebody in the community but he forgot the person's name. They also asked him about where he came from? He told them he came from a church and they decided to walk with him to the church. When they got to the church, they ask the people at the church whether they knew him? The people at church said yes they knew the man and they thanked them for protecting the man.[25]

References to members of the Watch Forum escorting suspects to the police or otherwise diffusing dangerous situations came up in at least two other town hall meetings.[26] Though relatively rare as a proportion of the total number of meetings, the observation protocol was not designed to screen for these events, and it is likely that others went unrecorded.

The results reported in this paper are also consistent with the third explanation, whereby the intervention's impact on knowledge, norms, and attitudes among residents as a whole made them less likely to resort to vigilantism. Not only were residents of treatment communities less supportive of mob violence, they were also more aware of the presence of local groups operating in coordination with the police in their communities. In particular and as reported previously reported, they were considerably more likely to either participate in security meetings or group activities (30%) or to be aware of them happening (50%), relative

[24] Kpelleh Town Back Road, 8/11/2018.
[25] Catholic Community, 9/23/2018.
[26] Nyanford Community, 10/21/2018; New Hope Community, 8/4/2018.

to residents of control communities (at 18% and 35%, respectively). In addition, analysis of additional outcomes from the endline survey indicates they were: eight percentage points more likely to agree or strongly agree with the statement "My community is well-organized in the fight against crime" (($p < 0.18$); thirteen percentage points more likely to agree or strongly agree that "Leaders in my community have a close working relationship with the police"($p < 0.03$); nine percentage points more likely to agree or strongly agree that "Members of my community do a good job of working together to help the police"($p < 0.08$); and five percentage points more agree or strongly agree that "Leaders in my community do a good job of organizing my community to combat crime" ($p < 0.38$). While these differences do not constitute direct evidence of substitution away from vigilantism, they do suggest that residents of treatment communities became more aware of local security groups and their ability to respond to crimes in a manner that complements rather than substitutes for the police, and that their perceptions of the efficacy and accessibility of these groups improved.

3.7 Conclusion

Police in weak states face many obstacles in trying to provide security, not least of which is a severe shortage of capacity. In the absence of effective policing, communities often resort to extralegal practices such as mob violence, lynching, and vigilantism. Police absence or ineffectiveness can also create a security vacuum in which non-state actors emerge to fill the void. Overtime, these actors can accumulate political power and come to act as "spoilers who reject the state-building project because they believe it poses a fundamental threat to their political or economic interests" (Menkhaus, 2007, 96).

Reforms designed to strengthen the police and establish their position as the predominant provider of security take decades to materialize. In the short term, addressing security needs in a way that strengthens rather than undermines police authority requires finding

ways to incorporate communities into the production of security while simultaneously reducing the risk of vigilantism.

Community policing programs have been at the forefront of governments' efforts to address this challenge. These programs aim to do this by building confidence in the police, dissuading citizens from reliance on extralegal practices, and providing legal, rights-respecting pathways through which communities may coproduce security. The hope is that these activities will divert communities away from vigilantism while also helping to improve police effectiveness.

But there is little evidence on whether these programs work as intended. I address this gap by experimentally evaluating the Liberian National Police's coproduction model of community policing. I show that program improved familiarity and perceptions of police and mobilized communities to coproduce security by forming local security groups and incorporating them into the police's Watch Forum initiative. I further show that these changes were accompanied by a roughly 40 percent reduction in the incidence of mob violence, suggesting that local security groups were seen as a viable alternative to extralegal practices.

I also find, however, that despite improvements in trust and coproduction, the program did not reduce crime, improve perceptions of security or increase crime reporting. I argue that these null effects most likely reflect the fact that capacity constraints within the LNP remained unchanged by the intervention. As a result, efforts by communities and security groups to provide crime tips or facilitate police investigations were seldom met with an effective police response, and most residents remained skeptical of the value of reporting crimes or otherwise cooperating with police.

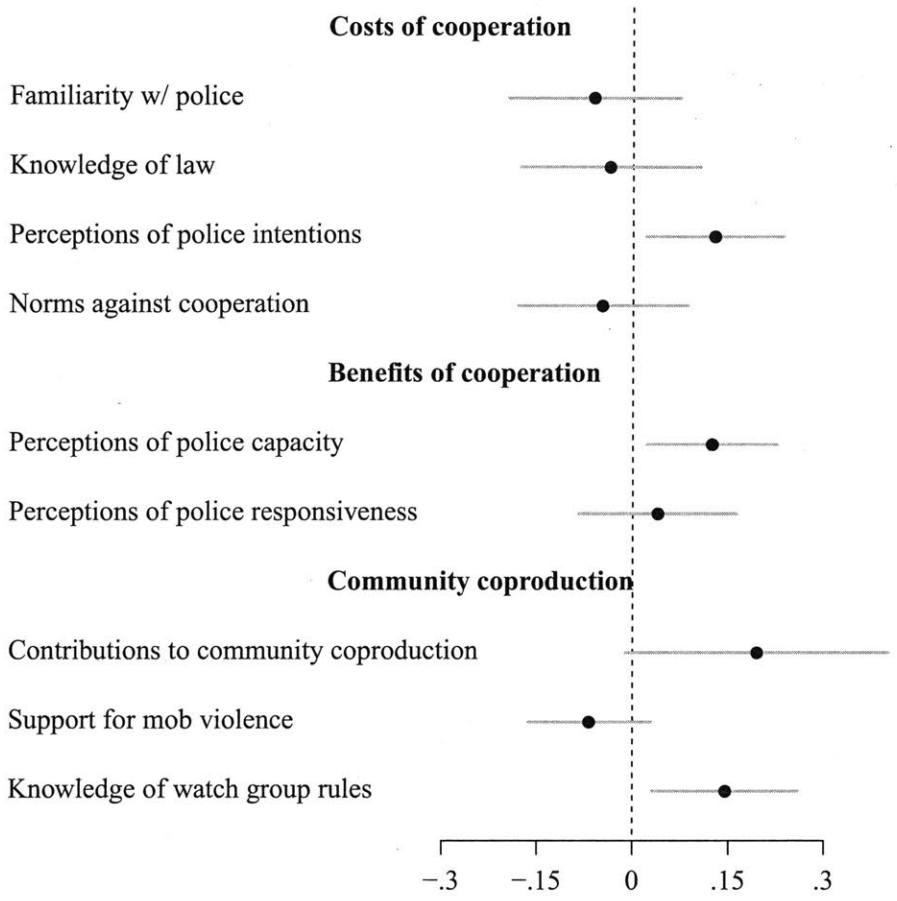
These findings have important implications for the literature on coproduction as a strategy to supplement weak state capacity in developing countries. Much of this literature assumes that coproductive strategies are most important precisely where capacity constraints are most severe, because states with low capacity can least afford to provide services on

their own. My findings, on the other hand, suggest a certain baseline level of capacity may be a prerequisite for successful coproduction. When capacity is so low that government institutions cannot reciprocate citizens' contributions with contributions of their own, coproduction is unlikely to improve the quality of services.

My findings also suggest, however, that while coproduction cannot substitute for the shortcomings of weak states, it can be effective at drawing communities away from harmful or rights-abusing practices. This finding has important implications for other service sectors where provision by non-state actors may conflict with international standards or human rights, such as the justice, health, and education sectors. When designing policies in these and other sectors where non-state providers are prevalent, policymakers should consider coproduction not only for its potential to supplement state capacity, but also for its potential to divert non-state actors away from harmful practices.

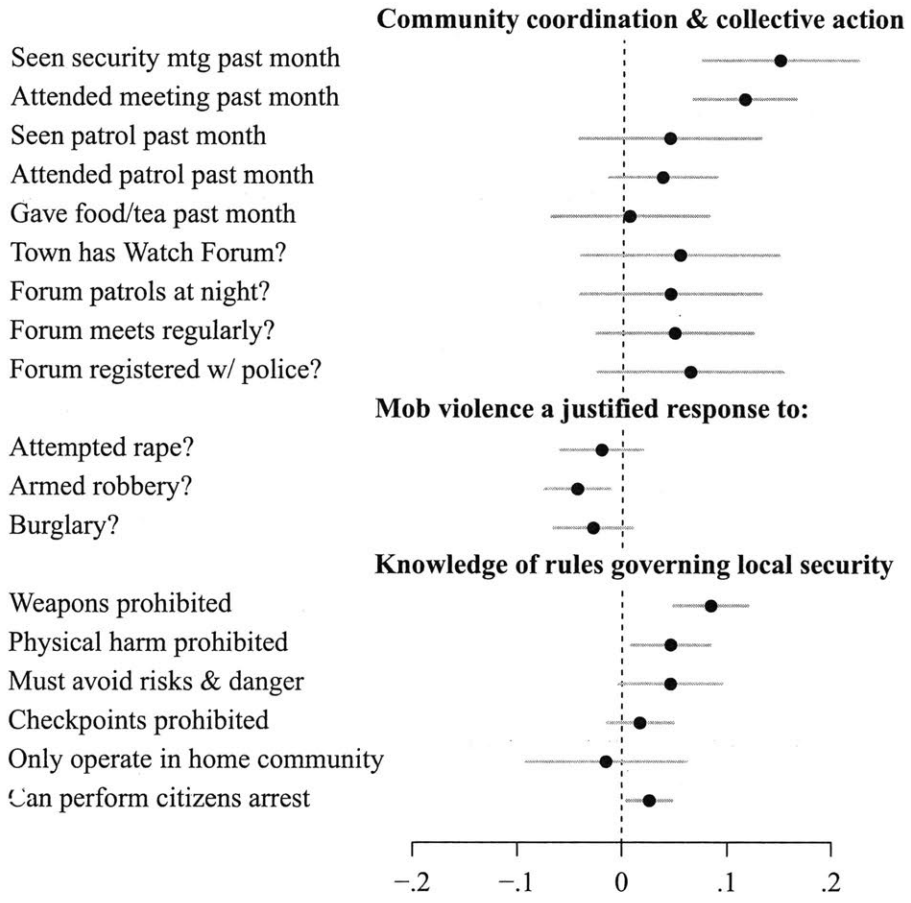
This study also contributes to the broader literature on state-building, which has long equated states' success at centralizing and consolidating power with their ability to engender compliance with the law (Migdal and Schlichte, 2005b). Contrary to this perspective, I show that *delegating* authority to local leaders and communities through coproduction can actually *strengthen* compliance with the law by converting potential rule-breakers into law-abiding coproducers. This finding is consistent with prior work on coproduction in more developed states, which shows that by bringing citizens and state officials together for a common cause, coproduction can build trust and social capital that spills-overs into increased compliance (Tsai, 2011; Cammett and MacLean, 2014). I extend this literature by showing that coproduction can lead to a similar dynamic in the security sector in weak states, improving relations between police and citizens, engendering norms against breaking the law, and integrating otherwise peripheral communities into the state's process of providing public goods. While further research is needed to verify the scope of this potential, coproduction appears to be a promising strategy for building trust and compliance in regions where the state and its rule of law are weak.

Figure 3-1: Effects on Hypothesized Mechanisms



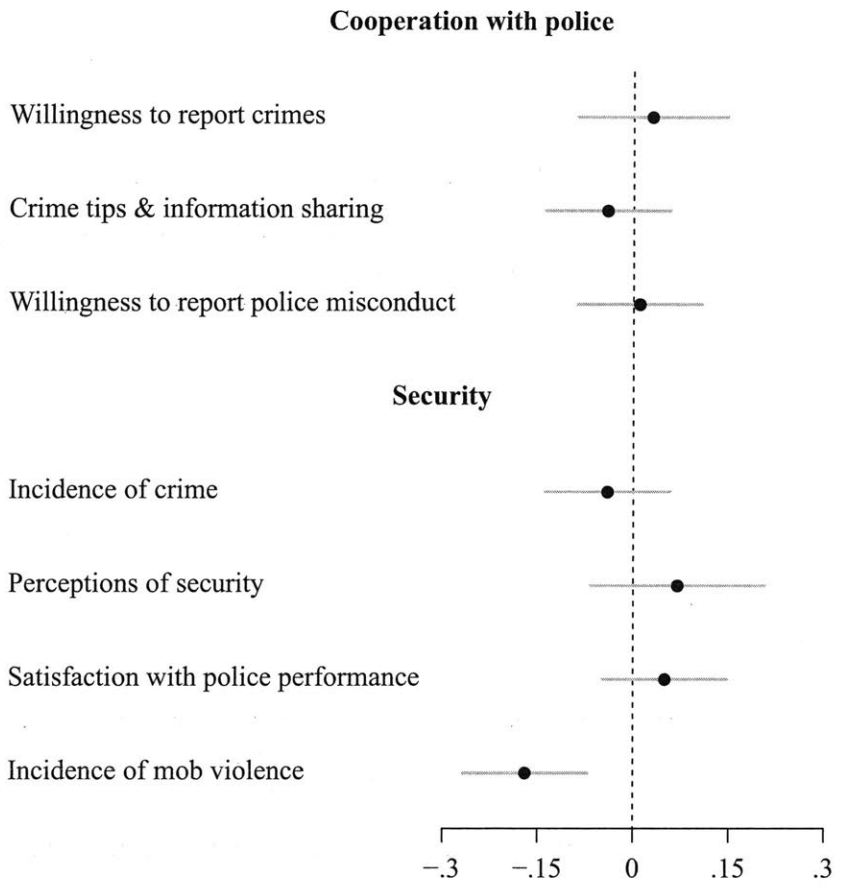
Notes: Average Treatment Effects on indices for hypothesized mechanisms. All indices standardized by baseline mean and standard deviation.

Figure 3-2: Effects on coproduction disaggregated by component variables for indices



Notes: Average Treatment Effects on component variables for coproduction indices. All outcome variables are binary.

Figure 3-3: Effects on Primary Outcomes



Notes: Average Treatment Effects on indices for primary outcomes. All indices standardized by baseline mean and standard deviation.

Chapter 4

Patrimonial Policing: Police Bias and Access to Justice in Liberia

Patrimonial Policing: Police Bias and Access to Justice in Liberia

Benjamin S. Morse - Massachusetts Institute of Technology

Abstract

Ensuring that rank-and-file officers adhere to standards of professionalism, fairness, and impartiality is a challenge for police departments the world over, but nowhere more so than in weak and fragile states. I examine whether officers of the Liberian National Police (LNP) discriminate against particular groups when resolving crimes, and whether this in turn leads to inequities in access to justice. Motivated by theories of bureaucratic bias and accountability, I test for discrimination along three dimensions: i) tasted-based discrimination against minorities, ii) discrimination against individuals with low SES, who lack the skills and resources to hold police accountable through formal channels, and iii) discrimination against individuals without personal connections to government officials, who lack the ability to hold police accountable through informal channels. Combining data from a conjoint experiment designed to elicit citizens' expectations about police discrimination with fine-grained data on nearly 2600 incidents of crime, I find little to no evidence of discrimination against minorities or low SES individuals but strong evidence of discrimination against those without personal connections to government officials. I argue that these results have important consequences for equity in access to justice as well as important implications for our understanding of bureaucratic accountability. In patrimonial bureaucracies, formal channels of accountability rooted in bureaucratic rules and procedures may be less important than informal channels rooted in personal connections to government elites.

4.1 Introduction

Impartiality in the exercise of power is a defining feature of legitimate policing (Tyler, 2004). Yet police departments the world over face enduring challenges in trying to ensure that rank-and-file officers adhere to standards of professionalism, fairness, and impartiality. Because much of police work is discretionary and performed by small, autonomous teams that are difficult to monitor, rank-and-file officers often have ample room to abuse their authority or discriminate on the basis of their personal biases (Lipsky, 1980). This is especially true of policing in weak and fragile states, where mid-level commanders often

lack the resources to monitor their subordinates (if they care to monitor at all), and where citizens' ability to hold officers to account through formal reporting channels is limited.

To what extent do these conditions lead to inequities in access to justice? When victims report crimes to the police, do officers treat all victims equally? Or do they exhibit bias against poor, minority, or otherwise disadvantaged individuals, neglecting their cases while prioritizing those from wealthy or well-connected individuals? To what extent does this deter members of disadvantaged groups from reporting crimes in the first place? And what can the answers to these questions tell us about the sources of accountability problems and performance shortfalls within bureaucracies?

Despite a growing body of research on police discrimination in developed countries, evidence on the extent and nature of discrimination in developing countries — and in weak states in particular — remains limited. While a handful of scholars have documented patterns of police corruption and discrimination at traffic checkpoints (Robinson et al., 2018; Fried et al., 2010), few have considered whether discrimination plagues police investigations and creates inequities in access to justice; fewer still have considered what this tells us about the sources of bureaucratic accountability.

Identifying the magnitude and nature of police discrimination during investigations has been difficult in part due to a lack of fine-grained data on crime, crime reporting, and case outcomes for reported crimes, as well as by a lack of accompanying data on complainant background characteristics. Such data are necessary if researchers are to identify differences in reporting patterns and case outcomes across groups that cannot be explained by alternative factors such as the nature or location of the crime, thereby suggesting discrimination.

To address this gap, I oversaw the administration of a crime victimization survey to a representative sample of 3600 residents of Monrovia, Liberia, one of sub-Saharan Africa's most crime-ridden cities.[1] Liberia is an especially "most likely" case for observing police

[1] According to Afrobarometer data collected in 2016, 65 percent of the city's residents reported that they or someone they knew was a victim of theft in the past year, 35 percent reported that they or someone they knew

discrimination because many of the factors that social science theories suggest would lead to discrimination — e.g., lack of oversight, poorly functioning channels of accountability, low pay, and capacity constraints — are particularly severe, and because the country's recent experience with civil conflict has made class and social divisions especially salient.

Respondents were asked if they or a member of their household was a victim of any of several types of crime in the past year, including robbery, armed robbery, simple assault, aggravated assault, and domestic violence. For each incident of crime, respondents were asked whether the case was reported to the police; those responding affirmatively were asked a follow-up set of questions on how they were treated, effortful actions the police may or may not have taken to solve their case, and case outcomes such as whether the perpetrator was ever brought to justice. In addition to data on these outcomes, the survey collected information on victims' background characteristics, the date and location of the crime, and the nearest police station, among other potentially confounding factors.

Empirically, my analysis is motivated by the intuition that individuals facing discrimination should report lower rates of satisfaction and less successful case outcomes when reporting crimes to the police, as well as be less likely to report crimes in the first place. Within this framework, I test for evidence of discrimination along four dimensions, each linked to a particular theory of bureaucratic bias. First, I test whether officers exhibit "taste-based" discrimination against Liberia's minority Muslim population (Becker, 1957), causing these individuals to report fewer crimes and to be less satisfied with how they were treated during those they did report. Second, motivated by "bargaining power" theories of discrimination in which bureaucrats exploit those least able to hold them to account through formal reporting channels (Banerjee et al., 2018, 2013), I test whether officers exhibit discrimination against either the poor, who may lack the time and resources to report misconduct, or those with limited education, who may lack an understanding of the law or

was physically assaulted, and 78 percent reported that in the past year, they felt unsafe in their neighborhood "several times", "many times", or "always." These figures are the highest of any of the 36 capital cities covered by Afrobarometer's Round 6 Survey.

the literacy skills required to report misconduct. Finally, drawing on theories of patrimonial governance in which authorities exercise power on the basis of their personal relationships rather than rational-legal criteria (Weber, 1964), I test for evidence that officers favor individuals with connections to elites in government, providing better treatment, exerting greater effort, and achieving more successful case outcomes.

As with all observational studies, the validity of my analysis rests on the assumption that unobserved factors do not account for the observed differences in case outcomes. I attempt to mitigate this risk by controlling for a large number of victim and crime-level factors, as well as by conducting an extensive set of supplementary analyses to diagnose, address, and rule out the most likely remaining sources of confounding. While I believe these steps go a long way towards mitigating the risk of bias, eliminating this risk entirely is of course impossible. For this reason, I complement my analysis of crime-level data with a conjoint survey experiment designed to elicit citizens' *expectations* about police discrimination. In the experiment, respondents were presented with a vignette in which a victim of crime reports their case to the police. The victims' attributes varied independently along four dimensions: income, education, religion, and political connections. After being read the vignette, respondents were asked a series of questions about how they expected the police to handle the case.

My use of expectations as test for discrimination rests on the assumption that expectations are indeed reflective of reality. This too may be a strong assumption, as a large literature suggests that humans sometimes process information in ways that are systematically biased, leading to misperceptions (Campbell et al., 1960; Zaller et al., 1992; Nyhan and Reifler, 2010). Yet this same literature also acknowledges that biased information processing is by no means the norm (Guess and Coppock, 2018), and that humans often have strong incentives to hold accurate beliefs and will readily update them in response to new information, regardless of whether it conforms to their priors (Bullock, 2009; Bullock et al., 2015). Research also indicates that beliefs are most likely to be accurate when informed by

personal experience, when the issues at hand are not highly politicized, and when accuracy incentives are strong, all of which weigh in favor of the view that Liberians' beliefs about the police are likely to be well-informed and reasonably accurate.

Nevertheless, the virtue of using both experimental and observational data is that they require distinct sets of assumptions. By using both in my analysis, I avoid the need to base my conclusions on any single assumption.

I organize my results into three sets of findings. First, I find little evidence to suggest that police exhibit taste-based discrimination against Liberia's minority Muslims population. Using crime-level data on reporting and complainant satisfaction, I find that Muslims are not significantly less likely to report crimes to the police, as compared to non-Muslims, and that when they do report, they are not significantly less likely to be satisfied with how they were treated or to experience successful case outcomes. These null results hold with and without controls for potential confounders, are close to zero and consistent across outcomes and specifications, and withstand a set of placebo tests for reporting biases and other most likely sources of unobserved confounding. They are also consistent with the results from the conjoint experiment: when Muslim victims report crimes to the police, respondents are just as likely to expect that officers will treat them with respect, investigate the case aggressively, and take other meaningful actions to resolve the case, as compared to crimes reported by Chrisian victims.

Evidence of discrimination against the poor and individuals with limited education is more mixed. On the one hand, the crime-level data indicates that these individuals are nearly just as likely to report crimes to the police as individuals with above median education or income. And when they do report, they are roughly just as likely to be satisfied with how they were treated and to experience successful case outcomes. However, results from the conjoint experiment reveal that citizens *expect* the police to discriminate against the poor, and to a lesser degree, against those who are illiterate. On the composite index of expected police performance, being a poor reduces scores .1 standard deviations. These

seemingly contradictory results suggest the possibility of a gap between citizens' expectations and reality when it comes to discrimination by police against those least able to hold them accountable.

Examining this possibility in further detail, I find that expected discrimination is driven in part by respondents who have previously experienced petty corruption at the hands of the police, most often in the form informal fees linked to case investigations. And yet the crime-level data indicates these fees affect all citizens equally, regardless of their socio-economic status. Moreover, examining effects on the sub-components of index, I find that lower expected overall performance for the poor is also driven in part by the expectation that poor victims will not be charged informal fees, suggesting that lower expected performance derives not simply discrimination but also from fewer resources available for investigation. Taken as a whole, I interpret these results as evidence that citizens misconstrue the police's "fee-for-service" model of service delivery as discrimination against the poor, when in fact the practice is applied without regard for victims' socio-economic status and closely linked to the lack of resources available to rank-and-file officers.

In contrast to the findings on (the absence of) discrimination against minorities, the poor, and those with limited education, I find strong and consistent evidence that individuals who are related to elites in government receive better quality services from the police (which, of course, equates to discrimination against those without connections). As compared to those without connections, victims with connections are significantly more likely to report their case to the police, and when they do report, they are significantly more likely to be satisfied with how they were treated and to experience successful case outcomes. These patterns do not appear to reflect differences in the nature of crimes that occur, reporting biases, resources, or bargaining power outside the realm of interactions with the state.

Results from the conjoint experiment provide further evidence of favoritism towards politically connected individuals: when the victim in the vignette is politically connected,

expectations of police performance improve by .08 standard deviations relative to vignettes in which the victim is not politically connected.

Taken as a whole, these findings are consistent with the idea that, in countries where patrimonial forms of governance prevail and formal channels of accountability are weak or irrelevant, individuals with connections to elites in government are able to advocate for services through informal channels, by using elite patrons to pressure frontline services providers to perform on their behalf, thereby short-circuiting formal but largely ineffective channels linked to official reporting processes.

This conclusion is a departure from conventional explanations for bureaucratic bias and poor performance, which typically appeal to principal-agent problems between mid-to-upper-level officials and street-level bureaucrats. In Liberia, street-level officers do not discriminate against minorities and they do not exploit those who lack the skills and resources to hold them account, despite the challenges they face with inadequate compensation, lack of equipment, and limited support and oversight. Rather, it appears that accountability problems within the LNP stem from the fact that power is organized according to a patrimonial logic, with resources concentrated in the hands of Commanders and accessible only to those in the upper echelon of society.

4.2 Theory

Like many street-level bureaucrats, police officers typically enjoy considerable discretion over how they perform their duties. It is common, for instance, for rank-and-file officers to make decisions about where to patrol, who to arrest, and which cases to prioritize for investigation. Police also tend to work in small, autonomous teams, often with only limited oversight from higher-level officials. In his seminal study of government bureaucracy, Lipsky (1980) argues that this combination of autonomy and discretion is common throughout government and gives rise to the role of street-level bureaucrats as de-facto "policy

makers" whose personal preferences and biases influence the nature and quality of public services. When these preferences are at odds with official policy objectives, a potentially large discrepancy can arise between the policies that citizens experience and the policies that policymakers intend. In the policing sector, studies have shown that officers operating autonomously and within their discretion are prone to racial bias when enforcing traffic violations (Goncalves et al., 2017; Lundman and Kaufman, 2003), racial profiling when identifying potential suspects (Gelman et al., 2007), and discrimination against minorities when investigating crimes (Eitle et al., 2002).

Problems of unequal treatment by street-level bureaucrats are not always the result of personal biases or "taste-based" discrimination, however. As Lipsky (1980, 107) points out, differential treatment can also result from capacity or resource constraints that force bureaucrats to ration services (e.g. by giving preferential access to those who seem most likely to benefit from services, triaging high-risk clients, or simply selecting those who are easiest or fastest to serve). Because rationing strategies may disproportionately exclude disadvantaged social groups, they can lead to unequal access to public services.

Most research on the causes and consequences of bureaucratic discrimination comes from developed countries. However, many of the factors that are believed to cause discrimination are especially pronounced in developing countries. Perhaps most notably, governments in these settings often underfund their agencies (Bardhan and Mookherjee, 2006; Lin et al., 2006), forcing bureaucrats to adopt "fee-for-service" models of service provision that inherently discriminate against the poor (Justesen and Bjørnskov, 2014). Fee-for-service models are a particularly common obstacle for victims of crime, who often have to have to pay fees throughout the course of an investigation, from case registration, to transportation to the crime scene, to the detention of suspects (Hunt, 2007; Kyed, 2017).

In many developing-country settings, these problems are compounded by inadequate oversight and failures of accountability. As Rose-Ackerman (2004, 112) points out, resource constraints can provide a pretext for police officers to become "part-time bandits"

who extort bribes from individuals and businesses to supplement their income.

The prevalence of monitoring challenges in developing countries can also provide an especially wide space for bureaucrats to discriminate based on ethnic or religious biases, which may be especially strong to begin with due to the salience of these kinds of cleavages in these settings. Indeed, a large literature on distributive politics in developing countries documents discrimination in the provision of public services (Kramon and Posner, 2013; Franck and Rainer, 2012; Hicken, 2011), with ethnic and religious minorities seemingly at greatest risk of being excluded or discriminated against (McClendon, 2016; Distelhorst and Hou, 2014). While this literature largely focuses on the distribution of resources by central governments across districts and counties, bureaucrats on the frontlines of service provision may be prone to similar dynamics.

Taste-based discrimination against ethnic and religious minorities is not the only source of discrimination that can arise in settings of weak accountability, however. A substantial body of research suggests that corruption by street-level bureaucrats disproportionately affects poor and uneducated citizens, because these citizens tend to be less knowledgeable about their rights and less able to pursue recourse through formal reporting channels (Fried et al., 2010; Robinson et al., 2018; Banerjee et al., 2018). Underlying these findings is the idea that citizen-bureaucrat interactions frequently entail a bargaining process in which citizens seek to access the services to which they're entitled while bureaucrats try to either shirk these obligations or solicit bribes.[2] In this interaction, citizens' bargaining power depends on their levels of wealth and education, because these factors determine whether they have the time, knowledge, and skills to pursue formal channels of accountability, such as contacting a higher-level manager or lodging a complaint with an official oversight body. Bureaucrats, for their part, can be expected to be strategic, exploiting poor and uneducated complainants who lack the skills to hold them accountable, while holding off on such behavior when interacting with those can.

