

Experiments in Post-Conflict Contexts

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Experiments are increasingly used to better understand various aspects of civil conflict. A critical barrier to peace is often conflict recurrence after a settlement or other attempt to end fighting between sides. This chapter examines the growing literature on experiments in post-conflict contexts to understand their contributions and limitations to our understanding of the dynamics in this period. It argues that work on post-conflict contexts takes two different perspectives: a *peace stabilization* approach emphasizes special problems from civil conflict, including how to sustain peace agreements, while a *peace consolidation* approach emphasizes problems common to statebuilding, including how to reconstruct communities. Both seek in part to prevent conflict recurrence, though, and that is the focus of this chapter. Although more existing theory links stabilization programs with enduring peace, more existing experiments examine consolidation programs. Both approaches would benefit from new work. Post-conflict contexts in general, however, are difficult environments in which to work, and so experiments face three interrelated challenges: first, these contexts present special ethical challenges due to both the high stakes of peace and the sensitivity of subjects; second, these are complex treatments often conducted simultaneously by different actors, and these are treatments that depend on both institutional change and behavioral responses, so change is the constant in these contexts; and, third, these contexts also face heterogeneity in terms of programs but also contexts that mean the lessons may not travel even among post-conflict settings. Despite these challenges, experiments in post-conflict contexts hold promise for advancing our understanding of enduring peace.

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Civil conflict causes more death and destruction in the modern era than interstate conflict. Even after a settlement or another end to the conflict, serious challenges to peace persist. Often, as states, communities, and individuals seek to rebuild, the conflict recurs, or related conflicts erupt. In these contexts, scholars seek to understand how to stabilize and consolidate peace, including at times by conducting experiments. These experiments assess interventions by foreign peacekeepers and domestic programs designed to reestablish stability, promote reconciliation, and rebuild as civil conflict subsides. While much of this research depends on observational data, existing experiments advance our understanding of post-conflict contexts, and with careful consideration of the way forward, future work can help establish what produces enduring peace.

This chapter argues that there are two main perspectives in political science on the critical impediments to enduring peace in post-conflict contexts. First, there is a largely top-down approach that develops a unified theoretical framework based on overcoming commitment problems primarily for group leaders. This approach focuses on special problems that emerge in states affected by civil conflict: how to sustain peace agreements and how to reintegrate fighters into the population. I call this approach the *peace stabilization* perspective. Second, there is a largely bottom-up approach that analyzes the effect of programs on individuals, communities, and their relationships to the state. This approach focuses on problems common to statebuilding whether post-conflict or not: how to police, provide justice, rehabilitate those traumatized, build social cohesion and reduce bias, generate economic growth, and build state institutions. I call this approach the *peace consolidation* perspective.²

Each perspective could use experimental evidence even more effectively. The peace stabilization perspective largely lacks experimental tests, likely at least in part because it often theorizes about the effect of state-level institutional changes on entire groups beginning with elites. The peace consolidation perspective tends to fail to tie the experiments conducted to theoretical frameworks about conflict recurrence specifically, likely at least in part because these programs often prioritize development outcomes even as the studies frame themselves in terms of peace. Both perspectives would benefit from new work. Specifically, new studies from the peace stabilization perspective should examine elite perspectives and mechanisms of institutional change, which may be limited to lab-in-the-field or survey experiments, but these randomized can still advance our knowledge of how peacekeeping works. New studies from the peace consolidation perspective should more explicitly tie to theoretical frameworks, including incorporating more outcome variables that are explicitly related to conflict recurrence, and feature coordinated sets of experiments using common treatments tied to arguments about how peace consolidates across contexts. Any new experimental work would also benefit from typologies systematizing studies and an advisory community recommending best practices for protecting those in these contexts.

Changing norms in the field, alongside innovative partnerships, increasingly encourage and allow researchers to do experimental work in post-conflict contexts. Recognizing these two

² I use these terms because this is in part about sequencing: stabilizing a peace agreement or otherwise securing an end to the sides fighting often comes first, and then consolidating society into a peaceful context comes next. However, at times programs for reconciliation will seek to reach individuals before the sides have stopped fighting, for example. Other terms, including top-down and bottom-up problems of conflict termination versus common problems of statebuilding, also often accurately describe these two perspectives.

different perspectives and adopting strategies to overcome the challenges that each perspective presents can make this research even more effective going forward. This research has the potential to make specific contributions. First, much of the experimental work in this post-conflict period examines actual programs designed to overcome the pressing policy challenge of producing enduring peace after civil conflict, and so there is an applied goal of “whispering in the ears of princes” (Roth 1995, p. 22). But, second, the work also can help develop theory about ending conflict and building resiliency in states, communities, and individuals in this unique context; understanding how these changes occur not only answers questions in this critical setting but also potentially holds lessons for broader stabilization.

This chapter draws on a wide range of existing work, including the growing set of experiments in post-conflict contexts. I consider studies to be post-conflict when they occur within ten years of a cessation of a civil conflict, defined by the number of casualties (Gleditsch et al. 2002), but I also include those the authors frame as post-conflict (for instance, when a group or region has stopped fighting, even as other conflict continues in the state). This is not a meta-analysis or review – which, indeed these could be written on each topic below – but I sought comprehensiveness in my initial pool of experimental studies. I therefore searched the top social science journals, I used keyword searches in the usual academic databases, I interviewed over a dozen individuals working on related topics, and I also examined the websites of IPA, J-PAL, and 3iE, major organizations supporting experimental studies, which had also recently put out reviews of related work. On a couple of topics, especially on community resilience and individual well-being, this produced dozens of experiments, and so I relied on reviews in these areas, explored topics with significant work including some inclusion of conflict outcomes – such as employment – and sought diversity across cases as much as possible. On topics with multiple types of experiments, I prioritized field experiments, wherein researchers alongside implementing partners randomize programs in the natural world, over lab-in-the-field, laboratory, and survey experiments due to a potential better match to reality (see Gerber 2011, p. 116; Humphreys and Weinstein 2009, p. 369).³ Finally, I relied on published or working papers, where available, over pre-registrations. The chapter first articulates two different approaches to peace in post-conflict contexts before turning to the experimental literature and then articulating some general challenges.

