February 7, 2017

Re: EGAP 19: Sciences Po, March 2017

Dear Participants,

Thank you in advance for your comments during the workshop. This paper is a first draft of a paper involving a larger field experiment I conducted in Liberia for my dissertation. The larger experiment involved household visits by male and female police officers separately, as well as the civilian information experiment mentioned in the paper. During Q&A, I am more than happy to talk about the larger project.

Because this is a first draft, I welcome extensive feedback including, but not limited to the following:

- Portions of the paper that need more or less elaboration
- How best to present and interpret results
- What other analysis can be conducted to strengthen the claims in the paper
- How best to present the two control groups in Experiment 1
- Whether or not the interpretation of spillover is correct. Other ways of handling the spillover problem
- Suggestions for imbalance
- Clarity of the treatments and research design
- Clarity and plausibility of the theoretical mechanisms
- Whether I provide enough context and background information
- What parts can be cut (and perhaps added to the appendix)

Thank you again.

Sincerely,

Sabrina Karim

Does the Messenger Matter for Improving Post-Conflict Governance? Results from Two Field Experiments on Security Sector Reform in Liberia

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February 7, 2017

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Abstract

Many post-conflict countries face unique governance challenges due to weakened institutions after war. In response, state builders often attempt to reform political, economical, and social institutions in war-torn countries. In order to gain support, however, state builders must do more than just roll out reforms; they must make the public aware of them. Without citizens' awareness of reform implementation, citizens are unlikely to have increased confidence in the states' institutions. This paper builds on this wisdom by assessing whether the messenger matters when governments expose citizens to reforms. The messenger could either be a member of the reformed institution (reform-insider) or a non-member of the reformed institution (reform-outsider). Using the security sector as an example institution, two experiments were conducted in rural Liberia to assess whether "insiders" or "outsiders" are more effective in eliciting positive perceptions of governance when delivering the message. In the first experiment, treated citizens were randomly exposed to newly professionalized police officers who provided information about police reforms. In the second, randomly chosen citizens were given the same information about police reforms by civilians. The results suggest that information by the police enhance perceptions of governance; there was no effect when civilians informed the public about police reforms. These findings indicate that the messenger does matter when it comes to communicating reforms to the public, and that reform-insiders could be an important avenue through which to improve public perceptions of governance.

1 INTRODUCTION

For decades, researchers have been studying how to achieve peace and order after a civil war. One of the key insights in this literature is that (re-)building functional and effective domestic institutions—whether political, economics, social, or security-related—is important for long-term stability, as post-conflict countries face unique governance challenges due to weakened institutions after war. Weak, post-conflict states are often plagued by an inadequate security sector and bureaucracy, damaged infrastructure, and declining rule of law. Unsuccessful implementation of domestic institutional reforms in such fragile, post-conflict states stand to jeopardize domestic and international security, which is why many have called on prioritizing security and state power in state building efforts (Call and Cousens 2008, Fukuyama 2011, 2014, Ghani and Lockhart 2009, Holsti 1996, Lake 2016)

While the focus of reforms is to build the actual capacity of post-conflict states to better govern the state, another potential added benefit of implementing reforms is improvement in citizens' perceptions of governance—which is a potential condition for a state's democratic legitimacy.[1] As state builders implement reforms, citizens are more likely to have a favorable opinion of the government. This confidence in the government may be exhibited in different ways such as improved perceptions of institutional restraint, inclusivity, rule of law, and/or effectiveness. However, positive public opinion dividends are unlikely to manifest unless citizens have exposure to the reforms or they are informed about them.

State builders enact a whole host of reforms, but a prerequisite for improved perceptions of governance is the public's cognizance of them. As such, states sometimes engage in awareness and information campaigns or take measures to expose citizens to reforms. The transfer of information about reforms may occur via two channels: by actors who embody or have experienced the reform (reform-insiders) or actors who are unrelated to the reform (reform-outsiders). Reform-insiders include those who are directly associated with the reform whether appointed to a newly created position due to the reform (such as civilian oversight boards, new ministries, new bureaucratic organizations, including the security sector), or undergone a revised training regime related to the reform (new standards, training institutes, programs etc.). Their presence demonstrates that

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[1] See for example Miller and Listhaug (1990).

the reform has been implemented, and could discourage concerns about government cheap talk. Additionally, information campaigns by reform-insiders may be perceived as service provision or expansion of expertise by the state. In contrast, reform-outsiders are those who have no direct relationship to the reform such as civil society groups, NGOs, media, researchers, and academics. They could be perceived as potential neutral sources of information. Given the two channels, this article assess whether reform-insiders or reform-outsiders are better messengers when it comes to improving perceptions of governance. For purposes of this paper, the reform in question is police professionalization—a type of security sector reform. Security sector reforms have recently been suggested as a priority area for state building (Fukuyama 2011, 2014, Lake 2016).

In order to test whether reform-insiders or reform-outsiders positively enhance perceptions about governance, two field experiments were conducted in one of Liberia's most rural counties in 2015. Security sector reform was implemented after Liberia's civil war, which ended in 2003. Despite the war's ending, nearly fifteen years ago, Liberia's security sector is still very weak and presents a security challenge for the region. The police have ongoing professionalization reforms in place to ensure that the police no longer abuse the population and so that they can effectively provide security. Moreover, regardless of police professionalization efforts, many civilians in Liberia are still unaware of police reforms, particularly those residing in rural areas, where police presence is largely absent.

In the "reform-insider" experiment, teams of two, professionalized Liberian National Police (LNP) officers spoke to rural residents about police professionalization. In total, 300 households were visited by the police and 225 households were not visited by police. In a separate "reform-outsider" experiment, 200 households were given the same information about reforms as the professionalized police gave to community members in the "reform-insider experiment," but this time by civilian enumerators conducting a survey. The control group of 300 households were not given any information. A household survey was conducted in both treated and control households to assess perceptions of governance.

The results from the "reform-insider" experiment show that when professionalized police delivered the message, it improved perceptions of police restraint, inclusiveness, rule of law, and effectiveness. However, some of these positive results were due to backlash by untreated citizens who knew about the police visits and perceived them as a government service that ignored them.

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When the treated households were compared to an alternative control group, where this "reverse-spillover" did not occur, perceptions of restraint, inclusiveness (discrimination) and to some extent, rule of law improved. The results from the "reform-outsider" experiment show that, aside from corruption, information about police professionalization by civilians has little effect on improving perceptions of governance. These findings indicate that messenger does matter when it comes to communicating reforms to the public, and that reform-insiders could help boost public perceptions of governance.

The article makes important theoretical and empirical contributions. First, it develops mechanisms through which governments might communicate information to their constituents in weak, post-conflict states. While the state building literature has covered the importance of institutional reforms, it ignores whether and how these changes reach citizens. The article develops a theory of reform messengers, which addresses the missing link between reform implementation and public approval of government institutions. By concentrating on reform messengers, the article broadens the scope of the post-conflict institutional reform literature from being focused on outcomes (i.e. outcomes related to conflict recurrence) to questions of public approval and government legitimacy. Second, it uses original field experiments to test whether reform-insider or reform-outsider messengers are more effective in improving perceptions of governance. The approach is rigorous and allows for causal claims to be made. Finally, the study focuses on the security sector and police, which are often forgotten actors in political science. Police reforms are particularly important to ensure stability in weak, post-conflict states, as the police are the main actors responsible for maintaining order. This study illuminates their potential role in state building, and to the author's knowledge, is one of the first interventions conducted that randomizes police officers visits to households in a weak, post-conflict country.[2]

Moving forward, the article first addresses state building, particularly related to the security sector, and governances in weak, post-conflict states, it then explores the mechanisms through which reform-insiders and reform-outsiders might improve perceptions. The next sections address the relevance of security sector reform for assessing the claims in this article, case selection, and research design of the two experiments. The article concludes with the results from the two experiments and a discussion of the implications from the findings.

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[2]One exception is Blair, Karim and Morse (2016).

2 State Building and Governance in Weak Post-Conflict Countries

Post-conflict states often face many unique governance challenges due to weakened state institutions as a result of war. They are often plagued by an inadequate security sector and bureaucracy, damaged infrastructure, and declining rule of law. States with weak institutions create transnational threats including the spillover of violence across borders (Call and Cousens 2008, Holsti 1996), become safe havens for illicit, criminal, and/or terrorist activity (Ghani and Lockhart 2009), facilitate the accidental or purposeful transfer of weapons of mass destruction (WMD), and cause global fluctuation in the price for oil, causing energy insecurity (Patrick 2011). Additionally, failed or weak states have a difficult time handling humanitarian crises, from natural disasters to famine, to disease (Patrick 2011). For this reason, scholars have suggested that since the end of the Cold War, weak or failing states are the single-most significant threat to world order, and that preventing state failure is imperative for international security (Fukuyama 2011, 2014, Lake 2016).

Much of the literature to date has focused on re-building political institutions as a way to prevent state failure—a trading in of bullets for ballots, of sorts. This has usually meant ensuring fair and competitive elections, the creation of a robust party system, and other electoral reforms (Matanock 2017, Paris 2004, Snyder 2000). However, Paris (2004) argues that states need a "rudimentary network of domestic institutions" before the introduction of electoral institutions. Such "rudimentary" institutions may take the form of security and administrative reforms (Ghani and Lockhart 2009). As a result, an alternative form of state building suggested by Lake (2016) argues that since the U.S.-led wars in Afghanistan and Iraq in 2002 and 2003, respectively, the focus of reform should be on providing security to win the hearts and minds of the people—this approach restores the fundamental social contract—security and order in exchange for compliance and loyalty. Lake (2016) suggests that state builders should move away from "institutionalism" and back to security and state power. As such, security sector reforms might be more of a priority area for state building as they serve Paris (2004)'s notion of a "rudimentary network of domestic institutions," and Lake (2016)'s push for the prioritization of re-building state power.

While there are ongoing debates about which types of reforms to prioritize,[3] the objective of

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[3]A reform is defined as changes made to state institutions and organizations to improve outcomes for citizens. It

implementing any type of reform—whether political or security-related—is to improve governance.[4] Governance is the process of decision-making and the process by which decisions are implemented (or not implemented) within an organization or entity (UN 2006). "Good governance" refers to the notion that governance is conducted in a way that maps onto certain international norms such as democracy, accountability, transparency, human rights, and open markets (Lake 2016, Paris 2004), and has been a major priority for international organizations starting in the 1990's. During this time, the strengthening of "good governance" in developing countries became both an objective of and a condition for development assistance (Santiso et al. 2001), and was part of a larger "liberal peace building" agenda (Lake 2016, Paris 2004).

International organizations have conceptualized "good governance" indicators in different ways. According to the the United Nations Development Program, there are five components: legitimacy and voice, direction, performance, accountability, and fairness (UN 1997). Others in the United Nations have included more components: participation, consensus-oriented, accountable, transparent, responsive, effective and efficient, equitable and inclusive, and rule of law (UN 2006). The Worldwide Governance Indicators include: voice and accountability, political stability and absence of violence, government effectiveness, regulatory quality, rule of law, and control of corruption (Kaufmann, Kraay and Mastruzzi 2011). From these policy documents, several commonalities emerge.[5] First, there is a focus on preventing corruption and ensuring accountability and transparency. This means a better behaved government that does not prey on citizens. The government and its institutions are *restrained* from abusing citizens, asking for bribes, stealing items and sexually assaulting civilians. Second, there is a focus on fairness, representation, and equity. This focus is on ensuring that institutions treat all citizens equally and do not discriminate based on sex, gender, race, ethnicity, socio-economic status, disability etc. This can be conceptualized as *inclusivity*. Moreover, the different policy documents all mention *rule of law*. Finally, there is a focus on efficiency, performance, direction, or *effectiveness*. Reforms can thus be conceptualized as observable outcomes that are directly related to these norms—for example, less bad behavior by the government (restraint), less discrimination by the state (inclusivity), more rule of law, and a

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is different than the provision of goods and services because it involves changes at the institutional level, which may or may not increase service and goods provision.

[4]There are other goals for reforms as well such as to decrease conflict recurrence, decrease poverty, etc. However, good governance is needed for long-term stability and for improved development-related outcomes.

[5]Characteristics that are in at least two of the documents.

better functioning, autonomous state with an increased ability to respond to and provide services to citizens (effectiveness). Reforms are implemented to improve these outcomes.

In addition to improved outcomes, reform implementation could affect citizens' *perceptions* of governance. When individuals experience reforms or service provision by the state, they may be more likely to support the government (Hibbing 2001). Several studies corroborate this intuition and find that in post-conflict states, policy changes and institutional reforms, especially related to service provision, have an impact on generating confidence in the state's ability to govern (Bakke et al. 2014, De Juan and Pierskalla 2014, Gates and Justesen 2016, Hutchison and Johnson 2011, Sacks and Larizza 2012). While "good governance" is largely outcome based, it is also a concept that can be perceived by citizens. That is, citizens also hold perceptions about government restraint, inclusiveness, rule of law, and effectiveness, which may change depending on reform implementation.[6]

Confidence in the state's ability to govern or public opinion about the government and its institutions is an important indicator for political leaders in democracies to assess whether the public is content with the current status of politics (Hetherington 1998). Positive public opinion gives the government legitimacy and public opinion provides a signal to policymakers about the popularity of certain reforms, allowing modifications to be made based on these opinions (Miller and Listhaug 1990, Mishler and Rose 2002, Verba, Schlozman and Brady 1995). Thus, reforms, whether focused on the security sector or political institutions, not only change actual outcomes related to governance, they might also change citizens' perceptions about governance, and these positive perceptions could be an important factor in a state's democratic legitimacy.

While public opinion matters for all types of governments, public opinion about reforms and how they affect government institutions might be particularly important in weak, post-conflict states. It is in these states that public opinion about governance is likely to be distorted or absent. Individuals in post-conflict countries may hold unfavorable opinions about the government. The war and violence associated with the war may have disrupted or distorted prior beliefs about government institutions, making them more negative. Additionally, in weak states, service provision and information may not reach many constituents, which means that many citizens may hold no

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[6]Actual outcomes may or may not be correlated with perceptions. The goal of this article is not to assess this potential link, but rather to evaluate how reforms can be implemented so as to enhance confidence in the state's ability to govern.

opinion about governance because they have not been governed. In this way, weak, post-conflict country governments might suffer from anti-government opinions and/or a vacuum of opinion. For this reason, the paper is limit to these types of countries, even though the mechanisms developed below may not be unique to such cases.