[2]See Banerjee et al. (2013) for a more formal treatment of this framework.

Implicit in this framework — and indeed, in much of the literature on bureaucratic discrimination — is the assumption that higher-level officials would like to ensure that agency rules and guidelines are adhered to, but are prevented from doing so by the difficulties they face when trying to monitor their staff. However, in many weak states, power is organized according to a personalistic or "patrimonial" logic grounded in patronage, clientelism, and solidarity among networks of elites (Lemarchand, 1972; Bratton and Van de Walle, 1994). In these systems, the offices of the state are often treated as "benefices" by officeholders who make little distinction between public and private spheres and hold little regard for the public interest (Hyden, 2012, 96). Because appointments are made on the basis of loyalty rather than merit (Pitcher et al., 2009), officeholders tend to lack commitment to the public interest, and may instead be beholden to patrons higher up the chain of command. To maintain relations with their patrons, and to expand their own base of supporters, officeholders face constant pressure to divert public resources towards "bestowals of favor" and acts of reciprocity to those to whom they're indebted (Weber, 1978, 958).

When power is organized in this way, ordinary citizens are likely to find that their ability to exercise formal channels of accountability is irrelevant when negotiating with bureaucrats, because their complaints will only be ignored by supervisors who hold little regard for the public interest. Instead, they may find that the most effective strategy when trying to access services from frontline bureaucrats is to leverage their connections to political elites. From an equity standpoint, patrimonial forms of authority imply that those without connections to political elites will be at a disadvantage when accessing services from the state, regardless of their wealth or education.

Predictions

The above discussion points to three potential power arrangements that could give rise to police discrimination, each with a distinct set of predictions. The first occurs when rank-and-file officers operate within the confines of agency rules and guidelines, but perform

work that is inherently autonomous and discretionary in nature. Under these conditions, individual officers may exhibit "taste-based" discrimination against minorities or socially marginalized groups, but we would not necessarily expect them to discriminate against those who lack the skills to hold them accountable — i.e. those who are poor or lack education — because their actions remain within the scope of their authority under agency rules and guidelines.

The second occurs when high-ranking officials and police commanders would like to ensure policing services are delivered professionally, impartially, and in accordance with the law, but are prevented from doing so by monitoring problems that enable rank-and-file officers to violate these standards. Under these conditions, officers can be expected to discriminate against those least able to alert their supervisors through formal channels of accountability — such as those with limited education or income — exploiting their lack of bargaining power as an opportunity to shirk, solicit bribes, or otherwise neglect their duties. We therefore expect members of these groups to be less likely to report crimes, and when they do report, to be less satisfied with the police and to experience less successful case outcomes.

The final arrangement that can lead to bias occurs when power is organized in a patrimonial manner, with police officials and commanders owing their positions of authority not to merit or qualification, but to their connections to other elites. Under these conditions, citizens' bargaining power vis a vi bureaucrats is likely to be determined less by their ability to exercise formal avenues of accountability than by their ability to leverage their relationships with government elites. These elites can solicit the help of their colleagues in the police force to pressure rank-and-file officers to perform, or they can intervene directly, demanding performance while signaling their ability to contact higher-level authorities if necessary. As a result of these dynamics, we expect officers to improve on their otherwise lackluster performance when interacting with politically connected individuals, exerting greater effort, soliciting fewer informal fees, and ultimately delivering higher-quality ser-

vice.

Reasons for optimism?

While the literature on public bureaucracies provides reasons to expect discrimination against poor, uneducated, minority, and politically-unconnected citizens, there are also reasons to be more optimistic. For one, the principle that all citizens are equal before the law is universally recognized as among the core obligations of the state, and many government agencies devote considerable attention to the importance of objectivity and impersonality in the provision of services when training bureaucrats.[3] This is particularly true in the criminal justice sector, and in developing countries that receive training and assistance from international organizations such as the United Nations (Malan, 2008). With so much attention given to equality before the law, police may have come to internalize this principle as a norm, integrating it into what it means to be a professional and committing to serve all citizens equally, despite whatever biases they may hold in private.

Second, where minorities and socially disadvantaged groups constitute important voting blocks, politicians may have strong political incentives to prevent discrimination by street-level bureaucrats. Indeed, research has shown that political incentives can be a powerful driver of service delivery generally and towards the poor in particular, even in countries with limited capacity to overcome the principal-agent problems that can arise between politicians and bureaucrats (Ross, 2006; Kudamatsu, 2012). If political incentives can improve the quality and quantity of services, then it stands to reason they may also help to minimize potential biases against politically important groups.

A final factor that may mitigate the potential for bureaucratic discrimination is that many governments have made efforts to ensure bureaucratic institutions are representative of the population they serve, at times going so far as to implement ethnic, gender, or

[3] See for instance, Article 26 of the International Covenant on Civil and Political Rights (ICCPR), adopted by the UN General Assembly on December 16, 1966; and the International Declaration of Human Rights, adopted by the UN General Assembly on December 10, 1946.

location-based quota systems (Bird, 2014). Such policies may mitigate discrimination by increasing descriptive representation within bureaucracies (Meier et al., 1999); by increasing contact between majority group bureaucrats and minorities (Allport et al., 1954); or by increasing the degree to which bureaucrats empathize with those they serve on the basis of their shared experiences.

4.3 Setting

Monrovia is the capital city of Liberia, a small West Africa nation of approximately 4 million people. Since the end of Liberia's civil war in 2003, Monrovia's population has grown to an estimated 2 million, up from 200,000 in 1984, before the war began.[4] The majority of Monrovia's residents are poor and live in densely-populated, informal settlements, though the city is also home to a class of political elites and a small but growing middle class. Data collected for this study suggests that 60% live on less than $2 day, 82% are either unemployed or work in the informal economy, and 55% have less than a high school education.

People from each of Liberia's 13 official ethnic groups can be found in Monrovia, and in roughly equal proportion to their share of the country as a whole. With the exception of Liberia's Christian/Muslim divide, ethnic divisions are not an especially prominent feature of residents' everyday lives. Intermarriage is common, and most communities are highly heterogeneous, though it is not uncommon to find neighborhoods within communities where the majority of inhabitants are from a single ethnic group.

Crime is a major problem in Monrovia. According to Afrobarometer data collected in 2016, 65 percent of the city's residents reported that they or someone they knew was a victim of theft in the past year, 35 percent reported that they or someone they knew was physically assaulted, and 78 percent reported they felt unsafe in their neighborhood "several

[4]Population is from the Liberian Institute for Statistics and Geo-Information Services (LISGIS), projected from the 2008 Census.

times", "many times", or "always." These figures are the highest of any of the 36 capital cities covered by Afrobarometer's Round 6 Survey.

In addition to its problem with crime, Monrovia has many of the characteristics that previous research suggests would lead to police discrimination. Much like the LNP as a whole, the Monrovia division of the LNP is notoriously under-resourced, with most stations lacking essential tools such as vehicles, radios, handcuffs, electricity, and even stationary (Baker, 2010). To fund their services, many stations have resorted to "fee-for-service" models of service delivery. This has led to complaints that victims face "a bewildering array of fees" when reporting crimes, including "registration fees, gas money for police investigators, requirements that victims pay the cost of food for the detained accused, lawyers' fees, bribes, and indirect costs such as money for transportation and time spent away from livelihoods" (Isser et al., 2009, 3).

Mechanisms to hold police accountable are also lacking. At the station level, Commanders are notorious for failing to adequately monitor their subordinates (Human Rights Watch, 2013). Absenteeism is pervasive, and the practice of "reassigning oneself" to a more profitable post (e.g. in the traffic sector) is not uncommon. While the LNP has taken strides to address these problems in recent years, most notably by establishing the Professional Standards Division for handling complaints of police misconduct, progress on the ground remains limited (Downie, 2013).

These dynamics have led to serious concerns about the impartiality of the LNP. In 2006, the International Crisis Group observed that "there is a crisis of confidence in the Liberian police and justice system because powerful individuals have used it as a political tool through which to exercise and legitimise their power" (International Crisis Group, 2006, 2). By 2009, most residents had come to view the police as little more than "a forum in which wealthy, powerful, and socially connected people assert their will" (Isser et al., 2009, 11). And in 2013, a report by Human Rights Watch reached a similar conclusion, reporting that "police corruption denies ordinary Liberians their basic rights to personal se-

curity and redress," and that "institutional neglect has led to the credible perception among many Liberians that wealth, not guilt, determines the outcome of a criminal case" (Human Rights Watch, 2013).

Though observers have largely focused on the potential for discrimination against the poor and "those living on the margins" of society, the culture of impunity that pervades the LNP also raises the risk that certain ethnic or religious groups will face discrimination. In Liberia, many citizens perceive Muslims from the Mandingo, Vai, and Mende ethnic groups as "foreigners" from Guinea and Sierra Leone, despite the fact that their ancestors emigrated as early as the 18th century (Beleky, 1973). Muslims from the Mandingo tribe, in particular, have long faced resentment for using political alliances to establish commercial privileges and secure land at the expense of 'native' tribes (Ellis, 2001, 38). During Liberia's civil war, divisions between Mandingos and predominantly Christian ethnic groups escalated into some of the war's worst atrocities (Ellis, 2001). Although Liberia has made some progress towards reconciling the divide between Christians and Muslims in recent years, these divisions remain salient today, and there is at least some evidence that they contribute to patterns of biased policing. A recent study by Blair et al. (2018), for example, uses lab-in-the-field experiments implemented with teams of officers from the LNP to argue that police discriminate against Muslim minorities when investigating crimes. Whether these patterns obtain outside the context of their experiments, however, remains an open question.

Liberia is also a country with a long history of patrimonialism in government, with appointments more often made on the basis of political criteria or personal connections rather than on competence for the job. Ellis (2001) describes patterns of state administration in twentieth century Liberia as a system in which "the use of official procedure for personal purposes operated at every level of administration," from presidential officials and cabinet members to rank-and-file staff (Ellis, 2001, 61). In this system, those in government would derive their position from their relationships with those above them, and would in turn use

their position to enhance their social and political standing, dispensing patronage to their own set of clients, kin, and retainers.

This patronage-based system of governance has been emulated by subsequent governments and persists today, more than a decade after the civil war has ended (Reno, 1995; Utas, 2012). Observers have been particularly critical of the government's failure to implement reforms that would centralize authority over recruitment and dismissal practices under the Civil Services Agency. The current system, which the International Crisis Group describes as fragmented, vulnerable to corruption, and plagued by "personalized politics" (ICG, 2011, 17), is believed to reinforce the personalized nature of Liberia's bureaucratic state because it indentures bureaucrats to patrons higher up the chain of command while also allowing them to build-out patronage networks of their own. The result, as Käihkö (2012, 188) describes, is that "patrons hold positions at every level of the state, from the presidency to the lowest public servant, and use the salaries and benefits they gain to feed their networks." Ordinary citizens, for their part, rely on patrons in government for advice, jobs, and social support, as well as to facilitate their interactions with the state. While this reliance on elite patrons has its origins in traditional notions of dependency and reciprocity in rural Liberia (Ellis, 2001), its operation within Liberia's contemporary bureaucratic state has contributed to the blurring of the distinction between the public and private spheres and led to allegations of favoritism towards those with connections to elites in government (Kaufmann, 2013, 164). The practice of calling on political elites to intervene on one's behalf is particularly common in the domain of criminal justice, where there is "a strong perception among many Liberians that the wealthy and the powerful are increasingly able to exercise undue influence" (Isser et al., 2009, 24).

4.4 Research design

My research design centers on two sources of data. The first is a conjoint survey experiment designed to measure citizens' expectations of police discrimination. In the experiment, respondents were presented with a vignette about an individual who was victimized by crime and reported their case to the police. After being read the vignette, respondents were asked a series of questions about how they expected the police to handle the case, including whether they thought the police would take the case seriously, treat the victim with respect, and go through the effort to visit the crime scene, among other questions. Respondents repeated this exercise twice; in each scenario, the attributes of the victim were randomly varied along four dimensions — religion, education, class, and political connections — while the type of crime was randomly varied along three levels of severity — a stolen phone, a nighttime burglary, and a nighttime burglary leading to aggravated assault with a cutlass. Further details on the experiment and its analysis are provided below.

The conjoint experiment is motivated by the idea that since citizens' are the ones who most often interact with the police, they are best-positioned to report on whatever biases the police may hold. Thus, a correspondence between their expectations and the hypotheses developed above may be viewed as support for those hypotheses, while a lack of correspondence may be viewed as evidence against them.

Of course, citizens may be subject to biases of their own. Indeed, research shows that negative experiences tend to have an outsize influence on citizens' perceptions of police (Brunson, 2007), and that citizens tend to overestimate the prevalence of crime (Esberg and Mummolo, 2018). More broadly, a large literature documents the prevalence of misperceptions among the masses (Bartels, 2002; Nyhan and Reifler, 2010; Campbell et al., 1960), raising the possibility that expectations bear little resemblance to reality.

This possibility motivates the second component of my research design, which tests for police discrimination against victims of crime in the real-world using survey data from a

crime victimization module that preceded the conjoint experiment. This analysis comes with its own set of assumptions and limitations, as I discuss below, but ones which are altogether distinct from those underlying the experimental analysis. Thus, I interpret a correspondence across the two analyses with respect to a particular hypothesis as a reliable test. Less obvious is how to handle cases in which there is not a correspondence. I return to this issue in the Discussion.

Tests of bureaucratic discrimination in the real-world have typically employed either of two research designs. The first involves running an audit-style experiment in which confederates with different socio-economic or racial attributes solicit information or services from government, and the frequency and quality of responses serve as the main outcome measures.[5] While these designs offer a high degree of internal validity and are well-suited for measuring 'point of contact' forms of discrimination, they are generally ill-suited for measuring more complex forms of discrimination that arise during the course of service delivery, after the initial point of contact. For instance, it would be unfeasible — not to mention unethical — to try to use this approach to measure discrimination during the course of a police investigation, as this would require a very elaborate deception scheme.[6]

The second approach — and the one employed in this paper — is to use observational data to compare outcomes across potentially advantaged and disadvantaged groups after controlling for as many confounding factors as possible. A recent study uses this approach to test for discrimination against African Americans in the use of force by the police, comparing differences in likelihood that force is used after controlling for encounter and neighborhood characteristics (Fryer Jr, 2019). This approach relies on the crucial assumption that unexplained variation in outcomes across groups reflects discrimination, rather than confounding due to unobserved factors. Because this assumption is only plausible when a

[5]In political science, scholars have used this approach to measure discrimination by local election officials (White et al., 2015), legislators (Butler and Broockman, 2011), and police at traffic checkpoints (Robinson et al., 2018; Fried et al., 2010), among others.

[6]Though see Blair et al. (2018) for an example of how lab-in-the-field experiments conducted with individual officers can be used to test for bias in the profiling of suspects.

large number of factors are accounted for, this approach requires especially rich data.

Unfortunately, detailed data on crime reporting, case outcomes, and complainant characteristics is scarce, especially in weak and fragile states.[7] To address this gap, I oversaw the administration of a crime victimization survey to a representative sample of 1800 adult residents of Monrovia, Liberia in July, 2017 and again in February 2019.[8] The resulting data set includes 2570 incidents of crime and includes information on crime reporting, complainant satisfaction, case outcomes, and a large set of potentially confounding factors.

While my reliance on survey data allows me to conduct quantitative analyses of police effort and case outcomes that would otherwise not be possible, this approach is not without limitations. One key limitation is that responses may be subject to recall bias, social desireability bias, or other forms of bias that lead to inaccurate data. It is also possible that respondents interpret questions in different ways, especially when posed subjective questions such as "During the course of this investigation, were you treated with respect?" Moreover, there is no guarantee that these differences do not correlate with class, education religion, or political-connectedness, thereby biasing my results. I attempt to mitigate these biases by focusing on questions that were pretested extensively to ensure that respondents understood them accurately and consistently; by complementing subjective measures with more objective outcomes such as "during the course of the investigation, did the police ever visit the scene of the crime to investigate?"; and by conducting a placebo test to show that social desireability bias is unlikely to vary by class, education religion, or political-connectedness (see Section 4.6.4). Nevertheless, my results should be interpreted with the limitations of survey-based measures in mind.

In what follows, I describe the sample, experimental data, and observational data in greater detail. I then outline my construction of key variables and explicate the strategies I

[7]Indeed, to the best of my knowledge, researchers have not yet collected or analyzed this type of fine-grained data to test for evidence of police discrimination in a developing-country setting.

[8]The survey is a panel at the community-level but not at the individual-level: the same communities were visited in 2017 and 2019, but respondents were sampled independently for each survey. For further details, see 4.4.1.

employ to mitigate the risk of confounding factors.

4.4.1 Sample

I employ observational and experimental survey data collected from 98 communities in Monrovia, Liberia. The data were collected in July 2017 and February 2019 as part of the baseline and endline surveys for an experimental evaluation of the LNP's community policing program.[9] The sample includes 35 communities nominated by the police for inclusion in the evaluation, as well as 63 communities selected at random from the remaining communities within Monrovia.[10] Within each community, 20 adult residents were selected to participate in the survey independently at baseline and endline via a random-walk procedure, resulting in a total sample size of 3920 respondents. Collectively, these respondents reported on 2570 crimes against either themselves or a member of their immediate family.

4.4.2 Data and measurement

Conjoint experiment

The conjoint experiment was included in the 2019 survey only. Respondents were presented with a vignette in which the victim's attributes were randomly varied along the dimensions listed in Table 4.1. An example of one of the vignettes is as follows:[11]

> Let's say that during the rainy season, a man named [Christian / Muslim name] had his phone stolen near his home in [Muslim / Christian town name]. After the incident, a neighbor came to [name] with information about who may have committed the crime and recommended he report the case to the police for further investigation.
>
> At first, [name] was scared to take the case to the police [because he is poor].

[9]Results from the evaluation are not included in this study but can be found in Morse (2019). Further details on the experimental study are available at: http://egap.org/registration/5472.

[10]The sampling frame for this study was provided by the Liberia Institute of Statistics and Geo-Information Services (LISGIS).

[11]The full set of vignettes is presented in the Appendix.

> He [also is not a book person and doesn't know much about the law], and [because he just recently moved to Monrovia, he doesn't know any big big people in government who can give him advice]. However, after a few days [name] was able to put his worries aside and decided take time off from his work as a [market vendor] to report the theft to the police.

After listening to the vignette, respondents were asked to rate on a four point scale how likely they think the victim would be to be satisfied with the police's hanlding of the case, as well as how likely they think the police would be to: i) behave professionally, ii) treat the victim with respect, iii) investigate aggressively, iv) visit the crime scene, v) interview witnesses, and vi) solicit a bribe or "case registration" fee. I construct a composite index by summing all 7 variables,[12] then resca ling so the index has a mean of zero and a standard deviation of one. I also report results on each of the component variables, coded as indicators for "Likely" or "Very likely" responses. Table 4.2 provides descriptive statistics for the outcomes from the vignette experiment.

Observational data

The crime victimization survey collected information about eight categories of crime: armed robbery, burglary, simple assault, aggravated assault, sexual violence, domestic violence, verbal domestic abuse, and property encroachment. Respondents were asked whether they or a member of their household was a victim of any of these types of crime in the past 6 months, and if so, whether the incident was reported to the police, courts, or local leaders. For each crime that was reported to one of these authorities, respondents were asked a set of follow-up questions about how their case was handled, including whether they had to pay informal fees, whether they felt they were respected and treated fairly, and, for cases reported to the police, whether the police interviewed witnesses, took written testimony, or visited the crime scene.

The resulting dataset provides incident-level data on 2,570 incidents of crime, 496 of

[12] I recode responses to the question on bribery so that higher values correspond to a lower expected likelihood of bribery.

which were reported to the police, 291 of which were reported to local leaders, and 84 of which were taken to court (Table 4.3).[13] Because the sample of court cases is too small for multivariate analysis, this paper focuses only on cases reported to the police and local leaders.

Case-level outcomes

Respondents who reported their case to the police were asked a set of questions about their satisfaction with how the police handled the case, including whether they felt the police i) treated them with respect, ii) took their case seriously, iii) were fair to both sides, iv) were available and accessible during the investigation, and v) whether they would report a similar case to the police in the future. For each of these questions, I code an indicator variable taking a value of 1 for an affirmative response. I analyze these outcomes individually and, to mitigate concerns about multiple hypothesis testing, as a single *subjective satisfaction index* constructed as the standardized average across all five individual outcomes.

I complement these subjective measures of police performance with a set of more objective outcomes drawn from questions about concrete, effortful actions the police may have taken while trying to address the case, including whether the police i) solicited a bribe, ii) visited the crime scene, or iii) interviewed witnesses; in addition, I consider whether the pepetrator was "brought to justice" and whether the case was ultimately "resolved" from the perspective of the respondent. Responses to each of these questions are coded as indicator variables denoting affirmative responses. Finally, I aggregate these variables additively into a single, standardized *police performance index*.[14]

[13]On average, respondents reported that they or a member of their family was a victim of .67 crimes in the past six months.

[14]Soliciting a bribe is reverse coded when constructing the *police performance index*, such that 1 indicates that the police did *not* solicit a bribe.

Sources of discrimination

In Section 4.2, I identified three groups of citizens at risk of police discrimination — Muslims, the poor, and those with limited education — as well as one group of citizens who may benefit from favoritism, namely those with personal connections to elites in government. I measure each of these characteristics using responses from the survey. For ease of presentation and interpretation, I measure income and education on a binary scale using indicator variables that take a value of 1 for any individual with below median household income or years of education, respectively.

My measure of personal connections to elites in government is based on the question "Are you related to any big big person in government?" In Liberia, this terminology is typically understood as referring to high-ranking officials such as legislators, ministers, deputy ministers, and senior staff. Though subject to some degree of ambiguity, pretesting revealed a surprising degree of consensus on who would qualify as a "bigman" in government, even among those with limited education and little understanding of government. During focus groups in slum communities, for instance, participants repeatedly and independently cited being issued a government car as the key indicator of whether one qualifies as "bigman" — a distinction which, according to the guidelines of Liberia's General Services Agency, is typically restricted to legislators, ministers, deputy ministers, and selected senior staff.

Among the sample of crime victims, 16% are Muslim, 40% have below median education,[15] 51% have below median wealth, and 20% are related to a high-level official in government. Notably, being related to a high-level official in government does not associate with wealth, education, or religion, and is only weakly associated with ethnicity.[16] While the absence of a strong association between political connections and ethnicity may seem at odds with much of the literature on patrimonial governance and ethnic politics, it is consistent with prevailing power arrangements in Liberia — because Liberia's population

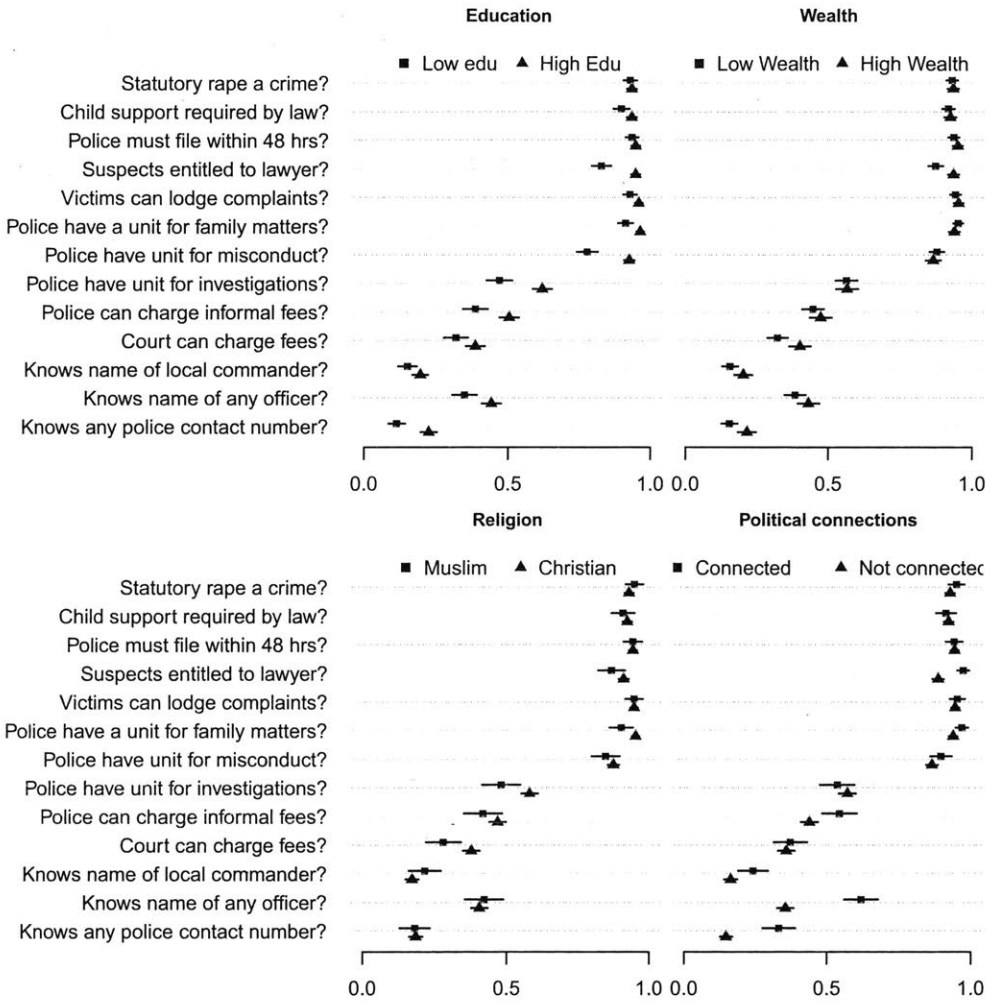
[15] Below median education includes anyone with less than a high school education.

[16] A detailed comparison of politically connected individuals to those without connections is presented in the Appendix.

is roughly equally divided across its thirteen officially-recognized ethnic groups, no one group one dominates its political institutions. Indeed, Liberia's postwar government campaigned on a platform of ethnic reconciliation and inclusion, and has subsequently been praised for following through on this commitment with the creation of a government that is broadly representative of the country's ethnic, religious, and regional diversity (ICG, 2011).

A key assumption underlying the theoretical motivation for this study is that knowledge of the criminal justice system and police accountability channels — and thus bargaining power when interacting with the police — is lower among individuals with limited education. Figure 4-1 tests this proposition by showing how knowledge varies by level of education, wealth, religion, and political connectedness. While there is little difference across religion, wealth, or political connectedness, there is a large difference in knowledge among those with below versus above-median education. These individuals are ten percentage points less likely to know about the Professional Standards Division of the LNP, eight percentage points less likely to know that it is illegal for police to charge informal fees, five percentage points less likely to know that it is illegal for Judges to charge informal fees, and six percentage points less likely to know the contact number of the police. These results suggest that those with limited education are indeed at a disadvantage when interacting with the police.

Figure 4-1: Knowledge gaps by education, wealth, religion, and political connections



Notes: Differences in knowledge of the law and police accountability channels by education, wealth, religion, and political connectedness. The X-axis displays the percentage of correct responses for each group.

4.5 Empirical strategy

Analysis of experimental data

Following the approach outlined in Hainmueller et al. (2014), I estimate the change in expected police performance associated with each of the four victim-level attributes via:

$$y_{ik} = \beta_0 + \beta_1 Muslim_{ik} + \beta_2 Poor_{ik} + \beta_3 Illiterate_{ik} + \beta_4 Connected_{ik} + e_{ik} \quad (4.1)$$

where $i \in \{1, 2, \ldots, 1895\}$ denotes the respondent and $k \in \{1, 2\}$ denotes the round.[17] The outcome is the composite index of expected police performance or one of its six component variables, and the explanatory variables correspond to each of the randomly varied attributes.

Analysis of crime-level data

Applied to crime-level data on reporting and complainant satisfaction, theories of bureaucratic discrimination predict that the police will be more likely to shirk, solicit bribes, or otherwise fail to seriously investigate cases brought to them by Muslims, the poor, and those with limited education. As a result, we expect members of these groups to be: i) less satisfied with how the police handle their cases, ii) less likely to experience successful case outcomes, and iii) less likely to report crimes to the police in the first place.