I. The problems of stabilizing and consolidating peace in post-conflict contexts

Post-conflict contexts present clear academic and policy challenges. Civil conflicts are usually defined as organized governments and rebel groups fighting over a political incompatibility and producing at least 25 battle deaths or, for full-scale war, 1,000 battle deaths per year (Gleditsch et al. 2002; Harbom and Wallensteen 2010). Since 1945, civil conflict has affected more than half of all countries (e.g. Blattman and Miguel 2010). Indeed, civil conflicts have produced millions of deaths in this period, and they have been five times as deadly as interstate wars (Fearon and Laitin 2003).

Civil conflicts are also especially destructive because they are fought within communities where they affect not only formal structures and economies but also the fabric of society. The termination of civil conflict typically requires opponents to send their children to the same

³ Given the aim of the chapter, I also prioritized studies that randomized their intervention. This means I did not include many excellent natural experiments. Also, except on a couple of theoretical points, I did not include studies that only used experiments to measure outcomes, such as a list experiment to report use of illegal institutions.

schools, shop in the same stores, and otherwise coexist. The proximity makes reconciliation necessary. Sharing or otherwise distributing power through governing institutions after civil conflict is also a substantial challenge. Most civil conflicts now end with negotiated settlements rather than victory by one side (e.g. Toft 2009), forcing changes to incorporate rebels into previously the government's state-level institutions at a minimum, but some similar dynamics also operate when one side largely wins (e.g. Kecskemeti 1957). In almost all civil conflicts, the former fighters and everyone else affected by conflict must then coexist in the state.

Conflict often recurs in these contexts, perhaps due to the challenges of coexistence at every level. For example, as much as 90 percent of civil conflict since 2000 has been linked to the recurrence of earlier conflicts (Collier et al. 2003). More specifically, fighting recurred within five years between the same sides in 40 percent of civil conflicts from 1975 to 2005 that ended with a peace agreement (Matanock 2017b). This chapter examines the period after a civil conflict ends, looking at attempts to stabilize and consolidate peace.

Research on how to secure enduring peace immediately after civil conflict is still developing. Post-conflict studies tend to take two different perspectives – which I call stabilization and consolidation – on producing enduring peace. Both perspectives potentially help inform our understanding of how to avoid conflict recurrence in these settings. I describe each perspective before turning to experimental work specifically.

1. Peace stabilization

What I call a “peace stabilization” perspective on post-conflict contexts animates many post-conflict studies in political science, and ties directly to many post-conflict programs, especially of intergovernmental organizations such as the United Nations. It focuses on overcoming commitment problems to stabilize peace especially through enduring settlements. This strand of the literature, while highly developed, still struggles to definitively test the mechanism and, related, to overcome potential selection bias to do so.

While not always explicit, this perspective is largely top-down, focusing on behavior and concerns of groups overall, presumably coordinated by their leaders. Some programs, however, seek to transform combatants into civilians, at least in part, to change the groups' incentives to renege on a settlement by making regrouping and rearming costlier. These primarily state-level programs, then, include monitoring, verification, and enforcement missions, along with political and security sector reform in state institutions, and, finally, disarmament, demobilization, and reintegration (DDR) efforts. These programs focus on problems that are particular to states emerging from civil conflict: how to sustain peace agreements and how to reintegrate fighters into the population. Studies of peace stabilization tend to examine missions that are provided by the United Nations, although they are also increasingly supplied by regional intergovernmental organizations (e.g. Weinstein 2018), and, at times, states seek to perform related functions such as monitoring on their own.

Studies of these programs of international intervention in post-conflict periods are mainly observational, and they show robust patterns, but they struggle to identify the specific mechanisms. Most scholars working in this area have focused on cross-national evidence on conflict recurrence when peacekeepers or other monitors are deployed, for example, and found

consistently positive associations between such post-conflict intervention and peace (Doyle and Sambanis 2000; Fortna 2008; Gilligan and Sergenti 2008; Hultman, Kathman and Shannon 2016; Matanock 2017b; Walter 2002). While not possible to fully detail here due to space constraints, a theoretical framework underpinning much of this work is that commitment problems can make peace precarious, even when conflict is costly and combatants can identify a settlement, but that international actors can provide a solution in many cases. Commitment problems arise when one side becomes temporarily weaker than an opponent, and then the stronger side has an incentive to grab more power than it was provided in the settlement; these problems often occur during the implementation process where institutional changes seek to match *de facto* and *de jure* power while demobilizing all sides (Fearon 2004; Lake 2003; Powell 2006; Walter 1997). Even the concern that an opponent will defect can cause preemptive conflict recurrence in the post-war period. Institutional changes that share power between ex-combatants, especially in the security sector, can make compliance with a settlement more beneficial and renegeing on its terms costlier (e.g. Hoddie and Hartzell 2007; Mattes and Savun 2009; Walter 1997). But international actors also can often provide “security guarantees” to transitioning groups under which they count on the outsider to reward compliance and sanction violations with a settlement (Walter 2002).