3   Reform Messengers: Communicating Information to the Public

In order for reforms to have an effect on changing public perceptions, they must be communicated to citizens (Von Kaltenborn-Stachau 2008). A prerequisite for improved perceptions of governance is the public's cognizance of reform implementation. Ideally reforms would lead to better outcomes such as less violence, less corruption, and more rule of law, a functioning, autonomous state, and increased ability to respond to and provide services to citizens, among other improved outcomes. Observing these improved outcomes, citizens might then develop positive perceptions of the government. However, implementation is a long-term process; there is no guarantee that the implementation of reforms will lead to the desired outcome; and, it is possible that citizens might not attribute improved outcomes to particular reforms implemented by the government. To speed up the receipt of positive dividends from reform implementation and to avoid misappropriation of credit, governments sometimes engage in awareness and information campaigns or take measures to expose citizens to reforms.[7] In particular, in rural peripheries, access to information about reforms is much more difficult than in urban settings, because of the lack of state institutions, which makes such outreach and information crucial in remote parts of weak states.

The underlying assumption behind these types of information campaigns is that they will improve perceptions about governance. One reason they might improve perceptions is because once citizens are aware of the reforms, they might use the government organization(s) that has been reformed, perhaps leading to satisfaction, and consequently more trust in government (Levi and Stoker 2000). Citizens cannot be satisfied with government agencies unless they have been informed about their services. Another reason information campaigns might improve perceptions is because it reduces the "knowledge gap" between governments and citizens (Cook, Jacobs and Kim

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[7]See, for example World Bank (2008).

2010). Citizens are appreciative when they receive more information about changes in government practices and policies. Additionally, greater communication between citizens and the state indicates more transparency and openness on the part of the government (Hood and Heald 2006).

While it is possible that any information campaign about reforms may yield positive views about governance, there is some evidence to suggest that not all forms of information campaigns are successful in eliciting positive perceptions. Nussio, Oppenheim and Pantoja (2016) find that communication interventions involving information about reforms sent via text messages had no effect on improving perceptions of the Colombian government. Moreover, Grimmelikhuijsen et al. (2013) find that too much information could erode trust in the state. One reason may be because of prior mistrust. If the level of mistrust in the government is high, citizens may not even believe the information that they receive. Another reason for these negative results may be that satisfaction with public services emerges from a comparisons between perceptions of government services and citizens' prior expectations (Van Ryzin 2007)—"satisfaction equals perceptions minus expectation (Nussio, Oppenheim and Pantoja 2016)."[8] Improved perceptions of governance, thus, might result from changes in perceptions or expectation. Information might raise expectations especially because information is not equivalent to access to the reformed institution and its services. Even if citizens have information about a reform, they may not benefit from it. The negative findings from Nussio, Oppenheim and Pantoja (2016) and Grimmelikhuijsen et al. (2013) might be due to heightened expectations from the reform or information about the reform, which the government was unable to meet.

Instead of assuming that all information yields positive perceptional dividends, it is possible that the success of information campaigns depends on the messenger. Specifically, success could depend on whether the messenger is a reform-insider or a reform-outsider. *Reform insiders* are actors who embody the reform, have undergone the reform, and/or include those who are directly associated with the reform. They may provide more credibility with regards to actual reform implementation—avoiding concerns about government cheap talk. Additionally, information campaigns by reform-insiders may be perceived as service provision by the state or an expansion of expertise by the state. In contrast, *reform-outsiders* are actors who are orthogonal to the reform and have no direct relationship to the reform. They could be perceived as potential neutral sources

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[8]See pg. 4

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of information. Both these channels have been used by state builders to promote reforms.

3.1 REFORM-INSIDERS

One of the main ways to change perceptions has been to expose individuals directly to those that embody the reform. That is, bureaucratic agents who were directly affected by the reform are the messengers of it. This could take different forms depending on the reform. For example, one reform may be to integrate new groups of people into particular institutions (i.e. ethnic/gender power sharing agreements or quotas). In this case the reform-insider messenger would be a member of the newly integrated group. Another reform might be the creation of new government agencies or organizations (i.e. a new ombudsman, ministry, oversight board, etc.), in which case the messenger would be a member of the newly created agency or organization. Yet another reform might have to do with professionalization of existing government bureaucracies (i.e. new standards, recruitment, promotion, training, etc.). If this is the case, a newly professionalized member of the government agency would be the messenger.

There may be several benefits to using reform-insiders as messengers. First, a reform-insider demonstrates that the reform has actually been implemented by the government. That is, if citizens are concerned that governments do not follow through with reforms, and instead engage in cheap talk, the only way to demonstrate that a reform has been implemented is to expose citizens to it first hand. This is achieved when the messenger embodies the reform, as the the reform-insider messenger reveals the reform's implementation, in addition to providing information about the reform. Governments may fail to implement reforms that are coveted by the public, perhaps because they prefer to divert those resources in different ways. However, they may be able to fool citizens into thinking they have implemented the reform simply by stating that they have implemented it. The government assumes that citizens will not pay the costs to find out if the government has actually implemented the reform. If governments want to prevent public fears of this cheap talk, they could simply expose citizens to the reforms once they have been implemented by sending a reform-insider to deliver a message.

Second, the very act of providing information by reform-insiders may be perceived as a form of service provision. Because reform-insiders embody the reform, when they deliver the message to citizens, recipients of the message may feel that they are receiving more than just information. For

example, if the information or outreach is by someone from a newly created government institution or a newly professionalized one, they may be seen as providing the service of that new or reformed institution. Moreover, exposure to personnel that embodies the reform also allows for an expert of the reform to provide information instead of a third party or reform-outsiders who may not have as much first-hand knowledge about the reform and its implications. If civilians have questions or comments about the reform, those that embody the reform or that implemented the reform are the best candidates to answer questions and address comments. Thus, even though third parties could provide information, they may not be able to provide the full range of information about particular reforms as those directly involved with the reform itself. In short, reform-insiders provide citizens more than just information—they demonstrate the government's follow through with reform implementation and provide a service and/or expertise.

H1: Delivery of information about the reform by reform-insiders will lead to positive perceptions of governance

3.2   Reform-Outsiders

Another way to spread awareness about reforms is to simply provide information to citizens about the reform through neutral, third parties such as civil society groups, NGOs, the media, researchers, and academics. Information could be spread in such areas through media ads, billboards, civilian canvassing programs, or other means to "get out the word." Governments vary in types of information campaigns they have implemented. Some policymakers in Hungary and Egypt used billboards to advertise reforms by the government as way to demonstrate that the government is helping the population.[9] In Liberia, as a way to spread the word about decentralization reforms related to the justice and security sector, the Peacebuilding Commission implemented a civilian canvassing program to raise awareness (Keane 2012).

The results from data, however, are mixed on the extent to which third party, informational campaigns work to change people's perceptions. The evidence on mass media interventions suggests

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[9]See, for example, the Hungarian billboard campaign government. ("Government launches new billboard campaign", *The Hungarian Times.* July 17, 2015.) See also the billboard campaign in Egypt ("New billboards across Egypt's capital extol austerity" ABC News, September 3, 2016).

that they do not necessarily lead to changed perceptions (Paluck 2009), or that they only have temporary effects (Paluck and Green 2009, Paluck et al. 2015). Canvassing by civilians from NGOs yields more positive results for changing perceptions (Broockman and Kalla 2016),[10] but this study's canvassing intervention was designed to "take an out-group's perspective" as a way to reduce prejudice, and was not aimed at providing information about the subject (in the case of the article transgender rights). Moreover, for Broockman and Kalla (2016), the goal was to change prejudiced attitudes, here the goal is to improve perceptions of governance. Thus, despite the positive findings related to canvassing in Broockman and Kalla (2016), the study is testing a different theory of attitudinal change than the one tested in this study, but nevertheless provides important information about third party canvassing.

Despite mixed results on canvassing by third parties or media campaigns, there may be some benefit of having neutral, third parties disseminate information. Unlike reform-insiders, reform-outsiders have no direct ties to the reform. As such, they provide an objective, and perhaps credible source of information to citizens who might be weary of hidden agendas by the government. Just like in the mediation literature, we can think of the third party as a mediator between an aggrieved population (due to the civil war and perhaps lack of services) and the government. The third party's job is to convince the aggrieved citizens that the government is enacting reforms for the benefit of the people. Mediators or third party information providers are more successful in this endeavor if they are unbiased (Kydd 2006). Thus, neutral third parties or reform-outsiders might be better vehicles for communicating reforms than reform-insiders specifically because they are perceived as unbiased.

H2: Delivery of information about the reform by reform-outsiders will lead to positive perceptions of governance

These two channels are not mutually exclusive and the hypotheses are not necessarily competing. Governments can send both reform-insiders and reform-outsiders to deliver messages. The goal is to understand whether information provision by reform-insiders and/or reform-outsiders has an impact on citizens' perception of governance. As such, the type of messenger may not have any

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[10]See pg. 221.

effect, they both may have an effect, one type of messenger or the other might have an effect, or one type of messenger may have a stronger affect than the other.

4   THE CASE FOR FOCUSING ON SECURITY SECTOR REFORM

As mentioned above, to date, much of the institution-building literature has focused on re-building *political* institutions (Matanock 2017, Paris 2004, Snyder 2000), but security sector reforms are often implemented prior to political and electoral reforms, and scholars have recently argued that these reforms should be prioritized to ensure order after a war ends (Lake 2016). This is because there appears to be an important link between state power projected through security and state legitimacy. Lake (2016) notes that "legitimacy derives from a mutually beneficial exchange in which the state provides social order to society, and society in turn complies with the extractions (e.g. taxes) and constraints on its behavior (e.g. law) that are necessary to the production of that order."[11] He claims that the central task for state building is to create a state that is regarded as legitimate by the people over whom it exercises authority. He goes on to assert that "failed states lose their monopoly, legitimacy, and most often both as they are pulled apart by societal conflicts," and "statebuilding is the process of restoring—or in some instances, creating for the first time—that monopoly of violence and especially its legitimacy."[12] Legitimacy is obtained when the state retains "sufficient public support for the monopoly of force (Lake 2016)."[13] It is *security sector reforms* that enable this legitimacy because they restore the fundamental social contract between citizens and the state. Thus, a key component of enhancing good governance relies on security sector reforms influencing citizens' perceptions of and behavior toward the state. This means that it might be prudent to study the effects of information about security sector reforms.

Security sector reforms are particularly important to implement in rural, peripheries, where state presence is largely absent. Research has shown that conflicts located at considerable distance from the main government stronghold, along remote international borders and in regions with valuable minerals last substantially longer (Buhaug, Gates and Lujala 2009) and that the risk of conflict increases with the distance from rebel groups to the capital (Cederman, Buhaug and Rød

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[11]See pg. 25.
[12]See pg. 8.
[13]See pg. 14.

2009). Others have shown that states with hostile neighbors weaken the state (Lee 2016). This means that along these corridors, where states are fragile, security sector reform is particularly important to not only bolster sovereignty, but also to assure civilians, who live in these areas, that the state is able to provide security.

The security sector is defined as "organizations and entities that have the authority, capacity and/or orders to use force or the threat of force to protect the state and civilians (Anderlini and Conaway 2004)."[14] This includes the military, police, militias, special forces, and other state-sponsored armed groups, and it also includes civilian organizations responsible for the oversight of the organizations that are authorized to use force such as intelligence agencies, Ministries of Defense, or oversight bodies that have direct control over the functions of armed sectors.

In the late 1990s, the international community coined the term "security sector reform" and incorporated it as a part of a "good governance" agenda, thereby extending ideas of accountability, transparency, and representation into the security sector as a way to build trust between the security sector and the civilian population. The focus of such reforms was on transforming the structure of security bodies by making them transparent and professional, reducing corruption, and fostering a cultural transformation so that previously excluded groups (ethnic minorities and women) were included (Anderlini and Conaway 2004, Brzoska and David 2013). The UN argues that reforming a country's security sector is essential to re-building confidence between the state and its peoples (Brzoska and David 2013). The sole purpose of major organizations such as the Geneva Centre for the Democratic Control of Armed Forces (DCAF) in 2000 was to pursue "good governance" and reform of the security sector. The OECD came up with a definition of SSR that encompassed this good governance agenda: a "transformation of the security system which includes all actors, their roles, responsibilities and actions working together to manage and operate the system in a manner that is more consistent with democratic norms and sound principles of good governance and thus contributes to a well functioning security framework (Hendrickson and Ball 2005)."[15]

In general, there is not much literature on the impact of security sector reform in weak, post-conflict countries. An exception is Toft (2010) who argues that SSR resolves the commitment problem and may prevent conflict recurrence. When the security sector is weak, the state is not

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[14]See pg. 1.
[15]See pg. 20.

strong enough to punish potential rebel groups either, what she calls "harm" (Toft 2010).[16] Without security sector reform, the costs of rebels taking up arms remains low, and deterrence may not be credible, making resolving the commitment problem more difficult. Thus, Toft (2010) hypothesizes that negotiated settlements are more likely to result in war recurrence without security sector reform. Using statistical analysis to assess whether types of war termination between 1942–1999 affect civil war recurrence, she finds evidence for her theory.

In Toft (2010)'s empirical analysis, she uses successful power-sharing or ethnic balancing in the military as the main independent variable. However, states can implement different types of security sector reforms—everything from disarmament, demobilizations, reintegration (DDR) programs, civilian oversight, professionalization, female and ethnic balancing, constitutional restrictions on soldiers, conscription, among many others. Instead of focusing on ethnic balancing or power sharing, here security sector reform is operationalized through professionalization of the police force. Professionalization refers to the restructuring of the security forces, vetting of recruits, training recruits through formal academies, setting standards for recruitment and promotion, and increasing the representation of different groups.

Security sector reform is operationalized through police professionalization for several reasons. First, when it comes to security sector reform, what usually comes to mind is professionalization as it incorporates different elements of reform such as criteria for new recruitmnet and training. Additionally, the police have more of an impact on citizens' daily lives than other parts of the security and citizens are more likely to interact regularly with police than other security forces, which means reform (or professionalization) of the police might be more important to them (Mazerolle et al. 2013). Finally, in weak, post conflict states such as Liberia, South Sudan, Haiti, and Somalia, entire security forces were destroyed by the war, which means that the state often starts out with minimal, if any at all, security force capability. It is in these contexts, where capacity is low, that civilians are likely to perceive the state security sector as abusive and ineffective. In these contexts, reforms, which professionalize the security sector, may yield significant effects because changes will be more noticeable and salient to the population.