I test these predictions using a simple OLS regression model of the following form:

[17] $N = 1895$ corresponds to the number of survey respondents in the February 2019 survey. Since each respondent reported their expectations for two vignettes, $N \approx 3790$ in the analysis.

$$y_{civs} = \beta_1 Muslim_i + \beta_2 Low\_Income_i + \beta_3 Low\_Edu_i + \beta_4 Connected_i + \alpha + X_{civ}\theta + e_{civ} \tag{4.2}$$

where y_{civ} is a measure of reporting or police performance for crime c against individual i in community v, X_{civ} denotes observable control variables, and e_{civ} captures all other, unobserved factors that may explain variation in reporting or police performance. If discrimination exists, then we would expect to observe a negative coefficient on $Muslim$, Low_Income, and/or $Low\_Education$, or a positive coefficient on $Politically\_Connected$, even after controlling the variables captured by X_{civ}, which include: type of crime, month the crime occurred, nearest police station, whether the identify of the alleged perpetrator was known at the time of the crime, and the age, gender, ethnicity, household size, and community of residence of the victim.[18]

In this type of discrimination analysis, there is always the concern that there may be some unobserved factor other than discrimination that explains the presence of religious or class-based differences and is not accounted for by observed control variables. For instance, it is possible that poor individuals tend to live in communities where it is inherently difficult to investigate crimes due to mistrust of the police and low community cooperation. Recognizing these challenges, police may reason that it is not worthwhile to expend effort investigating crimes that come from these communities. While the inclusion of community fixed-effects guards against this particular type of confounding, it is always possible that other, unobserved sources of confounding are not accounted for.

One way to gauge whether unobserved factors are likely to confound is to assess how our estimated differences change as more controls are added. If differences estimated from a "naive" regression model without control variables become small and insignificant once

[18]Technically I measure these variables for the *respondent* rather than the actual *victim*, the victimization module did not distinguish between crimes against the respondent and crimes against members of their household. However, apart from age and gender, these variables should be the same for both the victim and the respondent.

controls (X_{civ}) are added, then this is evidence that religous or class-based differences in performance and satisfaction are explained by differences in the type or nature of crimes that are reported, rather than discrimination. By extension, it also suggests that accounting for additional, unobserved factors would likely serve to further attenuate racial and class-based differences. Conversely, if differences estimated from a naive regression model remain large and statistically significant even after controlling for a robust set of potential confounders, then this would constitute evidence of discrimination. Finally, if differences are null in both the naive and fully controlled models, then this constitutes compelling evidence *against* discrimination, for it suggests that differences in the characteristics of crimes that are reported, whether observed or unobserved, are not confounding.

4.6 Results

4.6.1 General patterns of crime, crime reporting, and police performance

Before turning to the question of whether the police discriminate against certain groups, I consider general patterns of crime, crime reporting, and police performance when investigating crimes. The first observation of note in this regard is that crime is quite common, with 41% of respondents reporting that they or a member of their immediate family were victims of crime in the past year.[19] As shown in Table 4.3, the most common type of crime was burglary, which accounted for 39% of all incidents, followed by simple assault (12%), aggravated assault (10%), property disputes (10%), and armed robbery (8%). Perhaps surprisingly, given the amount of attention paid to issues of sexual and gender based violence in Liberia (Bacon, 2015), domestic and sexual violence were relatively rare, with only 3% of respondents reporting this type of victimization.[20]

A second pattern evident from Table 4.3 is that only a fraction of crimes that occur — about 25% — are ever reported to the police. In part, this may reflect the fact that some crimes are simply not serious enough to justify the costs of reporting. Yet even for more serious crimes, such as aggravated assault or armed robbery, rates of reporting are low, at 29% and 38% percent respectively, suggesting that many citizens lack confidence in the ability of the police to effectively investigate crimes.

Table 4.4 summarizes this study's measures of the police's performance while investigating reported crimes ($N = 496$). The results paint a mixed but generally unfavorable picture of police performance. While 84 percent of complainants reported that the police

[19]Note that the analyses below are restricted to the selective sample of respondents from the representative survey who reported a crime.

[20]It is, however, important to keep in mind that this may represent a lower bound on the incidence of these types of crimes, as male respondents may under report or simply be unaware of these kinds of victimizations among other members of their household.

were accessible and a similar percentage said they would report their case to the police again, 38 percent said the police were unfair in how they handled their case, 34 percent said they did not take their case seriously, and 27 percent said they failed to treat them with respect. In terms of effortful actions that the police might take to solve reported crimes, only 58 percent of complainants reported that the police visited the scene of the crime and only 53 percent reported that the police interviewed witnesses. More alarmingly, 46 percent of complainants reported having to pay informal fees, which on average totaled 9.8 USD, or roughly four days of income for the average Liberian.

4.6.2 Experimental results on citizens' expectations of police discrimination

Do expectations about police discrimination accord with the theories outlined in Section 4.2? Table 4.5 presents the results of the experiment respondents' expectations. Contrary to theories of "taste-based" discrimination against minorities, respondents do not expect Muslim victims to receive lower quality treatment when reporting crimes to the police. As shown Row 1 of the Upper and Lower panel of Table 4.5, the estimated effect of being a Muslim victim relative to being a Christian victim is close to zero and precisely estimated across all outcomes in the composite index.

The evidence on expected discrimination against the poor is mixed. On the one hand, respondents strongly associate being poor with lower police performance. When the victim is described as a "poor market vendor," respondents are 20 percentage points less likely to expect the police to be professional, 22 percentage points less likely to expect them to be respectful, 19 percentage points less likely to expect them to visit the crime scene, and 10 percentage points less likely to expect them to investigate the case aggressively, as compared to when victims are described as "having plenty money from their NGO job." But they are also eight percentage points less likely to expect the police to solicit a bribe or charge a "case registration" fee. While effect is of somewhat smaller magnitude than the others, it does suggest that lower overall expected police performance may be driven in part by the expectation that fewer resources will be available to support the investigation, consistent with the idea that respondents perceive petty bribes and informal fees to be a consequence of police lack of resources.

In terms of a "bargaining power" logic of discrimination against those with limited education, Table 4.5 shows that expected police performance is somewhat lower when the victim is described as someone who "is not a book person and doesn't know the law", as compared to when the victim is someone who "can read and write and knows the law very

well." However, these effects are small in magnitude — just .02 standard deviations for the composite index — suggesting that illiteracy and lack of knowledge of the law is not perceived as an important vulnerability that police exploit.

On the other hand, expectations of favoritism towards politically connected individuals are strong and consistent across most components of the composite index. When the victim is described as having "an uncle who is a bigman in the government" that they can turn to for advice, expected performance improves by .08 standard deviations in the composite index, as compared to victims who "just moved to Monrovia and don't know any big big people in government who can give them advice." This overall effect is driven by lower expected likelihood of being charged a bribe or informal fee, as well as greater expected professionalism, respectfulness, and effort to resolve the case.

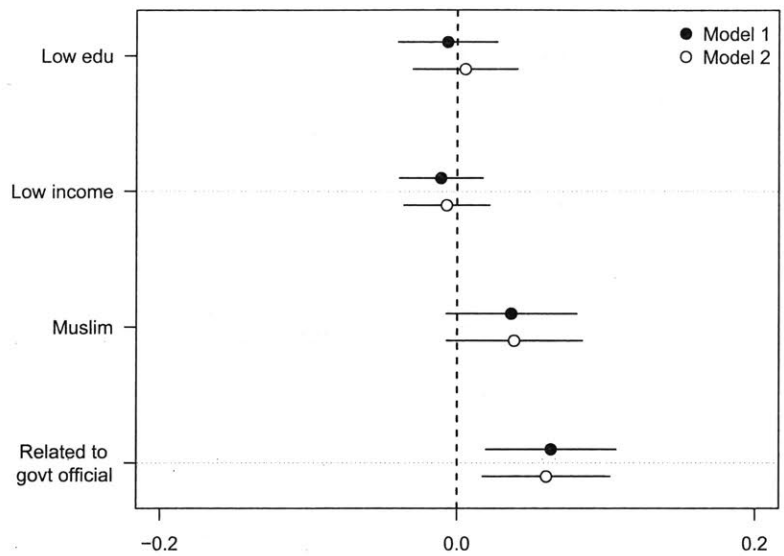
4.6.3 Evidence of police discrimination from crime-level data

To what extent do these extent do citizens' expectations about police performance and discrimination correspond to reality? To what extent do they deter seemingly disadvantaged groups from reporting crimes, or encourage those that are favored? This section continues the investigation of police discrimination and its effects on access to justice with an analysis of crime-level data on crime reporting and complainant satisfaction.

Figure 4-2 reports how crime reporting varies by religion, class, education, and political connectedness. In contrast to theories predicting taste-based discrimination against religious minorities, Muslims are if anything slightly *more* likely to report crimes to the police than non-Muslims, with an estimated association of 4 percentage points ($p < 0.12$). Figure 4-2 also shows that victims with below median education or below median income are not significantly more or less likely to report crimes to the police than those above the median, contrary to theories predicting bias against poor and low-class individuals. Finally, Figure 4-2 shows that victims who are related to elites in government are about six percentage points more likely to report crimes to the police than those without such connections, consistent with the hypothesis that politically connected individuals are favored by the police.

There are, of course, many reasons unrelated to expected bias and discrimination that may explain these differences in reporting. As a more direct test of police discrimination, Figure 4-3 examines patterns of complainant satisfaction among those who actually reported their case to the police (N=496). The results provide little evidence of discrimination based on class, religion, or education. Complainants with below-median levels of education or income express roughly the same level of satisfaction with how the police handled their case as complainants with above-median education or income, while Muslims, for their part, express roughly the same level of satisfaction as non-Muslims. These null results are consistent across multiple measures and are robust to the inclusion of controls

Figure 4-2: Differences in crime reporting by religion, income, education, and political-connections



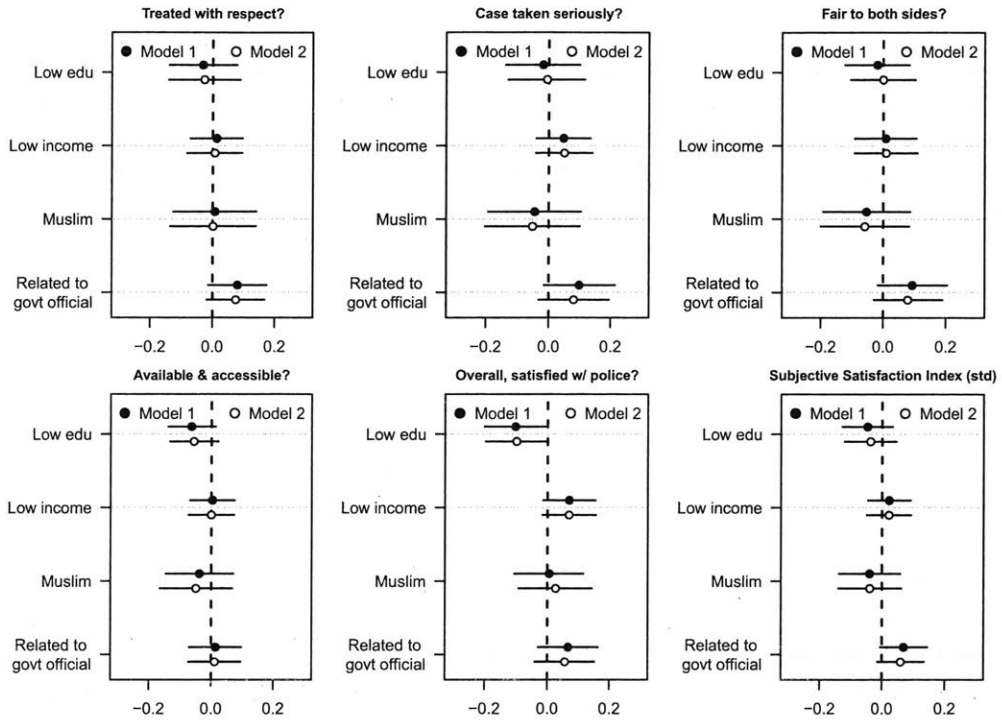
Notes: Differences in crime reporting by religion, income, education, and political-connections. Model 1 includes controls for type of crime only. Model 2 includes additional controls for crime location and complainant characteristics, following Equation 4.2 in Section 4.5. Standard errors clustered by community.

for complainant and crime-level characteristics, suggesting that differences in the nature of crimes reported are unlikely to account for these (null) results.

The results in Figure 4-3 do, however, support the hypothesis that the police favor politically connected individuals. Compared to those without connections to political elites, complainants with connections are eight percentage points more likely to feel the police treated them with respect, eight percentage points more likely to feel the police were fair, nine percentage points more likely to feel their case was taken seriously, and four percentage points more likely to report that they were satisfied with the police's handling of their case. While not all of these differences reach conventional levels of significance, they are consistent in their direction across each of the individual measures and marginally significant in the aggregate index ($p < 0.11$).

Complainants' subjective evaluations of how they were treated by the police are important because they influence perceptions of police legitimacy and willingness to cooperate

Figure 4-3: Differences in complainant satisfaction by religion, income, education, and political connections

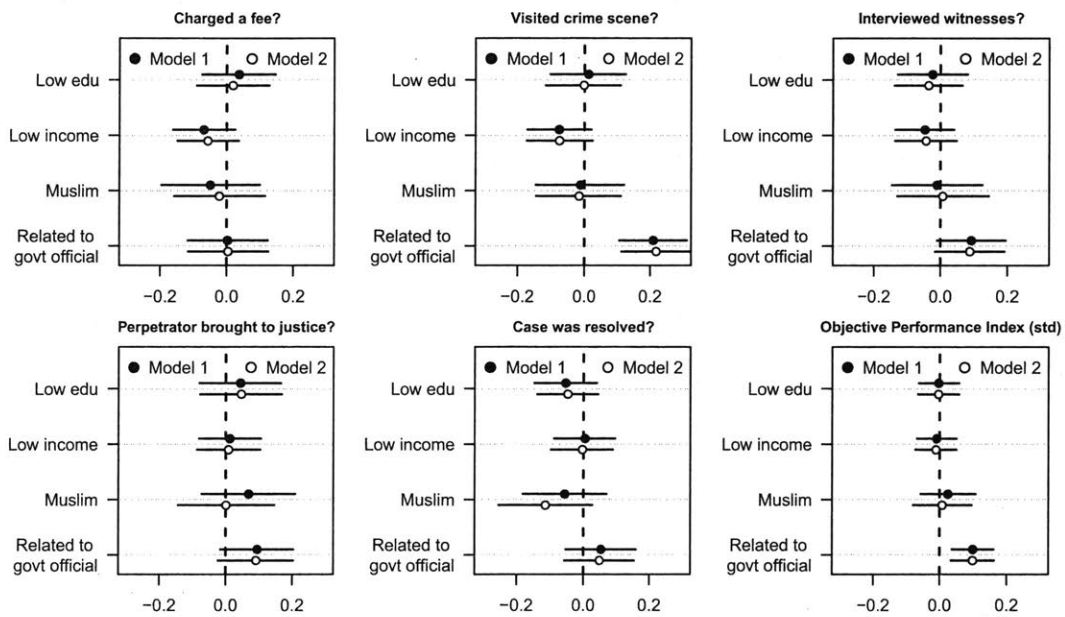


Notes: Differences in complainant satisfaction by religion, income, education, and political connections. Model 1 includes controls for type of crime only. Model 2 includes additional controls for crime location and complainant characteristics, following Equation 4.2 in Section 4.5. Standard errors clustered by community.

with the police (Tyler, 2006; Levi et al., 2009). However, subjective evaluations may or may not reflect more objective measures of police performance, such as whether cases are thoroughly investigated and resolved. Figure 4-4 complements the results reported in Figure 4-3 by reporting differences in objective measures of police effort and performance, including whether the complainant had to pay a bribe, whether the police investigated witnesses or visited the crime scene, whether the perpetrator was brought to justice, and whether the incident was ultimately resolved.

The results again provide little evidence to suggest the police discriminate on the basis of religion, class, or education, with differences across these dimensions small and statistically insignificant across all measures and specifications. Politically connected individuals, on the other hand, appear to experience greater police effort and more successful case outcomes. As compared to individuals without connections, these individuals were twenty-two percentage points more likely to report that the police visited the crime scene, nine percentage points more likely to report that the police interviewed witnesses, and eight percentage points more likely to report that the perpetrator was brought to justice. Adding controls for complainant and crime-level factors does little to alter these differences.

Figure 4-4: Differences in case outcomes by religion, income, education, and political connections



Notes: Differences in case outcomes by religion, income, and education. Model 1 includes controls for type of crime only. Model 2 includes additional controls for crime location and complainant characteristics, following Equation 4.2 in Section 4.5. Standard errors clustered by community.

4.6.4 Robustness of crime-level results & consideration of alternative mechanisms

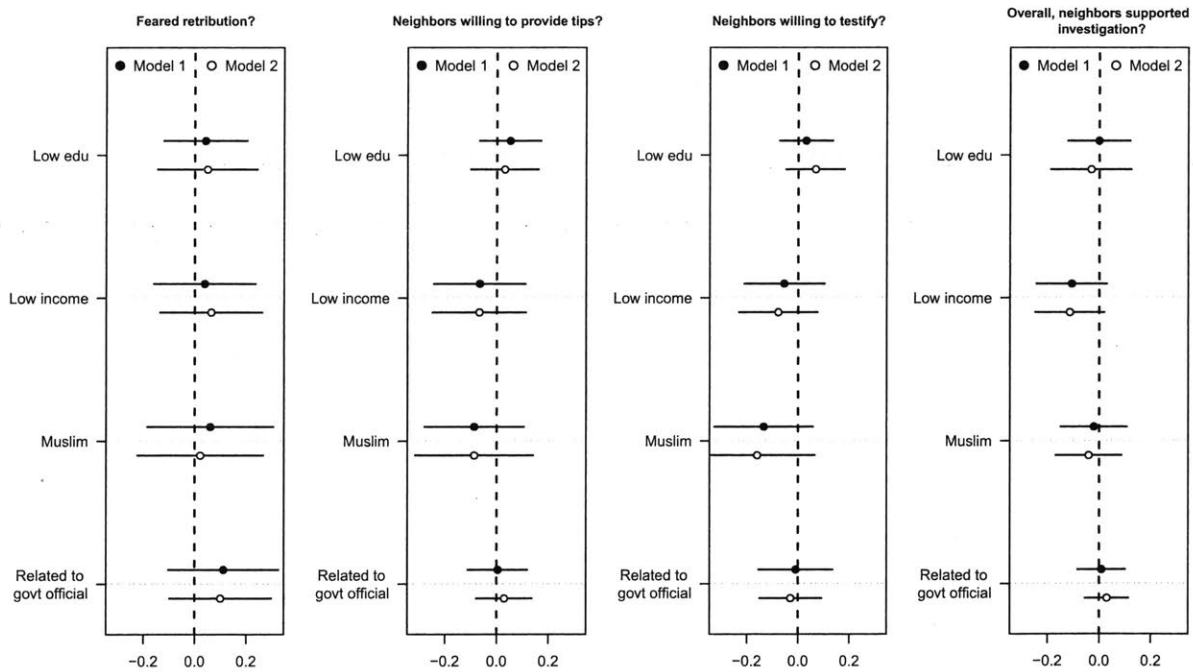
The crime-level data results presented above show that Muslims and individuals with below-median education or income are not significantly less likely to report crimes to the police, as compared to non-Muslims or individuals with above-median income or education, respectively. They further show that when these individuals do decide to report crimes to the police, they are not significantly less likely to be satisfied with how they were treated or to experience successful case outcomes. Individuals with connections to elites in government, on the other hand, are significantly more likely to report crimes to the police, as compared to individuals without these connections; moreover, when these individuals report crimes, they are significantly more likely to be satisfied with how they were treated and to experience successful case outcomes.

I interpret these patterns as evidence that the police favor politically connected individuals and evidence against the idea that the police discriminate against Muslims, the poor, or those with little education. However, as noted in Section 4.5, the validity of these interpretations rests on the assumption that unobserved factors associated with religion, education, income, or political connections do not also explain variation in reporting, satisfaction, or case outcomes. While it is impossible decisively rule out this possibility, I have attempted to limit the influence of confounders by controlling for a large set of observable complainant and crime-level characteristics, as outlined in Section 4.5. I have also shown that the results change little when moving from the sparse specification, which controls only for type of crime, to the full specification. This consistency indicates that observed factors are not confounding, and provides reason to believe that unobserved factors, insofar as they are similar in nature to observed factors, are also not confounding.

To further assess the possibility of unobserved confounding, this section identifies and evaluates the plausibility of four leading alternative explanations for my results. The first

is the possibility that certain types of individuals tend to come from neighborhoods where crimes are easier investigate, leading to greater investigative effort and more successful case outcomes. This scenario could lead me to falsely conclude that politically connected people are favored by the police, or conversely, to falsely conclude that Muslim, poor, or uneducated complainants are not discriminated against (in the sense that easier investigations could 'offset' the negative effect on case outcomes due bias against these individuals). While the robustness of the results to community fixed-effects makes this explanation unlikely, it is possible that a similar type of dynamic plays out on a more local scale, across blocks or neighborhoods within a community. To assess this possibility, I draw on survey questions asking complainants whether their fellow community members i) supported the decision to involve the police, ii) were willing to give information to help the police solve the case, and iii) would have been willing to provide written testimony if asked to do so.

Figure 4-5: Differences in community cooperation by religion, income, education, and political-connections



Notes: Differences in community cooperation by religion, income, education, and political-connections. Model 1 includes controls for type of crime only. Model 2 includes additional controls following Equation 4.2 in Section 4.5. Standard errors clustered by community.

Figure 4-5 reports how these responses to these questions vary according to complainants' religion, education, income, and political-connectedness. The results provide little evidence to suggest that these characteristics predict community cooperation, with differences close to zero across all four questions pertaining to community cooperation.

A second possibility is that some groups are simply more likely to offer socially desirable answers when asked questions about the police, calling into question the validity of this study's reliance on self-reported measures of police performance. To assess whether this is likely to be the case, I test for differences in respondents' perceptions of the police in general, outside the context of their personal experience when reporting a crime, on the assumption that biases leading to socially-desirable responses should manifest in these types of questions too. Examples of questions included in this placebo analysis include whether the police as a whole as trustworthy, whether the police "listen to the views of members of my community," and whether the police are corrupt, among others. To ensure this placebo analysis is not itself confounded by post-treatment bias among those reporting crimes to the police, I consider differences in perceptions among those who were never a victim of a crime ($N = 2189$) as well as those who were a victim of crime but did not report it to the police ($N = 1385$), in addition to differences among those who were a victim of a crime that was reported ($N = 392$). The results, reported in Appendix C.1, provide little evidence to suggest that social desireability bias influences the results. Across all three subgroups, differences in perceptions between Muslims and Christians, individuals with and without political connections, and individuals with above versus below median education are small and statistically insignificant. Although individuals with below median income are less likely to report favorable views of the police, this suggests that if anything, the null effects of low income on police performance are downward rather than upward biased.

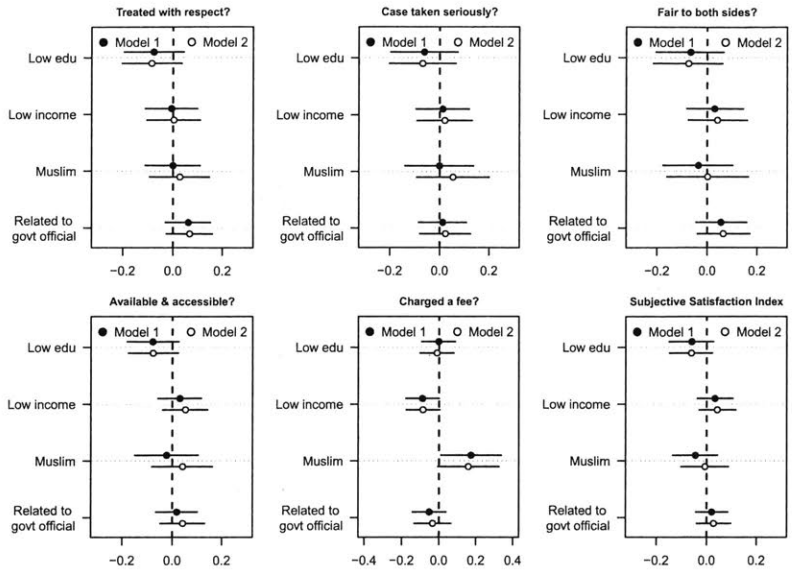
A third possibility that could lead to confounding would arise if certain types of individuals are better at advocating for themselves when interacting with authorities, for reasons unrelated to their political capital, religion, education, or income. It is possible, for ex-

ample, that politically connected families tend to be more cohesive and therefore devote greater effort and resources toward advocating for individual family members when they're in need. This scenario would lead us to falsely conclude that politically connected individuals are favored by the police, when in fact they're simply better positioned to draw on the help of their fellow family members.

To assess this possibility, I consider patterns of satisfaction among those reporting crimes to local leaders ($N = 291$). In Liberia, local leaders often handle property disputes and misdemeanor offenses such as simple assault or petty theft. If politically connected individuals are better at negotiating with authorities for reasons other than their political connections, then they should also report greater satisfaction when reporting cases to local leaders. The results, reported in Figure 4-6, show that this is not the case. Politically connected are not significantly more or less likely to feel that they were treated with respect, that their case was taken seriously, that the leaders were fair to both sides, or that the leaders were available and accessible, as compared to individuals without connections. Nor were they significantly more or less likely to pay a bribe. These results suggest that there may be something uniquely political about the advantages that politically connected individuals enjoy when interacting with the police.

The final alternative explanation considered here is that the apparent favoritism expressed towards politically connected individuals reflects the fact that these individuals are especially likely to share ethnic ties with police officers. If this were true, and if officers have a tendency to favor their coethnics, then this could lead us to falsely attribute differences in performance to political dynamics rather than shared ethnicity. While plausible, this explanation seems unlikely given that political-connectedness is only weakly associated with ethnicity, as discussed in Section 4.5, and given that the results hold even when controlling for complainants' ethnicity.

Figure 4-6: Differences in satisfaction with local leaders by religion, income, education, and political-connections



Notes: Differences in satisfaction by religion, income, education, and political-connections. Model 1 includes controls for type of crime only. Model 2 includes additional controls following Equation 4.2 in Section 4.5. Standard errors clustered by community.

4.7 Discussion

The results of the crime-level analysis accord with the results of the experimental analysis on three of the four forms of discrimination considered in this study: taste-based discrimination against Muslims, opportunistic exploitation of uneducated victims who lack the requisite skills and knowledge to hold police accountable, and discrimination against those without connections to political elites. But there is a mismatch between the experimental data and the crime-level data with respect to discrimination against the poor. Whereas the crime-level data provides little to no evidence of discrimination, the experimental data reveals that respondents expect police performance to be much lower when victims are poor as compared to when they are wealthy, and that this is only partly attributable to the perception that wealthy victims are better able to pay the informal fees that police sometimes charge to fund investigations.

Why are citizens more pessimistic about discrimination against the poor than the crime-level data suggests is warranted? In this section, I explore whether the apparent mismatch between expectations and reality might be explained by the conflation of *disparate impact* with *disparate treatment* in regards to the fees that victims sometimes have to pay to the police. As discussed in Section 4.3, the prevalence of informal fees for police services is a salient and contentious issue in Liberia, with critics alleging abuse of power and police on the one hand acknowledging the prevalence and illegality of this practice while on the other hand justifying it as an unavoidable consequence of their lack of resources. Notwithstanding the debate over its justification, few would disagree that the practice is closely linked to the perception that "justice is not for the poor" in Liberia (Human Rights Watch, 2013, 5).

In Table 4.6, I present the results of two tests of the hypothesis the mismatch between expectations and reality is at least partly explained by the conflation of disparate impact with disparate treatment. The first is a test of whether expected discrimination against the poor is stronger or weaker among the subset of respondents who've actually reported a crime to the police. Since we know from the crime-level data that these respondents on average received equal treatment regardless of their socioeconomic status (albeit of potentially low quality), then we would expect *expected discrimination* against the poor to be *lower* among these respondents if their expectations duly reflect their experiences. Conversely, if expected discrimination against the poor is driven by the salience of the police's "fee for service" model of service delivery, which although not disparately applied to the poor is easily construed as such by virtue of its disparate impact, then we would expect *expected discrimination* against the poor to be as strong or stronger among this population, since they will have had greater exposure to these practices. By the same logic, we would expect *expected discrimination* against the poor to be stronger among respondents who had previously reported a case to the police and been forced to pay a bribe or informal fee, relative to those who've not had these experiences.

Column 1 of Table 4.6 shows that the negative effect of the victim in the vignette being

poor as opposed to wealthy on expected police performance is slightly *stronger* among respondents who had previously reported a case to the police in the past 6 months. Although the interaction effect is not statistically significant, its direction is consistent with the idea that reporting crimes to the police heightens the salience of informal fees, which in turn becomes interpreted as disparate treatment or bias against the poor. Column 2 or Table 4.6 provides further evidence in support of this hypothesis, showing that the negative effect of the victim being poor is about 70 percent larger among those who previously reported a case to the police and were asked to pay a bribe or informal fee.