However, questions of *how* international intervention works remain, including whether commitment problems are truly a primary driver of conflict recurrence (e.g. Fortna and Howard 2008). Considering commitment problems, some studies seek to identify and test multiple implications of a theory of how international actors help. For example, Matanock (2017a; b) posits that these programs provide monitors – often peacekeepers but also election and other observers – to verify compliance and then work with donors and other partners to respond to the behavior of these groups with sticks, such as punishment by troops at the extreme, or carrots, such as aid or membership in international organizations. Additional cross-national data and case study evidence are also consistent with such a theory (also Girod 2012; Mattes and Savun 2009; Walter 2002; 1997). Other studies articulate sets of potential pathways, including those that address commitment problems and those that respond to other challenges (e.g. Fortna 2008; Howard 2019). They also show how at times leaders’ incentives may not match the groups’ (e.g. Cil and Prorok 2019). But these methods suffer from potential selection bias where international actors may choose particular strategies in response to the likely success of certain cases, posing a problem for even the best observational methods (e.g. Fortna 2008; Gilligan and Sergenti 2008).

Whether undergirded by a theory of commitment problems or another theory about what destabilizes peace, part of the argument often hinges on removing recruits. This can lessen the likelihood of conflict recurrence by removing the groups’ capacities to fight unilaterally (e.g. Hoddie and Hartzell 2007; Mattes and Savun 2009; Walter 1997). However, again, *how* to do so effectively is not resolved. Disarmament, demobilization, and reintegration (DDR) produce mixed results (e.g. Colletta and Muggah 2009). Most existing work has relied on case studies or, at best, rare natural experiments to understand the effects of DDR programs, though (e.g. Muggah 2008). For example, a study in Burundi, where some organizations set to distribute resources for reintegration were delayed, found that the program did not change former fighters allegiance to particular elites or even increase their satisfaction with post-conflict life (Gilligan, Mvukiyehe and Samii 2012). Experiments on international intervention and DDR programs could theoretically test if these mechanisms work and produce stable peace.

2. Peace consolidation

In contrast, what I call a “peace consolidation” perspective brings together a wider set of work on post-conflict programs conducted by donors in conjunction with other organizations and, at times, by the governments themselves. Much of this work nods to sustaining peace – for example, a program examining local dispute resolution frames the issue as being of concern to Liberian government and U.N. peacekeepers because “these local incidents could spark a national crisis” (Hartman, Blair and Blattman 2018, p. 6) – but instead focuses on development in terms of states, communities, and individuals. This strand of the literature, while rigorous in much of its methodology, still struggles to advance our understanding of conflict recurrence.

Most of these programs that have been studied take a bottom-up approach: they seek to build stronger state institutions by engaging the population, for instance. In fact, some have noted that state-level programs are important parts of these missions but not the focus of most of these studies (e.g. Mvukiyeye and Samii 2009), perhaps in part due to the feasibility of experimenting on sub-state programs. These programs, then, include governance efforts, peace and transitional justice processes, media campaigns, and even community-driven development and cash transfer programs. Peace consolidation programs focus on problems common to statebuilding whether post-conflict or not: how to police, provide justice, rehabilitate those traumatized, build social cohesion and reduce bias, generate economic growth, and build state institutions. These are often conducted by domestic or international organizations, although at times they are also run by state agencies or the United Nations and regional intergovernmental organizations. The programs are often funded by bilateral and multilateral international donors. The World Bank, for instance, has spent more than \$85 billion on local participatory development programs in the first decade of the century alone (Mansuri and Rao 2012). A major question that remains, however, is whether these programs work in the same ways in post-conflict context (discussed below).

Studies of these programs often emphasize the importance of development and examine development outcomes, where they often show improvements from their interventions, but, although they often frame the programs in terms of peace consolidation, they often do not rigorously describe their theory of change in terms of conflict recurrence or test related outcomes. Civil wars have been described as “development in reverse,” so development is necessary in many of these contexts (e.g. Voors et al. 2012), and these programs typically contribute to our understanding of development goals. An underpinning idea in many of these approaches is that factors likely to encourage statehood, community development, or individual well-being are also important to prevent conflict recurrence: for example, a joint U.N.-World Bank report, “Pathways for Peace” (2018), argued that preventing conflict, and presumably its recurrence, is to ensure resilience theory “inclusive and sustainable development” (p. xviii).

But questions remain as to whether, and how, these programs actually influence the consolidation of peace. Existing observational studies have found little systematic effect of World Bank post-conflict assistance on conflict recurrence (or even economic recovery) (e.g. Flores and Nooruddin 2009). But the experimental studies, which come from different fields, at times do frame from different angles how these post-conflict programs could produce more enduring peace (drawing generally on work on civil conflict and development, which I cannot fully detail here given space constraints, but see reviews including Blattman and Miguel 2010;

Findley 2018). A first angle taken by some studies draws on arguments in political science and related fields about the conditions under which conflict occurs rather than recurs specifically. There is more agreement on some causes of conflict onset. So this approach incorporates the idea that weak states are more likely to experience insurgency, for example, perhaps because fragile institutions create opportunities for disgruntled populations to attack successfully (e.g. Collier and Hoeffler 2004; Fearon and Laitin 2003), or because competing for resources peacefully becomes less attractive when contracts are not enforced and the option of violence is available (e.g. Blattman and Miguel 2010). Some of these theories of onset are especially applicable to post conflict contexts: for example, weak institutions combined with group concerns about strategic dilemmas and memories of prior conflict may present opportunities for political entrepreneurs to polarize society and reignite prior conflict (Lake and Rothchild 1996).