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[16]See pg. 42.

5 Case Selection: Grand Kru, Liberia

In order to evaluate whether the type of messenger affects information about security sector reforms (police professionalization), two field experiments were implemented in Grand Kru County, Liberia. Liberia and in particular, Grand Kru county,[17] represent countries and parts of a state that suffer from the unique post-conflict governance challenges mentioned above. Liberia is a weak, post-conflict state. The civil war in Liberia lasted nearly two decades and ended in 2003, after nearly two decades of fighting that killed over 250,000 people. The UN Mission in Liberia (UNMIL) arrived in 2003, and there has been peace since the mission's arrival. However, Liberia is currently one of the poorest post-conflict countries in the world and nearly all of its institutions were dysfunctional at the end of the war (Ciment 2013). By the end of the war, Liberia had fifteen different security agencies with overlapping functions; many police stations had been abandoned, destroyed, or taken over by rebel forces; the state lacked basic equipment, vehicles, fuel, and communications systems; and, many police officers and other government officials had fled the country (Friedman and MacAulay 2011). In short, most institutions in Liberia, including the security forces, were in dire need of restructuring and professionalization.

Within Liberia, Grand Kru County was chosen for conducting the experiments. The county is one of the most isolated in the country and fewer residents of Grand Kru County have prior experience with police than other counties. In 2011, only 16% of the population of the county had interacted with a police officer, which was one of the lowest percentages out of all the counties. About 54% of residents said that no one provided security, which was one of the highest percentages out of all the counties, and only 22% said police provide security, which was one of the lowest out of all the counties (Vinck, Pham and Kreutzer 2011). Most people in the county have minimal interactions with the police, which means that interacting with them may have more of an impact on changing perceptions and behavior, because the interactions are relatively novel and rare. Moreover, many counties in Liberia have been studied, but Grand Kru County is one that has not received much scholarly attention. Additionally, given that the presence of the UN Mission in Liberia (UNMIL) may be a factor that inhibits generalization, Grand Kru County has historically had very minimal UN presence and did not have any UN contingents stationed in the county during the time

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[17]A county is the country sub-unit much like individual states in the U.S.

of the study.

Liberia (and Grand Kru county) is not unlike other post-conflict countries or territories. The DRC, Haiti, Timor-Leste, to name a few, are also considered post-conflict countries with weak or fragile governance capacity. Thus, the arguments tested here may be applicable to other countries or parts of countries that resemble Liberia and Grand Kru County.

6 FIELD EXPERIMENT: RESEARCH DESIGN

Testing the conjectures above involves conducting two field experiments—one that tests the effect of reform-insiders delivering the message of police professionalization and one that tests the effect of reform-outsiders delivering the message of police professionalization.[18] As stated above, the experiments are not competing, thus they were conducted independently.

Reform-insiders, in this case, were professionalized police officers. Since 2005, the Liberian National Police (LNP) have undergone a massive professionalization scheme, where they replaced nearly their entire pre-war police force with new recruits who were vetted and who were required to meet certain standards.[19] They also underwent significant training at the Liberian National Police Training Academy (LNPTA). UNMIL's UN Police (UNPOL) division's sole task has been to mentor and supervise the LNP. In the past few years, the LNP have received bilateral training and aid from the U.S. State Department's Bureau of International Narcotics and Law Enforcement Affairs (INL), as well as from other bilateral arrangements from Swedish government, among others. All of these initiatives have served to better professionalize the police force. Even though the professionalization process has been in place since 2005, only recently are police being heavily deployed to rural parts of the country, which means that more citizens in rural parts of the country might start to receive services from professionalized police. Police officers who were selected for the experiment were chosen from the professionalized pool of new recruits, had similar personalities with one another, were all from the same ethnic group as community members from Grand Kru County, and were male.[20]

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[18]All parts of the study underwent an Internal Review Board process. On May 12, 2015, the Emory University IRB approved the project. The IRB is filed under IRB00073869.

[19]Around 80% of the police force was replaced (Karim and Gorman 2016). For more extensive, detail, see (Friedman and MacAulay 2011, Karim and Gorman 2016).

[20]See Appendix for details on selection process. This experiment was part of a larger project to understand the effect of police visits by male and female police officers. The female police officer part of the experiment is not

Reform-outsiders were civilians from the The Center for Applied Research and Training (CART) who carried out the survey.[21] Specifically, they were Liberian enumerators and researchers who stated that they were conducting surveys in Grand Kru County to help service provision to that county and asked questions about maternal health, security, and Ebola. They presented themselves as a neutral third party organization. Both police officers and civilian enumerators used the same script about police professionalization during their visits.[22]

Both experiments drew from a population of Grand Kru County, Liberia, of individuals (18 and older) that live in villages with more than 80 households. Using the 2008 census data from Liberia, there were forty-two villages in Grand Kru County that had 80 households or more. In total, twenty-five villages were randomly selected for both studies. The "reform-insider" experiment incorporated fifteen villages, whereas the "reform-outsider" experiment involved the remaining ten villages.

6.1 FIELD EXPERIMENT 1: "REFORM-INSIDER" TREATMENT

The treatment consisted of a message about police professionalization to community members delivered by two professionalized police officers. The officers memorized the script and alternated in delivering the message to the households. At the end of the visit, the officers gave the household cards with phone numbers of the local police. These cards were later used to identify whether individuals within the households had been treated. The officers spent between 1-3 days in each village.

The police officers visited randomly selected households in villages. The randomization was first conducted at the village level—selection of twenty-five from the potential forty-two from the census, then the random selection of fifteen for this experiment from the potential twenty-five. It was then conducted at the village quarter or neighborhood level. When the team arrived in the villages, they asked for a list of the quarters in the village.[23] Quarters are considered organized neighborhoods within villages. Quarters were randomly assigned to treatment and control, with at least two quarters randomly selected for treatment. On average, in the fifteen villages selected

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considered in this paper. However, it is important to note that the results below do not change if they are included.

[21] Now called the Center for Action, Research, and Training

[22] Both scripts can be found in the Online Appendix.

[23] All strangers must be given approval by village leaders to enter the village.

for treatment, each village included four quarters, and on average, each quarter contained about 56 households. Police officers did not leave their assigned quarters. Finally, using a random walk technique, the officers visited twenty households in the selected quarters. The total sample size for the treatment consisted of 300 households.

While the police visits were ongoing in the treated quarters, an enumerator interviewed randomly selected fifteen households in the control quarters (households that did not receive visits from the same village, but different quarters as where the police were located), resulting in a total of 225 control households. Table 1 provides the details of the experimental research design.

Table 1: Research Design for Reform-Insider Treatment

Group	Number of Villages	Households per village	Total Households
Control: No Infomration	15	15	225
Treatment: Information by Police Officers	15	20	300
Total	15	35	525

Three weeks after the officer visits, The Center for Applied Research and Training (CART) surveyed the treated households. The survey included questions that ranged from maternal health care and Ebola, as well as a section on security. The enumerators presented themselves as an independent organization doing a survey on health, security, and Ebola in the county. The enumeration team surveyed the exact same households as those that were visited by police officers. They were able to recognize the households based on maps that were drawn by the enumerators that were part of the police team and by a code that was written discreetly adjacent to the household.[24] All treated households were located.[25]

It is possible that spillover took place, as community members not assigned to treatment may have seen or even interacted with police officers. However to the extent that spillover occurred, it could take the form of attenuation bias. If spillover occurred—individuals that were not treated experienced some form of interaction with police officers—then this could minimize the treatment effect if the expectation is that officers' visits to villages is looked upon favorably by community members. It is also possible that there might be backlash among community members who did not

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[24]This practice of coding houses for the census and other nationwide surveys is common in Liberia.

[25]Thus, attrition was not a problem in the study, as the enumerators were able to locate all of the treated households.

directly receive the treatment—a delivery of the message by the police officers at their home.[26]

Nevertheless, treatment was assigned at the quarter level precisely to avoid spillover. Quarters are generally far apart, which means that those in a control quarter would not interact with police officers in a treated quarter. Moreover, control surveys were done at the time of the treatment, so that spillover such as community members talking to their neighbors about the police visits would not affect results. In other words, had surveys been conducted three weeks later (as was done for the treated households), then there would have been ample time for treated household members to speak about the visits to those in the control group. The simultaneous implementation of the survey may have avoided the spillover problem. However, in order to alleviate concerns about potential bias from the control group survey, the treated households are compared to an alternative control group from the second experiment to ensure that results are consistent across two different control group pools.

When the police officers visited the households, they treated the entire household; they spoke to the entire household when they visited them. However, one-sided non-compliance may have been an issue if those that received the treatment were not home when the survey was conducted, or if the random selection of the individual in the household for the survey did not yield a person that was treated (present during the officer visit). In other words, not all the household members may have been treated. Based on the data, however, 96% of the respondents were treated (said they had experienced a visit by a police officer in the last month) or were present during the officers' visits, and 98% could produce the card that the LNP officers gave them with phone numbers. Of those that said they did not meet the police (13 people), one person said they had not heard of the police coming to their household. Thus, the threat to one-sided non-compliance is quite low, with only one person reportedly not experiencing the treatment in any way. Regardless, the study uses an intent-to-treat research design. This approach summarizes the net impact of the treatment, and not of the treatment that was assigned.

6.2 FIELD EXPERIMENT 2: "REFORM-OUTSIDER" TREATMENT

The treatment was the delivery of the exact same message about police professionalization, verbatim, that the police delivered to community members but civilian enumerators delivered the mes-

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[26]This type of spillover is explored below.

sage. These civilians were researchers and enumerators from CART and were trained and briefed on police reforms prior to the implementation of the treatment and were also of the same ethnic group as the community members. The information was included as a part of the survey carried out in the villages.[27] The same randomization scheme that applied for Experiment 1 applied for this experiment as well. Randomization first occurred at the village level—selection of twenty-five from the potential forty-two from the census, then the random selection of ten for this experiment from the potential twenty-five. It was then conducted at the village quarter or neighborhood level. When the team arrived in the villages, they asked for a list of the quarters. The enumerators randomly chose quarters in which to implement the surveys. Using a random walk technique, the enumerators visited fifty households in each village, with twenty surveys containing the information treatment and thirty surveys containing no information on reforms. At the selected households, the surveys with the script/information treatment were randomly distributed to different enumerators in each village. Thus, the experiment was double-blind, because enumerators did not know if each household would be treated or not until they reached that part of the survey. Table 2 provides the details of the experimental research design.

   Spillover is not a consideration in this experiment because the surveys of the control and treated households were conducted simultaneously.

Table 2: Research Design for Information Treatment

Group	Number of Villages	Households per village	Total Households
Control: No Information	10	30	300
Treatment: Information by Civilians	10	20	200
Total	10	50	500

6.3 OUTCOMES

The survey included questions that pertained to the four governance outcome indicators mentioned above.[28] The groupings were pre-specified in a pre-analysis plan developed before either of the

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[27] The exact same survey as Field Experiment 1: Exposure.

[28] The questions were asked in Liberian English and are presented below as they were asked.

experiments were conducted.[29] In general, most questions were asked in statement form and respondents were asked whether they agree or disagree. [30] Table 3 and Table 4 show the distribution of the outcome variables used in the two experiments.[31]

For restraint, the questions were asked in the negative, so the expectation below is that there should be negative associations between the treatment and the questions.

Restraint

- The police can sometimes steal things from me or the community

- The LNP can sometimes do man woman business with women in the community even if the woman does not agree [rape]

- The LNP are causing problems (shouting, yelling, causing confusion etc.) if they come into the community

- The LNP sometimes behave like criminals

- The LNP are corrupt and eating money

As with restraint, the questions for inclusivity were also asked in the negative, so the expectation below is that there should be negative associations between the treatment and the questions.

Inclusivity

- The LNP discriminates based on religion/ethnicity/tribe

- The LNP treat women unfairly/badly

The questions related to rule of law capture both knowledge about laws and also one question about the perception of formal Liberian law. The expectation is that there should be positive

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[29]See EGAP registration number: 20151128AA. Some modifications were made based on data and are explained below.

[30]All models below drop "refuse to answer" (of which there were very few) and "I don't know" answers. Future analysis will unpack whether there were fewer "I don't know" answers in the treated group.

[31]Note that there are 501 surveys conducted in Experiment 2 and not 500. Also, the "I dont know" and "Refuse to answer" categories are removed. Later analysis will incorporate them into the study.

relationships between the treatment and the questions.

Rule of Law

- Is beating your wife or woman a crime?

- Is it a crime to do man/woman business with someone under the age of 18?

- Is it a crime for everyone to beat/jump on a person that commits a crime (mob justice/violence)?

- Is it a crime if a man does not support his outside child?

- Is giving people sassywood (trial by ordeal) a crime?

- People from Liberian formal law side like the courts in this district and county or the County Commissioner can settle a palava better than the elders from the community

It is important to note that the questions on rule of law include one perceptional question and five questions on knowledge about rule of law. These questions were added in order to gauge whether the community members learned anything from the visits about the rule of law. By including these questions, it is also possible to assess whether respondents recalled information after being provided that information whether by police or civilians.

The questions on effectiveness ask about how well the police are able to provide protection and help the population. The expectation is that there should be positive relationships between the treatment and the questions.

Effectiveness

- I feel safer when the LNP are in the village.

- The LNP are good at catching criminals.

- There will be less crime in the community if the LNP come to the village.

- It is easy to get help from the LNP.