4.8 Conclusion

Ensuring that rank-and-file officers adhere to standards of professionalism, fairness, and impartiality is a challenge for police departments the world over, but nowhere more so than in weak and fragile states, where resource constraints and principal-agent problems are pervasive. In these settings, many rank-and-file officers shirk, solicit bribes, and engage in other forms of petty corruption because they are underpaid, and because they recognize that their commanders lack either the will or the ability to monitor their behavior.

Not only do these challenges undermine public trust and cooperation (Tyler, 2004), they also raise concerns about equity. Theories of bureaucratic bias suggest that accountability failures provide ample room for rank-and-file officers to engage in misconduct or to discriminate on the basis of their personal biases. Bargaining power theories of corruption predict that citizens who are poor and uneducated may be especially vulnerable to police misconduct, as these individuals lack the knowledge and skills required to advocate for their rights through formal accountability mechanisms. And a large literature on neopatrimonialism suggests that accountability failures can lead to favoritism towards politically connected individuals who can leverage their connections to pressure bureaucrats through informal channels.

This paper tests for discrimination along these dimensions among those reporting crimes to the police. Drawing on original crime-level data on reporting and case outcomes for 2600 incidents of crime, I find that while acts of shirking and petty bribery are common in general, individuals who are poor, lacking in education, or who identify with the religious minority do not appear to be more vulnerable to these behaviors than others. After controlling for a range of case-level factors, members of these groups are not significantly more or less likely to be satisfied with how they were treated, to experience successful case outcomes, or to report crimes in the first place, as compared to the rest of the population. I do, however, find evidence that politically connected individuals are favored by the police.

Compared to those without connections, these individuals are significantly more likely report crimes, to be satisfied with how they were treated, and to experience successful case outcomes.

I find a similar set of patterns using data from a conjoint experiment designed to elicit respondents' expectations about police discrimination. Whereas expected performance is the same for Muslim and non-Muslim victims and nearly the same for victims with low versus high levels of education, expected performance is roughly .08 standard deviations higher for politically connected victims relative to unconnected victims. And while I find expected performance is on average .10 standard deviations lower for victims who are poor relative to victims who are wealthy, this effect appears to be driven by a) the expectation that the poor will not be able to pay informal fees, resulting in fewer resources for the investigation, and b) the conflation of the police's "fee-for-service" model of service delivery — which the crime-level data indicates is applied without regard to socio-economic status — with discrimination against the poor. But while the police's practice of charging fees to investigate crimes may have a *disparate impact* on the poor, it does not constitute of violation of legal principles forbidding *disparate treatment*.

Though I cannot decisively identify the mechanism underlying favoritism towards the politically connected, I have argued that this finding is consistent with the idea that, in settings characterized by patronage and patrimonial forms of governance, informal channels of accountability rooted in personal connections to politically powerful individuals are more effective than formal channels grounded in bureaucratic rules, hierarchies, and reporting mechanisms. In Liberia, access to state resources has long been more closely associated with personal connections to elites in government than with the rules and guidelines set by law. These power relations persist today (Sawyer, 2008), and are likely to be one of the reasons why individuals with connections to government elites receive better services from the police and greater access to justice when victimized by crime.

The idea that informal channels of accountability trump formal channels rooted in rules

and bureaucratic hierarchy is also consistent with the null effects on income and education, for if formal channels were effective, then we would expect those best positioned to pressure for better performance through these channels — i.e. those with a steady income, education, and knowledge of the law and formal avenues of recourse — to be more successful at doing so than individuals without these skills and resources, and thus to report greater satisfaction and more successful case outcomes. That fact that we do not observe these patterns may suggest these formal, rule-based channels are ineffective.

For scholars and policymakers interested in police reform in weak and fragile states, these results provide both cause for optimism and cause for concern. On the one hand, the fact that rank-and-file police don't appear to exhibit religious or class-based biases is encouraging, especially given the apparent strength of these biases in the informal sector, and more generally, the long history of ethnic, religious, and class-based cleavages in Liberia. In this regard, the null effects on class and religious discrimination allude to the potential for formal-sector institutions to serve as fair and impartial providers of justice and security, even in weak states with highly salient social cleavages. That these null results come from Liberia, a seemingly "most likely" case for discrimination, should give pause to those characterizing bureaucratic bias as a "ubiquitous phenomenon" (Lipsky, 1980, p. 111), and points to the need for further research on the factors and conditions that either cause and prevent such bias.

On the other hand, the fact that police performance is lackluster overall, and that sources of favoritism appear to stem from connections to elites in government rather than biases by rank-and-file officers, suggests that police officials and commanders are complicit in the production of discriminatory policing, and that failures of accountability stem from way power is controlled and exercised among these officials rather than from principal-agent problems between these officials and their rank-and-file. Moving forward, policymakers should consider directing their attention to this mid-to-upper tier of government, through reforms designed to ensure commanding officers distribute rather than horde resources and

equip rank-and-file officers with the resources they need to perform, even for those without political connections.

Table 4.1: Crime Victim Attributes Randomized in Conjoint Experiment

Attribute	Level 1	Level 2	Level 3
Religion	Christian	Muslim	NA
Class	Poor (Market vendor)	Rich (NGO job)	NA
Related to govt official	No	Yes (Uncle)	NA
Education	Illiterate	Literate	NA
Crime	Theft of phone	Burglary	Burglary, assault

Table 4.2: Summary of outcomes in vignette experiment

% of responses "Likely" or "Very likely"	
Victim will be satisfied?	68%
Police will be professional?	76%
Police will be respectful?	79%
Police will investigate aggresively?	55%
Police will visit crime scene?	82%
Police will interview witnesses?	81%
Police will solicit bribe or fee?	69%

Notes: $N = 3788$ vignettes.

Table 4.3: What types of crimes are most common and where are they reported?

		# of cases (out of 2570)
Type of crime:		
Burglary	39%	1004
Simple assault	12%	307
Aggravated assault	10%	251
Property Encroachment	10%	253
Armed robbery	8%	201
Domestic abuse (verbal)	5%	115
Domestic violence	3%	92
Sexual violence	1%	23
Other	4%	84
Crime reported to:		
Police	19%	496
Court	3%	84
Local leaders	11%	291
Nowhere	67%	1729

Table 4.4: Summary of outcomes for those reporting crimes to the police

Complainant satisfaction outcomes	
Police treated you with respect?	73%
Police took your case seriously?	65%
Police fair to both sides?	62%
Police available and accessible?	84%
Would report your case to the police again?	79%
Police effort & case outcomes	
Had to pay the police?	46%
If had to pay: how much?	$9.8
Police visited scene of crime?	58%
Police interviewed witnesses?	53%
Case was resolved?	52%

Notes: N=496 reported cases.

Table 4.5: Average conditional marginal effects of attributes on expected performance indicators

	Composite index	Victim will be satisfied	Police will: be professional	be respectful
Muslim	-0.00	0.00	-0.02	-0.01
	(0.01)	(0.02)	(0.01)	(0.01)
Poor	-0.10***	-0.10***	-0.20***	-0.22***
	(0.01)	(0.01)	(0.01)	(0.01)
Illiterate	-0.02**	-0.04*	-0.05***	-0.03*
	(0.01)	(0.02)	(0.01)	(0.01)
Connected	0.08***	0.07***	0.14***	0.14***
	(0.01)	(0.01)	(0.01)	(0.01)
Y mean	0.00	0.68	0.76	0.80
N	3788	3788	3788	3788
		Police will:		
	investigate aggresively	visit crime scene	interview witnesses	charge fees
Muslim	0.02	0.01	0.02	-0.02
	(0.02)	(0.01)	(0.01)	(0.02)
Poor	-0.10***	-0.19***	-0.07***	-0.08***
	(0.02)	(0.01)	(0.01)	(0.02)
Illiterate	-0.01	-0.02^{+}	-0.01	-0.01
	(0.02)	(0.01)	(0.01)	(0.01)
Connected	0.02	0.11***	0.05***	-0.05**
	(0.02)	(0.01)	(0.01)	(0.02)
Y mean	0.56	0.82	0.82	0.69
N	3788	3788	3788	3788

Robust standard errors in parenthesis, clustered by respondent.* $p < 0.05$, ** $p < .01$, *** $p < .001$. Composite index is standardized with mean 0 and standard deviation 1. All other outcomes are binary indicators for "Likely" or "Very Likely" response.

Table 4.6: Heterogeneous effects by crime reporting & exposure to bribery

	Composite index of police performance	
Muslim	-0.00	-0.00
	(0.01)	(0.01)
Poor	-0.09***	-0.09***
	(0.01)	(0.01)
Illiterate	-0.02**	-0.02*
	(0.01)	(0.01)
Connected	0.07***	0.08***
	(0.01)	(0.01)
Reported crime	0.01	
	(0.02)	
Reported crime × Muslim	0.00	
	(0.02)	
Reported crime × Poor	-0.03	
	(0.02)	
Reported crime × Illiterate	0.00	
	(0.02)	
Reported crime × Connected	0.02	
	(0.02)	
Reported crime & paid bribe		-0.00
		(0.03)
Reported crime & paid bribe × Muslim		0.01
		(0.03)
Reported crime & paid bribe × Poor		-0.07*
		(0.03)
Reported crime & paid bribe × Illiterate		-0.02
		(0.03)
Reported crime & paid bribe × Connected		0.01
		(0.03)
N	3788	3788

Robust standard errors in parenthesis, clustered by respondent.* $p < 0.05$, ** $p < .01$, *** $p < .001$. Composite index is standardized with mean 0 and standard deviation 1.

Appendix A

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

A.1 Map of Liberia and sample communities

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

A.2 Implementation timeline

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

A.3 Sample selection

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

A.4 Measurement

We use our survey to measure most of the outcomes in our analysis. To measure **knowledge of Liberian law**, we asked respondents about their and the government's legal obligations in each of seven hypothetical scenarios.[1]) 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

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

Crime Victimization Survey. We focused on five categories of crime in particular: armed robbery, theft and burglary,[2] simple assault, aggravated assault,[3] and domestic violence.[4] We asked respondents whether they were victims of any of these crimes in the past twelve months, and—because these are rare events—whether they had witnessed or heard about any similar incidents in their communities in the past 12 months. (For crimes in the community, we added rape as well.)

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

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

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, 2018a). For each scenario, we asked respondents whether their

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

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

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

community would be likely to use trial by ordeal, and whether they personally would support its use. Finally, we measured **security of property rights** by asking respondents whether they were worried about encroachment on their "house spot" or farmland; whether they had made major improvements to their house spot or farmland in the last year, or planned to do so in the coming year; whether they left their farmland fallow in the past year, or planned to do so in the coming year; and whether they were involved in a dispute over their house spot or farmland in the past year.

A.5 Descriptive statistics

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

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

Perceptions of the police were mixed, but generally unfavorable. Half of all respondents believed they would have to pay for the police to investigate a crime, and only 26% believed the police would take their case seriously. More than half (56%) described the LNP as corrupt, though a large majority (80%) believed the LNP treats all tribes equally, and only a

small minority believed the LNP would verbally or physically abuse them if they were ever held in police custody (9% and 10%, respectively). Perceptions of the government were similarly mixed, with 54% describing the government as corrupt and only 40% describing the government's decisions as transparent, but 90% agreeing that the government treats all tribes the same.

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

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

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

A.6 Correlation matrix for dependent variables

To illustrate how our outcomes relate to one another, we estimate the pairwise correlation between composite indices constructed for each of our outcome clusters. We construct each composite index by taking the average of the component variables within each cluster, then rescaling so the index has a mean of zero and a standard deviation of one. The results are displayed in Table A.1.

A.7 Balance tests

Table A.2 reports balance tests across a range of community-level variables gleaned from the 2008 census. We find no evidence of imbalance on any of these variables.

A.8 Results with multiple comparisons adjustments

Tables A.3 and A.4 report the results from Figures 1 and 2 in the paper, respectively, including p-values adjusted for multiple comparisons. We use the Average Effect Size estimator to control Type II errors within clusters of outcomes, and therefore only adjust p-values across clusters. As we discuss in the paper, it is not obvious that such an adjustment is necessary in this case: adjusting across clusters only makes sense if we wish to assess whether the Confidence Patrols program had any effect on any outcome at all, which is not a hypothesis we intended to test. Nonetheless, for completeness we report Holm and Benjamini and Hochberg corrections for the 10 outcome clusters in Figure 1, and also for the six outcomes in Figure 2. After adjusting our p-values, the positive AES on knowledge of Liberian law is no longer statistically significant at conventional levels (though it is just shy of statistical significance at the 10% level using the Benjamini and Hochberg correction). Our other results are unchanged.

A.9 Results with and without controls

Table A.5 presents the AES for each of our outcome clusters, estimated with and without controls. As expected, effects vary little across specifications.

A.10 Average treatment effects on component dependent variables

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

A.11 Effects on crime reporting using LNP data

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

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

reporting, given by

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

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

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

A.12 Conditioning on crime occurrence when estimating differences in crime reporting

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

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

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

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

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

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

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

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

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

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

This bias will be zero whenever $\mathbb{E}[Y_i(0)|C_i(1) = 1] = \mathbb{E}[Y_i(0)|C_i(0) = 1]$. While it is not possible to verify this assumption empirically, we can test whether $\mathbb{E}[X_i|C_i(1) = 1] = \mathbb{E}[X_i|C_i(0) = 1]$—that is, whether crimes in treatment and control communities are similar to one another along observable characteristics, such as the age, ethnicity, level of education, and gender of the victim, and the distance of the victim to the nearest police station. If crimes in treatment and control communities are similar along these dimensions, then it is likely they are also comparable in terms of their potential reporting outcomes. Table A.6 shows that crimes in treatment and control communities are indeed similar, lending some credence to the assumption that $\mathbb{E}[Y_i(0)|C_i(1) = 1] = \mathbb{E}[Y_i(0)|C_i(0) = 1]$.

As an additional robustness check, we rerun our analysis using an approach that avoids conditioning on crime occurrence. This approach is drawn from the pre-analysis plan for the Evidence in Governance and Politics (EGAP) network's Metaketa IV initiative.[5] For each incident of crime, we construct three variables, each defined for the entire sample: an indicator for whether the crime occurred, an indicator for whether the crime occurred and was reported to a formal authority (with zero indicating that the crime either did not occur, occurred and was reported to an informal authority, or occurred and was not reported at all), and an indicator for whether the crime occurred and was *not* reported to a formal

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

authority (with zero indicating either that the crime did not occur, or occurred and was reported to a formal authority). We then calculate the Average Effect Size across each of the three sets of variables, following the procedure outlined in the paper. This approach is not perfect, because individually these effects are of ambiguous interpretation—e.g. a decrease in the the number of crimes that occurred and were reported could indicate fewer crimes, fewer reported crimes, or a combination of both—but when analyzed together, they are potentially informative.

Table A.7 reports our results. We find that the program had a negative but not statistically significant effect on the likelihood that a crime occurred; a positive but not statistically significant effect on the likelihood that a crime occurred and was reported; and a negative and not statistically significant effect on the likelihood that a crime occurred and was not reported. Focusing on misdemeanors alone, we find that the program had a negative and statistically significant effect on the likelihood that a crime occurred; a negative but not statistically significant effect on the likelihood that a crime occurred and was reported; and a negative and not statistically significant effect on the likelihood that a crime occurred and was not reported. Focusing on felonies instead, we find that the program had a positive but not statistically significant effect on the likelihood that a crime occurred; a positive and statistically significant effect on the likelihood that a crime occurred and was reported; and a negative but not statistically significant effect on the likelihood that a crime occurred and was not reported. While these results are difficult to interpret in isolation, together they are consistent with the conclusion we draw in the paper: the Confidence Patrols program increased crime reporting, and it did so primarily by reducing the number of crimes that were not reported at all.

A.13 Heterogeneity in crime reporting

Tables A.8, A.9, and A.10 report heterogeneity in our survey-based measure of crime reporting by gender, ethnicity, and age, respectively, comparing treatment and control communities. In general we do not find any evidence of heterogeneity along these dimensions.

A.14 Additional pre-specified heterogeneous 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.11. 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:

Index of exposure to government violence during the war

Index of exposure to rebel violence during the war

Whether the respondent is a leader in the community

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

Total number of patrols

Number of months since the last patrol

We do not have sufficient statistical power to conduct this last analysis due to a lack of variation: each treatment community was last patrolled in either August or September 2015, two and three months before the start of our endline, respectively. Table A.12 reports 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.15 Additional themes from qualitative field reports

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

A.15.1 Increasing trust in the police

One of the primary purposes of the Confidence Patrols program was to increase citizens' trust in the police and assuage their fears of increased police presence in and around their communities. The officers typically began their presentation by acknowledging the troubled and often violent history of policing in Liberia, especially during the civil war. They

(rather inaccurately) described the years preceding the conflict as a period of trust and co-operation 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.[6]

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."[7]

The officers' presentations often involved direct and personal appeals to attendees. In his opening remarks to residents in Bunadin, Nimba County, for example, one of the patrolling officers explained that he and his colleagues "had come to extend an arm of friendship so that you may have trust and confidence in us and work together with us as UNMIL plans to leave."[8] 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."[9]

---

[6]Yila Town, 2/4/15.
[7]Yila Town, 2/4/15
[8]Bunadin 4/5/2015.
[9]Yila Town, 2/4/15.

The officers also allowed citizens to air grievances against the LNP and PSU,[10] and to provide the officers with feedback, including on their own performance during the patrols.[11] 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;"[12] 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];"[13] to a husband whose wife "can't respect him, and has the constant habit of refusing him in bed;"[14] and to a wife whose husband "does not want to support the children and [her]."[15] The officers offered advice in all of these cases, often stepping out of their role as police officers to do so.[16]

Finally, and more controversially, the officers attempted to re-frame grievances against the LNP in ways that might elicit sympathy for the challenges facing an underfunded and understaffed police force. Perhaps the most common of these grievances, both in our sample and in Liberia as a whole, related to the fees that police officers routinely charge to investigate crimes. The PSU attempted to re-frame these not as bribes, but rather as fees-for-service. While the officers clarified that civilians are not legally required to pay the LNP to investigate crimes—a point on which there was much confusion, given the ubiquity of the practice[17]—they also warned that some fees may simply be unavoidable if the police

[10]Zolowee 4/6/2015; Jinnepeleta 8/20/2015

[11]Kpayaquelleh 2/19/2015; Gbenequelle 3/3/2015.

[12]Zowienta 4/21/2015.

[13]Tassah 2/6/2015.

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

[15]Tassah 2/6/2015.

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

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

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."[18] 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."[19] 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."[20] 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."[21]

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?"[22]

> "We have serious problems with some people who are doing drugs business in this town. Some hardcore youths that are involved in taking in these drugs are also causing problems for us. On numerous occasions, we have reported the case to the police at Botota. The police will come and arrest the people and carry them. After two days, we see these people back in the town doing the same business. How can we solve this problem?"[23]

In some cases, suspects are prematurely in exchange for bribes. But as the PSU officers repeatedly explained, in many other cases, premature release results from a misunderstanding

[18]Galai Town 2/5/2015.
[19]Zowienta 4/21/2015.
[20]Dean Town 2/5/2015.
[21]Zowienta 4/21/2015.
[22]Turkpah Town 2/7/2015
[23]Dean Town 2/5/2015.

of the habeas corpus provisions of Liberian law, which require that detainees be released after 48 hours if no formal complaint is registered against them.

The officers were sometimes defensive on this point: "We are faced with these kinds of problems every day, and you people shift the blame on the police. The problem is not with the 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."[24] 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."[25] 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."[26]

A.15.2 Encouraging crime reporting

These messages were part of a more general effort to encourage citizens to report crimes to the police, despite disappointing experiences in the past. In addition to simply instructing citizens to report, the officers attempted to clarify the categories of crime that must and may not be reported to the police under Liberian law. Equally important, the officers suggested strategies for reporting crime without risking social sanction by other members of the community, sometimes using elaborate hypothetical scenarios to elicit additional ideas from citizens.[27]

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

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

[24] Turkpah Town 2/7/2015.
[25] Gbarlorkpala 3/3/2015.
[26] Turkpah Town 2/7/2015.
[27] Gbanway 4/11/2015.

> 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."[28]

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.[29]

Advice of this sort may have encouraged citizens to report despite fears of ostracism, and may have indirectly reduced the incidence of domestic abuse and SGBV by leading potential perpetrators to believe they would be reported and arrested.

The officers also emphasized that if citizens were dissatisfied with the state's response to a case, they would have recourse to the appellate process, itself made more accessible by the proximity of the Hub. For example, when a resident of Turkpah asked what he should do if he suspects a magistrate court is "playing with [his] case," one of the officers responded that "you can take your case to the Hub and the Hub will provide you justice. That is the sole purpose of the Hub. It is built to bring justice and security close to you."[30] 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."[31] (Of course, these promises may have also raised citizens' expectations of the police and courts to unreasonable levels.)

[28] Wainsue 2/6/2015.
[29] Flumpa 8/1/2015.
[30] Turkpah 2/7/2015.
[31] Doe Town 3/4/2015.

A.15.3 Discouraging reliance on extrajudicial punishment

At the same time they encouraged reporting to the police, the patrolling officers explicitly discouraged the use of illegal or extrajudicial mechanisms of adjudication and dispute resolution, especially vigilantism, mob justice, and trial by ordeal. They urged citizens not to harm or "mock" those suspected of petty crimes,[32] nor to torture, kill, or unlawfully detain those suspected of more serious crimes, lest they themselves be charged with assault.[33] 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."[34]

A.15.4 Encouraging support for Community Watch Forums

As an alternative to extrajudicial punishment, the officers encouraged citizens to organize Community Watch Forums. In places where a Community Watch Forum already existed, they encouraged citizens to provide its members with food and supplies. They described these groups as "the eyes of the police in the town,"[35] responsible for being "vigilant of incoming criminals" and keeping "watch over the town to protect and keep the town safe."[36] They also emphasized that members of Community Watch Forums are volunteers: they are doing "voluntary service, and the community should assist them."[37] The officers also discouraged members of Community Watch Forums from engaging in vigilantism themselves.[38]

[32]Gbahn 8/2/2015.
[33]Tassah 2/6/15.
[34]Neignbain 2/15/2015.
[35]Gbarlorkpala 3/3/2015.
[36]Gbahn 8/2/2015.
[37]Loyee 8/1/2015.
[38]Gbahn 8/2/2015.

A.15.5 Increasing knowledge of Liberian law

The line between vigilantism and self-defense is ambiguous and poorly understood in most Liberian communities, and much of the Q&A was spent answering basic factual questions about Liberian law. In one community, for example, a farmer asked "if someone is in the constant habit of stealing from my farm, do I have the right to shoot him on sight?" (he does not);[39] 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").[40]

Citizens also asked whether the police are required by law to detain witnesses as suspects in criminal cases (they are not);[41] whether victims are legally obliged to pay to transport police investigators to and from the scene of a crime (they are not);[42] whether "there is a penalty for sexual assault or harassment" (there is);[43] and whether rape or other cases of SGBV can legally be settled (or "compromised") outside of court (they cannot).[44]

A.15.6 Increasing knowledge of the police

Lessons in Liberian law were accompanied by primers on the roles and responsibilities of the LNP. In many cases citizens were unsure about the types of grievances that do and do not fall under police jurisdiction. In one community, for example, a citizen asked whether "if an individual refuses to do town work and doesn't want to listen to the town chief, should we call the police or PSU?"[45] Another said that he "noticed that [his] wife has a boyfriend," and asked whether he could call the police to "intervene."[46]

[39] Zowienta 2/4/2015.
[40] Tassah 2/6/2015.
[41] Wainsue 2/6/2015; Gbenequelle 3/3/2015; Konia 4/11/2015.
[42] Yila Town 2/4/2015; Galai Town 2/5/2015; Dean Town 2/5/2015; Kollie-Ta 2/6/2015.
[43] Kpaiyea 8/8/2015.
[44] Wainsue, 2/6/2015 Konia 4/11/2015; Flumpa 8/1/2015.
[45] Yila 3/4/2015.
[46] Yila 3/4/2015.

In other cases citizens asked about the division of labor between the police and courts;[47] between the police and the town chief;[48] or between the police and the Community Watch Forums (which are authorized to assist the police but not to make arrests or adjudicate crimes).[49] 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?";[50] "If we caught a criminal in our town, what should we do to him?";[51] or "If someone is causing serious problems in the town, and the person does not want to change, what do we need to do?"[52]

A.15.7 Reducing crime

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

The officers repeatedly emphasized that domestic abuse is a crime for which perpetrators could expect to be punished, and explicitly discouraged husbands from beating their wives in response to perceived slights.[53] They delivered similar messages about rape and other forms of SGBV, which they repeatedly characterized as a non-bailable offense over which the state claims original jurisdiction, and which therefore cannot be "settled" or "compromised" informally, even if the victim or the victim's family would prefer to do so.[54]

[47] Kpayea 4/11/2015; Dean Town 4/21/2015.
[48] Tukpah 3/4/2015.
[49] Loyee 8/1/2015.
[50] Gbalorkpala 2/5/2015.
[51] Jinnepelata 2/6/2015.
[52] Yila 2/4/2015.
[53] Kpaiyea 8/8/2015.
[54] Konia 4/11/2015; Ganglota 4/13/2015; Flumpa 8/1/2015.

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.[55] 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."[56]

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.[57]

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."[58] The guidance the officers provided may have helped raise awareness of the variety of mechanisms available to resolve non-violent land disputes, and their promises to intervene to diffuse violent ones may have helped reassure property owners that conflicts over land use or boundaries would not be allowed to spiral out of control.

[55]Gbenequelle 3/3/2015; Tukpah 4/20/2015; Wainsue 4/23/2015.
[56]Gbenequelle 3/3/2015.
[57]Jinnepeleta 2/6/15.
[58]Doe Town 2/7/2015.