A similar angle taken by some studies incorporates ideas about conflict occurrence and recurrence but with more tentative theoretical and empirical links. For instance, programs may seek to reduce grievances, but grievance's precise role in conflict occurrence or recurrence is not resolved: although many indicators of grievance such as income disparities or ethnic divisions are too widely experienced to drive conflict onset (e.g. Fearon and Laitin 2003), others find stark evidence of its producing conflict in particular cases (e.g. Moore 1993; Straus 2006; Wood 2003) and even link it to conflict in some cross-national evidence (e.g. Cederman, Weidmann and Gleditsch 2011). Opening democracy, especially at the local level (e.g. Mvukiyehe and Samii 2017), is similarly used – but its effect on conflict is also unresolved (e.g. Risley and Sisk 2005; Stedman, Rothchild and Cousens 2002). Community- or state-level approaches often seek to win hearts and minds broadly, under the assumption that beneficial policies toward these populations can produce long-term support for the state rather than its competitors, but most assessment is only in the short-term (e.g. Berman and Matanock 2015).

Finally, another angle – at times by studies outside political science in particular – draws on theories specifically about what happens after conflict. For instance, taking a sociological and psychological perspective, and at times incorporating even public health, some suggest fear or other emotions beyond grievances, collective identification with a violent group, and the loss of constraining norms allow individuals to commit acts that they otherwise avoid, perhaps driven by leaders, often in a belligerent “haze” in which many fight (e.g. Collins 2008; Littman and Paluck 2015). And then, focusing on conflict recurrence, this work then argues that cycles of violence persist wherein fighters have committed to violent groups and otherwise broken norms that make it more likely that they will continue to fight over time (e.g. Littman and Paluck 2015). More broadly, this theoretical framework fits with a set of potential socio-psychological barriers to peace, including societal beliefs about in-groups and/or outgroups, intergroup mistrust, cognitive and motivational processes, and affective and emotional factors (Bar-Tal and Halperin 2011). This theory also ties to those from political science described above on intergroup mistrust, for example. However, even many theories on dimensions of reconciliation or resilience within communities stop short of articulating a pathway back to conflict, making it less helpful on the particular question of recurrence (e.g. Staub 2006).

A theoretical framework is important to understand the link between these peace consolidation programs and conflict recurrence, of course, including by making clear the assumptions and scope conditions. Considering individual programs that generate employment, for example,

many rely on the premise that the jobs and welfare benefits offered by fighting exceed those civilian options, and so increasing these payoffs from peace can reduce the incentives to fight, but the evidence on conflict is mixed. Some individuals, or even entire conflicts, may be animated not by profit but by social incentives or something else – or some conflicts may require different recruits (see Becker 1968; Dube and Vargas 2013; Gilligan 2016; Nolan, Knox and Kenny 2019; Weinstein 2007). Similarly, considering community characteristics, it is not clear what is needed to prevent conflict recurrence: many programs in post-conflict contexts seek to develop collective decision-making processes, but some also explicitly aim to promote independent thought. The scope conditions can make a difference as large-scale one-sided conflict, as in Rwanda (see below), may require a different process than rebellion (and social cohesion is a contested concept in general; see Chan, To and Chan 2006).

Empirical studies in post-conflict contexts, however, often do not articulate and test specific components of theories explicitly linking their programs to enduring peace. Most frame themselves using at least one of these angles and assert that risk of conflict recurrence can be mitigated by capable but also responsive state institutions, cooperative communities that can resolve differences through normal processes and are generally prosocial and not discriminatory, and individuals who are mentally and economically healthy and tied into their communities. But the existing empirical literature still tends not to rigorously tie its increasingly sophisticated methods into theories of conflict recurrence.⁴ Many experiments, as I show in the next sections, do not test conflict outcomes despite their peace framing. Part of the reason may be that many theories do not focus on precisely what makes conflict more likely to recur. Part of the reason also may be that so many different possible theories across fields at times produce conflict implications. Some experiments do test conflict outcomes but then produce mixed results. Even these, though, tend not to link the findings back to theory development. Therefore, as I will show below, experimental studies and other work can help assess whether these post-conflict programs change states, communities, and individuals in ways that can prevent conflict recurrence and, if so, under what circumstances – but we do not yet know much about this particular question because much of this work is not yet tied to specific theory on conflict recurrence.

II. Learning from experiments in post-conflict contexts

Existing experimental work has made progress from both peace stabilization and peace consolidation perspectives. However, much less of this work focuses on peace stabilization than on peace consolidation. There are both structural reasons and practical reasons that these special problems of post-conflict contexts receive less experimentation than do more common problems of statebuilding. A weakness of many of the experimental studies on peace consolidation, though, as noted above, is that they do not tie directly to outcomes or even theory on peace. In this section, I describe the challenges but also the existing experimental work in each category, and I offer suggestions for paths forward.

1. Peace stabilization

Much of the theoretical work prioritizes changing state-level institutional environments to overcome commitment problems and incentivize elites to implement peace agreements. As noted above, cross-national evidence shows consistent patterns and case studies even trace the

⁴ There are notable exceptions, often reviews such as Ditlmann, Samii and Zeitzoff 2017), and I discuss some of these below.

mechanisms through which outside guarantees work, but they face potential selection bias, and so experiments could add rigor to assessing how peacekeeping works. From this perspective, then, experiments could usefully test hypotheses that these missions provide monitoring and conditional incentives, changing beliefs and behavior of governments and their rebel challengers.