Table 3: Summary Statistics: Reform-Insider

Variable	Mean	Std. Dev.	N
The police can sometimes steal things from me or the community	0.38	0.48	502
The LNP sometimes behave like criminals	0.40	0.49	467
The LNP are causing problems if they come into the community	0.21	0.41	524
The LNP can sometimes do man woman business with women if the woman does not agree [rape]	0.12	0.32	508
There will be less crime in the community if the LNP come to the village	0.90	0.30	518
It is easy to get help from the LNP	0.57	0.50	525
The LNP are good at catching criminals	0.72	0.45	512
I feel safer when the LNP are in the village	0.80	0.40	525
The LNP treat women unfairly/badly	0.07	0.25	510
The LNP discriminates based on religion/ethnicity/tribe	0.36	0.48	462
The LNP are corrupt and eating money	0.57	0.50	527
Is beating your wife or woman a crime?	0.87	0.34	527
Is it a crime to do man/woman business with someone under the age of 18?	0.94	0.24	527
Is it a crime for everyone to beat/jump on a person that commits a crime (mob justice/violence)?	0.80	0.40	527
Is it a crime if a man does not support his outside child?	0.85	0.36	527
Is giving people sassywood a crime?	0.69	0.46	527
People from Liberian formal law side can settle a palava better than the elders from the community	0.62	0.49	498

Table 4: Summary Statistics: Reform-Outsider

Variable	Mean	Std. Dev.	N
The police can sometimes steal things from me or the community	0.27	0.44	404
The LNP sometimes behave like criminals	0.28	0.45	409
The LNP are causing problems if they come into the community	0.21	0.40	468
The LNP can sometimes do man woman business with women if the woman does not agree [rape]	0.10	0.30	394
There will be less crime in the community if the LNP come to the village	0.88	0.33	476
It is easy to get help from the LNP	0.58	0.49	472
The LNP are good at catching criminals	0.92	0.27	421
I feel safer when the LNP are in the village	0.85	0.36	501
The LNP treat women unfairly/badly	0.06	0.23	437
The LNP discriminates based on religion/ethnicity/tribe	0.29	0.45	451
The LNP are corrupt and eating money	0.69	0.46	407
Is beating your wife or woman a crime?	0.84	0.37	501
Is it a crime to do man/woman business with someone under the age of 18?	0.85	0.36	501
Is it a crime for everyone to beat/jump on a person that commits a crime (mob justice/violence)?	0.73	0.45	501
Is it a crime if a man does not support his outside child?	0.74	0.44	501
Is giving people sassywood a crime?	0.70	0.46	500
People from Liberian formal law side can settle a palava better than the elders from the community	0.64	0.48	474

One concern about the experiments may be bias due to the timing of when the survey was implemented. The survey for the "reform-insider treatment" was implemented three weeks after the treatment, whereas the survey for the "reform-outsider treatment" was implemented immediately after the treatment. For this reason, among others, the experiments are not comparable. If we believe the treatments to be equally "potent," then the timing of the "reform-outsider" survey might yield stronger effects because the survey questions on governance immediately followed.

The design of the "reform-outsider" experiment raises questions about whether the survey is testing perceptional change due to information or the ability for participants to recall information. To determine whether the responses in the "reform-outsider" experiment are measuring perceptional change or recall, the rule of law questions were included, which specifically test recall or the ability of participants to accurately retrieve or "recall" information they were just told. Because enumerators provided information about laws pertaining to domestic abuse, underage sexual relationships, mob justice, child support, and sassywood (trial by ordeal), the expectation is that community members should be able to voice correct answers about the rule of law. Indeed, if the respondents recall this information later on in the survey, then we might suspect that the answers are measuring recall and not perceptional change. However, if they are unable to recall answers, then responses may be due to changes in perception due to the information itself.

6.4 MODELS

The models use randomization inference. Two-tail tests are used in the analyses to assess whether delivery of the message by reform-insiders or reform-outsiders had a positive or negative effect on perceptions. Because the outcomes included more than one question on the survey or clusters of thematically similar questions, the Average Effect Size (AES) is also estimated.[32] This mitigates the possibility of Type I and Type II errors (Clingingsmith, Khwaja and Kremer 2009, Kling, Liebman and Katz 2007). In addition, logit models are presented with at table of predicted probabilities for individual questions.

All models are presented using village-level clustered errors. Conventional p-value levels are

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[32]The AES across J related dependent variables is given by $\tau = \sum_{j=1}^{J} \frac{\pi_j}{\sigma_j}$, where π_j is the average treatment effect on each dependent variable and σ_j is the standard deviation of dependent variable j in the control group. To test the null hypothesis of no average effect, the effects π_j are jointly estimated using seemingly unrelated regression. The J dependent variables are stacked to compute a variance-covariance matrix for testing the statistical significance of τ, the AES. For further details see Clingingsmith, Khwaja and Kremer (2009), Kling, Liebman and Katz (2007).

used with p-values that are below 0.05 for the AES and 0.10 for the logit models.[33] The data are presented in the appendix using covariates on the variables that do not balance as well as controls for if they were suspicious that the enumerators were a part of the Liberian National Police,[34] as well as if they know and LNP officer.

6.5 RANDOMIZATION CHECK

If randomization worked, we should expect that the characteristics of participants are similar across treatment and control groups and that a priori characteristics of respondents should not predict the treatment.[35] For the first experiment assessing the effect of message delivery by reform-insiders, Table 5 presents the coefficient estimates from a basic logic model predicting the effects of relevant observable characteristics on the probability of receiving the treatment.[36] It suggests that randomization worked for all variables except for two. The co-variate *Female* is negative and statistically significant, suggesting that police officers were less likely to reach women with their message. About 47% of the community members who were treated were women, whereas 53% of the community members who were treated were men. One reason for this could be that women were less likely to partake in the intervention because of cultural norms related to speaking to men and fear of male police.[37] If there are slightly more males than females in the sample, this could bias the results because males may be more likely to perceive the police positively. In another study by the author, however, which includes police visits by female police officer, no such discrepancy was found and the results below were consistent.[38]

The co-variate *Traditional Leader* is also negative and statistically significant suggesting that police were less likely to communicate a message to traditional leaders in the community. This might be due to the fact that upon entering the community, traditional leaders in the villages met with police officer to grant permission to conduct household visits. As such, it is possible that the police did not visit their households because they already interacted with them prior to the

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[33]The results below were tested with enumerators fixed effects, and they do not change.

[34]The latter questions was filled out by the enumerator after the survey was complete.

[35]A table of descriptive statistics of the co-variates are in the appendix along with p-values from t-tests comparing means between control and treatment groups.

[36]The appendix shows the descriptive statistics for the treated and controls groups in more depth.

[37]This discrepancy is not due to survey bias because there is no discrepancy in the reform-outsider experiment nor in another paper by the author that looked at differences between male and female policing (Karim 2016).

[38]In Karim (2016), male and female police visits are compared to the control group and the results below do not change.

door-to-door visits.

Table 5: Randomization Check: Reform-Insider Experiment

	Dependent variable:
	Received Treatment
Female	−0.33** (0.16)
Age	−0.001 (0.01)
Traditional Leader	−1.02** (0.32)
Farm	0.25 (0.29)
Head of Household	0.11 (0.22)
Household Number	0.07 (0.04)
Born in the Village	−0.28 (0.28)
Cognitive Ability	−0.35 (0.28)
Victim of Crime	−0.07 (0.51)
Experienced War Trauma	−0.23 (0.20)
Own Land	−0.13 (0.21)
Kru	−0.74 (0.53)
Grebo	0.52 (0.55)
Christian	1.22 (1.33)
Can Read	0.07 (0.19)
Constant	−0.85 (1.27)
Observations	525
Log Likelihood	−328.71

Note: $^*p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$
Village Level Clustered Standard Errors

Unfortunately, the data for the balance checks come from the surveys implemented post-treatment (in the case of Experiment 1). A baseline survey was not possible for several reasons including that it would have aroused suspicion among community members.[39] The communities are not used to receiving visits by strangers,[40] which means that if they received a baseline survey, and then either visits by police and/or and end line survey, community members may have assumed that they were related. The study took great care to make every attempt to disassociate the police visits form the survey—adding another survey would have made it difficult to decouple the surveys from the treatment.

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[39]About 10% of the respondents were suspicious about the survey being related to the police visit.
[40]About 62% have never interacted with community outsiders.

For the second experiment, assessing the effect of message delivery by reform-outsiders, Table 6 presents the coefficient estimates from a basic logic model predicting the effects of relevant observable characteristics on the probability of receiving the treatment.[41] It suggests that randomization worked for all variables except for *Traditional Leader*, *Head of Household*, and *Land*. The same explanation as above could be true for *Traditional Leader* as the enumerators also had to gain permission to enter the community from traditional leaders. About 66% of control households owned land whereas 53% of treated households owned land. The higher number of land owners might be due to the way that randomization occurred at the quarter level. It is possible that in some neighborhoods in the villages, there were fewer land owners and those were the ones randomly selected. Treated household had on average 6 members whereas control household had almost 7.[42] It is not immediately clear how the difference in household number might bias results.

Table 6: Randomization Check: Reform-Outsider Experiment

	Dependent variable:
	Received Treatment
Female	0.10 (0.23)
Age	-0.001 (0.01)
Traditional Leader	-0.76** (0.34)
Farm	-0.22 (0.30)
Head of Household	0.28 (0.32)
Household Number	-0.08*** (0.03)
Born in the Village	0.22 (0.15)
Cognitive Ability	0.16 (0.25)
Experienced War Trauma	-0.10 (0.23)
Own Land	-0.48*** (0.17)
Kru	-0.08 (0.58)
Grebo	-0.31 (0.43)
Christian	0.14 (0.27)
Can Read	-0.06 (0.11)
Constant	0.43 (0.73)
Observations	525
Log Likelihood	-323.14
Note:	*p<0.1; **p<0.05; ***p<0.01
	Village Level Clustered Standard Errors

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[41]The appendix shows the descriptive statistics for the treated and controls groups in more depth.

[42]This difference is not a result of higher numbers of children in control households as the number of children in control and treated households are balanced.

7 RESULTS

We now turn to the set of results for Field Experiment 1: "Reform-Insider." Looking first changes between the control and treated groups, we observe that across the board, the treatment improved perceptions of restraint, inclusiveness, rule of law, and effectiveness. Those that were given information about police professionalization by police were more likely to perceive the police as restrained, inclusive, effective, and know about and believe in the rule of law. The largest effects were for restraint, where police visits reduced perceptions of police stealing by 53%, of police raping by 17%, police behaving like criminals by 34%, police causing problems by 21%, and police behaving corruptly by 15%. Simply assessing these changes does little to demonstrate the magnitude of the effect. As such, Table 8 shows that the Average Effect Size (AES), which is statistically significant for all outcomes. There is a 69% of a standard deviation increase in perceptions of restraint, a 63% of a standard deviation increase in inclusiveness, a 22% increase in a standard deviation in the rule of law, and a 58% increase in a standard deviation increase in effectiveness. This means that the average person in the experimental group was 0.69 and 0.63 standard deviations below the average person in the control group with respect to perceiving the police as abusive and discriminatory and unfair; and they were 0.22, and 0.58 standard deviations above the average person in the control group with respect to knowledge and perceptions about the rule of law and police effectiveness.

The ITT and the AES estimated do not consider potential confounding co-variates. Using logit model which include control variables on which the treatment and control groups did not balance, we can look at the effects of the treatment on individual outcomes.[43] The treatment is statistically significant for all the outcomes except knowledge about mob justice and perceptions of less crime in the community with police presence.[44] The predicted probabilities are presented in Table 9. Again, they show that for most individual variables, police visits had a positive influence on perceptions of governance.

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[43]These include female, traditional leader, Kru, Grebo, and also whether the individuals had family or friends in the police force, whether they had heard about the treatment, and whether they were suspicious of the treatment being related to the survey.

[44]The full tables can be found in the Appendix.

Table 7: Impact of "Reform-Insider" Treatment

Variables	Control Mean	Treatment Mean	Percent Change	p-value
Restraint				
Police Steal	0.69 (0.03)	0.16 (0.02)	-0.53 (0.04)	0.00
Police Rape	0.22 (0.03)	0.05 (0.01)	-0.17 (0.03)	0.00
Police Like Criminals	0.63 (0.04)	0.26 (0.04)	-0.38 (0.04)	0.00
Police Cause Problems	0.33 (0.03)	0.13 (0.02)	-0.21 (0.04)	0.00
Police Corrupt	0.66 (0.04)	0.52 (0.03)	-0.15 (0.05)	0.002
Inclusiveness				
Police discriminate	0.64 (0.03)	0.15 (0.02)	-0.49 (0.04)	0.00
Police are unfair to women	0.09 (0.02)	0.05 (0.01)	-0.04 (0.02)	0.06
Rule of Law				
Beat Wife is Crime	0.83 (0.03)	0.89 (0.02)	0.06 (0.03)	0.02
Sex with Under age is Crime	0.89 (0.02)	0.98 (0.01)	0.09 (0.02)	0.00
Mob Justice is Crime	0.80 (0.03)	0.80 (0.02)	0.00 (0.04)	0.92
No Child Support is Crime	0.77 (0.03)	0.91 (0.02)	0.15 (0.03)	0.00
Sassywood is Crime	0.66 (0.03)	0.72 (0.03)	0.05 (0.04)	0.19
Formal Law Better	0.49 (0.04)	0.70 (0.03)	0.21 (0.04)	0.00
Effectiveness				
Less Crime	0.85 (0.02)	0.93 (0.01)	0.08 (0.03)	0.004
Catch Criminal	0.54 (0.03)	0.85 (0.02)	0.31 (0.04)	0.00
Police Help	0.27 (0.03)	0.78 (0.02)	0.51 (0.04)	0.00
Feel Safer	0.72 (0.03)	0.85 (0.02)	0.13 (0.04)	0.00

Table 8: Average Effect Size (Reform-Insider): Perceptions of Restraint, Inclusiveness, Rule of Law, and Effectiveness

	(1)	(2)	(3)	(4)
Restraint	**-0.69*** (0.12)**			
Police Steal	-0.57*** (0.08)			
Police Rape	-0.15*** (0.04)			
Police Like Criminals	-0.43*** (0.06)			
Police Cause Problems	-0.31*** (0.08)			
Police Corrupt	-0.12** (0.05)			
Inclusiveness		**-0.63*** (0.12)**		
Police discriminate		-0.51*** (0.09)		
Police are unfair to women		-0.05* (0.03)		
Rule of Law			**0.22*** (0.06)**	
Beat Wife is Crime			0.08** (0.03)	
Sex with Under age is Crime			0.09*** (0.02)	
Mob Justice is Crime			0.01 (0.05)	
No Child Support is Crime			0.15*** (0.05)	
Sassywood is Crime			0.03 (0.06)	
Formal Law Better			0.21*** (0.08)	
Effectiveness				**0.58*** (0.09)**
Less Crime				0.09 (0.03)
Catch Criminal				0.30*** (0.07)
Police Help				0.50*** (0.05)
Feel Safer				0.16*** (0.05)
Observations	429	497	498	502