Figure A-1: Map of Liberia and sample communities

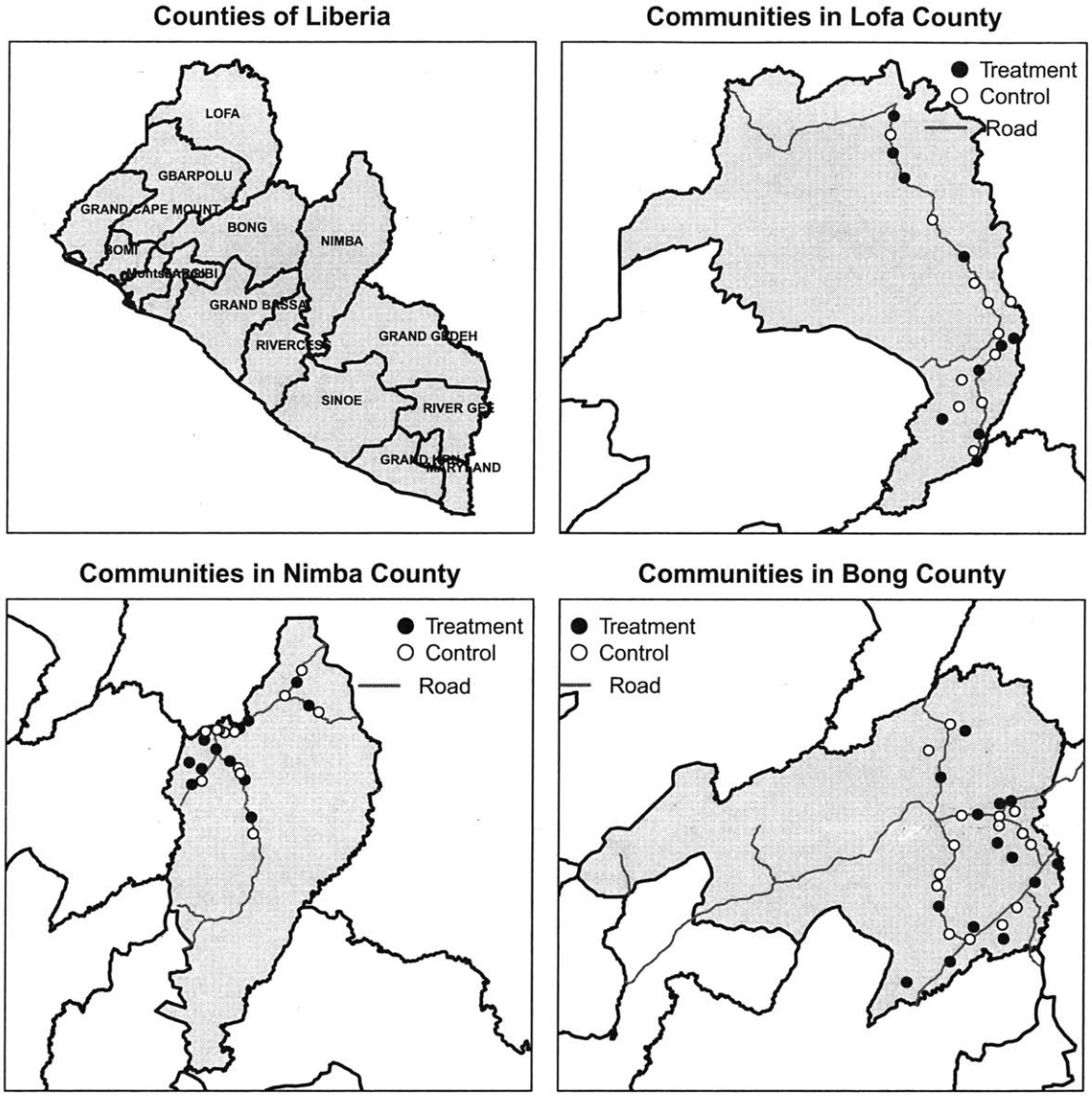


Figure A-2: Implementation timeline, June 2014-December 2015

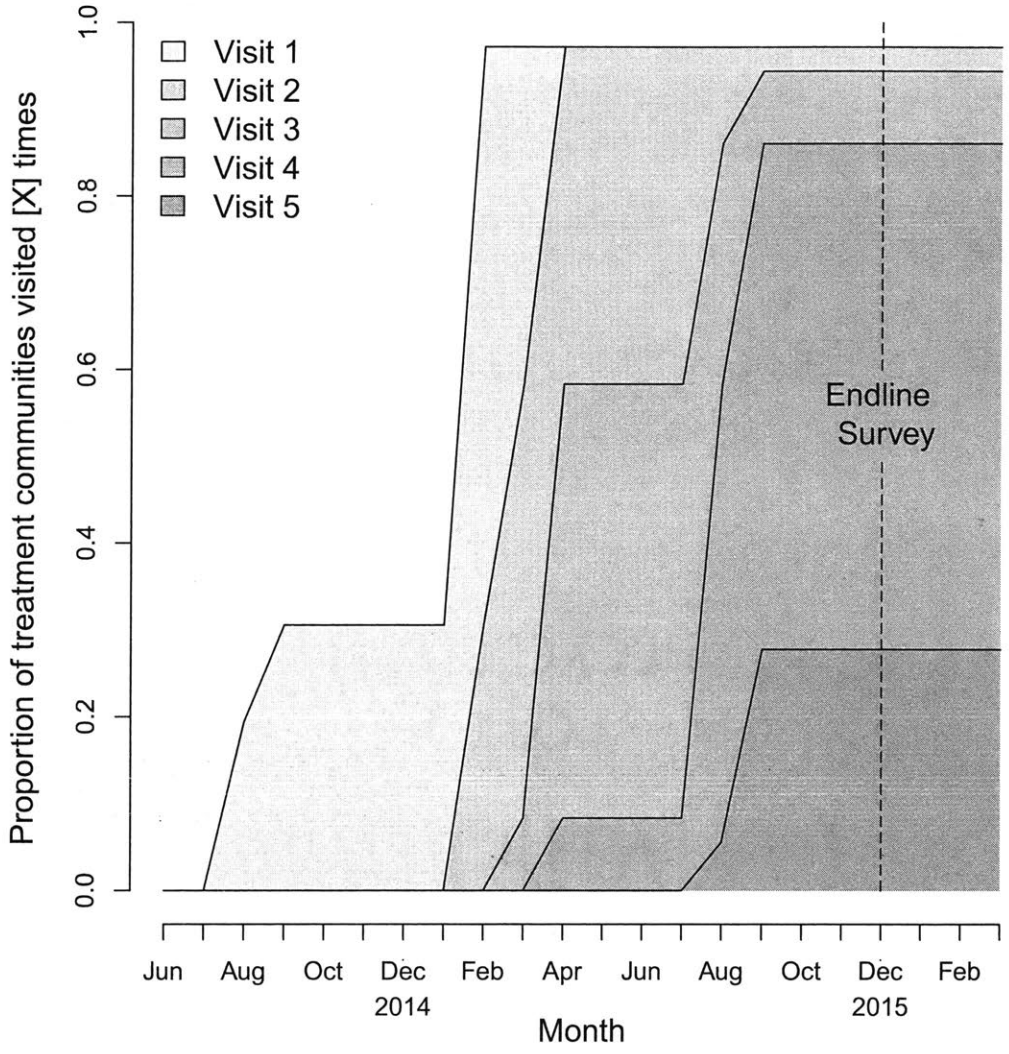


Table A.1: Descriptive statistics

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

Table A.0: 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.1: Correlation matrix for dependent variables

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

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

Table A.2: Balance

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

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

Table A.3: Average Effect Sizes with and without controls

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

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

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

Table A.4: Average treatment effects on crime with multiple comparisons adjustments

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

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

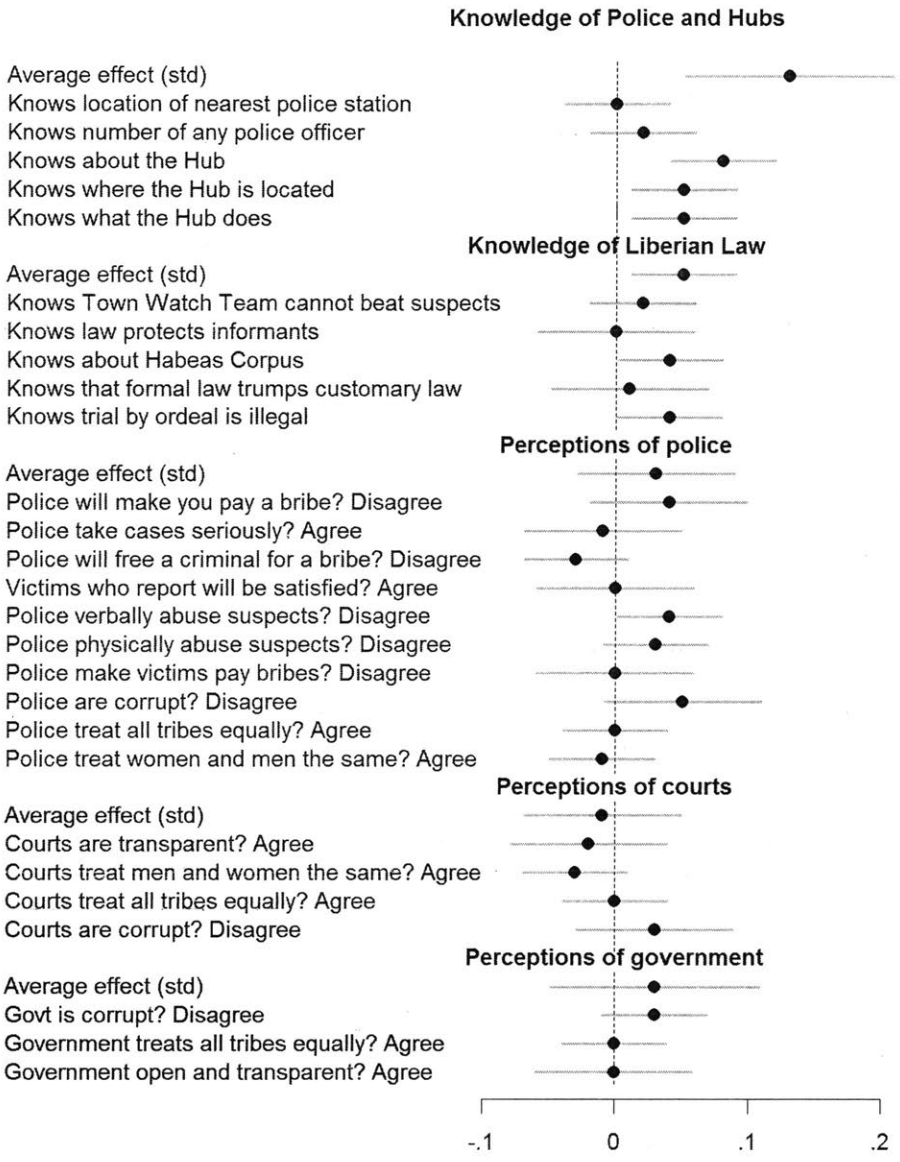
Table A.5: Average Effect Sizes with and without controls

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

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

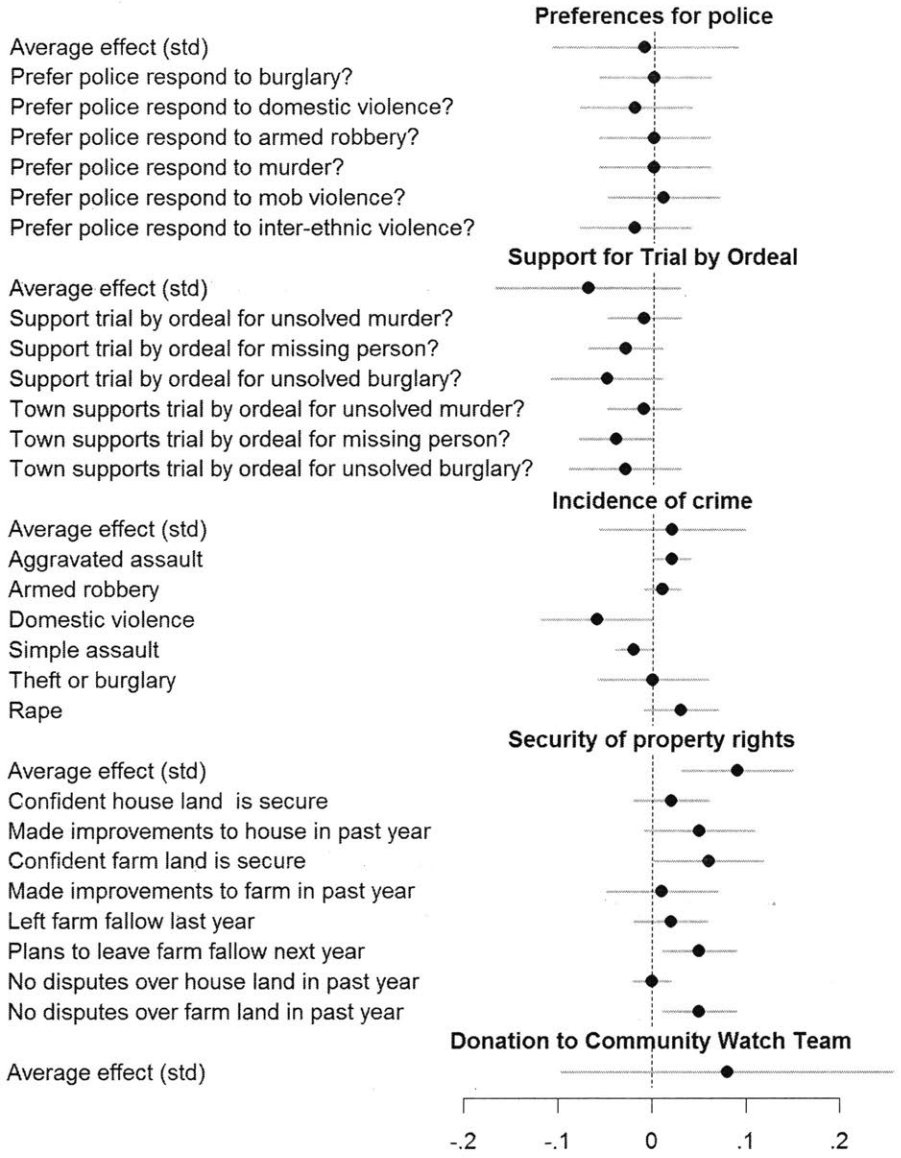
Average Effect Sizes (AES) for each cluster of outcomes. AES coefficients are interpreted in terms of standard deviations from the control group mean. Standard errors in parentheses, clustered by community. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Not all outcomes were measured for local leaders,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

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



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.

Table A.6: Balance across crimes in treatment and control communities

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

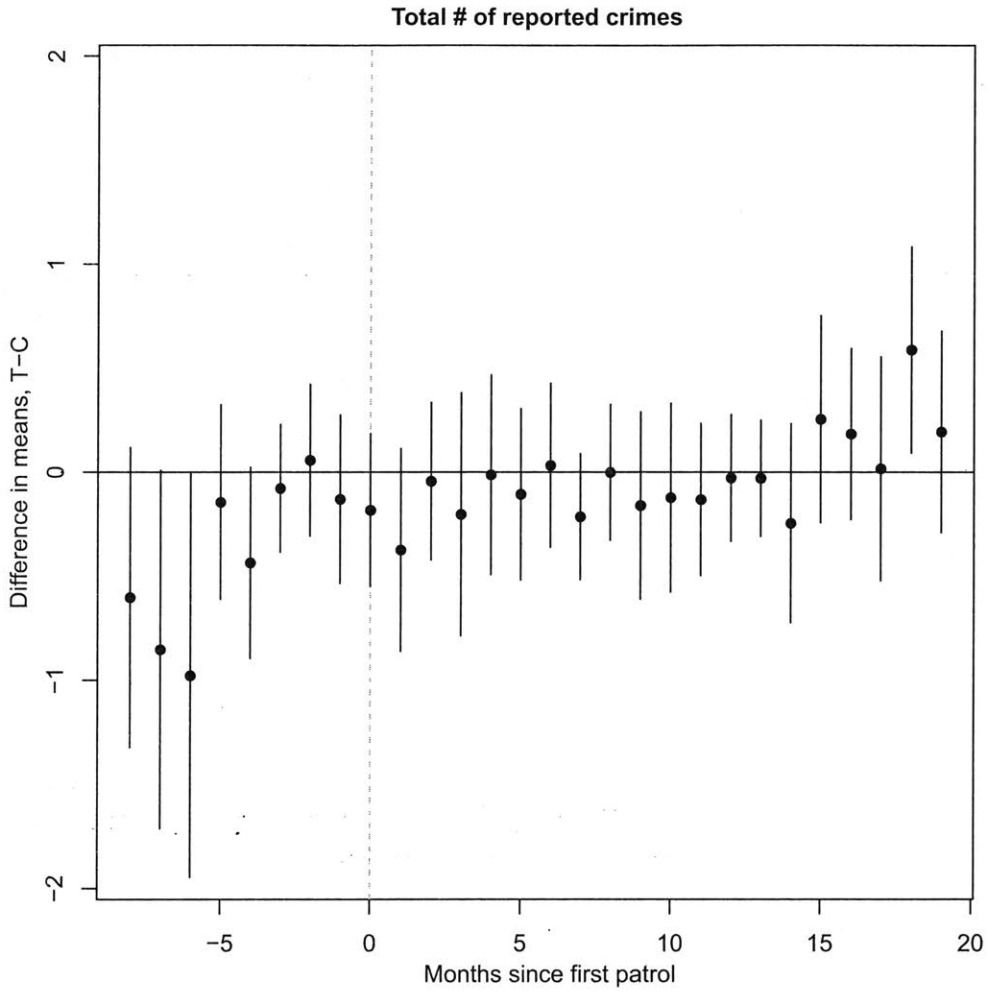
Differences in characteristics of crime victims across treatment and control communities. N=1,813 crimes.

Table A.7: Effects on crime reporting without conditioning on crime occurrence

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

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

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



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

Table A.8: Heterogeneous treatment effects on crime reporting by gender

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

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

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

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

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

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

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

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

Table A.11: Effects on secondary outcomes not reported in paper

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

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

Table A.12: Additional pre-specified heterogeneous treatment effects analyses

	Knowledge of police					Knowledge of law					Perceptions of police				
Treatment	0.13***	0.14**	0.10*	0.15***	0.34**	0.05*	0.04	0.04	0.06*	0.07	0.03	0.04	0.00	0.03	0.21*
	[0.04]	[0.05]	[0.04]	[0.04]	[0.12]	[0.03]	[0.04]	[0.03]	[0.03]	[0.09]	[0.03]	[0.04]	[0.03]	[0.03]	[0.09]
Treatment x leader															
Treatment x rebel violence		-0.00					0.01					-0.01			
		[0.04]					[0.02]					[0.03]			
Treatment x gov't violence			0.08					0.02					0.07*		
			[0.05]					[0.03]					[0.03]		
Treatment x police depot				-0.15					-0.07					-0.02	
				[0.16]					[0.09]					[0.05]	
Treatment x # patrols					-0.05					-0.01					-0.05*
					[0.03]					[0.02]					[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. $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$.; $^{***}p < 0.001$. Not all outcomes and independent variables measured among leaders; sample sizes vary accordingly.

Table A.11: Additional pre-specified heterogeneous treatment effects analyses (cont.)

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

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

Table A.10: Additional pre-specified heterogeneous treatment effects analyses (cont.)

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

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

Table A.9: Additional pre-specified heterogeneous treatment effects analyses (cont.)

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

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

Appendix B

Appendix for: Reducing Vigilantism in Fragile States: Evidence from a Field Experiment with the Liberian National Police

B.1 Intervention pamphlet

B.2 Descriptive statistics

Figure B-1: Example of a pamphlet distributed during patrols and meetings

Police accountability and the Professional Standards Division (PSD) of the LNP

- The Professional Standards Division is a special unit of the LNP responsible for handling incidents of Police Misconduct.
- If you witness an instance of police misconduct, you should report it to the PSD. You may do so by calling 0770800XXX, or by visiting the PSD office in person at LNP headquarters.
- You can also report police misconduct to Victor X, Chief of Patrol for Greater Monrovia (0770800XXX), the Zone Commander for your Zone, or any officer of the LNP.
- If you still don't get a response, bring your complaint to a Call In Show on the radio.

Remember, *no police officer* has the right to:

- Solicit a bribe from you
- Make you pay a fine without issuing a you citation
- Intervene in civil cases like land disputes or money business – that one is for the courts.
- Use excessive force against civilians

Community Watch Teams

A Community Watch Team is a community group comprised of community leaders and volunteers chosen and vetted by the community and the police.

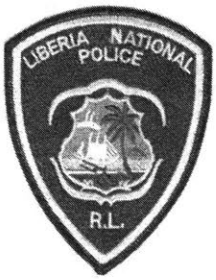
Responsibilities of the community watch team

1. Conduct night patrols in the community to deter criminal activity
2. Report all suspects immediately to the police
3. Provide information on suspected criminal activity to the LNP
4. Respect freedoms and rights of individuals arrested as violators or suspects
5. Protect the lives, security and properties of citizens and other persons in their respective communities

Rules governing the community watch team

1. Members MUST report all crimes to the Police
2. Members have arresting power but should report violators immediately to the Police
3. Membership is a Voluntary Service and not money making
4. Members should *never use weapons or engage in mob violence*
5. Members should not associate with known criminals or engage in collusion with said criminals

If your community would like to start a watch team, ask your Chairman/Chairlady contact the Community Services Section at the Zone Base.



Liberia National Police

Community Policing Program

POLICE ZONE 2

What is community policing?

Community policing is an approach to policing which recognizes the interdependence and shared the responsibility of the police and the community in ensuring a safe and secure environment.

Goals of community policing

- Prevent crime in the community through increased police presence and visibility
- Encourage communities to share information with the police
- Empower the community on issues of crime and their legal rights
- Promote open and honest relations between police and the residents
- Promote greater accountability in policing

Benefits associated with community policing

- Improved relationships between citizens and police
- Increased trust in police as well as information flow from the community
- Residents feel more comfortable approaching police officers due to greater familiarity
- Officers feel less hostility from the residents due to improved relations
- Greater police effectiveness due to improved team work between police and residents
- Greater police accountability

Women and Children Protection Services (WACPS)

- ✓ The Women and Children Protection Services (WACPS) division is trained to deal with cases of sexual and gender based violence, domestic abuse, and child abuse.
- ✓ If you know someone who is a victim of rape, domestic violence, child abuse, human trafficking, child marriage, or child labor, report it to WACPS
- ✓ Officers from WACPS are available at every police station. They have safe houses where victims can access food and shelter, and they can assist victims in getting help from other government agencies and NGOs, such as medical treatment, counseling, and legal assistance.

WACPS would like you to know:

- ✓ Sexual intercourse between someone over 18 and someone under 18 is called statutory RAPE. Statutory rape is a crime, just like forcible rape, and must be reported to the police.
- ✓ Be suspicious when you see uncles, step-fathers, neighbors, teachers, and mothers' boyfriends who are unusually friendly with young girls, as these people are the most common perpetrators of statutory rape.
- ✓ Child labor is also a common problem in Liberia. If you know of a child who has to work or sell instead of going to school, report it to WACPS

Key contacts at Zone 2 Police Base

Zone 2 Commander
Chief Superintendent James H. X
0770-800- XXX

Executive Officers / Patrol
Superintendent Veronica X
0770-800- XXX
Chief Inspector Sam B. X
0770-800- XXX

Chief of Operations
Chief Inspector Kamata T. X
0770-800- XXX

Community Services Section
Chief Inspector William R. X
0778-481- XXX / 0776-616- XXX
Inspector Edward X
0778-481- XXX / 0776-905- XXX
Sergeant Elaine T. Farley
0778-481- XXX

Women & Children Protection Services
Chief Inspector Anthony X
0770-800- XXX

Crime Services Department (CSD)
Inspector Edward X
0770-800- XXX
Sergeant J. Cyrus X
0770-800- XXX

Traffic Section
Inspector Muhammed X
0776-551-XXX

Radio Number: 0770-800-911

Other Important Numbers

Table B.1: Descriptive statistics

	Mean	N
Demographics		
Male	0.48	1,851
Age	37.3	1,851
Christian	0.88	1,851
No education	0.12	1,851
Elementary education	0.063	1,851
Junior High education	0.19	1,851
High schools education	0.42	1,851
Post secondary education	0.21	1,851
Literate	0.60	1,851
Familarity w/ police		
Knows about WACPS	0.94	1,851
Knows about PSD	0.87	1,851
Knows about CSD	0.40	1,851
Knows WACPS by name	0.55	1,851
Knows PSD by name	0.25	1,851
Knows local commander	0.20	1,851
Knows officer by name	0.40	1,851
Knows officer's number	0.21	1,851
Knowledge of law		
Knows statutory rape illegal	0.91	1,851
Knows about habeas corpus	0.94	1,851
Knows right to lawyer	0.91	1,851
Knows rights when reporting	0.96	1,851
Knows about child support	0.92	1,851
Knows obligation to report serious crimes	0.34	1,851
Knows informal fees illegal	0.32	1,851
Knows bond fees illegal	0.31	1,851
Police intentions		
Police corrupt?	0.48	1,851
Police treat all equal	0.42	1,851
Police take cases seriously	0.67	1,851
Police fair to all sides	0.54	1,851
Police care about citizens' safety	0.69	1,851
Police respect citizens	0.56	1,851
Police objective	0.62	1,851
Police respect victims	0.70	1,851

Notes: Apart from *Age*, all variables are coded dichotomously.

Table B.2: Descriptive statistics (cont.)

	Mean	N
Cooperation norms		
Ppl get mad if you report a burglary?	0.28	1,851
Ppl get mad if you report domestic violence?	0.43	1,851
Ppl get mad if you report land disputes?	0.29	1,851
Ppl should always obey the police	0.33	1,851
Ppl get mad if you give info about armed robbery?	0.26	1,851
Ppl get mad if you give info about domestic violence?	0.30	1,851
Ppl get mad if you give info about stolen motorcycle?	0.23	1,851
Ppl get mad if you give info about child abuse?	0.30	1,851
Trust in police capacity		
Police able to respond quickly	0.62	1,851
Police able to investigate effectively	0.80	1,851
Police act on citizens' feedback	0.60	1,851
Police include citizens in decision making	0.75	1,851
Support for mob violence		
Mob violence justified for rape?	0.16	1,851
Mob violence justified for armed robbery?	0.22	1,851
Mob violence justified for burglary?	0.19	1,851
Community coproduction of security		
Seen security mtg past month	0.43	1,849
Attended security mtg past month	0.24	1,850
Seen patrol past month	0.39	1,848
Attended patrol past month	0.19	1,851
Gave food/tea past month	0.32	1,850
Town has Watch Forum?	0.27	1,850
Forum patrols at night?	0.22	1,851
Forum meetings regularly?	0.18	1,851
Forum registered with police?	0.23	1,848
Knowledge of watch team rules		
Weapons prohibited	0.23	1,851
Physical harm prohibited	0.73	1,851
Must avoid risks & danger	0.37	1,851
Checkpoints prohibited	0.17	1,851
Only operate in home community	0.49	1,851
Can perform citizens' arrest	0.95	1,851

Notes: All variables are coded dichotomously.

Table B.3: Descriptive statistics (cont.)

	Mean	N
Prefer police respond to:		
Land disputes	0.27	1,851
Violent land disputes	0.63	1,851
Burglaries	0.80	1,851
Domestic violence	0.44	1,851
Armed robbery	0.88	1,851
Crime tips & information sharing		
Reported suspicious activity past 6m	0.091	1,851
Helped police find suspect past 6m	0.068	1,849
Given info for investigation past 6m	0.097	1,849
Provided testimony past 6m	0.068	1,851
Willingness to report police misconduct		
Would report illegal checkpoint	0.68	1,849
Would report drunk officer	0.49	1,849
Would report police beating	0.85	1,851
Crime		
# of armed robberies	0.41	1,844
# burglaries	1.39	1,833
# of aggravated assaults	0.48	1,840
# simple assaults	0.65	1,837
# sexual violence	0.069	1,850
# domestic violence	0.64	1,842
# domestic violence	0.91	1,817
# violence land disputes	0.26	1,846
# non violent land disputes	0.20	1,817
# other violent crimes	0.027	1,803
# other non violent	0.069	1,818
# murders	0.050	1,851
# child abuse	0.32	1,838
Perceptions of security		
Fears violent crime	0.52	1,851
Fears non-violent crime	0.53	1,851
Fears walking at night	0.34	1,851
Fears home invasions	0.27	1,851
House boundaries secure	0.58	1,851
House items secure	0.45	1,851
Motorbike secure outside	0.093	1,851
Generator secure outside	0.076	1,851
Overall satisfaction with police performance		
Trust police?	0.62	1,851
Satisfied w/ police performance?	0.61	1,851

Notes: Apart from crime variables, all variables are coded dichotomously.

B.3 Tests of balance

Table B.4 shows the estimated mean for the control group (Column 2) and the estimated difference in means between the treatment and control groups. The estimates are produced using WLS regression of each outcome on treatment and block fixed effects to account for block randomization, with weights accounting for unequal probabilities of assignment.

Table B.4: Tests of balance

	(1) Difference in means	(2) Control mean
male	0.01	0.44***
	(0.03)	(0.02)
age	-1.13	37.42***
	(0.78)	(0.56)
rel_Christian	0.03	0.87***
	(0.03)	(0.02)
education	0.07	4.36***
	(0.16)	(0.11)
literate	0.01	0.62***
	(0.03)	(0.02)
know_pol_idx	-0.05	0.02
	(0.08)	(0.06)
know_idx_lbr	0.01	-0.00
	(0.05)	(0.04)
intentions_idx_lbr	0.11*	-0.06
	(0.06)	(0.04)
norm_idx_lbr	0.00	-0.00
	(0.07)	(0.04)
police_capacity_idx	0.05	-0.02
	(0.06)	(0.04)
responsive_act	0.01	-0.00
	(0.05)	(0.04)
ca_sec_idx	-0.01	0.00
	(0.10)	(0.07)
sup_mobviol_idx	-0.12*	0.06
	(0.06)	(0.04)
crimeres_idx_lbr	-0.08	0.04
	(0.05)	(0.05)
crime_tips_idx_lbr	0.02	-0.01
	(0.06)	(0.04)
police_abuse_report_idx_lbr	0.07	-0.03
	(0.08)	(0.06)
crime_num_lbr	0.13	-0.06*
	(0.07)	(0.03)
cmob_num	0.03	-0.02
	(0.08)	(0.04)
future_security_idx_lbr	-0.01	0.01
	(0.07)	(0.04)

Standard errors clustered by community in parentheses. N=1858.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

B.4 Effects on crime reporting

Table B.5 reports estimates of the program's effect on crime reporting, following Equation 3.3 in the paper. The program did not have a statistically significant impact on crime reporting to either the police, local leaders, or nowhere at all.