However, so far, little experimental work exists on this topic due to two crucial and interrelated challenges that derive from the theory. First, the theory primarily depends on state-level variation, or perhaps variation at the conflict- or dyad-level of fighting factions, because it expects change in leaders' incentives based on outside guarantees. It therefore is often not amenable to the typical subnational experimental design (also on this point, see Hyde 2015). Second, these experiments also may require this specialized population of elites, including fighting faction leaders, or at least tests showing that they behave in similar ways as other experimental subjects. Recent advances, however, have begun to overcome these challenges.

I describe existing experiments in this section that point us to important directions forward, arguing that peace stabilization programs potentially represent an area for tremendous growth in experimental work, especially with studies focused on elites and "micro-foundations" of the theory. Questions such as whether policymakers condition foreign aid on elite compliance with peace agreements or whether compliance is worse with less monitoring are the subject of new experiments, primarily based on informational treatments (see Grose's chapter in this volume Hyde 2010b).

Before turning to the literature that seeks to experiment primarily on leaders, experiments can help examine how average citizens' attitudes toward peace processes are formed, what forces they respond to, and whether they are an independent stabilizing force. Recent work argues for more inclusion of average citizens and civic society in peace processes either to stabilize or consolidate peace (e.g. Nilsson 2012), and some studies even argue that changing incentives can be a bottom-up process (e.g. Nanes 2018). However, a set of survey experiments that offer different information about aspects of peace agreements, or options for potential agreements, show strong effects of elite cues on citizens' attitudes in addition to substantive factors like retributive justice (Assouline and Trager 2017; 2015; Fabbe, Hazlett and Sinmazdemir 2019; Garbiras-Diaz, Garcia-Sanchez and Matanock 2019; Kao and Redlich Revkin 2018; Khadka and Haas forthcoming; Matanock and Garbiras-Díaz 2018; Tellez 2019).⁵ Especially as citizens decide whether to support costly concessions in environments of limited information, they look to elites, both positively and negatively: for example, during the recent peace process in Colombia, endorsements by different political leaders strongly influenced the attitudes of their supporters and opponents, in opposite directions, on policies related to the settlement (Garbiras-Diaz, Garcia-Sanchez and Matanock 2019; Matanock and Garbiras-Díaz 2018).⁶ This evidence

⁵ Some of these studies are post-conflict, in keeping with the scope conditions of this chapter, but these questions are also important during conflict, at times during an active peace process, and the experiment on Syrians was even conducted in a refugee camp (although I exclude questions of integrating refugees from the rest of this review because, although crucial, these studies are somewhat distinct and space constraints bind).

⁶ This work also relates to survey experiments that seek to accurately measure average citizens' attitudes toward different faction leaders, showing both strong positive and negative affect toward elites, primarily in conflict contexts (Blair et al. 2013; Blair, Imai and Lyall 2014; Bullock, Imai and Shapiro 2011; Fair et al. 2018; Fair, Malhotra and Shapiro 2014; 2010; Lyall, Blair and Imai 2013; Matanock and García-Sánchez 2018).

suggests that taking a top-down approach focused on elites may be necessary in many of these cases, but more work on any independent influence of average citizens on peace would be useful.

Taking a perspective focused on elites, and given the challenges, new experiments still are making progress on testing the mechanisms that potentially underpin the peace stabilization perspective. In the past, studies have sought to overcome potential selection bias by explaining how particular cases are chosen for missions (Fortna 2008), even matching on these observable factors (Gilligan and Sergenti 2008), or using natural experiments such as the rotating U.N. Security Council presidency and alternation of its seats by region (e.g. Carnegie and Mikulaschek 2017). However, recent studies have added experimental tools, especially informational treatments, to explore the mechanisms through which international intervention works. One crucial question in overcoming commitment problems is when international actors will actually condition aid or other incentives. Recent studies use vignettes to experimentally examine when policymakers choose to change funding levels in a post-conflict context. A survey of over 1,000 officials from donor and implementing agencies, for example, showed that they endorsed decreasing budgetary aid and development aid but increasing humanitarian aid in scenarios of renewed tensions, and the opposite in scenarios of sustained peace, while they wanted to increase transitional aid in any scenario (Campbell and Spilker 2018). Other ongoing work seeks to assess the particular conditions under which noncompliance by a former fighting faction would cause donors to suspend funding (Matanock and Johnson 2020). This experimental approach to examining international conditionality could grow like work on support for international intervention broadly has done (e.g. early studies on interstate war, Beer et al. 1995, and then civil conflict, Berinsky and Kinder 2006).