Note:
*p<0.10; **p<0.05; ***p<0.01
Standard Errors clustered at the village level

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Table 9: Predicted Probabilities (Reform-Insider): Restraint, Inclusiveness, Rule of Law, and Effectiveness

	Predicted Probabilities:		
	Perceptions of Restraint, Inclusiveness, Rule of Law, and Effectiveness		
Variables	Control	Treatment	Change in Percentage
LNP Steal	0.67 (0.48–0.85)	0.16 (0.10–0.22)	−0.47 (−0.66− −0.28)
LNP Rape	0.16 (0.07–0.24)	0.05 (0.01–0.09)	−0.12 (−0.20− −0.03)
Like Criminal	0.57 (0.38–0.75)	0.28 (0.22–0.35)	−0.27 (−0.48− −0.06)
Cause Problem	0.26 (0.14–0.38)	0.13 (0.07–018)	−0.14 (−0.27− −0.01)
LNP Corrupt	0.66 (0.54–0.79)	0.52 (0.45–0.59)	−0.14 (−0.27− −0.11)
LNP Discriminate	0.59 (0.42–0.77)	0.16 (0.10–0.22)	−0.41 (−0.62− −0.20)
Law Against Beating Wife	0.82 (0.76–0.89)	0.91 (0.87–0.94)	0.09 (0.01–0.16)
Law for Child Support	0.73 (0.62–0.84)	0.93 (0.89–0.97)	0.21 (0.08–0.33)
Law Against Sassywood	0.62 (0.51–0.72)	0.75 (0.70–0.80)	0.13 (−0.003–0.27)
Believe in formal law	0.37 (0.24–0.50)	0.79 (0.70–0.88)	0.38 (0.23–0.52)
Less Crime	0.87 (0.82–0.92)	0.93 (0.89–0.97)	0.06 (−0.01–0.13)
Catch Criminal	0.60 (0.45–0.76)	0.84 (0.79–0.89)	0.23 (0.10–0.37)
Police Help	0.30 (0.21–0.39)	0.79 (0.68–0.91)	0.45 (0.29–0.61)

Note:

Predicted Probabilities with 95% CI
Variables included in the model include female, kru, grebo traditional leader
know someone in LNP, heard about LNP visits, and suspicious
which are held at their means

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The large effect size and consistent statistically significant relationships between the treatment and perceptions of restraint, inclusiveness, rule of law, and effectiveness may give pause for concern. In particular, the results need validation because of the potential for spillover. In order to check the robustness of these results, we assess the impact of the treatment on an independent, alternate control group—the same control group as the reform-outsider treatment (below). This control group consists of those surveyed in the other ten villages, who were not a part of the first experiment.[45] There is no reason to believe that spillover would be a concern when comparing these two groups.

Table 10 shows the averages of the treated and alternative control group. Based on the table, the treatment improved perceptions of restraint, inclusiveness, and rule of law, albeit the effects are much smaller than when using the original control group. Moreover, unlike the original experiment, the treatment leads to mixed perceptions about effectiveness, with no consistency among the questions. These results are further corroborated in Table 11, which shows the AES. Again, there is evidence for enhanced perceptions of restraint and rule of law even though the effect sizes are much smaller than the AES estimates from the original experiment. When using logit models that include co-variates, these results are further corroborated.[46] The results show a decrease in perceiving the police as likely to steal (-11%) and as corrupt (-13%)—both related to restraint. It also led to a decrease in the likelihood of perceiving the police as discriminatory (-17%) and an increase in the likelihood of knowing about child support laws (9%).[47]

One reason for the smaller effect size could be because of "reverse spillover" in the original experiment. In other words, those in the control group knew about the treatment and developed negative opinions of the police precisely because they did not receive the treatment. They may have perceived the absence of police visits as a rebuke to their household. Moreover, if reform-insiders are perceived as service providers (in addition to messengers), those that did not receive the messenger might have perceived themselves as losing out on a government service. It is possible

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[45]The balance checks between this alternative control group and the treated group can be found in the appendix. Unfortunately, balance is not very good when comparing these groups. As such the models include unbalanced co-variates.

[46]Models included female, Kru, Grebo, Christian, traditional leadership, land, farm, born in the same town, current victim, war trauma, heard of police, and know someone in the police. They were tested using both village level fixed effects and standard errors clustered at the village level.

[47]The logit model without village level fixed effects provide support for an increased likelihood in knowing about domestic violence laws, underage sex, and perceiving the formal law to be better. However, including village level fixed effects, the significance goes away.

Table 10: Impact of "Reform-Insider" Treatment (Alternative Control)

Variables	Alternative Control Mean	Treatment Mean	Percent Change	p-value
Restraint				
Police Steal	0.29 (0.03)	0.16 (0.02)	-0.13 (0.04)	0.00
Police Rape	0.09 (0.02)	0.05 (0.01)	-0.05 (0.01)	0.04
Police Like Criminals	0.28 (0.03)	0.26 (0.03)	-0.02 (0.02)	0.56
Police Cause Problems	0.21 (0.02)	0.13 (0.02)	-0.08 (0.03)	0.01
Police Corrupt	0.78 (0.03)	0.52 (0.03)	-0.26 (0.04)	0.00
Inclusiveness				
Police discriminate	0.34 (0.03)	0.16 (0.02)	-0.18 (0.04)	0.00
Police are unfair to women	0.03 (0.01)	0.05 (0.01)	-0.02 (0.02)	0.30
Rule of Law				
Beat Wife is Crime	0.82 (0.02)	0.89 (0.02)	0.07 (0.03)	0.01
Sex with Under age is Crime	0.88 (0.02)	0.98 (0.01)	0.10 (0.02)	0.00
Mob Justice is Crime	0.73 (0.03)	0.80 (0.02)	0.07 (0.03)	0.05
No Child Support is Crime	0.81 (0.02)	0.91 (0.02)	0.10 (0.03)	0.00
Sassywood is Crime	0.73 (0.03)	0.72 (0.03)	-0.01 (0.04)	0.74
Formal Law Better	0.64 (0.03)	0.70 (0.03)	0.06 (0.04)	0.09
Effectiveness				
Less Crime	0.90 (0.02)	0.93 (0.01)	0.3 (0.02)	0.24
Catch Criminal	0.93 (0.02)	0.85 (0.02)	-0.08 (0.03)	0.002
Police Help	0.60 (0.03)	0.78 (0.02)	0.18 (0.04)	0.00
Feel Safer	0.91 (0.02)	0.85 (0.02)	-0.05 (0.03)	0.03

Note: *Each item in the third column represents the ITT change.*

Table 11: Average Effect Size (Reform-Insider) with Alternative Control Group

	(1)	(2)	(3)	(4)
Restraint	**-0.22*** (0.07)**			
Police Steal	-0.15*** (0.05)			
Police Rape	-0.08** (0.04)			
Police Like Criminals	-0.05 (0.04)			
Police Cause Problems	-0.04 (0.04)			
Police Corrupt	-0.15*** (0.05)			
Inclusiveness		**-0.09 (0.08)**		
Police discriminate		-0.11*** (0.04)		
Police are unfair to women		-0.01 (0.02)		
Rule of Law			**0.18*** (0.06)**	
Beat Wife is Crime			0.07* (0.04)	
Sex with Under age is Crime			0.11*** (0.02)	
Mob Justice is Crime			0.06 (0.04)	
No Child Support is Crime			0.10*** (0.04)	
Sassywood is Crime			-0.02 (0.04)	
Formal Law Better			0.06 (0.07)	
Effectiveness				**-0.09 (0.08)**
Less Crime				0.04** (0.02)
Catch Criminal				-0.09*** (0.03)
Police Help				0.14*** (0.05)
Feel Safer				-0.10*** (0.03)
Observations	412	525	578	541

Note:

that members of the control group knew about the police visits because of the community entry process and because if strangers come to visit a village, this is a rare event and word travels fast. Indeed, there were forty-five respondents in the original control group who stated that they met a police officer in the past month. It is precisely these respondents who may have believed that they were entitled to the service (police visit) but were denied this benefit. As such, they might be *more* inclined to view the police negatively, thereby decreasing the baseline level of positive perceptions for the control group. Compared to the alternative control group where this "reverse spillover" was not a problem, the baseline levels of beliefs about the police were indeed much lower. Additionally, when looking at those in the original control group who heard about the police, they were much less likely to perceive the police as restrained, inclusive, and effective.[48] This analysis suggests that when the messenger is an insider, the messenger may also be perceived as a service provider, and if individuals know that a service is being provided, but do not not benefit from it, it could lead to a decrease in positive perceptions of governance.

Overall, the first experiment shows us that reform-insiders can improve perceptions of governance. There was consistent evidence regardless of the control group demonstrating that perceptions of restraint, inclusiveness (at least ethnic discrimination), and to some extent rule of law improved when community members received visits from the professionalized police or reform-insiders. It might be possible, however, for the equivalent results with reform-outsiders. We turn next to the second experiment.

In Field Experiment 2: "Reform-Outsider," the outcomes are measured in the same way, using the same questions as above. Recall that the treatment here is information provided by civilian research assistants during the implementation of a survey. From Table 13, we immediately observe that no outcomes are statistically significant, except for effectiveness, but it is in the wrong direction. Inclusiveness and rule of law are also in the wrong direction.

One exception may be that information delivered by civilians reduces perceptions of corruption. The individual question about corruption is significant and has an AES of 0.20. This means that there is a 20% of a standard deviation decrease in perceptions of corruption when civilians inform citizens about police reforms. Using the logit model, those that received information about police reforms were 18% less likely to perceive the police as corrupt. The effect is stronger for

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[48]See Appendix "Reform-Insider Comparison with Alternative Control Group" section for coefficient table.

the reform-outsider messenger than the reform-insider messenger, suggesting that perhaps neutral third parties are more effective in convincing citizens that government institutions are less corrupt after reforms.

Despite the positive findings for corruption, other individual questions related to the rule of law and effectiveness were significant, but in the opposite direction as expected. Fewer people who received information about police professionalization from civilians knew about the laws pertaining to underage sex, child support, and mob justice. These negative results mean that individuals were not able to recall information they were just told. The underlying implication could be that citizens simply don't remember information provided by reform-outsiders, even in the very short-term.

Treated individuals were less likely to feel safe. This might be due to the fact that citizens generally feel safer with police presence, but if they are told that the police are reformed and not present in their community, this could lead them to doubt their effectiveness. In other words, for perceptions of effectiveness to change, there must be some display of police. With only information, citizens may feel cheated out of the service.

The overall evidence suggests that when civilians provide information through canvassing about police professionaization, there is no effect on changing attitudes about governance.

Table 12: Impact of "Reform-Outsider" Treatment

Variables	Control Mean	Treatment Mean	Percent Change	p-value
Restraint				
Police Steal	0.29 (0.03)	0.22 (0.03)	-0.07 (0.04)	0.11
Police Rape	0.09 (0.02)	0.10 (0.02)	0.01 (0.03)	0.80
Police Like Criminals	0.28 (0.03)	0.28 (0.04)	-0.004 (0.05)	0.92
Police Cause Problems	0.21 (0.02)	0.20 (0.03)	-0.01 (0.04)	0.78
Police Corrupt	0.78 (0.03)	0.58 (0.04)	-0.20 (0.05)	0.00
Inclusiveness				
Police discriminate	0.34 (0.03)	0.22 (0.03)	-0.12 (0.04)	0.01
Police are unfair to women	0.03 (0.01)	0.09 (0.02)	0.06 (0.01)	0.01
Rule of Law				
Beat Wife is Crime	0.82 (0.02)	0.87 (0.02)	0.05 (0.03)	0.13
Sex with Under age is Crime	0.87 (0.02)	0.82 (0.03)	-0.06 (0.03)	0.06
Mob Justice is Crime	0.73 (0.03)	0.72 (0.03)	0.01 (0.04)	0.77
No Child Support is Crime	0.81 (0.02)	0.63 (0.03)	-0.18 (0.04)	0.00
Sassywood is Crime	0.73 (0.03)	0.66 (0.03)	0.07 (0.04)	0.08
Formal Law Better	0.64 (0.03)	0.65 (0.03)	0.1 (0.04)	0.77
Effectiveness				
Less Crime	0.90 (0.02)	0.88 (0.02)	-0.06 (0.03)	0.02
Catch Criminal	0.93 (0.02)	0.91 (0.02)	-0.02 (0.03)	0.56
Police Help	0.60 (0.03)	0.54 (0.02)	-0.05 (0.05)	0.23
Feel Safer	0.91 (0.02)	0.75 (0.03)	-0.16 (0.03)	0.00

Note: *Each item in the third column represents the ITT change.*

Table 13: Average Effect Size (Reform-Outisder): Perceptions of Restraint, Inclusiveness, Rule of Law, and Effectiveness

	(1)	(2)	(3)	(4)
Restraint	**-0.15 (0.10)**			
Police Steal	-0.04 (0.07)			
Police Rape	-0.07 (0.04)			
Police Like Criminals	-0.02 (0.07)			
Police Cause Problems	0.02 (0.05)			
Police Corrupt	-0.22*** (0.06)			
Inclusiveness		**0.10 (0.13)**		
Police discriminate		-0.05 (0.08)		
Police are unfair to women		0.06* (0.03)		
Rule of Law			**-0.10 (0.08)**	
Beat Wife is Crime			0.05 (0.04)	
Sex with Under age is Crime			-0.06** (0.03)	
Mob Justice is Crime			-0.17*** (0.05)	
No Child Support is Crime			-0.16*** (0.05)	
Sassywood is Crime			-0.06 (0.06)	
Formal Law Better			0.10 (0.05)	
Effectiveness				**-0.22** (0.11)**
Less Crime				-0.002 (0.04)
Catch Criminal				-0.01 (0.03)
Police Help				0.03 (0.04)
Feel Safer				-0.17*** (0.05)
Observations	259	405	471	397

Note: *p<0.05; **p<0.01; ***p<0.001
Standard Errors clustered at the village level

8 DISCUSSION AND CONCLUSION

The article started out by asking whether the messenger matters in the provision of information about reforms in weak, post-conflict states. Based on the two field experiments conducted here, the answer is yes. The evidence suggests that when the messenger is an insider, there were improved perceptions of restraint, inclusiveness (discrimination), and citizens were more likely to know about child support. In contrast, when the messenger was an outsider, no improved perceptions or knowledge about laws ensued, with perhaps the one exception of a reduction in the perception of police corruption.