Table B.5: Average treatment effects on crime reporting

	Crime reported to:		
	Nowhere	Police	Leaders
All crimes			
Treatment	-0.01	-0.02	0.02
	(0.02)	(0.02)	(0.02)
Ctrl mean	0.55	0.28	0.11
N	3763	3763	3763
Violent crimes			
Treatment	-0.04	-0.01	0.04
	(0.02)	(0.03)	(0.02)
Ctrl mean	0.50	0.33	0.10
N	1770	1770	1770
Non-violent crimes			
Treatment	0.03	-0.04	0.01
	(0.03)	(0.02)	(0.02)
Ctrl mean	0.60	0.23	0.12
N	1993	1993	1993

Robust standard errors in parenthesis, clustered by community. * $p < 0.05$, ** $p < .01$, *** $p < .001$

B.5 Effects on primary outcomes with multiple comparisons adjustments

Tables B.6 reports the results on this study's primary outcome indices with p-values adjusted for multiple comparisons following Benjamini and Hochberg (1995). After adjusting the p-values, the negative effect on incidence of mob violence remains significant at the 1% level; likewise, effects on all of the other outcomes remain not statistically significant, as reported in the paper.

Table B.6: Average treatment effects on primary outcomes with multiple comparisons adjustments

	Willingness to report crimes)		Crime tips & info sharing		Willingness to report misconduct		Total # of crimes reported	
Treatment	0.01	0.01	-0.02	-0.04	0.01	0.01	-0.12	-0.31
	(0.02)	(0.02)	(0.05)	(0.05)	(0.03)	(0.03)	(0.37)	(0.36)
Ctrl mean	0.60	0.60	0.01	0.01	2.80	2.80	5.21	5.21
P-value	0.65	0.59	0.74	0.45	0.68	0.81	0.75	0.40
P-value (BH)	0.75	0.69	0.75	0.63	0.75	0.81	0.75	0.63
N	1851	1851	1851	1851	1850	1850	1673	1673

	Perceptions of security		Satisfaction w police performance		Incidence of mob violence	
Treatment	0.10	0.10	0.05	0.05	-0.32**	-0.34***
	(0.08)	(0.07)	(0.06)	(0.05)	(0.10)	(0.10)
Covariates	N	Y	N	Y	N	Y
Ctrl mean	-0.06	-0.06	2.23	2.23	0.96	0.96
P-value	0.17	0.18	0.35	0.37	0.00	0.00
P-value BH	0.59	0.63	0.75	0.63	0.00	0.00
N	1851	1851	1846	1846	1845	1845

Each outcome is an index standardized by the baseline mean. Robust standard errors in parenthesis, clustered by community. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < .01$, *** $p < .001$

B.6 Effects on indices and component dependent variables

Familiarity with police

Table B.7: Average treatment effects on familiarty with police

	Familiarity index (std)		Knows about WACPS		Knows about PSD		Knows about CSD		Knows WACPS by name	
treatment	-0.04	-0.06	-0.02*	-0.03*	-0.01	-0.01	-0.03	-0.03	-0.07*	-0.08*
	(0.07)	(0.07)	(0.01)	(0.01)	(0.02)	(0.02)	(0.04)	(0.04)	(0.03)	(0.03)
Controls	N	Y	N	Y	N	Y	N	Y	N	Y
Ctrl mean	0.02	0.02	0.96	0.96	0.88	0.88	0.42	0.42	0.59	0.59
N	1851	1851	1851	1851	1851	1851	1851	1851	1851	1851

	Knows PSD by name		Knows Commander by name		Knows officer by name		Knows officers' number	
treatment	-0.06^{+}	-0.06^{+}	0.05*	0.04^{+}	0.04	0.03	0.03	0.02
	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)
Controls	N	Y	N	Y	N	Y	N	Y
Ctrl mean	0.28	0.28	0.17	0.17	0.38	0.38	0.19	0.19
N	1851	1851	1851	1851	1851	1851	1851	1851

Robust standard errors in parenthesis, clustered by community. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < .01$, *** $p < .001$

Knowledge of law

Perceptions of police intentions

Table B.8: Average treatment effects on knowledge of law

	Knowledge of law idx (std)		Knows statutory rape illegal		Knows about habeas corpus		Knows right to lawyer		Knows rights when reporting	
Treatment	-0.04	-0.04	0.00	0.00	-0.02	-0.02	-0.01	-0.01	-0.01	-0.01
	(0.07)	(0.07)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Covariates	N	Y	N	Y	N	Y	N	Y	N	Y
Ctrl group mean	0.02	0.02	0.91	0.91	0.95	0.95	0.92	0.92	0.97	0.97
N	1851	1851	1851	1851	1851	1851	1851	1851	1851	1851

	Knows about child support		Knows obligation to report serious crimes		Knows informal fees illegal		Knows bond fees illegal	
Treatment	-0.03	-0.03	0.00	0.01	0.00	0.00	0.01	0.01
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Covariates	N	Y	N	Y	N	Y	N	Y
Ctrl group mean	0.93	0.93	0.35	0.35	0.31	0.31	0.31	0.31
N	1851	1851	1851	1851	1851	1851	1851	1851

Robust standard errors in parenthesis, clustered by community. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < .01$, *** $p < .001$

Norms against cooperation

Perceptions of police capacity

Table B.9: Average treatment effects on perceptions of police intentions

	Trust police intentions idx (std)		Police corrupt		Police treat all equal		Police take cases seriously		Police fair to all sides	
Treatment	0.12*	0.13*	-0.06*	-0.05*	0.05+	0.04	0.03	0.03	0.00	0.01
	(0.06)	(0.05)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
Covariates	N	Y	N	Y	N	Y	N	Y	N	Y
Ctrl group mean	-0.06	-0.06	0.51	0.51	0.40	0.40	0.65	0.65	0.54	0.54
N	1850	1850	1851	1851	1851	1851	1851	1851	1851	1851

	Police care about citizens' safety		Police respect citizens		Police objective		Police respect victims			
Treatment	0.06*	0.05*	0.03	0.04	0.02	0.02	0.04+	0.04*		
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)		
Covariates	N	Y	N	Y	N	Y	N	Y	N	Y
Ctrl group mean	0.66	0.66	0.55	0.55	0.61	0.61	0.68	0.68		
N	1851	1851	1851	1851	1851	1851	1851	1851		

Robust standard errors in parenthesis, clustered by community. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < .01$, *** $p < .001$

Perceptions of police responsiveness

Contributions to community coproduction

Table B.10: Average treatment effects on norms against cooperation

	Norms against cooperation idx		People get mad if you report: a burglary?		domestic violence?		land disputes?		Ppl should always obey the police	
Treatment	-0.03	-0.03	-0.01	-0.01	-0.05^{+}	-0.04^{+}	-0.02	-0.02	0.02	0.02
	(0.05)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
Covariates	N	Y	N	Y	N	Y	N	Y	N	Y
Ctrl group mean	1.62	1.62	0.28	0.28	0.45	0.45	0.30	0.30	0.32	0.32
P-value	0.51	0.49	0.82	0.79	0.09	0.09	0.52	0.53	0.30	0.39
N	1850	1850	1851	1851	1851	1851	1851	1851	1851	1851

	People get mad if you give info about: armed robbery?		domestic violence?		stolen motorcycle?		child abuse?	
treatment	-0.01	-0.02	-0.04	-0.04	0.00	-0.00	-0.01	-0.01
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Covariates	N	Y	N	Y	N	Y	N	Y
Ctrl group mean	0.26	0.26	0.32	0.32	0.23	0.23	0.31	0.31
P-value	0.61	0.58	0.20	0.15	0.96	0.97	0.76	0.72
N	1851	1851	1851	1851	1851	1851	1851	1851

Robust standard errors in parenthesis, clustered by community. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < .01$, *** $p < .001$

Table B.11: Average treatment effects on perceptions of police capacity

	Trust police capacity idx (std)		Police able to respond quickly		Police able to investigate effectively	
Treatment	0.12*	0.12*	0.06*	0.06*	0.02	0.02
	(0.06)	(0.05)	(0.03)	(0.03)	(0.02)	(0.02)
Ctrl group mean	-0.06	-0.06	0.59	0.59	0.79	0.79
P-value	0.03	0.02	0.02	0.01	0.31	0.29
N	1844	1844	1851	1851	1851	1851

Robust standard errors in parenthesis, clustered by community. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < .01$, *** $p < .001$

Support for mob violence

Knowledge of watch group rules

Table B.12: Average treatment effects on perceptions of police responsiveness

	Police responsiveness idx (std)		Police act on citizens' feedback		Police include citizens in decision making	
Treatment	0.04	0.03	0.00	-0.00	0.02	0.01
	(0.05)	(0.05)	(0.03)	(0.02)	(0.03)	(0.03)
Ctrl group mean	2.41	2.41	0.60	0.60	0.74	0.74
P-value	0.48	0.53	0.91	0.87	0.56	0.62
N	1841	1841	1851	1851	1851	1851

Robust standard errors in parenthesis, clustered by community. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < .01$, *** $p < .001$

Table B.13: Average treatment effects on contributions to community coproduction

	Coproduction idx (std)		Seen security mtg past month		Attended security mtg past month		Seen patrol past month		Attended patrol past month	
Treatment	0.21^{+}	0.20^{+}	0.16***	0.15***	0.13***	0.11***	0.05	0.04	0.05^{+}	0.04
	(0.11)	(0.10)	(0.04)	(0.04)	(0.03)	(0.03)	(0.05)	(0.04)	(0.03)	(0.03)
Covariates	N	Y	N	Y	N	Y	N	Y	N	Y
Ctrl group mean	-0.10	-0.10	0.35	0.35	0.18	0.18	0.36	0.36	0.17	0.17
P-value	0.06	0.06	0.00	0.00	0.00	0.00	0.26	0.32	0.10	0.16
N	1851	1851	1849	1849	1850	1850	1848	1848	1851	1851

	Gave food/tea past month		Town has Watch Forum		Forum patrols at night		Forum meets regularly		Forum registered with police	
Treatment	0.01	0.01	0.06	0.05	0.05	0.04	0.05	0.05	0.07	0.06
	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)
Covariates	N	Y	N	Y	N	Y	N	Y	N	Y
Ctrl group mean	0.32	0.32	0.25	0.25	0.19	0.19	0.16	0.16	0.19	0.19
P-value	0.79	0.88	0.26	0.26	0.31	0.31	0.19	0.20	0.15	0.15
N	1850	1850	1850	1850	1851	1851	1851	1851	1848	1848

Robust standard errors in parenthesis, clustered by community. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < .01$, *** $p < .001$

Willingness to report crimes to police

Crime tips & information sharing

Table B.14: Average treatment effects on support for mob violence

	Support for mob violence idx (std)		Mob violence justified for: rape?		armed robbery?		burglary?	
Treatment	-0.06	-0.07	-0.02	-0.02	-0.04*	-0.04*	-0.03	-0.03
	(0.05)	(0.05)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Covariates	N	Y	N	Y	N	Y	N	Y
Ctrl mean	0.03	0.03	0.16	0.16	0.25	0.25	0.20	0.20
P-value	0.21	0.17	0.42	0.32	0.02	0.01	0.22	0.15
N	1847	1847	1851	1851	1851	1851	1851	1851

Robust standard errors in parenthesis, clustered by community. $^{+}$ $p <$ 0.10, * $p < 0.05$, ** $p < .01$, *** $p < .001$

Table B.15: Average treatment effects on knowledge of rules governing local security groups

	Knowledge of rules idx (std)		Weapons prohibited		Physical harm prohibited		Must avoid danger	
treatment	0.03**	0.03*	0.08***	0.08***	0.05*	0.05*	0.05^{+}	0.05^{+}
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Covariates	N	Y	N	Y	N	Y	N	Y
Ctrl group mean	0.55	0.55	0.19	0.19	0.71	0.71	0.35	0.35
P-value	0.01	0.01	0.00	0.00	0.01	0.02	0.07	0.07
N	1851	1851	1851	1851	1851	1851	1851	1851

	Checkpoints prohibited		Only operate in home community		Can perform citizens' arrest	
Treatment	0.02	0.02	-0.01	-0.01	0.03*	0.03*
	(0.02)	(0.02)	(0.04)	(0.04)	(0.01)	(0.01)
Covariates	N	Y	N	Y	N	Y
Ctrl group mean	0.16	0.16	0.49	0.49	0.93	0.93
P-value	0.18	0.29	0.72	0.71	0.01	0.02
N	1851	1851	1851	1851	1851	1851

Robust standard errors in parenthesis, clustered by community. $^{+}$ $p < 0.10$, * $p <$ 0.05, ** $p < .01$, *** $p < .001$

Willingness to report police misconduct

Incidence of crime

Table B.16: Average treatment effects on willingness to report crimes to police

	Crime resolution idx		Prefers police resolve: Land disputes		Violent land disputes		Burglaries		Domestic violence		Armed robbery	
treatment	0.01	0.01	0.03	0.03	0.01	0.01	-0.01	-0.01	0.03	0.03	-0.03^{+}	-0.03^{+}
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)
Covariates	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
Ctrl group mean	0.60	0.60	0.26	0.26	0.63	0.63	0.81	0.81	0.42	0.42	0.90	0.90
P-value	0.65	0.59	0.18	0.12	0.60	0.54	0.66	0.70	0.24	0.22	0.08	0.10
N	1851	1851	1851	1851	1851	1851	1851	1851	1851	1851	1851	1851

Robust standard errors in parenthesis, clustered by community. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < .01$, *** $p < .001$

Table B.17: Average treatment effects on willingness to report crimes to police

	Crime tips & info sharing idx (std)		Reported suspicious activity past 6m		Helped police find suspect past 6m		Gave info for investigation past 6m		Provided testimony past 6m	
treatment	-0.02	-0.04	-0.01	-0.01	0.00	-0.00	-0.01	-0.02	0.00	-0.00
	(0.05)	(0.05)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Covariates	N	Y	N	Y	N	Y	N	Y	N	Y
Ctrl mean	0.01	0.01	0.10	0.10	0.07	0.07	0.10	0.10	0.07	0.07
P-value	0.74	0.45	0.67	0.43	0.85	0.98	0.41	0.19	0.88	0.91
N	1851	1851	1851	1851	1849	1849	1849	1849	1851	1851

Robust standard errors in parenthesis, clustered by community. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < .01$, *** $p < .001$

Perceptions of security

Satisfaction with police performance

Table B.18: Average treatment effects on willingness to report police misconduct

	Willingness to report police misconduct idx		Would report illegal checkpoint		Would report drunk officer			Would report police beating
Treatment	0.01	0.01	0.05*	0.04*	-0.00	-0.01	0.03	0.02
	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Covariates	N	Y	N	Y	N	Y	N	Y
Ctrl group mean	2.80	2.80	0.66	0.66	0.49	0.49	0.83	0.83
P-value	0.68	0.81	0.03	0.05	0.84	0.78	0.10	0.16
N	1850	1850	1849	1849	1849	1849	1851	1851

Robust standard errors in parenthesis, clustered by community. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < .01$, *** $p < .001$

B.7 Effects on crime reporting using LNP data

Figure B-2: Effect on crime reporting using LNP data

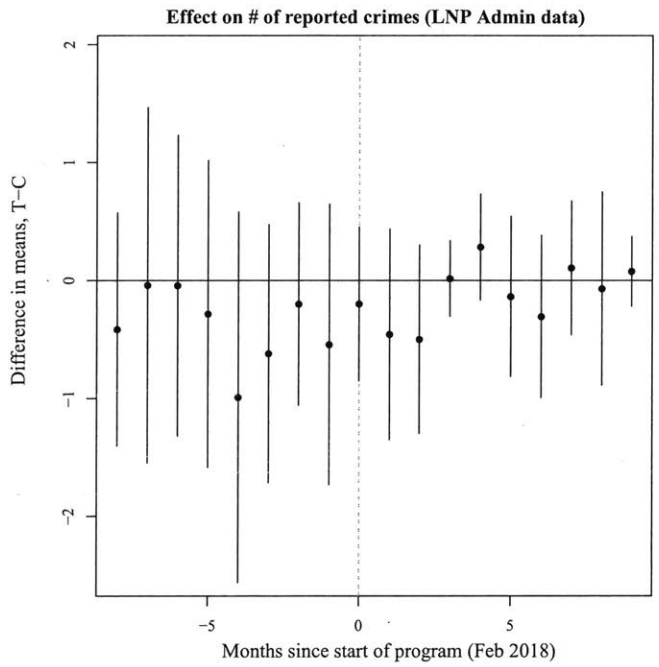


Table B.19: Average treatment effects on crime

	Total # crimes reported		# of armed robberies		# of burglaries		# of aggravated assaults		# of simple assaults	
Treatment	-0.12	-0.31	-0.06	-0.05	-0.01	-0.06	0.04	0.03	-0.02	-0.04
	(0.37)	(0.36)	(0.07)	(0.08)	(0.16)	(0.16)	(0.08)	(0.09)	(0.10)	(0.10)
Covariates	N	Y	N	Y	N	Y	N	Y	N	Y
Ctrl group mean	5.21	5.21	0.44	0.44	1.41	1.41	0.47	0.47	0.67	0.67
P-value	0.75	0.40	0.38	0.52	0.95	0.73	0.67	0.71	0.87	0.68
N	1673	1673	1844	1844	1833	1833	1840	1840	1837	1837

	# of sexual violence		# of domestic violence		# of domestic verbal abuse		# of violent land disputes		# of non violent land disputes	
Treatment	-0.00	-0.00	-0.06	-0.11	-0.36*	-0.41*	0.00	0.01	0.02	0.01
	(0.02)	(0.02)	(0.10)	(0.10)	(0.16)	(0.16)	(0.05)	(0.05)	(0.05)	(0.05)
Covariates	N	Y	N	Y	N	Y	N	Y	N	Y
Ctrl group mean	0.07	0.07	0.67	0.67	1.10	1.10	0.26	0.26	0.19	0.19
P-value	0.85	0.84	0.53	0.27	0.02	0.01	0.92	0.92	0.73	0.83
N	1850	1850	1842	1842	1817	1817	1846	1846	1817	1817

	# of other violent crimes		# of other nonviolent crime		# of murders		# of child abuse	
Treatment	0.01	0.00	-0.06*	-0.06^{+}	0.01	0.01	0.02	0.02
	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.09)	(0.09)
Covariates	N	Y	N	Y	N	Y	N	Y
Ctrl mean	0.02	0.02	0.10	0.10	0.04	0.04	0.31	0.31
P-value	0.74	0.87	0.04	0.06	0.49	0.55	0.80	0.83
N	1803	1803	1818	1818	1851	1851	1838	1838

Robust standard errors in parenthesis, clustered by community. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < .01$, *** $p < .001$

Table B.20: Average treatment effects on perceptions of security

	Perceptions of security idx (std)		Fears violent crime		Fears non-violent crime		Fears walking at night		Fears home invasions	
Treatment	0.10	0.10	-0.03	-0.02	-0.02	-0.01	-0.02	-0.02	0.00	0.00
	(0.08)	(0.07)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Covariates	N	Y	N	Y	N	Y	N	Y	N	Y
Ctrl mean	-0.06	-0.06	0.53	0.53	0.54	0.54	0.36	0.36	0.28	0.28
P-value	0.17	0.18	0.36	0.48	0.55	0.72	0.51	0.48	0.95	0.90
N	1851	1851	1851	1851	1851	1851	1851	1851	1851	1851

	House boundaries secure		House items secure		Motorbike secure outside		Generator secure outside	
Treatment	0.00	0.00	0.03	0.02	0.05*	0.05*	0.04^{+}	0.04^{+}
	(0.04)	(0.04)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
Covariates	N	Y	N	Y	N	Y	N	Y
Ctrl mean	0.58	0.58	0.44	0.44	0.07	0.07	0.06	0.06
P-value	0.97	0.90	0.38	0.44	0.05	0.04	0.09	0.09
N	1851	1851	1851	1851	1851	1851	1851	1851

Robust standard errors in parenthesis, clustered by community. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < .01$, *** $p < .001$

Table B.21: Average treatment effects on satisfaction with police performance

	Satisfaction w/ police performance idx		Trusts police			Satisfied w/ police performance
Treatment	0.05	0.05	0.01	0.01	0.02	0.02
	(0.06)	(0.05)	(0.02)	(0.02)	(0.02)	(0.02)
Covariates	N	Y	N	Y	N	Y
Ctrl group mean	2.23	2.23	0.61	0.61	0.59	0.59
P-value	0.35	0.37	0.62	0.70	0.25	0.38
N	1846	1846	1851	1851	1851	1851

Robust standard errors in parenthesis, clustered by community. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < .01$, *** $p < .001$

B.8 Results for hypotheses included in the pre-analysis plan but excluded from the paper

As part of its participation in the EGAP Metaketa Initiative, this study pre-specified that it would report impacts on trust in government and trust within communities. Results on these outcomes were excluded from the main paper but are included in Table B.22, below.

Table B.22: Average treatment effects on secondary outcomes excluded from main paper

	Trust in government			Trust in other community members
treatment	-0.04	-0.08	-0.06	-0.07
	(0.06)	(0.06)	(0.05)	(0.05)
Ctrl mean	2.87	2.87	2.29	2.29
P-value	0.50	0.18	0.26	0.21
N	1805	1805	1840	1840

Robust standard errors in parenthesis, clustered by community. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < .01$, *** $p < .001$

B.9 Measurement of Mechanisms

B.9.1 Familiarity with the police

I measure *familiarity with the police* using the following nine questions:

1. The police have special officers responsible for dealing with issues of sexual assault and child abuse. True or False?

2. IF YES: What is the name of the unit to which these officers are assigned? [ANSWER: Women and Child Protection Services (WACPS). ENUMERATOR: IS THE RESPONDENT CORRECT?]

3. The police have a special unit responsible for handling citizen complaints about police misconduct or abuse. True or false?

4. What is the name of this unit? [ANSWER: Professional Standards Division. ENUMERATOR: IS THE RESPONDENT CORRECT?]

5. The police have a special unit responsible for investigating crimes and gathering evidence. What is the name of this unit? [ANSWER: Crime services division. ENUMERATOR: IS THE RESPONDENT CORRECT?]

6. Do you know where the nearest police station is? [ENUMERATOR: IS RESPONDENT CORRECT?]

7. Do you know the name of the COMMANDER at the police station that is nearest to you? [ENUMERATOR: IS RESPONDENT CORRECT?]

8. Do you know the name of any police officer at the police station that is nearest to you?

9. Do you know the PHONE NUMBER of any police officer at the police station that is nearest to you?

For each question, I construct a dummy variable indicating whether the respondent answered affirmatively or correctly, as appropriate. Endline responses are standardized by the baseline mean and standard deviation; the composite index *know_pol_idx* takes the average of these nine standardized variables.

B.9.2 Knowledge of the criminal justice system

To measure *knowledge of the criminal justice system*, I ask the following nine questions:

1. *know_law_suspect*: If you see a dead body lying in the street and you report it to the police, Liberian law says the police must hold you as a suspect. True or false?

2. *know_law_lawyer*: If you take your case to court and you don't have money to pay a lawyer, Liberian law says the government must provide a lawyer for you. True or false?

3. *know_law_fees*: If you take a case to the police, Liberian law says the police can charge a fee to register the case. True or false?

4. *know_report_station*: Do you know where the nearest police station is? [ENUMERATOR: IS RESPONDENT CORRECT?]

5. *know_law_statrape*: If a man does man-woman business with a woman under age 18, Liberian law says that is rape, even if the woman consents. True or false?

6. *know_law_childsup*: According to Liberian law, a man does not have to provide for his children if he never married the mother and they are seperated. True or false?

7. *know_law_habeasc*: If the police put someone in jail and no one comes to carry a case against that person, Liberian law says the police have to let him go free. True or false?

8. *know_law_bondfee*: If you take a case to court, Liberian law says the Judge can charge you a fee before he can hear the case. True or False?

9. *know_law_complain*: If you report a serious crime to the police like murder or rape and the police fail to take it seriously or investigate, Liberian law says you have the right to file a complaint against the police. True or False?

Questions 1 - 4 were harmonized across all studies in Blair et al. (2018)'s meta-analysis; Questions 5 - 9 are unique to Liberia and address issues that were to be emphasized in the intervention. The meta-analysis will report results on the harmonized index, *know_idx*, which will be constructed from questions 1 - 4; this study will report results on the more-extensive, Liberia-specific index, *know_idx_lbr*, which will be constructed from all 9 questions. For each question, I construct a dummy variable indicating whether the respondent

answered correctly; endline responses will be standardized by the baseline mean and standard deviation, and each of the composite indices will take the average of the relevant standardized variables.

B.9.3 Perceptions of police intentions

To measure *perceptions of police intentions*, the survey included the following ten questions:

1. *polint_corrupt*: The police are corrupt or eating money. Agree or disagree?
2. *polint_quality*: The police treat all citizens equally. Agree or disagree?
3. *polcaseserious*: Imagine someone is the victim of armed robbery in your community. The police will take the case seriously and investigate. Agree or disagree?
4. *polcasefair*: Imagine someone is the victim of armed robbery in your community. The police will be fair to all sides in the investigation. Agree or disagree?
5. *pol_care*: The police care about the safety and well-being of people in my community. Agree or disagree?
6. *polint_digresp*: Police treat citizens with dignity and respect. Agree or disagree?
7. *polint_decfact*: Police make their decisions based upon facts, not their personal biases or opinions. Agree or disagree?
8. *polcaserespect*: Imagine someone is the victim of armed robbery in your community. The police will treat the victim with dignity and respect. Agree or disagree?

Questions 1 - 4 were harmonized across all studies participating in Blair et al. (2018)'s meta-analysis; Questions 5 - 8 are unique to Liberia. Accordingly, the Meta-analysis reports results on the harmonized index, *intentions_idx*, which is constructed from questions 1 - 4; this study reports results on the more-extensive, Liberia-specific index, *intentions_idx_lbr* , which is constructed from all 8 questions.

Response options range from "strongly agree" to "strongly disagree" including a neutral "neither agree nor disagree" option, in addition to options for "Do not know" and "Refuse to answer." Responses are coded on an ordinal scale ranging from 0 ("strongly disagree")

to 4 ("strongly agree"), with "Do not know" and "Refuse to answer" treated as missing. At endline, these variables are standardized by the baseline mean and standard deviation, and each of the composite indices takes the average of the relevant standardized variables.

B.9.4 Perceptions of police capacity

To measure *perceptions of police intentions*, I ask:

- The police have the ability to respond to incidents of crime in a timely manner. Agree or disagree?
- The police have the ability to investigate crimes and gather evidence effectively and professionally. Agree or disagree?

Response options range from "strongly agree" to "strongly disagree" including a neutral "neither agree nor disagree" option, in addition to options for "Do not know" and "Refuse to answer." Responses are coded on an ordinal scale ranging from 0 ("strongly disagree") to 4 ("strongly agree"), with "Do not know" and "Refuse to answer" treated as missing. At endline, these variables are standardized by the baseline mean and standard deviation, and the composite index, *police_capacity_idx*, takes the average of the standardized variables.

B.9.5 Perceptions of police responsiveness

To measure *perceptions of police responsiveness*, I ask:

1. *responsive_listen*: The police give people in my community a chance to express their views before making decisions. Agree or disagree?
2. *responsive_act*: The police act upon citizen comments and complaints about security in my community. Agree or disagree?

Question 1 was harmonized across all studies in Blair et al. (2018)'s meta-analysis; Questions 2 was unique to Liberia. The meta-analysis reports results on *responsive_act* alone, while this study reports results on the more-extensive, Liberia-specific index, *pol_responsiveness_lbr* , which is constructed from both questions.

Response options range from "strongly agree" to "strongly disagree" including a neutral "neither agree nor disagree" option, in addition to options for "Do not know" and "Refuse to answer." Responses are coded on an ordinal scale ranging from 0 ("strongly disagree") to 4 ("strongly agree"), with "Do not know" and "Refuse to answer" treated as missing. At endline, these variables are standardized by the baseline mean and standard deviation, and the *pol_responsiveness_lbr* index takes the average of the relevant standardized variables.

B.9.6 Norms of citizen cooperation with police

To measure *norms of citizen cooperation with the police*, I ask:

1. *reportnorm_theft*: If there is a BURGLARY in your community, people can get angry if you take it to the police. Agree or disagree?
2. *reportnorm_abuse*: If a MAN BEATS HIS WIFE in your community, people can get angry if you take it to the police. Agree or disagree?
3. *obeynorm*: You should do what the police tell you to do even when you do not understand the reasons for their decisions. Agree or disagree?
4. *reportnorm_land*: If there is a LAND DISPUTE in your community, people can get angry if you take it to the police. Agree or disagree?
5. *helppolnorm_armedrob*: If a member of the community provides the police with information that helped catch the perpetrator of the [ARMED ROBBERY], other people can get angry with him. Agree or disagree?
6. *helppolnorm_domviol*: If a member of the community provides the police with information that helped catch the perpetrator of the [DOMESTIC VIOLENCE], other people can get angry with him. Agree or disagree?
7. *helppolnorm_moto*: If a member of the community provides the police with information that helped catch the perpetrator of the [MOTORBIKE THEFT], other people can get angry with him. Agree or disagree?
8. *helppolnorm_childabuse*: If a member of the community provides the police with information that helped catch the perpetrator of [CHILDABUSE], other people can get angry with him. Agree or disagree?