Beyond experiments across cases, even if national leaders make most decisions about renegeing on settlements, the theory implies some variation in compliance based on monitoring and conditional incentives. Therefore, experiments could potentially induce variation at a subnational level. For example, in El Salvador, the newly-formed rebel party accused the government of voter disenfranchisement in rebel strongholds to shift the outcome of elections, and outside actors investigated and froze aid to combat this violation of the bargain (e.g. Matanock 2017b). While not yet conducted in post-conflict contexts, randomized audits could examine effects on such violations and displacement, following innovative experiments on monitoring elections (in contexts of peace and conflict, respectively, Hyde 2010a; Callen and Long 2015; also, on audit studies, see Butler and Crabtree's chapter in this volume). Studies combining natural and lab-in-the-field or survey experiments move in this direction. First, in a lab setting in Kosovo, over 200 Serb and Albanian participants played an iterated trust games that randomized assignment of outside interveners and showed that enforcement produced a significant reduction in "violating" trust norms; then, using "as if" random variation in the quality of enforcement by different NATO interveners assigned by location – where French forces monitored but did not enforce – just over 460 participants play dictator games and showed more prosocial behavior just outside the French zones than just inside it (Mironova and Whitt 2015). Programs such as peacekeeping patrols do vary at a subnational level in ways that could facilitate experimentation along these lines (e.g. Ruggeri, Dorussen and Gizelis 2017).⁷ Other studies also take abstract approaches to

⁷ Non-experimental work has begun to test potential micro-foundations of U.N. peacekeeping guarantees, although it has focused on citizen responses, not elite responses: these studies tend to rely on imperfect information in deployment and patrolling decisions alongside some matching strategy and regression analysis. They find little

third-party sanctions in lab-in-the-field experiments, showing similar results, including in post-conflict cases such as Bosnia-Herzegovina and Papua New Guinea (e.g. Alexander and Christia 2011; Bernhard, Fehr and Fischbacher 2006; Henrich et al. 2006; on lab-in the-field experiments, also see Eckel and Londono's chapter in this volume).

Experimentation around subnational variation in peace stabilization programs could also develop further in two ways. First, these studies often implicitly or explicitly challenge whether programs indeed work in a unified way that necessarily relies on national leaders. Some theoretical work describes local leaders as important in committing to peace processes (e.g. Fortna 2008; Ruggeri, Gizelis and Dorussen 2013), making variation in patrols described above even more important to study, for instance. But theory about which are the critical elites for such peace stabilization – considering who faces the commitment problems and, if not convinced about their opponents' credibility, can drive an entire faction back to fighting – is not yet fully developed. Factors such as the authority structure of groups fighting likely matter: if national leaders have centralized commands, studying their decisions may be more important, whereas if local leaders play a central role in decision-making, then studying their decisions through subnational experiments becomes more plausible. Second, as noted, some studies also argue that removing crucial resources may change even national leaders' decisions by reducing the options available to them. Making fighting more difficult by disarming, demobilizing, and reintegrating recruits is therefore a specific set of programs that could be usefully examined experimentally. Given DDR and other similar programs on the state side are often constrained, as resources to provide payouts for fighters and the like can come in slowly, it is plausible that implementers may agree to randomize more aspects of these going forward. Some studies experimentally seek to assess if programs can help former fighters overcome trauma or reintegrate into their communities (for example, the former in demobilization camps in the DRC, see: Hermenau et al. 2013; Köbach et al. 2017, and the latter in Nigeria as fighters left Boko Haram, see: Blair et al. Forthcoming, and in Colombia after the rebels signed a settlement, see: Ugarriza and Nussio 2017); however, the primary aims of those studies are often not to change the conflict dynamics but to improve former fighters' lives generally (although the latter may lead to the former).

Overall, then, considering potential directions forward, survey or perhaps lab-in-the field experiments may be the only options available for elite samples, but field experiments may be an option sub-nationally, depending in part on willing partners. The survey or lab-in-the-field experiments struggle to match real-world complexity, at times lacking convincingly costly outcomes to make policymakers think through their decisions in the same way they might otherwise, and measuring only short-term effects in most cases. However, these experiments allow more rigorous testing of the mechanism posited by the theory; they follow other studies that experiment on elites in similar ways, and so researchers studying post-conflict contexts can draw on their lessons learned about how to design surveys that reach and engage elites, or perhaps similarly identify when other populations provide comparable responses to elites (e.g. Grose's chapter this volume, Hafner-Burton, Hughes and Victor 2013; Kertzer, Renshon and

effect on security (but do find effects on economic wellbeing). For example, in Sierra Leone, peacekeeper exposure was not correlated with a reduction in civilian victimization (already low) or beliefs about whether conflict would recur, although citizens were optimistic that the UNOCI-monitored Zone of Confidence reduced the chance of conflict recurrence (Mvukiyehe and Samii 2009). In Liberia, U.N. base areas also do not correlate with security or social infrastructure (Mvukiyehe and Samii 2010).

Yarhi-Milo 2019; Loewen, Rubenson and Wantchekon 2010; Tomz, Weeks and Yarhi-Milo 2019; Yarhi-Milo, Kertzer and Renshon 2018). Field experiments are also possible to test the mechanism posited by the theory on peace stabilization, particularly when the theory implies subnational variation, although scholars will need to work in conjunction with the organizations conducting these programs. While many aid organizations have expressed their willingness to cooperate as such with researchers (Millennium Challenge Corporation and USAID funded documents call “randomized impact evaluation” the “ideal research design”; see Hyde 2010a), developing similar research with democracy observation organizations was slow (e.g. Hyde 2010a). Some have expressed concern that peacekeeping may be more difficult to experiment on (e.g. Humphreys and Weinstein 2007), but the same kind of persistent outreach may work with these policymakers as it has in other sectors. Especially as their resources shrink, peacekeepers may not be able to patrol an entire state’s territory, for example, and they may be even more open to experiments to understand what works best. In fact, some have already formed partnerships with state security forces or United Nations peacekeepers to allow randomization, such as in Liberia (see the “State” section below). So far, however, peace stabilization has not been subjected to the same experimental scrutiny as peace consolidation.

Peace stabilization programs potentially represent an area that could benefit from more experimental work going forward, especially considering work focused on elites and “micro-foundations” of the theory; so far, these studies have been limited to informational treatments through survey or lab-in-the-field experiments, but future work could adopt this approach and also try for more ambitious field experiments with new partners on the mechanisms, in particular.