There are two potential explanations for why reform-insiders may be more effective than reform-outsiders in enhancing perceptions about governance. First, reform-insiders embody the reform, which means that citizens see successful implementation of a reform when they interact with a reform-insider. In this way, citizens' concerns about government cheap talk about reform implementation may be alleviated. For example, governments may be especially reluctant to implement reforms that are restraining because it diminishes the state's power (Toft 2010), which means that they may announce restraint-increasing reforms to the public, but not actually carry them out. As an illustration, governments might state that they are implementing civilian oversight bodies to preside over the security sector, but never actually create one. Citizens may fear this type of cheap talk. But if governments did actually create a civilian oversight board, if citizens meet with members of the oversight committee, in addition to receiving information about the creation of the body, they recognize that the new institution has actually been created. The same could be true for professionalization reform, as seen in this case.

Second, it is possible that reform-insiders represent service delivery and expertise. When governments expand their reach, they usually do it through personnel. As such, police officers, bureaucrats, and politicians represent the government and when they interact with civilians in this capacity, they are providing the service of their office to the public. In the study, this was most clearly seen in the first experiment, where lower baseline levels of perceptions of governance in the control group may have been due to the fact that citizens knew the police were in the community, but did not receive information from them in the form of a household visit (service-delivery). Moreover, it is possible that reform-insiders were seen as experts on the reform compared to reform-

outsiders. As experts and service providers, they could better answer questions and perhaps even do something about the problems in the community. More questions were asked to police officers than civilians.[49] Survey enumerators also asked respondents if they had any information they wanted to share with the police; only 0.71% provided information to the civilians. This is compared to 25% who provided information to the police. Thus, reform-insiders are perhaps seen as a more credible messenger because of their perceived expertise.

This latter point is important because it signals that neutrality may not be an important factor in trusting the messenger. Given that civilians were neutral actors, unaffiliated with the government, we might have expected perceptions to improve because individuals from war-torn countries may be less likely to trust a government source for information. Yet, this does not seem to be the case, at least, not in rural Liberia.

Neither information delivered by reform-insiders nor reform-outsiders consistently improved perceptions of police effectiveness. One reason for the null results could be because the simple provision of information about reforms does not demonstrate that the police (or other bureaucratic agency) is effective. Rather, citizens require experiencing an effective police force. In this sense, perceptions of effectiveness could be correlated with feelings of satisfaction after experiencing police services. If the police handle a crime or respond to an incident in a satisfactory way, the experience may elicit positive perceptions of effectiveness. Indeed, the policing literature suggests that when the police respond in a satisfactory manner, perceptions of the institution improve (Mazerolle et al. 2013).

Perceptions of restraint may have improved with police visits in this study because not only did the police provide information, but they also demonstrated that they are reformed with respect to restraint. When in the communities, the police did not steal items from civilians, behave like criminals, rape women, cause problems or ask for bribes— in the past, the police in Liberia did engage in this type of behavior. Moreover, they were not discriminatory in their information provision. In this way, they behaved like a professionalized police force during the information delivery process, and this may have been key in changing perceptions. Yet, even though they behaved in a professionalized way, they did not directly demonstrate their policing abilities during their visit, because the goal of their visits was not to handle crimes, but simply to

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[49]In both interactions, civilians were given the opportunity to ask questions.

provide information. As such, they did not display their capabilities as police officers; there was no signal to citizens that the police were more effective. Thus, information delivery accompanied by a demonstration of the reform may be key to improved perceptions.

Despite the positive results from the first experiment, several caveats are worth mentioning. First, the results from both experiments are only short-term effects. For the "reform-insider" experiment, the survey was conducted three weeks after the police visits, which means that the results should not be interpreted as having lasting effects. Given that the effects from most one-time interventions usually taper off over time (Paluck and Green 2009), another survey would have to be done to gauge whether the the police visits had medium-longer term effects. Yet, the study does provide some optimism, because short term effects are a necessary condition for longer-term effects. And, perhaps with more consistent message delivery by reform-insiders (such as a sustained community policing program)—rather than a one-off household visit—changes in perceptions could have a lasting impact.

Second, both experiments were conducted in villages in Grand Kru County, Liberia, which means that they may not be able to be generalizable to other counties and other countries. However, both Liberia and Grand Kru county were chosen strategically for this study. Liberia represents a weak post-conflict state and Grand Kru county is a rural, peripheral county with little state presence. It is precisely these types of states and counties where state building is most necessary to prevent future conflict and state failure. Thus, even though the results here cannot be applied more broadly, they do provide insight about whether certain types of messengers are important for enhancing perceptions of governance in the rural peripheries of weak post-conflict states.

Third, there is variation in the types of security sector reforms that states implement and variation in government reforms more generally. In addition to professionalization, states may decide to implement civilian oversight, conscription, add new operational units, disarm, demobilize and reintegrate soldiers, ethnic balance the forces etc.[50] Other studies would do well to test whether other types of security sector reforms, such as the creation of new operational units,[51] and whether different types of political, economic, and social reforms improves perceptions of

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[50] Currently, no body of literature had developed a way to categorize the different types of security sector reforms with the exception of the author's dissertation (Karim 2016).

[51] For example, a study by Blair, Karim, and Morse shows that patrols by elite units can sometimes improve outcomes in Liberia (Blair, Karim and Morse 2016).

restraint, inclusiveness, rule of law, and effectiveness through reform-insider (or reform-outsider) messengers. However, not all of these reforms may be amenable to an information campaign with reform-insiders. For example, it might not be prudent to send demilitarized soldiers door-to-door to advertise that DDR has been complete. Even with the above-mentioned civilian oversight body example, governments may not wish to spend scarce resources sending their few board members to canvass. The same could be true for reforms outside the security sector. Not all political, economic, or social reforms necessitate information campaigns. Thus, governments should be expedient in their decisions about which reforms to promote.

Finally, it is possible that the question and results presented in this article are not specific to weak-post-conflict countries. The messenger might matter in contexts outside of the scope conditions presented here. In "strong," authoritarian states, leaders need effective messengers and in non-conflict countries, crime, terrorism, and other violence serve to delegitimize the government's authority, which means that effective messengers are also needed. In this way, the theoretical underpinning of this article could be applied to a much broader context. Nevertheless, testing them first in weak, post-conflict states where information is often absent, and where information is distorted due to the war provides much needed information about how to make these states stronger and more legitimate in the eyes of their publics.

Given that post-conflict countries face unique governance challenges due to weakened institutions, understanding how best to overcome these challenges is important for scholars and policymakers alike. The key to overcoming these challenges may be through institution-building, but any important reform must be communicated to the masses in order to improve perceptions of governance. Given that the messenger matters, hopefully more work will shed light on how governments effectively communicate to the public.

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APPENDIX:
Does the Messenger Matter for Improving Post-Conflict Governance? Results from Two Field Experiments on Security Sector Reform in Liberia

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February 7, 2017

1 Appendix

1.1 List of Villages (Grand Kru County)

Table 1: List of Villages

Exposure Experiment	Information Treatment
Behwan City	Allawala
Behwan-B	Chenrinken
Garaway-A	Norkwia
Garaway-B	Filoken
Tuaken	Barclayville 2
Blebo B	Grandcess
Gbanken	Big Suehn
Barclayville 1	Beloken
Picnicess City	Deneken
Gennoyah	Juduken
Barforwin	
Weteken	
Newaken	
Doeswan	
Wilsonville	

1.2 BALANCE

Table 2: Balance: "Reform-Insider"

Variables	Control	Treatment	p-value
Age	35.70	34.50	0.46
Women	0.50	0.47	0.57
Kru	0.46	0.22	0.00
Grebo	0.47	0.72	0.00
Christian	0.99	0.99	0.18
Traditional Leader	0.10	0.05	0.03
Naval String	0.74	0.71	0.43
Own Land	0.41	0.42	0.78
Farm	0.34	0.39	0.24
Head of Household	0.34	0.34	0.82
Household Number	6.10	6.70	0.03
Read	0.59	0.60	0.84
Cognitive Ability	0.57	0.50	0.16
Experience Wartime Violence	0.50	0.47	0.59
Victim of Crime	0.13	0.15	0.49

The imbalance may be due to randomization at the quarter level. The imbalances for Kru and Grebo are concentrated in Doeswan, Barfowin, Barclayville, Behwan B, Genoyah City, Tuaken, Wilsonville, Garaway A, and Garaway B villages, which means that in these villages, quarters may have contained a higher number of Kru or Grebo. This appears to be the case due to the organization of households in many villages. In rural villages, members of the same tribe live in the same quarter.[1] With respect to the imbalanced co-variates, we can assess which villages in the sample of fifteen villages are imbalanced using t-test comparisons for each village. Imbalance in individual villages is limited to a few village for each variable. Four villages were imbalanced on household number (Barclayville, Newaken, Genoyah City, Garaway B). Given that some individual villages appear to be responsible for some imbalance, rather than more systematic imbalance across all villages, it is possible that the proportions would have balanced if a higher number of villages had been included.

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[1]This is reported from the enumerators on the ground.

Table 3: Balance: "Reform-Outsider"

Variables	Control	Treatment	p-value
Age	36.63	34.11	0.05
Women	0.49	0.51	0.62
Kru	0.40	0.44	0.31
Grebo	0.58	0.52	0.17
Christian	0.78	0.83	0.16
Traditional Leader	0.19	0.09	0.003
Naval String	0.87	0.87	0.77
Own Land	0.66	0.53	0.004
Farm	0.63	0.54	0.04
Head of Household	0.44	0.43	0.85
Household Number	6.6	6.1	0.03
Read	0.49	0.51	0.68
Cognitive Ability	0.55	0.56	0.73
Children	0.85	0.85	0.97
Experience Wartime Violence	0.68	0.64	0.28
Current Victim of Crime	0.24	0.15	0.02

1.3 CHOOSING OFFICERS FOR THE EXPOSURE EXPERIMENT

Officers for the project were chosen based on similarity in their personalities. This was done to ensure that the personality type of the officer did not drive the treatment effect. Or, in other words, this ensured that officers' personality type was controlled for in the experiment. The LNP in Monrovia provided fifteen Kru/Grebo-speaking officers for the study. Each of the fifteen officers was then filmed speaking to an audience about the role of the LNP. The videos were then shown to a group of Kru/Grebo people in Monrovia who rated the officers on different characteristics.[2] Based on the similarity of scores, four officers were chosen (two female and two male). In each group of two—female and male—one of the officers was a regular LNP officer, and the other one was a member of a specialized unit, the Police Support Unit (PSU). [3] While in reality, officers for community policing are not chosen based on the similarity of their personality, this procedure ensured the internal validity of the experiment. In addition to "controlling" for personality type, two officers were sent to each household to ensure that one officer would not drive the results.

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[2]After watching the video of each officer, they were asked: Do you want this officer to be your friend? Do you think the officer knows his or her work good? Are you afraid of the officer? Does the officer make you feel tired? Would you follow the officer?s instructions? Do you like this officer? Would you talk to this officer if they came to your home? Would you feel fine asking the officer questions? Would you feel fine reporting a crime to the officer? Should the officer be sent to Kru/Grebo-speaking communities to promote the LNP? List some words to describe the officer. The suggestions for the questions and their wording came from the Liberian enumeration team.

[3]The PSU are a tactical unit responsible for responding to larger-scale security threats such as riots.

1.4 Reform-Insider Comparison with Alternative Control Group

Table 4: Randomization Check: Alternative Control Group

	Dependent variable:
	Received Treatment
Female	−0.47** (0.18)
Age	0.03 (0.01)
Traditional Leader	−1.54*** (0.41)
Farm	−0.70** (0.32)
Head of Household	−0.21 (0.22)
Household Number	−0.01 (0.03)
Born in the Village	−0.48 (0.34)
Cognitive Ability	−0.36 (0.29)
Victim of Crime	−0.71 (0.26)
Experienced War Trauma	−0.08 (0.39)
Own Land	−0.62** (0.28)
Kru	−0.98 (0.65)
Grebo	−0.07 (0.60)
Christian	−4.55*** (1.03)
Can Read	0.11 (0.22)
Constant	−3.08* (1.57)
Observations	604
Log Likelihood	−321.71

Note: *p<0.1; **p<0.05; ***p<0.01
Village Level Clustered Standard Errors

Table 5: Balance: "Reform-Insider," Alternative Control

Variables	Control	Treatment	p-value
Age	36.63	34.49	0.06
Women	0.49	0.47	0.63
Kru	0.40	0.22	0.00
Grebo	0.58	0.72	0.00
Christian	0.78	0.99	0.00
Traditional Leader	0.19	0.05	0.00
Naval String	0.87	0.71	0.00
Own Land	0.66	0.42	0.00
Farm	0.63	0.39	0.00
Head of Household	0.44	0.34	0.01
Household Number	6.61	6.70	0.70
Read	0.49	0.60	0.01
Cognitive Ability	0.54	0.50	0.29
Experienced Wartime Violence	0.56	0.47	0.04
Victim of Crime	0.24	0.15	0.01

Table 6: Perceptions of Governance: Met with Police in the Last Month (Control Group Only)

Variables	Coefficient	p-value
Restraint		
Police Steal	1.39 (0.57)	0.02
Police Rape	2.61 (0.42)	0.00
Police Like Criminals	2.00 (0.61)	0.001
Police Cause Problems	0.82 (0.02)	0.003
Police Corrupt	2.00 (0.91)	0.03
Inclusiveness		
Police discriminate	0.82 (0.48)	0.09
Rule of Law		
Beat Wife is Crime	-0.86 (0.28)	0.002
Sex with Under age is Crime	-0.25 (0.57)	0.66
Mob Justice is Crime	-0.66 (0.25)	0.009
No Child Support is Crime	-0.91 (0.26)	0.001
Sassywood is Crime	-0.70 (0.29)	0.01
Formal Law Better	0.28 (0.34)	0.42
Effectiveness		
Less Crime	0.59 (0.50)	0.24
Catch Criminal	-2.11 (0.51)	0.00
Police Help	-2.37 (0.67)	0.00
Feel Safer	0.21 (0.35)	0.54
Note:	*Logit models with standard errors clustered at the village level*	

This table shows the coefficients for separate logit models predicting different perceptions of governance among the original control group only. (excludes those that were treated in the fifteen villages) The independent variable is those who heard about or met with a police officer. In other words, the independent variable is those who received "partial treatment," who knew that police officers were in the village but that did not receive information from them. Overwhelmingly, the independent variable predicts perceptions of *less* restraint and inclusiveness or more abuse/discrimination, less knowledge of rule of law, and less effectiveness. The analysis suggests that police interactions that are not perceived as service may drastically diminish positive perceptions of governance.