Questions 1 - 3 were harmonized across all studies in Blair et al. (2018)'s meta-analysis; Questions 4 - 8 are unique to Liberia. The meta-analysis reports results on the harmonized

index, *norm_idx*, which will be constructed from questions 1 - 3; this study reports results on the more-extensive, Liberia-specific index, *norm_idx_lbr*, which is constructed from all 8 questions.

Response options range from "strongly agree" to "strongly disagree" including a neutral "neither agree nor disagree" option, in addition to options for "Do not know" and "Refuse to answer." Responses are coded on an ordinal scale ranging from 0 ("strongly disagree") to 4 ("strongly agree"), with "Do not know" and "Refuse to answer" treated as missing. At endline, these variables are standardized by the baseline mean and standard deviation, and each of the composite indices takes the average of the relevant standardized variables.

B.9.7 Support for mob violence

To measure *support for mob violence*, we ask respondents to judge whether mob violence would be justified in the following three scenarios:

1. Let's say somebody bust into a lady's house and tries to rape her in the night. As he is running away, the community people catch the man and say they want to flog him rather than carry him to the police because they know the police do not have enough manpower to investigate and prepare the case for court. Would you say the actions of the community people are justified, somewhat justified, or not at all justified?

2. Let's say somebody bust into a man's house with a gun, terrorizes his family, and runs away with a his TV and generator. As he is running away, the community people catch the man and want to flog him rather than carry him to the police because they know the police will just release him. Would you say the actions of the community people are justified, somewhat justified, or not at all justified?

3. Let's say somebody busts into another man's home and steals a bag of rice. As he runs away, the community people catch the man and want to flog him rather than carry him to the police because they know the police will not take the case seriously. Would you say the actions of the community people are justified, somewhat justified, or not at all justified?

Response options include "justified", "somewhat justified", and "not at all justified", in addition to options for "Do not know" and "Refuse to answer." Responses will be coded on an ordinal scale ranging from 0 ("Not at all justified ") to 2 ("Justified"), with "Do

not know" and "Refuse to answer" coded as missing. At endline, these variables are standardized by the baseline mean and standard deviation, and the composite indices take the average of the three standardized variables.

B.9.8 Knowledge of rules governing local security groups

Knowledge of rules governing local security groups is measured by the following seven questions:

1. *know_cwt_arrest*: Members of the Community Watch Forum may arrest someone provided they carry them to the police and do not physically harm them. True or false?

2. *know_cwt_risk*: If members of the Watch Forum encounter a gang of violent criminals, they are required to engage the criminals even if it puts them at risk. True or false?

3. *know_cwt_checkpoint*: Members of the Community Watch Forum may put up checkpoints if they deem in necessary for security. True or False?

4. *know_cwt_jurisdiction*: Members of the Community Watch Forum may patrol in communities other than their own if they think it is necessary. True or false?

5. *know_cwt_violent*: If a VIOLENT crime is reported to the Community Watch Forum, they are required by law to report it to the police. True or False?

6. *know_cwt_beat*: If a criminal is resisting arrest, the Community Watch Forum has the right to flog him until he can no longer resist. True or False?

7. *know_cwt_cutlass*: Members of the Community Watch Forum have the right to carry cutlasses for protection at night. True or false?

For each question, a dummy variable indicating whether the respondent answered correctly is constructed. Endline responses are standardized by the baseline mean and standard deviation, and the composite index, *know_cwt_idx* takes the average of these seven standardized variables.

B.9.9 Coproduction of security

The degree to which residents *contribute to the coproduction of security* is measured using the following eight questions:

1. In the past month, have you seen or heard of members of your commnunity organizing a security meeting?
2. In the past month, have you attended any security meetings organized by members of your community?
3. In the past month, have you seen or heard of members of your community conducting security patrols at night?
4. In the past month, have you or a member of your family participated in a security patrol organized by members of your community?
5. In the past month, have you or a member of your family donated money, tea, bread, or any other thing to help members of your community conduct security patrols?
6. Does your community currently have an community watch team or community watch forum?
7. (IF YES): Does the watch team/forum conduct nighttime patrols on a regular basis?
8. (IF YES): Does the watch team/forum organize community meetings on a regular basis?

For each question, an indicator variable taking a value of one if the respondent answered affirmatively and zero otherwise is constructed. Endline responses are standardized by the baseline mean and standard deviation, and the composite index, *ca_sec_idx*, takes the average of these eight variables.

B.9.10 Police presence

Police presence is measured using three questions:

1. *compliance_patrol*: About how often do you see police officers patrolling your area on FOOT?
2. *compliance_freq*: About how often do you see police officers patrolling your area while in a vehicle or on a motorbike?

3. *compliance_meeting*: In the past 6 months, have you HEARD ABOUT, SEEN, OR ATTENDED community meetings with police officers in your area?

Responses for each question are standardized by the baseline mean and standard deviation and used to construct *compliance_idx*, which takes the mean of these three standardized variables.

B.10 Measurement of Primary Outcomes

B.10.1 Incidence of crime

The survey asks the following questions about crime:

1. *armedrob_any*: In the past 6 months, were you or anyone in your family the victim of any ARMED ROBBERY? [ROBBERY WITH ANY KIND OF WEAPON, INCLUDING GUNS, CUTLASSES, STICKS, ETC.]

2. *burglary_any*: In the past 6 months, besides any armed robbery, were you or anyone in your family the victim of BURGLARY or THEFT OF PROPERY? [ROBBERY WITHOUT WEAPON]

3. *aggassault_any*: In the past 6 months, has anyone attacked you or a member of your household WITH A WEAPON? [INCLUDING GUNS, CUTLASSES, STICKS, ETC.]

4. *simpleassault_any*: In the past 6 months, has anyone attacked you or a member of your household WITHOUT a weapon?

5. *sexual_any*: In the past 6 months, have you or anyone in your household been forced or coerced to engage in unwanted sexual activity?

6. *domestic_phys_any*: Apart from what you have told me, in the past 6 months, has anyone in your household ever PHYSICALLY ABUSED you or another person in your household? [INCLUDING PUSHING, SLAPPING, PUNCHING, KICKING, CHOKING, ETC.]

7. *domestic_verbal_any*: Besides any physical abuse, in the past 6 months, has anyone in your household ever been VERBALLY ABUSED by another person in this household? [INCLUDING SHOUTING, CUSSING, THREATS OF ABUSE, ETC.]

8. *land_viol_any*: In the past 6 months, did you or a member of your household have a dispute over land or property that involved THREATS OR VIOLENCE? This include disputes that are still ongoing up to now.

9. *land_nviol_any*: In the past 6 months, did you or a member of your household have a dispute over land or property that did not involve threats or violence? This include disputes that are still ongoing up to now.

10. *other_any*: In the past 6 months, have you or a member of your household been a victim of any OTHER CRIME that we haven't mentioned already?

11. *carmedrob_any*: In the past 6 months, was anyone you know in this community a victim of ARMED ROBBERY? [ROBBERY WITH ANY KIND OF WEAPON, INCLUDING GUNS, CUTLASSES, STICKS, ETC.]

12. *cburglary_any*: In the 6 months, was anyone you know in this community a victim of BURGLARY or THEFT? [ROBBERY WITHOUT WEAPON]

13. *caggassault_any*: In the past 6 months, was anyone you know in this community attacked WITH A WEAPON? [INCLUDING GUNS, CUTLASSES, STICKS, ETC.]

14. *csimpleassault_any*: In the past 6 months, was anyone you know in this community attacked WITHOUT a weapon? [SIMPE ASSAULT]

15. *csexual_any*: In the past 6 months, was anyone you know in this community SEXUALLY ABUSED? [INCLUDING RAPE]

16. *cdomestic_phys_any*: In the 6 months, was anyone you know in this community been PHYSICALLY ABUSED by someone in their own household? [INCLUDING PUSHING, SLAPPING, PUNCHING, KICKING, CHOKING, ETC.]

17. *cdomestic_verbal_any*: In the past 6 months, was anyone you know in this community VERBALLY ABUSED by someone in their own household? [INCLUDING SHOUTING, CUSSING, THREATS OF ABUSE, ETC.]

18. *cland_viol_any*: In the past 6 months, did anyone you know in this community have a LAND DISPUTE over their house land or farm land involving THREATS OR VIOLENCE? This includes disputes that started in the past and have been resovled or ones that are still ongoing up to now.

19. *cland_nviol_any*: In the past 6 months, did anyone you know in this community have a LAND DISPUTE over their house land or farm land that did not involve threats or violence? This includes disputes that started in the past and have been resovled or ones that are still ongoing up to now.

20. *cmurder_any*: In the past 6 months, was anyone you know in this community MURDERED?

21. *cchildabuse_any*: In the past 6 months, was any CHILD you know of in your community a victim of CHILD ABUSE? [INCLUDING LACK OF FOOD, LACK OF SUPPORT, HITTING, BEATING, CHILD LABOR, OR SEXUAL ABUSE?]

22. *cother_any*: In the past 6 months, was anyone you know in your community a victim of any OTHER CRIME that we haven't mentioned already?

For each affirmative response, a follow-up question asks "How many times did this happen in the past 6 months?" Questions 3, 5, 6, 7, 8, 9, 18, 19, and 21 were only asked in Liberia; the remaining questions were asked in Liberia as well as in the other studies participating in Blair et al. (2018)'s meta-analysis.

For the meta-analysis, *violentcrime_num*, denoting the total number of violent crimes a respondent reports occurred in their community in the past 6 months, is constructed from questions on armed robbery, simple assault, aggravated assault, sexual assault, murder, domestic violence, and violent crimes in the "other" category. Questions on burglary and non-violent crimes in the "other" category are used to construct *nonviolentcrime_num*, denoting the total number of non-violent crimes a respondent reports occurred in their community in the past 6 months. Both of these indices are used to construct *crime_num*, the composite index for this cluster, denoting the total number of crimes a respondent reports occurred in their community in the past 6 months.

For this study, I report effects on *crime_num_lbr*, an index denoting the total number of crimes reported across all categories measured in Liberia. In addition, I report effects on each of the following individual categories of crime: burglary, armed robbery, simple assault, aggravated assault, sexual violence, domestic violence, domestic abuse, and land conflict (violent). For each category, a continuous variable is constructed as the sum of i) crimes against the respondent or their family members and ii) crimes that occurred to other people the respondent knows in their community.

B.10.2 Crime reporting

For each affirmative response to the crime questions listed below, a follow-up question asks (with respect to the most recent time the crime occurred): "Where did you report this case?" with answer options: nowhere, police, courts, town leader, elders, commu-

nity watch group/forum, settled directly with perpetrator, and other.[1] Following the meta-analysis PAP, these questions are used to construct *crime_report_num*, the total number of crimes reported to the police; *violentcrime_report_num*, the total number of violent crimes reported to the police; and *nonviolentcrime_report_num*, the total number of non-violent crimes reported to the police.

The survey also asks the following questions about *support for crime reporting* in hypothetical scenarios:

1. *burglaryres*: If there's a BURGLARY in your community, where do you think the case should be reported, if anywhere?
2. *dviolres*: If a MAN BEAT HIS WOMAN in your community, where do you think the case should be reported, if anywhere?
3. *armedrobres*: If there's an ARMED ROBBERY in your community, where do you think the case should be reported, if anywhere?

with response options: nowhere, police, courts, town leader, elders, community watch group/forum, and other. For each scenario, I code a dummy variable that takes a value of one for "police" responses and zero otherwise. The index *crimeres_idx* takes the average of these five variables. The primary outcome index for crime reporting, *crime_reporting_idx*, is constructed as the average of: *crime_report_num* and *crimeres_idx*.

As discussed in Section 3.5.4 of the paper, I complement the meta-analysis approach to analyzing effects on crime reporting with a crime-level analysis of crime reporting, as outlined in Section 3.5.4. In particular, for each affirmative response to the 22 categories of crime listed above, I construct an indicator variable for whether or not the crime was reported to the police or courts, using Equation 3.3 to compare rates of crime reporting in treatment versus control communities for i) all crimes, ii) felonies,[2] and iii) misdemeanors.[3]

[1]For questions about crimes against other people they know in their community, the crime reporting question is phrased "As far as you know, where was this case reported?"

[2]Following Liberia's penal code, I classify the following as felonies: armed robbery, aggravated assault, sexual assault, domestic violence, murder, violent land disputes, and incidents of child abuse involving violence.

[3]Following Liberia's penal code, I classify the following as misdemeanors: burglary/petty theft, simple assault, domestic abuse (verbal), and incidents of child abuse not involving violence.

Administrative crime data

Administrative data from the LNP is used to construct the following variables:

1. *armedrob_num* : Total number of armed robberies in community in past 6 months
2. *aburglary_num* : Total number of burglaries in community in past 6 months
3. *aaggassault_num* : Total number of aggravated assaults in community in past 6 months
4. *asimpleassault_num* : Total number of simple assaults in community in past 6 months
5. *asexual_num* : Total number of sexual violence incidents in community in past 6 months
6. *adomestic_phys_num* : Total number of domestic violence incidents in community in past 6 months
7. *adomestic_verbal_num* : Total number of verbal domestic violence incidents in community in past 6 months
8. *aland_num* : Total number of armed robberies in land conflicts in community in past 6 months
9. *aland_violent_num* : Total number of violent land conflicts in community in past 6 months
10. *amob_num* : Total number of mob violence incidents community in past 6 months
11. *ariot_num* : Total number of riots in community in past 6 months
12. *amurder_num* : Total number of murders in community in past 6 months
13. *aother_any* : Total number of other crimes in community in past 6 months

Primary outcome indices *acrime_num* and *aviolentcrime_num* are constructed from these variables.

B.10.3 Crime tips and information sharing

I measure *information sharing* with four questions:

1. *contact_pol_susp_activity*: In the past 12 months, have you ever contacted the police to alert them to suspicious or criminal activity in your community?

2. *give_info_pol_investigation*: In the past 12 months, have you ever given information to the police to assist with an investigation?

3. *contact_pol_find_suspect*: In the past 12 months, have you ever contacted the police to help them find a suspected criminal in your community?

4. *testify_police_investigation*: In the past 12 months, have you ever served as a witness or provided testimony as part of a police investigation?

5. *name_witness*: Suppose there was a robbery in your community. You didn't see the crime occur, but you know someone in your community who did. How likely would you be to give that person's name to the police?

6. *identify_hideout*: Suppose there is a suspect accused of car jacking hiding in your community and the police are looking for him. Let's say you happen to know where that person is hiding. How likely would you be to give that information to the police?

7. *identify_ghetto*: Suppose there is an area of your community where people take drugs and plan petty crimes. How likely would you be to report that information to the police?

8. *guide_police*: Suppose a police officer wants to familiarize himself with your community. How willing would you be to spend a day showing him around your community?

9. *give_testimony*: Suppose you witness a crime in your community and the police ask you to give written testimony at the police station. How likely would you be to go to the station to give written testimony?

Questions 1 - 2 were harmonized across all studies participating in Blair et al. (2018)'s meta-analysis; Questions 3 - 9 are unique to Liberia. The meta-analysis reports results on the harmonized index, *tips_idx*, which is constructed from questions 1 - 2; this study reports results on the more-extensive, Liberia-specific index, *tips_idx_lbr* , which is constructed from all nine questions, with each input variable standardized by the baseline mean and standard deviation.

B.10.4 Willingness to report police abuse

Willingness to report police abuse is measured using three questions:

1. *checkpoint_report*: Suppose you see a group of police officers running an illegal checkpoint just to take money from motorists. How likely would you be to report that situation?

2. *dutydrink_report*: Suppose you see a uniformed police officer drinking alcohol in your community. How likely would you be to report that situation?

3. *policebeating_report*: Suppose you see a group of officers beating someone in your community. How likely would you be to report that situation?

Questions 1 - 2 were included in all studies participating in Blair et al. (2018)'s meta-analysis; Question 3 is unique to Liberia. The Meta-analysis reports results on the harmonized index, *police_abuse_report_idx*, which is constructed from Questions 1 - 2; this study reports results on the more-extensive, Liberia-specific index, *police_abuse_report_idx_lbr*, which is constructed from all three questions, with each input variable standardized by the baseline mean and standard deviation.

B.10.5 Perceptions of security

To measure *perceptions of security*, I ask:

1. *fear_violent*: How worried are you that you or a member of your household will be the victim of a VIOLENT CRIME in the coming year? [INCLUDING ARMED ROBBERY, ASSAULT WITH A WEAPON, ASSAULT WITHOUT A WEAPON, ETC.]

2. *fear_nonviolent*: How worried are you that you or a member of your household will be the victim of a NON-VIOLENT CRIME in the coming year? [INCLUDING BURGLARY, THEFT, ETC.]

3. *feared_walk*: In the past 6 months, how often, if ever, have you or anyone in your family felt unsafe walking in your neighbourhood?

4. *feared_home*: In the past 6 months, how often, if ever, have you or anyone in your family feared crime in your own home?

5. *hssecure*: How sure are you that the boundaries of your house spots are secure? That is, no one can leave from his side to come and sit down on your side?

6. *hsitemssecure*: How sure are you that the valuable items in and around your house are secure? (e.g. generators, phones, computers, TVs, furniture)

Questions 1 - 3 were included in all studies in Blair et al. (2018)'s meta-analysis; Questions 4-6 were unique to Liberia. The meta-analysis reports results on the harmonized index, *future_security_idx*, which is constructed from Questions 1 - 3; this study reports results on the more-extensive, Liberia-specific index, *future_security_idx_lbr*, which is constructed from all six questions, with each input variable standardized by the baseline mean and standard deviation.

B.10.6 Satisfaction with police performance

To measure *satisfaction with police performance*, I ask:

1. *satis_trust*: In general, I trust the police. Agree or disagree?
2. *satis_general*: I am satisfied with the service that police provide. Agree or disagree?

Response options range from "strongly agree" to "strongly disagree" including a neutral "neither agree nor disagree" option, in addition to options for "Do not know" and "Refuse to answer." Responses are coded on an ordinal scale ranging from 0 ("strongly disagree") to 4 ("strongly agree"), with "Do not know" and "Refuse to answer" treated as missing. At endline, these variables are standardized by the baseline mean and standard deviation; the composite index *satis_idx* takes the average of these two standardized variables.

B.11 Measurement of Secondary Outcomes

B.11.1 Trust in government

To measure *Trust in government* (S1) I ask: "Currently, how much do you trust the government?", with response options: "Not at all", "2-Just a little", "Somewhat", "A lot", "Do

not know", and “Refuse to answer". Responses to these questions are standardized by the baseline mean and standard deviation.

B.11.2 Communal trust

To measure *communual trust* (S2), I ask:

1. *trust_community*: Most people in my community can be trusted. Agree or disagree?
2. *days_comm_work*: In the past 30 days, how many days did you spend doing community work, like cleaning dirt or brushing the road?
3. *cgroups*: How many community groups that meet regularly do you participate in?
4. *trust_keys*: Would you be willing to leave one of your neighbors the keys to your home while you went away for the afternoon?
5. *comm_help*: Most members of [COMMUNITY NAME] are willing to help me when I'm in need. Agree or disagree?

Questions 1 was included in all studies participating in the meta-analysis; Questions 2-5 were unique to Liberia. The Meta-analysis reports results on the harmonized index, *trust_community*; this study reports results on a more-extensive, Liberia-specific index, *comm_cohesion*, which is constructed from all five questions, with each input variable standardized by the baseline mean and standard deviation.

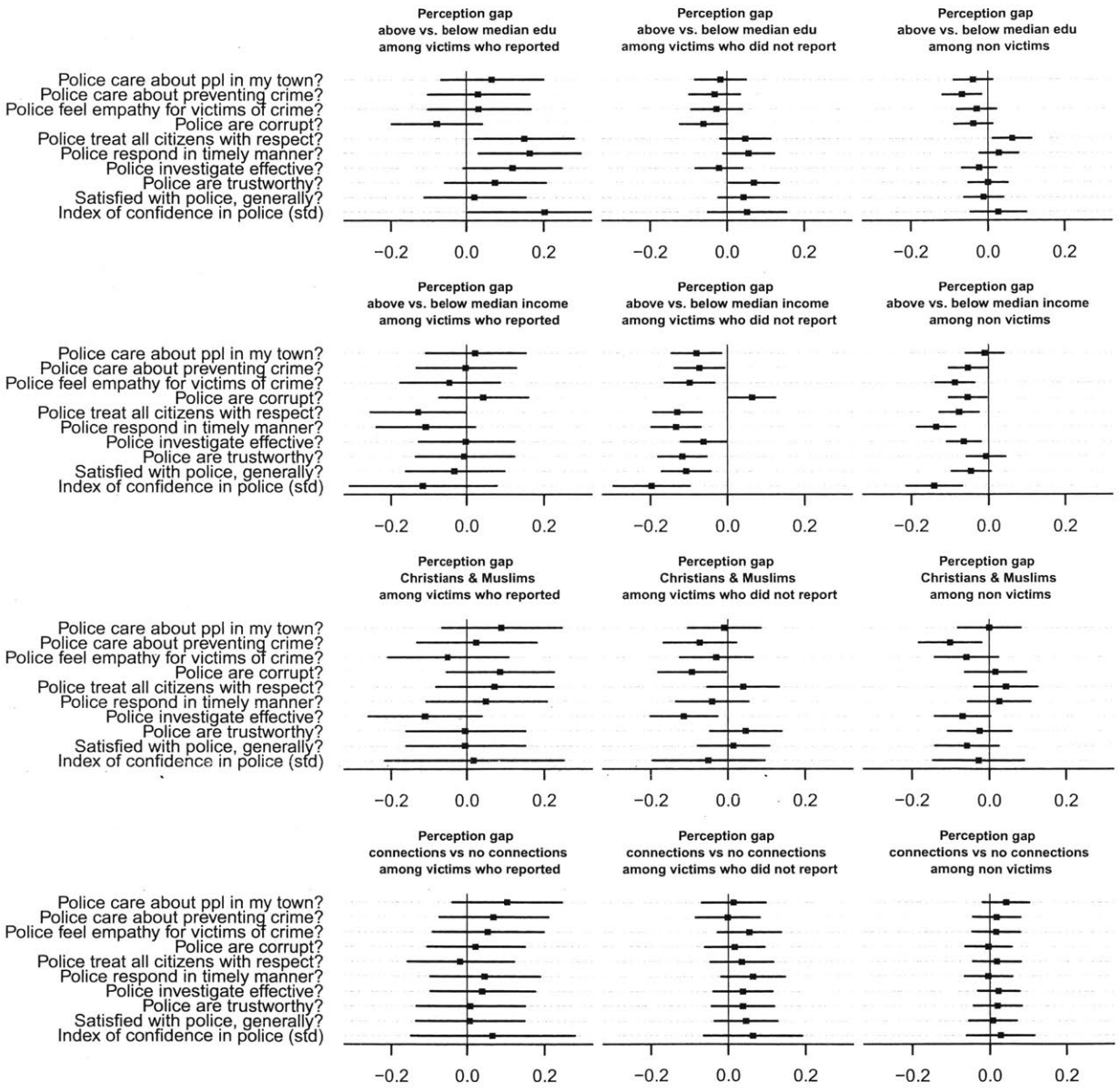
Appendix C

Appendix for: Patrimonial Policing: Police Bias and Access to Justice in Liberia

C.1 Appendix 1 Tests of social desireability bias

Figure C-1 displays differences in respondents' perceptions of the police in general, outside the context of their personal experience when reporting a crime, as a test of whether certain types of individuals are more likely to offer socially desirable responses, whether when asked questions about reported crimes or the police in general. To ensure this placebo analysis is not itself confounded by post-treatment bias among those reporting crimes to the police, Figure C-1 displays differences in perceptions among those who were never a victim of a crime (N=1,142) as well as those who were a victim of crime but did not report it to the police (N=724), in addition to differences among those who were a victim of a crime that was reported (N=214). The results offer little evidence to suggest social desireability bias influences the results. Across all three subgroups, differences in perceptions between Muslims and Christians, individuals with and without political connections, and individuals with above versus below median education are small and statistically significant. Although individuals with below median income are less likely to report favorable views of the police, this suggests that if anything, the null effects of low income on police performance are downward rather than upward biased.

Figure C-1: Differences in perceptions of police by religion, income, education, and political-connections



Notes: Raw differences in perceptions of police by religion, income, education, and political-connections.

C.2 Appendix 2 Comparison of politically connected individuals to those without connections

Table C.1 provides a comparison of politically connected individuals to those without connections.

Table C.1: Comparison of politically connected individuals to those without connections

	(1) Related to govt official?
Below median wealth	-0.02
	[0.02]
Below median education	0.03
	[0.02]
Below median income	-0.04
	[0.02]
Muslim	-0.07
	[0.02]
Female	-0.08**
	[0.03]
Age	-0.00*
	[0.00]
Voted in 2011/2017	0.04
	[0.04]
Voted for incumbent in 2011/2017	0.01
	[0.02]
Bassa	-0.16*
	[0.07]
Gio	-0.12
	[0.07]
Kissi	0.03
	[0.11]
Kpelle	-0.07
	[0.08]
Kru	0.07
	[0.07]
Lorma	0.05
	[0.11]
Mandingo	0.03
	[0.11]
Vai	-0.01
	[0.09]
Constant	0.29***
	[0.08]
Observations	2546

Standard errors in brackets, clustered by community

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

C.3 Appendix 3 Vignettes used in the conjoint experiment

Part 1: Gender, Religion, Crime

Theft of phone

Let's say that during the rainy season, a [gender] named [Christian / Muslim name] had [ppronoun] phone stolen near [ppronoun] home in [Muslim / Christian town name]. After the incident, a neighbor came to [name] with information about who may have committed the crime and recommended [pronoun] report the case to the police for further investigation.

Burglary

Let's say that during the rainy season, a [vig_gender] named [Christian / Muslim name] had someone break into [vig_ppronoun] house in [Muslim / Christian town name] while [vig_pronoun] wasn't there and stole all of [vig_ppronoun] furniture and valuables. After the incident, a neighbor came to [name] with information

Burglary & Aggravated assault

Let's say that during the rainy season, a [vig_gender] named [Christian / Muslim name] was assaulted with a cutlass at night near [vig_ppronoun] home in [Muslim / Christian town name]. After the incident, a neighbor came to [name] with information about who may have committed the crime and recommended [vig_pronoun] report the case to the police for further investigation.

Part 2: Wealth, Literacy, Political Connections

Poor, Illiterate, Unconnected

At first, [vig_name] was scared to take the case to the police because [vig_pronoun] is poor. [vig_pronoun] also is not a book person and doesn't know the law, and [vig_pronoun] just moved to Monrovia and doesn't know any big big people in government who can give [vig_ppronoun] advice. However, eventually [vig_pronoun] put [vig_ppronoun] worries aside and took time off from [vig_ppronoun] petty business to report the [vig_crime] to the police.

Poor, Illiterate, Connected

At first, [vig_name] was scared to take the case to the police because [vig_pronoun] is poor. [vig_pronoun] also is [vig_pronoun] is not a book person and doesn't

know the law. However, after talking to [vig_ppronoun] uncle, who is a bigman in the government, [vig_pronoun] decided it was best to take the case to the police. Shortly thereafter, [vig_name] took time off from [vig_ppronoun] petty business and reported the [vig_crime] to the police.

Poor, Literate, Connected

At first, [vig_name] was scared to take the case to the police because [vig_pronoun] is poor. Even though [vig_pronoun] can read and write and knows the law good good, [vig_pronoun] was still scared because [vig_pronoun] was poor. However, after talking to [vig_ppronoun] uncle, who is a bigman in the government, [vig_pronoun] decided it was best to take the case to the police anyway. Shortly thereafter, [vig_name] took time off from [vig_ppronoun] petty business to report the [vig_crime] to the police.

Poor, Literate, Unconnected

At first, [vig_name] was scared to take the case to the police because [vig_pronoun] is poor. [vig_pronoun] can read and write and knows the law good good, but [vig_pronoun] just moved to and doesn't know any big big people in government who can give [vig_ppronoun] advice. However, eventually [vig_pronoun] put [vig_ppronoun] worries aside and took time off from [vig_ppronoun] petty business to report the [vig_crime] to the police.