2. Peace consolidation

In contrast to peace stabilization theories, many of the peace consolidation studies use experiments. In the post-conflict context, the studies, and the programs and processes they analyze, are typically designed to: (1) build stronger state institutions by engaging the population, (2) shape resilient communities by increasing multiple outcomes including civic participation, social capital, and pro-sociality, and (3) improve the well-being of those affected by conflict. The expected effects are on average citizens, making experimentation at a subnational level appropriate, and, as noted above, many of these programs are run by development agencies and other actors supportive of this method. Some of these studies and programs are explicit in seeking to consolidate peace or deter conflict, and many are at least framed in terms of their importance for peace, but most do not tie to specific theory on how they work to achieve these outcomes. Given the marginal improvements identified in many of these studies, and lacking more links to theoretical frameworks, the studies often evoke questions about how much trust citizens need to have in the state before its institutions deter competition, how cooperative communities need to be before they avoid violence when disputes arise, and how well-off individuals need to be before they refuse to rebel. This diverse set of programs begins to produce results, however, that may develop such an understanding. In this section, I examine the implications and limitations of sets of these studies, focusing on those that potentially relate the most to conflict recurrence, beginning with those that focus on engagement with the state, then the community, and finally individuals themselves. I suggest that review essays that systematically examine subsets of this literature and relate it to theory are an important next step, but that also developing experiments with outcomes explicitly related to

theory and, ideally, employing sets of experiments that have common treatment arms tied to a broader argument about peace consolidation across contexts. This section is divided broadly by state, community, and individual, assessing studies that focus on each, although some overlap.

States: A first set of such experiments assesses whether various programs can build stronger state institutions by engaging the population, potentially deterring competitors, although studies so far do not focus much on this outcome. This is a concern for post-conflict states but also any statebuilding. One group of these studies examine citizens' engagement with state institutions, often expected with security reforms, and they mostly show some increased use but also some worsened perceptions. A field experiment that randomized patrols by newly-retrained and better-equipped police in Liberia found the patrols increased security and citizens' crime reporting to police, especially among low-income individuals, but did not change reported trust in state institutions (Blair, Karim and Morse 2018).⁸ This study randomized aspects of the security sector reform, but such experiments are rare, and most others are conducted longer after conflict. Some, however, still assess what is explicitly post-conflict programming. A survey experiment in Guatemala found United Nations investigators and prosecutors were seen as more effective than the state in responding to cases about corrupt systems that had developed during the civil conflict and, as in the prior study, reduced the rate of those who thought the government should be in charge of dealing with similar crimes (Matanock and Garbiras-Díaz 2019). A field experiment that randomized assignment of permanent community police officers by village, who were recruited as part of post-conflict stabilization in Papua New Guinea, found the officers increased women's positive beliefs about and reported use of these state institutions, but had the opposite effects on men (Cooper 2019). A field experiment in Colombia found randomized hot-spot policing did not increase in satisfaction with these state institutions, although this study and another showed some reduction in assaults (Blattman et al. 2017; Collazos et al. 2019). Finally, a recent experiment that randomized a whole set of related activities including community policing, data collection on crime, and improved related infrastructure and at-risk youth programs in Central America found improvements in effectiveness but the package treatments made the mechanisms harder to assess (Berk-Seligson et al. 2014). Experiments underway on these topics, including a set of coordinated policing studies including some post-conflict sites, for example (Blair et al. 2019a), are likely to expand our current understanding. These studies so far suggest a role of security reforms in changing relations to the state, potentially increasing citizen use if not trust, although outside actors providing security or other governance functions potentially complicate the relationship.

A related set of experiments seeks to assess the effect of altering representation in these state institutions on citizen engagement. It shows decidedly mixed results. For example, a field experiment that randomized household visits by gender in Liberia, when security sector reform added female police officers, found that the presence of female officers increased approachability and demand for services (Karim 2016). A lab-in-the-field experiment that randomized police teams by gender to work on cooperative tasks in Liberia found no change in competence on hypothetical sexual or gender-based violence cases but more team cohesion with female officers (Blair et al. 2019b). Another survey and lab-in-the-field experiment, also in

⁸ However, a field experiment that randomized free legal aid also in Liberia found no change in attitudes or behaviors with regard to state institutions, even as the program improved case and later economic outcomes (Sandefur and Siddiqi 2015).

Liberia, however, found that police teams with minority officers were *more* discriminatory toward minority civilians although their presence did not affect team cooperation (Blair et al. 2019b). But, a survey experiment in Baghdad, which was described as assessing post-conflict security sector reform and having lower levels of fighting even though Iraq was mid-conflict, found that providing minority respondents with randomized information on police integration decreased support for anti-government violence (Nanes 2018). The effects of increased representation on satisfaction and even effectiveness are therefore mixed in these studies. Moreover, while these studies examine how to engage citizens, potentially through better representation, which could potentially deter competitors, only this final study actually measured anti-government violence and even then as a self-reported survey response.