1.5 ADDITIONAL TABLES

Table 7: "Reform-Insider" Experiment: Restraint

	Dependent variable:				
	Steal	Rape	Like Criminals	Cause Problems	Corrupt
	(1)	(2)	(3)	(4)	(5)
Treatment	−2.41*** (0.29)	−1.83*** (0.40)	−1.76*** (0.25)	−0.89*** (0.28)	−0.77*** (0.24)
Female	−0.19 (0.26)	0.03 (0.31)	0.04 (0.22)	−0.53** (0.25)	0.02 (0.21)
Kru	−1.17** (0.57)	−0.84 (0.61)	−0.55 (0.49)	−1.23** (0.53)	−0.41 (0.49)
Grebo	−1.69*** (0.56)	−0.98 (0.61)	−0.54 (0.47)	−0.86* (0.52)	−0.24 (0.48)
Christian	0.66 (1.19)		0.94 (1.32)	0.03 (1.27)	1.53 (1.30)
Traditional Leadership	0.23 (0.51)	−1.62 (1.08)	−0.36 (0.43)	−0.15 (0.49)	0.33 (0.44)
Know Someone in LNP	−0.84*** (0.29)	0.14 (0.39)	−0.39 (0.24)	−1.42*** (0.33)	0.43* (0.23)
Suspicious of Enumerator	−0.44 (0.63)	0.03 (1.09)	1.64*** (0.52)	−1.45 (1.08)	0.07 (0.46)
Constant	2.99** (1.24)	0.46 (0.70)	1.23 (1.37)	1.39 (1.32)	0.49 (1.35)
Observations	501	507	466	523	461
Log Likelihood	−202.79	−143.08	−252.86	−213.09	−284.65
Akaike Inf. Crit.	451.58	330.15	551.71	472.19	615.31

Note: *p<0.1; **p<0.05; ***p<0.01
Logit model that includes village level fixed effects

Table 8: "Reform-Insider" Experiment: Inclusiveness

	Dependent variable:	
	Discriminatory	Treat Women Unfairly
	(1)	(2)
Treatment	−2.67*** (0.29)	−0.42 (0.45)
Female	−0.07 (0.24)	0.21 (0.38)
Kru	−0.31 (0.51)	−0.42 (0.79)
Grebo	−1.01** (0.50)	−0.50 (0.79)
Christian	−0.35 (1.36)	−1.38 (1.38)
Traditional Leadership	0.61 (0.47)	0.39 (0.70)
Know Someone in LNP	0.51* (0.28)	−0.37 (0.45)
Suspicious of Enumerator	1.28** (0.50)	0.84 (0.72)
Constant	2.61* (1.41)	−0.91 (1.51)
Observations	510	509
Log Likelihood	−232.66	−110.35
Akaike Inf. Crit.	511.32	266.71

Note: *p<0.1; **p<0.05; ***p<0.01
Logit model that includes village level fixed effects

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Table 9: "Reform Insider" Experiment: Rule of Law

			Dependent variable:			
	Beat Wife	Under Age Sex	Mob Justice	Child Support	Sassywood	Belief in Formal Law
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.86*** (0.30)	2.06*** (0.54)	−0.24 (0.27)	1.47*** (0.32)	0.38* (0.23)	1.36*** (0.25)
Female	−0.35 (0.27)	−1.18*** (0.44)	−0.51** (0.23)	−0.43 (0.27)	−0.25 (0.20)	0.11 (0.21)
Kru	−1.42 (1.07)	−0.01 (0.58)	−2.17** (1.05)	−0.28 (0.63)	−0.28 (0.48)	−0.29 (0.48)
Grebo	−1.88* (1.05)		−1.93* (1.04)	−0.24 (0.62)	−0.44 (0.47)	−0.11 (0.48)
Traditional Leadership	0.41 (0.58)	1.05 (1.08)	0.64 (0.57)	0.43 (0.60)	−0.07 (0.40)	0.75 (0.46)
Know Someone in LNP	−0.16 (0.31)	0.19 (0.52)	0.70** (0.27)	−0.28 (0.32)	−0.17 (0.22)	−0.97*** (0.25)
Suspicious of Enumerator		−0.17 (1.13)	0.20 (0.54)	−0.80 (0.57)	−0.15 (0.43)	0.82* (0.49)
Constant	2.69** (1.11)	2.20*** (0.83)	3.49*** (1.12)	0.92 (0.70)	0.95* (0.56)	1.34** (0.63)
Observations	526	526	526	526	526	497
Log Likelihood	−194.96	−87.03	−244.80	−191.44	−312.43	−279.84
Akaike Inf. Crit.	431.92	216.06	533.60	426.88	668.86	603.67

Note: *p<0.1; **p<0.05; ***p<0.01

Logit model that includes village level fixed effects

Table 10: "Reform Insider" Experiment: Effectiveness

	Dependent variable:			
	Less Crime	Catch Criminal	Police Help	Feel Safer
	(1)	(2)	(3)	(4)
Treatment	0.76** (0.36)	1.10*** (0.27)	2.34*** (0.26)	0.29 (0.27)
Female	0.33 (0.31)	−0.05 (0.24)	−0.15 (0.23)	0.05 (0.24)
Kru	1.35** (0.54)	0.37 (0.54)	0.04 (0.55)	0.45 (0.54)
Grebo	1.52*** (0.54)	0.74 (0.54)	−0.47 (0.54)	0.64 (0.54)
Christian		−0.26 (1.29)	1.19 (1.46)	−0.004 (1.30)
Traditional Leadership	0.15 (0.66)	−0.15 (0.50)	1.44*** (0.48)	−0.01 (0.52)
Know Someone in LNP	0.36 (0.37)	1.07*** (0.29)	1.15*** (0.26)	0.79*** (0.29)
Suspicious of Enumerator	−0.24 (0.80)	0.94 (0.79)	2.15** (1.06)	1.79* (1.05)
Constant	−0.13 (0.66)	−0.88 (1.35)	−3.16** (1.50)	0.34 (1.35)
Observations	517	511	524	524
Log Likelihood	−153.41	−221.41	−246.42	−228.19
Akaike Inf. Crit.	350.82	488.81	538.85	502.37

Note: *p<0.1; **p<0.05; ***p<0.01
Logit model that includes village level fixed effects

Table 11: "Reform Outsider" Experiment: Restraint

	Dependent variable:				
	Steal	Rape	Like Criminals	Cause Problems	Corrupt
	(1)	(2)	(3)	(4)	(5)
Treatment	−0.37 (0.27)	−0.23 (0.42)	0.09 (0.27)	−0.34 (0.29)	−0.79*** (0.26)
Age	0.01 (0.01)	0.02 (0.02)	0.02 (0.01)	0.01 (0.01)	0.03** (0.01)
Grebo	−0.62** (0.28)	−1.03** (0.47)	−0.55** (0.28)	−1.41*** (0.31)	0.004 (0.29)
Current Victim	0.91*** (0.31)	0.51 (0.51)	1.46*** (0.30)	−0.02 (0.36)	1.14*** (0.37)
War Trauma	−0.34 (0.28)	−0.54 (0.44)	−0.94*** (0.30)	−0.24 (0.30)	0.18 (0.29)
Household Number	0.02 (0.04)	−0.06 (0.07)	−0.04 (0.04)	−0.06 (0.05)	−0.04 (0.05)
Traditional Leader	0.002 (0.41)		0.17 (0.36)	0.26 (0.40)	0.49 (0.48)
Own Land	−0.14 (0.27)	1.02** (0.45)	−0.28 (0.28)	−0.04 (0.29)	0.50* (0.29)
Heard about Police Visit	1.37*** (0.49)	1.16 (0.76)	1.54*** (0.50)	1.86*** (0.51)	0.66 (0.67)
Know Someone in LNP	−0.59** (0.29)	−0.99** (0.50)	−0.46* (0.28)	−1.42*** (0.35)	−1.88*** (0.28)
Suspicious of Enumerator	0.35 (0.49)	1.73** (0.71)	−0.03 (0.54)	0.91 (0.57)	0.07 (0.50)
Constant	−0.16 (0.66)	−0.90 (0.97)	−0.17 (0.65)	1.21* (0.68)	0.23 (0.68)
Observations	402	392	407	466	405
Log Likelihood	−209.42	−98.96	−206.83	−190.20	−195.41
Akaike Inf. Crit.	460.83	237.92	455.67	422.40	432.83

Note: *p<0.1; **p<0.05; ***p<0.01

Logit model that includes village level fixed effects

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Table 12: "Reform Outsider" Experiment: Inclusiveness

	Dependent variable:	
	Discriminatory	Treat Women Unfairly
	(1)	(2)
Treatment	−0.53** (0.26)	0.98** (0.47)
Age	0.04*** (0.01)	−0.0000 (0.02)
Grebo	−0.79*** (0.28)	−0.48 (0.51)
Current Victim	0.82*** (0.29)	0.33 (0.60)
War Trauma	0.63** (0.29)	0.15 (0.49)
Household Number	−0.04 (0.04)	−0.03 (0.08)
Traditional Leader	−0.25 (0.35)	0.49 (0.73)
Own Land	0.47* (0.28)	−0.35 (0.49)
Heard about Police Visit	0.78 (0.48)	1.30* (0.75)
Know Someone in LNP	−0.04 (0.27)	−0.68 (0.55)
Suspicious of Enumerator	−0.19 (0.52)	−0.38 (1.12)
Constant	−2.06*** (0.66)	−2.59** (1.12)
Observations	449	436
Log Likelihood	−221.74	−87.02
Akaike Inf. Crit.	485.48	216.04

Note:	*p<0.1; **p<0.05; ***p<0.01
	Logit model that includes village level fixed effects

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Table 13: "Reform Outsider" Experiment: Rule of Law

	Dependent variable:					
	Beat Wife	Under Age Sex	Mob Justice	Child Support	Sassywood	Belief in Formal Law
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.47 (0.30)	−0.32 (0.28)	−0.26 (0.23)	−0.76*** (0.23)	−0.34 (0.21)	0.10 (0.23)
Age	−0.04*** (0.01)	0.02* (0.01)	−0.01 (0.01)	−0.001 (0.01)	−0.01 (0.01)	0.01 (0.01)
Grebo	0.56* (0.31)	0.27 (0.31)	−0.08 (0.25)	0.69** (0.27)	0.16 (0.24)	0.49* (0.25)
Current Victim	0.01 (0.39)	0.77* (0.43)	−0.74*** (0.27)	0.44 (0.33)	−0.54** (0.26)	−0.49* (0.28)
War Trauma	0.72** (0.32)	0.15 (0.30)	0.02 (0.25)	0.38 (0.26)	0.11 (0.23)	−0.71*** (0.24)
Household Number	0.13** (0.05)	0.01 (0.05)	0.05 (0.04)	−0.03 (0.04)	0.04 (0.04)	−0.01 (0.04)
Traditional Leader	0.95** (0.46)	−0.75* (0.39)	−0.59* (0.31)	0.46 (0.37)	0.17 (0.32)	0.20 (0.33)
Own Land	−0.25 (0.32)	−0.06 (0.30)	−0.37 (0.25)	0.14 (0.26)	0.04 (0.23)	−0.15 (0.24)
Heard about Police Visit	−0.56 (0.56)	−0.32 (0.60)	−0.07 (0.45)	0.56 (0.61)	0.43 (0.47)	−0.80* (0.46)
Know Someone in LNP	1.13*** (0.39)	0.01 (0.30)	0.16 (0.24)	−0.38 (0.25)	0.39 (0.24)	0.61** (0.24)
Suspicious of Enumerator	−1.00** (0.48)	0.01 (0.58)	−0.08 (0.41)	−0.28 (0.45)	0.07 (0.40)	−0.83** (0.40)
Constant	0.60 (0.67)	0.02 (0.70)	2.22*** (0.60)	−0.16 (0.60)	0.88 (0.55)	−0.15 (0.57)
Observations	499	499	499	499	498	472
Log Likelihood	−180.18	−192.13	−270.71	−242.52	−293.70	−268.20
Akaike Inf. Crit.	402.36	426.25	583.41	527.04	629.39	578.39

Note: *p<0.1; **p<0.05; ***p<0.01

Logit model that includes village level fixed effects

Table 14: "Reform Outsider" Experiment: Effectiveness

	Dependent variable:			
	Less Crime	Catch Criminal	Help	Feel Safer
	(1)	(2)	(3)	(4)
Treatment	−0.94*** (0.32)	−0.50 (0.43)	−0.38* (0.22)	−1.41*** (0.30)
Age	−0.04*** (0.01)	−0.06*** (0.02)	−0.01 (0.01)	−0.02* (0.01)
Grebo	−0.40 (0.37)	−0.24 (0.46)	0.01 (0.24)	0.73** (0.33)
Current Victim	0.15 (0.46)	0.14 (0.53)	−0.01 (0.27)	0.09 (0.41)
War Trauma	0.34 (0.36)	0.84 (0.52)	0.26 (0.24)	0.58* (0.32)
Household Number	−0.02 (0.06)	−0.01 (0.07)	0.06 (0.04)	−0.06 (0.05)
Traditional Leader	−0.24 (0.43)	0.02 (0.64)	0.08 (0.32)	0.64 (0.50)
Own Land	−0.98** (0.39)	−0.45 (0.50)	−0.15 (0.23)	0.04 (0.32)
Heard about Police Visit	−0.03 (0.69)		−0.70 (0.45)	−1.71*** (0.51)
Know Someone in LNP	1.15*** (0.41)	1.16** (0.58)	1.00*** (0.24)	1.83*** (0.48)
Suspicious of Enumerator	−0.43 (0.62)	−0.89 (0.69)	−0.71* (0.40)	−0.90* (0.49)
Constant	4.53*** (0.85)	4.89*** (1.14)	0.51 (0.56)	2.25*** (0.73)
Observations	474	419	470	499
Log Likelihood	−149.85	−91.13	−289.58	−173.96
Akaike Inf. Crit.	341.69	222.26	621.16	389.92