Rich, Illiterate, Unconnected

At first, [vig_name] was scared to take the case to the police. Even though [vig_pronoun] has plenty money from [vig_ppronoun] NGO job, [vig_pronoun] is not a book person and doesn't know the law, and [vig_pronoun] just moved to Monrovia and doesn't know any big big people in government who can give [vig_ppronoun] advice. However, eventually [vig_name] put [vig_ppronoun] worries aside and took time off from [vig_ppronoun] NGO job to report the [vig_crime] to the police.

Rich, Illiterate, Connected

At first, [vig_name] was scared to take the case to the police. Even though [vig_pronoun] has plenty money from [vig_ppronoun] NGO job, [vig_pronoun] is not a book person and doesn't know the law. However, after talking to [vig_ppronoun] uncle, who is a bigman in the government, [vig_pronoun] decided it was best to take the case to the police. Shortly thereafter, [vig_name] took time off from [vig_ppronoun] NGO job and reported the [vig_crime] to the police.

Rich, Literate, Connected

At first, [vig_name] was scared to take the case to the police. Even though [vig_pronoun] has plenty money from [vig_ppronoun] NGO job and [vig_pronoun] can read and write and knows the law good good, [vig_pronoun] was still

scared. However, after talking to [vig_ppronoun] uncle, who is a bigman in the government, [vig_pronoun] decided it was best to take the case to the police. Shortly thereafter, [vig_name] took time off from [vig_ppronoun] NGO job and reported the [vig_crime] to the police.

Rich, Literate, Unconnected

At first, [vig_name] was scared to take the case to the police. Even though [vig_pronoun] has plenty money from [vig_ppronoun] NGO job and [vig_pronoun] can read and write and knows the law good good, [vig_pronoun] just moved to Monrovia and doesn't know any big big people in government who can give [vig_ppronoun] advice. However, [vig_name] eventually put [vig_ppronoun] worries aside and took time off from [vig_ppronoun] NGO job to report the [vig_crime] to the police.

Bibliography

Abrahams, R. G. (1998). *Vigilant Citizens: vigilantism and the state*. Polity Press.

Aldashev, G., I. Chaara, J.-P. Platteau, and Z. Wahhaj (2012). Formal Law as a Magnet to Reform Custom. *Economic Development and Cultural Change 60*(4), 795–828.

Allport, G. W., K. Clark, and T. Pettigrew (1954). The nature of prejudice.

Andrews, M., M. Woolcock, and L. Pritchett (2017). *Building state capability: Evidence, analysis, action*. Oxford University Press.

Ansell, B. and J. Lindvall (2013). The political origins of primary education systems: Ideology, institutions, and interdenominational conflict in an era of nation-building. *American Political Science Review 107*(3), 505–522.

Bacon, L. (2015). Liberia's gender-sensitive police reform: Improving representation and responsiveness in a post-conflict setting. *International Peacekeeping 22*(4), 372–397.

Baker, B. (2009). *Security in post-conflict Africa: the role of nonstate policing*. CRC Press.

Baker, B. (2010). Resource constraint and policy in liberia's post-conflict policing. *Police Practice and Research: An International Journal 11*(3), 184–196.

Baker, B. and E. Scheye (2007). Multi-layered justice and security delivery in post-conflict and fragile states: Analysis. *Conflict, Security & Development 7*(4), 503–528.

Baldwin, K. (2015). *The Paradox of Traditional Chiefs in Democratic Africa*. New York: Cambridge University Press.

Baldwin, K. (2016). *The paradox of traditional chiefs in democratic Africa*. Cambridge University Press.

Banerjee, A., R. Hanna, J. Kyle, B. A. Olken, and S. Sumarto (2018). Tangible information and citizen empowerment: Identification cards and food subsidy programs in indonesia. *Journal of Political Economy 126*(2), 451–491.

Banerjee, A., S. Mullainathan, and R. Hanna (2013). Corruption. In R. Gibbons and J. Roberts (Eds.), *The Handbook of Organizational Economics*, pp. 1109–1147.

Banerjee, A. V., R. Chattopadhyay, E. Duflo, D. Keniston, and N. Singh (2014). Improving police performance in rajasthan, india: Experimental evidence on incentives, managerial autonomy and training. NBER Working Paper No. 17912.

Baranyi, S., P. Beaudet, and U. Locher (2011). World development report 2011: conflict, security, and development.

Bardhan, P. and D. Mookherjee (2006). Decentralization, corruption, and government accountability. *International handbook on the economics of corruption 6*, 161–188.

Bartels, L. M. (2002). Beyond the running tally: Partisan bias in political perceptions. *Political behavior 24*(2), 117–150.

Becker, G. S. (1957). *The economics of discrimination.* University of Chicago press.

Beleky, L. P. (1973). The development of liberia. *The Journal of Modern African Studies 11*(01), 43–60.

Benjamini, Y. and Y. Hochberg (1995). Controlling the false discovery rate: A practical and powerful approach to multiple testing. *Journal of the Royal Statistical Society. Series B (Methodological)*, 289–300.

Bird, K. (2014). Ethnic quotas and ethnic representation worldwide. *International Political Science Review 35*(1), 12–26.

Blair, G., F. Christia, C. Samii, J. Weinstein, E. Badran, R. Blair, A. Cheema, T. Fetzer, G. Grossman, D. Haim, R. Hanson, A. Hasanain, B. Kachero, D. Kronick, J. Shapiro, B. Morse, R. Muggah, B. Silva, M. Nanes, S. Pedro, L. Tsai, and S. Hyde (2018). Meta-analysis pre-analysis plan: Community policing metaketa project.

Blair, R., S. Karim, M. J. Gilligan, and K. C. Beardsley (2018). Policing Ethnicity: Lab-in-the-Field Evidence on Discrimination, Cooperation and Ethnic Balancing in the Liberian National Police. SSRN Scholarly Paper 2772634.

Blair, R., S. Karim, and B. Morse (2016). Building trust in a reformed security sector: A field experiment in liberia.

Blair, R., S. Karim, and B. Morse (2019). Establishing the rule of law in weak and war-torn states: Evidence from a field experiment with the liberian national police. *American Political Science Review.*

Blair, R. A. (2018a). International intervention and the rule of law after civil war: Evidence from liberia. *International Organization Forthcoming.*

Blair, R. A. (2018b). Legitimacy after violence: Evidence from two lab-in-the-field experiments in liberia. SSRN Scholarly Paper 2326671.

Blair, R. A., B. S. Morse, and L. L. Tsai (2017). Public health and public trust: Survey evidence from the Ebola Virus Disease epidemic in Liberia. *Social Science & Medicine 172*, 89–97.

Blattman, C., A. C. Hartman, and R. A. Blair (2014). How to Promote Order and Property Rights under Weak Rule of Law? An Experiment in Changing Dispute Resolution Behavior through Community Education. *American Political Science Review 108*, 100–120.

Boege, V., M. A. Brown, and K. P. Clements (2009). Hybrid political orders, not fragile states. *Peace Review 21*(1), 13–21.

Bowers, K. J., S. D. Johnson, R. T. Guerette, L. Summers, and S. Poynton (2011). Spatial displacement and diffusion of benefits among geographically focused policing initiatives: A meta-analytical review. *Journal of Experimental Criminology 7*(4), 347–374.

Braga, A. A. (2008). *Problem-Oriented Policing and Crime Prevention* (2nd ed.). New York: Willow Tree.

Braga, A. A., B. C. Welsh, A. V. Papachristos, C. Schnell, and L. Grossman (2014). The growth of randomized experiments in policing: The vital few and the salience of mentoring. *Journal of Experimental Criminology 10*(1), 1–28.

Bratton, M. and N. Van de Walle (1994). Neopatrimonial regimes and political transitions in africa. *World politics 46*(4), 453–489.

Brunson, R. K. (2007). Police don't like black people?: African-american young men's accumulated police experiences. *Criminology & Public Policy 6*(1), 71–101.

Bryden, A., B. N'Diaye, and F. Olonisakin (Eds.) (2008). *Challenges of Security Sector Governance in West Africa*. Zurich: Lit Verlag and Geneva Centre for the Democratic Control of Armed Forces (DCAF).

Bullock, J. G. (2009). Partisan bias and the bayesian ideal in the study of public opinion. *The Journal of Politics 71*(03), 1109–1124.

Bullock, J. G., A. S. Gerber, S. J. Hill, and G. A. Huber (2015). Partisan bias in factual beliefs about politics. *Quarterly Journal of Political Science 10*, 519–578.

Butler, D. M. and D. E. Broockman (2011). Do politicians racially discriminate against constituents? a field experiment on state legislators. *American Journal of Political Science 55*(3), 463–477.

Buur, L. and S. Jensen (2004). Introduction: vigilantism and the policing of everyday life in south africa. *African Studies 63*(2), 139–152.

Cammett, M. and L. M. MacLean (2014). *The politics of non-state social welfare*. Cornell University Press.

Campbell, A., P. E. Converse, W. E. Miller, and E. Donald (1960). *Stokes. 1960. The American Voter*. New York: Wiley.

Caparini, M. (2014). *Extending State Authority in Liberia: The Gbarnga Justice and Security Hub*. Oslo, Norway: Norwegian Institute of International Affairs. NUPI Report No. 5.

Carothers, T. (2006). *Promoting the rule of law abroad: in search of knowledge*. Brookings Institution Press.

Carothers, T. (2009). Rule of Law Temptations. *Fletcher Forum of World Affairs 33*, 49–61.

Cheng-Hopkins, J. and C. Tah (2013). Rebuilding Liberia, one hub at a time. *Mail & Guardian* (February 10).

Chirayath, L., C. Sage, and M. Woolcock (2005). Customary law and policy reform: Engaging with the plurality of justice systems.

Clingingsmith, D., A. I. Khwaja, and M. Kremer (2009). Estimating the Impact of the Hajj: Religion and Tolerance in Islam's Global Gathering. *Quarterly Journal of Economics 124*(3), 1133–1170.

Cooper, J. (2018). State capacity and gender inequality: Experimental evidence from papua new guinea. Working paper.

Darley, J. M., K. M. Carlsmith, and P. H. Robinson (2000). Incapacitation and just deserts as motives for punishment. *Law and Human behavior 24*(6), 659–683.

Denney, L. (2014). *Justice and Security Reform: development agencies and informal institutions in Sierra Leone*. Routledge.

Denney, L. and S. Jenkins (2013). Securing communities: The what and the how of community policing. *Overseas Development Institute Background Paper*.

Dinnen, S. and G. Peake (2013). More than just policing: Police reform in post-conflict bougainville. *International peacekeeping 20*(5), 570–584.

Distelhorst, G. and Y. Hou (2014). Ingroup bias in official behavior: A national field experiment in china. *Quarterly Journal of Political Science*.

Downie, R. (2013). *Building police institutions in fragile states: case studies from Africa*. Center for Strategic and International Studies.

Eitle, D., S. J. D'Alessio, and L. Stolzenberg (2002). Racial threat and social control: A test of the political, economic, and threat of black crime hypotheses. *Social Forces 81*(2), 557–576.

Ellis, S. (1995). Liberia 1989-1994: A Study of Ethnic and Spiritual Violence. *African Affairs 94*(375), 165–197.

Ellis, S. (2001). *The mask of anarchy: the destruction of Liberia and the religious dimension of an African civil war*. NYU Press.

Ellis, S. (2006, September). *The Mask of Anarchy: The Destruction of Liberia and the Religious Dimension of an African Civil War*. New York: New York University Press.

Esberg, J. and J. Mummolo (2018). Explaining misperceptions of crime.

Ferreira, B. R. (1996). The use and effectiveness of community policing in a democracy. *Policing in Central and Eastern Europe: Comparing Firsthand Knowledge with Experience from the West. College of Police and Security Studies, Ljubljana, Slovenia.*

Franck, R. and I. Rainer (2012). Does the leader's ethnicity matter? ethnic favoritism, education, and health in sub-saharan africa. *American Political Science Review 106*(2), 294–325.

Fried, B. J., P. Lagunes, and A. Venkataramani (2010). Corruption and inequality at the crossroad: A multimethod study of bribery and discrimination in latin america. *Latin American Research Review*, 76–97.

Fryer Jr, R. G. (2019). An empirical analysis of racial differences in police use of force. *Journal of Political Economy*.

García-Ponce, O., L. Young, and T. Zeitzoff (2018). Anger and support for punitive justice in mexico?s drug war.

Gelman, A., J. Fagan, and A. Kiss (2007). An analysis of the new york city police department's "stop-and-frisk" policy in the context of claims of racial bias. *Journal of the American Statistical Association 102*(479), 813–823.

Gerber, A. S. and D. P. Green (2012). *Field Experiments: Design, Analysis, and Interpretation*. New York: W. W. Norton.

Gill, C., D. Weisburd, C. W. Telep, Z. Vitter, and T. Bennett (2014a). Community-oriented policing to reduce crime, disorder and fear and increase satisfaction and legitimacy among citizens: A systematic review. *Journal of Experimental Criminology 10*(4), 399–428.

Gill, C., D. Weisburd, C. W. Telep, Z. Vitter, and T. Bennett (2014b). Community-oriented policing to reduce crime, disorder and fear and increase satisfaction and legitimacy among citizens: A systematic review. *Journal of Experimental Criminology 10*(4), 399–428.

Goldstein, M. and C. Udry (2008). The Profits of Power: Land Rights and Agricultural Investment in Ghana. *Journal of Political Economy 116*(6), 981–1022.

Goncalves, F., S. Mello, et al. (2017). A few bad apples?: Racial bias in policing.

Gordon, E. (2014). Security sector reform, statebuilding and local ownership: Securing the state or its people? *Journal of Intervention and Statebuilding 8*(2-3), 126–148.

Guerette, R. T. and K. J. Bowers (2009). Assessing the Extent of Crime Displacement and Diffusion of Benefits: A Review of Situational Crime Prevention Evaluations. *Criminology 47*(4), 1331–1368.

Guess, A. and A. Coppock (2018). Does counter-attitudinal information cause backlash? results from three large survey experiments. *British Journal of Political Science*, 1–19.

Hainmueller, J., D. Hopkins, and T. Yamamoto (2014). Causal inference in conjoint analysis: Understanding multi-dimensional choices via stated preference experiments. *Political Analysis*.

Hartman, A. C., R. A. Blair, and C. Blattman (2018). Engineering Informal Institutions: Long-run Impacts of Alternative Dispute Resolution on Violence and Property Rights in Liberia. NBER Working Paper No. 24482.

Herbst, J. (2014). *States and power in Africa: Comparative lessons in authority and control*. Princeton University Press.

Hicken, A. (2011). Clientelism. *Annual Review of Political Science 14*, 289–310.

Hidalgo, D. and B. Lessing (2015). Endogenous state weakness in violent democracies: paramilitaries at the polls. *Work. Pap., Mass. Inst. Technol., Cambridge, MA*.

Hillenbrand, E. (2006). Improving traditional-conventional medicine collaboration: Perspectives from cameroonian traditional practitioners. *Nordic Journal of African Studies 15*(1).

Hills, A. (1999). *Policing Africa: internal security and the limits of liberalization*. Lynne Rienner Publishers.

Holm, S. (1979). A simple sequentially rejective multiple test procedure. *Scandinavian Journal of Statistics*, 65–70.

Human Rights Watch (2013). "no money, no justice": Police corruption and abuse in liberia.

Hunt, J. (2007). How corruption hits people when they are down. *Journal of Development Economics 84*(2), 574–589.

Hyden, G. (2012). *African politics in comparative perspective*. Cambridge University Press.

ICG, I. C. G. (2011). *Liberia: How Sustainable Is the Recovery?* Number 177. International Crisis Group.

International Crisis Group (2006). *Liberia: Resurrecting the Justice System*. Number 148. International Crisis Group.

International Crisis Group (2009). *Liberia: Uneven Progress in Security Sector Reform*. Number 148. International Crisis Group.

Isser, D. (Ed.) (2011). *Customary Justice and the Rule of Law in War-Torn Societies.* Washington, DC: United States Institute of Peace.

Isser, D. H., S. C. Lubkemann, and S. N'Tow (2009). *Looking for Justice: Liberian Experiences and Perceptions of Local Justice Options.* Washington, DC: United States Institute of Peace.

Johnson, D. (2009). Anger about crime and support for punitive criminal justice policies. *Punishment & Society 11*(1), 51–66.

Justesen, M. K. and C. Bjørnskov (2014). Exploiting the poor: Bureaucratic corruption and poverty in africa. *World Development 58*, 106–115.

Kagoro, J. (2019). The crime preventers scheme: A community policing initiative for regime security in uganda. *Journal of Intervention and Statebuilding 13*(1), 41–56.

Käihkö, I. (2012). Big man bargaining in african conflicts. *African conflicts and informal power: Big men and networks*, 181–204.

Kantor, A. and M. Persson (2010). *Understanding vigilantism: informal security providers and security sector reform in Liberia.* Folke Bernadotte Academy.

Karim, S. (2018). Do male or female messengers matter for improving the rule of law in post-conflict countries? Working paper.

Karim, S., M. J. Gilligan, R. Blair, and K. Beardsley (2018). International Gender Balancing Reforms in Postconflict Countries: Lab-in-the-Field Evidence from the Liberian National Police. *International Studies Quarterly 62*(3), 618–631.

Kaufmann, A. (2013). *Spaces of imagination: associational life and the state in post-war, urban Liberia.* Ph. D. thesis, University_of_Basel.

King, E. and C. Samii (2014). Fast-track institution building in conflict-affected countries? insights from recent field experiments. *World Development 64*, 740–754.

Kramon, E. and D. N. Posner (2013). Who benefits from distributive politics? how the outcome one studies affects the answer one gets. *Perspectives on Politics 11*(2), 461–474.

Krasner, S. D. and T. Risse (2014). External actors, state-building, and service provision in areas of limited statehood: Introduction. *Governance 27*(4), 545–567.

Kudamatsu, M. (2012). Has democratization reduced infant mortality in sub-saharan africa? evidence from micro data. *Journal of the European Economic Association 10*(6), 1294–1317.

Kyed, H. M. (2017). Inside the police stations in maputo city. *Police in Africa: The Street Level View*, 213.

Leander, A. (2002). Wars and the Un-making of States: Taking Tilly Seriously in the Contemporary World. In S. Guzzini and D. Jung (Eds.), *Copenhagen Peace Research: Conceptual Innovations and Contemporary Security Analysis*, pp. 69–80. London: Routledge.

Lemarchand, R. (1972). Political clientelism and ethnicity in tropical africa:* competing solidarities in nation-building. *American political science review 66*(1), 68–90.

Levi, M. (1989). *Of Rule and Revenue*. University of California Press.

Levi, M., A. Sacks, and T. Tyler (2009). Conceptualizing Legitimacy, Measuring Legitimating Beliefs. *American Behavioral Scientist 53*(3), 354–375.

Li, Y., L. Ren, and F. Luo (2016). Is bad stronger than good? The impact of police-citizen encounters on public satisfaction with police. *Policing: An International Journal of Police Strategies and Management 39*(1), 109–126.

Lin, J. Y., R. Tao, and M. Liu (2006). Decentralization and local governance in china?s economic transition. *Decentralization and local governance in developing countries: A comparative perspective*, 305–327.

Lipsky, M. (1980). *Street-Level Bureaucracy: Dilemmas of the Individual in Public Service*. Russell Sage Foundation.

Little, K. (1965). The political function of the poro. part i. *Africa 35*(4), 349–65.

Lubkemann, S. C., D. H. Isser, and P. A. Z. Banks III (2011). Unintended Consequences: Constraint of Customary Justice in Post-Conflict Liberia. In D. Isser (Ed.), *Customary Justice and the Rule of Law in War-Torn Societies*, pp. 193–237. Washington, DC: United States Institute of Peace.

Lundman, R. J. and R. L. Kaufman (2003). Driving while black: Effects of race, ethnicity, and gender on citizen self-reports of traffic stops and police actions. *Criminology 41*(1), 195–220.

Mack, A., S. Merz, M. Bui, and T. Cooper (2013). *Human security report 2013: the decline in global violence: evidence, explanation, and contestation*. Human Security Report Project, Simon Fraser University.

Malan, M. (2008). Security sector reform in liberia: Mixed results from humble beginnings. Technical report, Army War College Strategic Studies Institute.

Mazerolle, L., E. Antrobus, S. Bennett, and T. R. Tyler (2013). Shaping Citizen Perceptions of Police Legitimacy: A Randomized Field Trial of Procedural Justice. *Criminology 51*(1), 33–63.

McClendon, G. H. (2016). Race and responsiveness: An experiment with south african politicians. *Journal of Experimental Political Science 3*(1), 60–74.

Meier, K. J., R. D. Wrinkle, and J. L. Polinard (1999). Representative bureaucracy and distributional equity: Addressing the hard question. *The Journal of Politics 61*(4), 1025–1039.

Menkhaus, K. (2007). Governance without government in somalia: Spoilers, state building, and the politics of coping. *International security 31*(3), 74–106.

Migdal, J. S. (1988). *Strong societies and weak states: state-society relations and state capabilities in the Third World.* Princeton University Press.

Migdal, J. S. (1994). The State in Society: An Approach to Struggles for Domination. In J. S. Migdal, A. Kohli, and V. Shue (Eds.), *State Power and Social Forces: Domination and Transformation in the Third World*, pp. 7–36. New York: Cambridge University Press.

Migdal, J. S., A. Kohli, and V. Shue (1994). *State power and social forces: domination and transformation in the Third World.* Cambridge University Press.

Migdal, J. S. and K. Schlichte (2005a). Rethinking the State. In K. Schlichte (Ed.), *The Dynamics of States: The Formation and Crises of State Domination*, pp. 1–40. Aldershot, England: Ashgate.

Migdal, J. S. and K. Schlichte (2005b). Rethinking the state. *The dynamics of states: The formation and crises of state domination*, 1–40.

Moore, M. H. (1992). Problem-solving and community policing. *Crime and justice 15*, 99–158.

Morse, B. (2019). Reducing vigilantism in fragile states: Evidence from a field experiment with the liberian national police.

Nagin, D. S. and C. W. Telep (2017). Procedural Justice and Legal Compliance. *Annual Review of Law and Social Science 13*(1), 5–28.

Nix, J., S. E. Wolfe, J. Rojek, and R. J. Kaminski (2015). Trust in the Police: The Influence of Procedural Justice and Perceived Collective Efficacy. *Crime & Delinquency 61*(4), 610–640.

Nyhan, B. and J. Reifler (2010). When corrections fail: The persistence of political misperceptions. *Political Behavior 32*(2), 303–330.

OECD (2008). *OECD Handbook on Security System Reform: Supporting Security and Justice*. Organisation for Economic Co-operation and Development.

Ostrom, E. (1996). Crossing the great divide: coproduction, synergy, and development. *World development 24*(6), 1073–1087.

Pitcher, A., M. H. Moran, and M. Johnston (2009). Rethinking patrimonialism and neopatrimonialism in africa. *African Studies Review 52*(1), 125–156.

Post, A. E., V. Bronsoler, and L. Salman (2017). Hybrid regimes for local public goods provision: a framework for analysis. *Perspectives on Politics 15*(4), 952–966.

Qi, Z. (2013). Who traditional medicine strategy. 2014-2023. *Geneva: World Health Organization.*

Reeve, R. and J. Speare (2012). Human security in liberia. local perspectives on formal and informal security sectors. *Accord: an international review of peace initiatives 23.*

Rennie, L. (2006). Vigilantes take on liberian gangs. *British Broadcasting Corporation* (September 19).

Reno, W. (1995). Reinvention of an african patrimonial state: Charles taylor's liberia. *Third World Quarterly 16*(1), 109–120.

Robinson, A. L., B. Seim, et al. (2018). Who is targeted in corruption? disentangling the effects of wealth and power on exposure to bribery. *Quarterly Journal of Political Science 13*(3), 313–331.

Rose-Ackerman, S. (2004). *When states fail: causes and consequences*, Chapter Establishing the rule of law, pp. 182–221. Princeton University Press.

Ross, M. (2006). Is democracy good for the poor? *American Journal of Political Science 50*(4), 860–874.

Rotberg, R. (2004). *The Failure and Collapse of Nation-States: Breakdown, Prevention, and Collapse*, Chapter 1, pp. 201–213. Princeton, NJ: Princeton University Press.

Sahin, N., A. A. Braga, R. Apel, and R. K. Brunson (2017). The Impact of Procedurally-Just Policing on Citizen Perceptions of Police During Traffic Stops: The Adana Randomized Controlled Trial. *Journal of Quantitative Criminology 33*(4), 701–726.

Sandefur, J. and B. Siddiqi (2013). Extending the Shadow of the Law: Theory and Experimental Evidence. Presented at the Working Group in African Political Economy, The World Bank, May 20.

Sawyer, A. (2005). *Beyond Plunder: Toward Democratic Governance in Liberia.* Boulder, CO: Lynne Rienner.

Sawyer, A. (2008). Emerging patterns in liberia's post-conflict politics: Observations from the 2005 elections. *African Affairs 107*(427), 177–199.

Scott, J. C. (1985). *Weapons of the weak: Everyday forms of peasant resistance.* Yale University Press.

Siddiqi, B. and J. Sandefur (2009). *Appendix: Community-Based Justice and the Rule of Law in Liberia.* Washington, DC: United States Institute of Peace.

Skogan, W. G. (2006). Asymmetry in the Impact of Encounters with Police. *Policing and Society 16*(2), 99–126.

Skogan, W. G. and K. Frydl (2004). *Fairness and Effectiveness in Policing: The Evidence*. National Academies Press.

Steinberg, J. (2008). *Thin Blue Line — the unwritten rules of policing South Africa*.

Sunshine, J. and T. R. Tyler (2003). The Role of Procedural Justice and Legitimacy in Shaping Public Support for Policing. *Law & Society Review 37*(3), 513–548.

Tamanaha, B. Z. (2008). Understanding Legal Pluralism: Past to Present, Local to Global. *Sydney Law Review 30*, 375–411.

Telep, C. W., D. Weisburd, C. E. Gill, Z. Vitter, and D. Teichman (2014). Displacement of crime and diffusion of crime control benefits in large-scale geographic areas: A systematic review. *Journal of Experimental Criminology 10*(4), 515–548.

Tsai, L. L. (2011). Friends or foes? nonstate public goods providers and local state authorities in nondemocratic and transitional systems. *Studies in Comparative International Development 46*(1), 46–69.

Tyler, T. and J. Fagan (2006). Legitimacy and Cooperation: Why Do People Help the Police Fight Crime in Their Communities? *Ohio State Journal of Criminal Law 6*, 231–275.

Tyler, T. R. (2004). Enhancing Police Legitimacy. *The Annals of the American Academy of Political and Social Science 593*(1), 84–99.

Tyler, T. R. (2006). *Why people obey the law*. Princeton University Press.

Tyler, T. R. and J. Fagan (2008). Legitimacy and Cooperation: Why Do People Help the Police Fight Crime in Their Communities? *Ohio State Journal of Criminal Law 6*, 231–275.

Tyler, T. R. and Y. J. Huo (2002). *Trust in the Law: Encouraging Public Cooperation with the Police and Courts*. New York: Russell Sage Foundation.

United Nations (2016). *Guidelines for Police Operations in United Nations Peacekeeping Operations and Special Political Missions*. United Nations Department of Peacekeeping Operations.

United Nations (2018). *Community-Oriented Policing in United Nations Police Operations*. United Nations Department of Peacekeeping Operations.

Utas, M. (2012). *African conflicts and informal power: Big men and networks*. Zed Books.

Valters, C., E. Van Veen, and L. Denney (2015). Security progress in post-conflict contexts. *London: Overseas Development Institute*.

Weber, M. (1964). *The theory of social and economic organization*. Simon and Schuster.

Weber, M. (1978). *Economy and society: An outline of interpretive sociology*. Univ of California Press.

White, A. R., N. L. Nathan, and J. K. Faller (2015). What do i need to vote? bureaucratic discretion and discrimination by local election officials. *American Political Science Review 109*(1), 129–142.

Wilson, J. Q. (1978). *Varieties of Police Behavior: The Management of Law and Order in Eight Communities*. Cambridge, MA: Harvard University Press.

Wisler, D. and I. D. Onwudiwe (2008). Community policing in comparison. *Police Quarterly 11*(4), 427–446.

Wolfe, S. E., J. Nix, R. Kaminski, and J. Rojek (2016). Is the Effect of Procedural Justice on Police Legitimacy Invariant? Testing the Generality of Procedural Justice and Competing Antecedents of Legitimacy. *Journal of Quantitative Criminology 32*(2), 253–282.

Zaller, J. R. et al. (1992). *The nature and origins of mass opinion*. Cambridge university press.

Zanker, F. (2017). Moving beyond hybridity: The multi-scalar adaptation of community policing in liberia. *Journal of Intervention and Statebuilding 11*(2), 166–185.