Note: *p<0.1; **p<0.05; ***p<0.01

Logit model that includes village level fixed effects

Table 15: "Reform Insider" Experiment (Alternative Control): Restraint

	Dependent variable:				
	Steal	Rape	Like Criminals	Cause Problems	Corrupt
	(1)	(2)	(3)	(4)	(5)
Treatment	−2.43** (1.01)	−0.03 (0.50)	−0.77 (0.85)	−1.94* (1.03)	−1.62* (0.95)
Female	0.10 (0.24)	0.38 (0.39)	−0.16 (0.23)	−0.35 (0.27)	0.01 (0.21)
Kru	−0.78 (0.59)	−1.72** (0.85)	0.82 (0.59)	−0.25 (0.71)	0.30 (0.54)
Grebo	−1.58*** (0.57)	−2.10*** (0.80)	0.46 (0.57)	−0.98 (0.69)	0.25 (0.51)
Victim of a Crime	0.72** (0.30)	0.69 (0.47)	1.55*** (0.27)	0.05 (0.35)	0.73** (0.29)
War Trauma	0.21 (0.25)	−0.64 (0.40)	−0.04 (0.24)	0.21 (0.29)	0.26 (0.23)
Traditional Leadership	0.20 (0.42)	0.18 (0.56)	−0.26 (0.36)	0.10 (0.41)	0.17 (0.40)
Farm	−0.30 (0.29)	0.33 (0.44)	−0.23 (0.27)	0.20 (0.33)	−0.34 (0.25)
Own Land	0.20 (0.28)	0.44 (0.44)	0.32 (0.27)	0.29 (0.31)	0.79*** (0.24)
Born in Town	0.19 (0.32)	0.18 (0.56)	−0.42 (0.29)	−0.17 (0.34)	−0.40 (0.28)
Christian	−1.36*** (0.45)	−1.10* (0.59)	−0.59 (0.39)	−0.90** (0.39)	−1.04* (0.56)
Heard of Police Visit	0.79** (0.38)	−0.31 (0.51)	0.26 (0.33)	0.52 (0.42)	−0.33 (0.32)
Know Someone in LNP	−0.80*** (0.28)	−0.98** (0.50)	−0.44* (0.24)	−1.41*** (0.31)	−0.26 (0.22)
Suspicious of Enumerator	−0.46 (0.50)	−0.79 (1.06)	0.42 (0.43)	−0.19 (0.55)	−0.30 (0.41)
Constant	1.96** (0.89)	0.03 (1.00)	−0.62 (0.86)	1.53 (0.94)	2.49** (1.06)
Observations	539	514	535	580	520
Log Likelihood	−238.61	−112.06	−273.60	−207.99	−292.65
Akaike Inf. Crit.	551.23	252.13	621.20	489.97	659.30

Note: *p<0.1; **p<0.05; ***p<0.01

Logit model that includes village level fixed effects

Table 16: "Reform-Insider" Experiment: Inclusiveness

	Dependent variable:	
	Discriminatory	Treat Women Unfairly
	(1)	(2)
Treatment	−4.47*** (1.23)	−1.75 (1.74)
Female	0.29 (0.24)	0.71 (0.51)
Kru	0.61 (0.61)	−1.04 (1.32)
Grebo	−0.35 (0.58)	−1.44 (1.28)
Victim of a Crime	−0.03 (0.29)	0.18 (0.66)
War Trauma	0.62** (0.26)	0.61 (0.53)
Traditional Leadership	−0.02 (0.36)	−0.44 (1.21)
Farm	−0.91*** (0.28)	0.39 (0.62)
Own Land	0.83*** (0.29)	0.01 (0.57)
Born in Town	−0.25 (0.31)	0.74 (0.75)
Christian	−0.35 (0.36)	−0.27 (1.20)
Heard of Police Visit	0.76** (0.38)	1.65* (0.92)
Know Someone in LNP	0.58** (0.26)	0.32 (0.57)
Suspicious of Enumerator	0.74* (0.42)	0.32 (0.71)
Constant	−0.21 (0.84)	−3.29* (1.90)
Observations	559	552
Log Likelihood	−246.31	−70.58
Akaike Inf. Crit.	566.61	215.16

Note: *p<0.1; **p<0.05; ***p<0.01
Logit model that includes village level fixed effects

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Table 17: "Reform Insider" Experiment: Rule of Law

	Beat Wife	Under Age Sex	Mob Justice	Child Support	Sassywood	Belief in Formal Law
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.25 (0.86)	1.78*** (0.61)	−0.51 (0.76)	2.38* (1.25)	−0.19 (0.75)	0.64 (0.75)
Female	−0.17 (0.27)	−0.42 (0.35)	−0.48** (0.22)	−0.40 (0.27)	−0.58*** (0.21)	0.27 (0.21)
Kru	−1.10 (1.10)		−1.93* (1.08)	−1.52 (1.12)	−1.37* (0.80)	−0.38 (0.60)
Grebo	−0.75 (1.08)	0.58* (0.35)	−1.52 (1.07)	−1.21 (1.11)	−1.39* (0.79)	0.34 (0.59)
Victim of a Crime	0.43 (0.35)	1.34** (0.57)	0.04 (0.27)	0.16 (0.34)	−0.67*** (0.25)	−0.44* (0.27)
War Trauma	0.09 (0.28)	0.21 (0.37)	0.25 (0.24)	0.33 (0.29)	−0.07 (0.22)	−0.46** (0.22)
Traditional Leadership	0.76 (0.47)	−0.89** (0.45)	−0.75** (0.33)	0.54 (0.47)	−0.19 (0.33)	0.39 (0.33)
Farm	0.23 (0.30)	−0.10 (0.38)	0.36 (0.25)	0.16 (0.30)	0.09 (0.23)	−0.01 (0.24)
Own Land	−0.95*** (0.32)	−0.23 (0.40)	−0.35 (0.25)	0.09 (0.31)	−0.41* (0.23)	0.26 (0.24)
Born in Town	0.43 (0.34)	0.74* (0.44)	0.01 (0.30)	0.15 (0.36)	0.09 (0.27)	0.25 (0.28)
Christian	0.55 (0.41)	0.25 (0.45)	−0.16 (0.37)	0.61 (0.40)	−0.42 (0.37)	1.04*** (0.34)
Heard of Police Visit	−0.31 (0.43)	0.14 (0.63)	−0.58 (0.35)	0.57 (0.50)	0.05 (0.32)	−0.61* (0.34)
Know Someone in LNP	0.17 (0.29)	0.03 (0.38)	0.46* (0.24)	−0.30 (0.29)	−0.13 (0.22)	0.30 (0.22)
Suspicious of Enumerator	−0.73* (0.44)	−0.38 (0.58)	0.05 (0.39)	−0.49 (0.44)	−0.08 (0.36)	0.37 (0.38)
Constant	1.50 (1.27)	1.13 (0.77)	2.98** (1.23)	0.85 (1.23)	3.34*** (0.98)	−1.11 (0.81)
Observations	604	604	604	604	603	579
Log Likelihood	−212.11	−133.61	−294.06	−203.75	−329.38	−307.17
Akaike Inf. Crit.	498.22	295.23	662.12	481.50	732.76	688.35

Dependent variable:

Note: *p<0.1; **p<0.05; ***p<0.01

Logit model that includes village level fixed effects

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Table 18: "Reform Insider" Experiment: Effectiveness

	Dependent variable:			
	Less Crime	Catch Criminal	Police Help	Feel Safer
	(1)	(2)	(3)	(4)
Treatment	0.19 (0.41)	−0.62 (0.38)	0.19 (0.89)	−0.82 (1.32)
Female	0.21 (0.32)	0.17 (0.28)	−0.34 (0.21)	−0.08 (0.29)
Kru	1.54** (0.71)	0.57 (0.66)	0.47 (0.56)	0.81 (0.81)
Grebo	1.19* (0.66)	0.50 (0.63)	0.26 (0.54)	0.85 (0.79)
Victim of a Crime	−0.08 (0.41)	−0.13 (0.37)	0.12 (0.27)	0.20 (0.41)
War Trauma	0.22 (0.33)	0.26 (0.29)	−0.24 (0.23)	0.17 (0.32)
Traditional Leadership	−0.77* (0.45)	1.16 (0.77)	0.43 (0.34)	0.68 (0.69)
Farm	0.06 (0.35)	−0.09 (0.32)	−0.13 (0.24)	−0.18 (0.35)
Own Land	0.09 (0.35)	0.05 (0.32)	−0.49** (0.24)	0.50 (0.35)
Born in Town	−1.00* (0.52)	0.15 (0.35)	0.29 (0.28)	0.37 (0.37)
Christian	−2.15** (1.05)	−1.32 (1.05)	0.80** (0.33)	0.29 (0.59)
Heard of Police Visit	0.44 (0.43)	−0.25 (0.33)	0.14 (0.34)	−1.25*** (0.44)
Know Someone in LNP	0.21 (0.35)	0.82*** (0.32)	0.77*** (0.23)	1.68*** (0.38)
Suspicious of Enumerator	0.57 (0.76)	0.07 (0.58)	0.55 (0.41)	1.06 (0.67)
Constant	3.55*** (1.29)	2.65** (1.21)	0.43 (0.80)	1.22 (1.41)
Observations	582	558	592	602
Log Likelihood	−155.22	−185.44	−308.18	−177.03
Akaike Inf. Crit.	340.44	400.89	690.36	428.06

Note: *p<0.1; **p<0.05; ***p<0.01
Logit model that includes village level fixed effects

1.6 SCRIPTS

1.6.1 SCRIPT

I am here to talk to you about how we can all live together in peace, how you can see us as your friends, brother, sister, security and partner because I am part of the LNP and our work is to protect you, your properties and to make sure the whole county is safe so that no one can come from anywhere to harm you. Is that clear?

I know that this place is peaceful and that is why you don't see plenty police here. But, the LNP wants you people to work along with us. We know you are peaceful, but there are some people here that are can do small small bad bad things, I wrong?

The police are supposed to catch the bad people. So, we want you people to help us report crimes and violence. Such as rape, killing, armed robbery, fighting with knifes, cutlasses, taking in drugs, stabbing. We also want to advise you to stop mob violence. When plenty people come to do something, they just can't jump on the person. If you caught a bad criminal or somebody for doing bad things, please do not beat them, just arrest the person and turn the person over to the police. I also beg you, we should not beat on your woman if she does any bad thing please talk with her and make her to know that what she is doing is not good or tell the old people about it or any family member or else one day you will harm or kill her and then you will have to face the law. If a man borns a child, he should help pay for the child to grow up. Rape is a serious crime and it is not good because it can damage the person or even kill the person. Once you have sex with someone below 18, the law says it is wrong. Also, sassywood is against the law. If these things are happening in your community, please report it. We all have to respect each other and respect the law in our country.

We have not forgotten about this County to send enough police officers because we are not many but as soon as we have more police officers, as soon as they graduate from the Police Academy, we will send some of them here.

The Liberian National Police are now professional. Before and during the war, we did not have much training. But, now we have been trained by Americans police officers and police officers from other countries. The LNP are trained to protect you and your properties. Some of our police officers are high school graduates, university graduates, and some even have masters degree. To

avoid tribalism or one tribe to be plenty in the LNP, the LNP got people from all the tribes, we got in our country. Also, there are plenty more women in the LNP now. Soon, one out of every three LNP officers will be women. We also do not recruit bad bad people into the LNP. If someone committed a crime during the war, they are not allowed to work for the LNP. So, the police are a more professional institution than before.

I want you to please encourage your children to join the Police because when they graduate, they will come back and serve you.

So, everything we say, you understand it good good, and you have any question to ask? Any question at all? Please ask us so we can all talk about it and any other question you have about the LNP.

1.6.2 Civilian Script

Ok, now I am going to read something to you about the Liberian National Police. Please listen to what I have to say. The LNP's work is to protect you, your properties and to make sure the whole county is safe so that no one can come from anywhere to harm you. Is that clear? This place is peaceful and that is why you don?t see plenty police here. But, the LNP wants you people to work along with them. We know that you are peaceful, but there are some people here that are can do small small bad bad things, I wrong?

The police are supposed to catch the bad people. So, they want you people to help report crimes and violence. Such as rape, killing, armed robbery, fighting with knives, cutlasses, taking in drugs, stabbing. They also want to advise you to stop mob violence. When plenty people come to do something, they just can?t jump on the person. If you caught a bad criminal or somebody for doing bad things, please do not beat them, just arrest the person and turn the person over to the police. They also beg you, we should not beat on your woman if she does any bad thing please talk with her and make her to know that what she is doing is not good or tell the old people about it or any family member or else one day you will harm or kill her and then you will have to face the law. If a man borns a child, he should help pay for the child to grow up. Rape is a serious crime and it is not good because it can damage the person or even kill the person. Once you have sex with someone below 18, the law says it is wrong. If these things are happening in your community, please report it. We all have to respect each other and respect the law in our country.

The Liberian National Police are now professional. Before and during the war, we did not have much training. But, now we have been trained by Americans police officers and police officers from other countries. The LNP are trained to protect you and your properties. Some of our police officers are high school graduates, university graduates, and some even have masters degree. To avoid tribalism or one tribe to be plenty in the LNP, the LNP got people from all the tribes, we got in our country. Also, there are plenty more women in the LNP now. Soon, one out of every three LNP officers will be women. We also do not recruit bad bad people into the LNP. If someone committed a crime during the war, they are not allowed to work for the LNP. So, the police are a more professional institution than before.

The police want you to please encourage your children to join the Police because when they graduate, they will come back and serve you.

So, everything we say, you understand it good good, and you have any question to ask? Any question at all? Please ask us so we can all talk about it and any other question you have about the LNP.

1.7 Photos

Figure 1: Police-Community Meeting in Grand Kru County.

Figure 2: Police Visits in Grand Kru County.

Figure 3: Road Conditions in Grand Kru County.