Other programs, run by both states and outside actors, also have the potential to engage average citizens in ways that potentially reduce violence and support for state competitors. For example, in these post-conflict contexts, field experiments that randomized informational civic education programs and other community institutions improved participation in elections, mitigated some concerns about violence, and even reduced actual violence around these power transfers (for example, in Liberia, see Mvukiyehe and Samii 2017, in Nigeria, during a lull in active conflict, see Collier and Vicente 2014). However, experiments on these programs also found increased intimidation and fear related to voting (in Liberia, Mvukiyehe and Samii 2017, and, in Cote d'Ivoire, Arriola et al. 2020). Other programs also try to increase engagement with the state in these contexts but often secondarily to their development goals. For instance, a field experiment on education that randomized outsourcing schooling to private companies in Liberia found that treated students held more supportive views of elections and less biased views of other ethnic groups among other outcomes (Romero, Sandefur and Sandholtz 2017); a field experiment on health that randomized increased community monitoring and status awards for nurses in government-run clinics in Sierra Leone found increased use and perceptions of at least these institutions (Christensen et al. 2018); a field experiment that randomized advocacy for general local issues and responsiveness training for local officials had negative effects on satisfaction, however, although this perhaps indicated a more open environment in which to question the government (in Rwanda, for a program that began after a conflict and was described as applicable to post-conflict contexts, see Nichols-Barrer et al. 2015). In addition, studies assessing the effect of general foreign aid programs or more negative intervention such as sanctions or even election interference on relationships with the state are growing, including in some post-conflict contexts (e.g. Blair and Roessler 2018; Brancati 2014; Dietrich, Mahmud and Winters 2018). The focus of these programs on the whole is not always peace consolidation, but some of their outcomes may be related, although they do at times show negative effects through increasing fear or disaffection in post-conflict contexts so far.

Explicitly at the local-level, and so beginning to transition to the community studies examined next, a set of experiments assesses the effect of community-driven reconstruction (CDR) programs in post-conflict contexts on diverse outcomes. These programs typically randomize grants for local public goods that communities decide how to spend. In post-conflict contexts, experiments show that CDR programs improved public goods and, at times, broadened participation on community goals in these instances, but they largely failed to transform broader community decisions or collective action – and thus they have not reduced the likelihood of conflict recurrence either (see reviews of these studies including Barron 2011; Casey 2018;

Casey and Glennerster 2016; King and Samii 2014). The cases examined after conflict include in Sierra Leone (Casey, Glennerster and Miguel 2012, which found no effects on collective action, increased involvement of outgroups, or collective decision-making, and Laudati, Mvukiyehe and van der Windt 2018, which also did not find any female empowerment even with gender distribution requirements in leadership) and Liberia (Fearon, Humphreys and Weinstein 2009; 2015, which found increased contributions to a development project but only with a mixed gender groups overseeing distribution). The cases examined with some active fighting, but after peace agreements or during some break in the conflict, include Eastern Congo (Humphreys, Sánchez de la Sierra and Van der Windt 2019, which found no effect on the quality of local governance or participation) and Sudan (Avdeenko and Gilligan 2015, which found an increase in civic participation in newly opened governance institutions but no increase in social networks or norms). The type of program may matter, too: for example, CDRs led by skilled members of the community performed better on the projects in Sierra Leone (Casey et al. 2018), and CDRs with direct voting produced higher satisfaction with project chosen in Indonesia (Olken 2010). While these programs may be beneficial for specific development projects, there is little evidence that they contribute to citizen engagement directly tied to strengthening state institutions against competitors, or conflict reduction directly, based on the evidence so far.

Communities: A second set of experiments assesses whether various programs can build more resilient communities, although what produces resilience or potentially reduces propensity for conflict recurrence ranges from increased cohesion to dissent, depending on the particular study. Some of these studies are development programs that happen to be run in post-conflict contexts, but they are not tied to any theory of peace consolidation, although most at least use conflict prevention in their framing. A first set of studies examines programs that provide non-violent mechanisms for local dispute resolution, potentially keeping a minor disagreement from spiraling into conflict, although this link is not always explicit or, indeed, limited to post-conflict contexts. Field experiments that randomized training to 15 percent of adults in villages on alternative dispute resolution mechanisms in Liberia found fewer violent land disputes despite initially more disagreements and extrajudicial punishment; however, only those with political connections felt their property rights were more secure, whereas everyone else felt theirs were less secure (Blattman, Hartman and Blair 2014; Hartman, Blair and Blattman 2018). Lab-in-the-field and survey experiments in Mali that randomized the perceived presence of U.N. peacekeepers also found positive effects on local dispute resolution, but that the specific outside actor, and in particular whether it is viewed as impartial, shaped the response (Nomikos 2019). These studies interestingly build alternatives to formal institutions. Their effect on conflict is tenuous, however: the violence studied in Liberia, for instance, is “low-level and limited” such as “trampling of crops, defacement, fist fights,” although the authors note that “sometimes tit-for-tat reprisals escalate,” and so the Liberian government and U.N. peacekeepers worry that “these local incidents could spark a national crisis” (Hartman, Blair and Blattman 2018, p. 6). However, the process of escalation is not discussed much in this work, and these studies do not assess any measure related to escalation.

Next, a set of programs encourage reconciliation, often through transitional justice, and they mostly show positive effects on group relations, but few of the studies examine measures related to conflict recurrence or peace. Beyond survey experiments that randomized vignettes on transitional justice and explored effects on forgiveness (e.g. David 2017; Gibson 2002), recent

field experiments randomized these programs and others. For example, a field experiment that randomized truth and reconciliation efforts at the village level in Sierra Leone found improved community outcomes, including strengthened social networks and forgiveness of perpetrators, but worsened individual outcomes such as post-traumatic stress disorder, even in the longer term (Cilliers, Dube and Siddiqi 2016). In addition, the study did not change beliefs that ex-combatants and other members in the community would return to conflict. A field experiment that randomized assignment of trained facilitators to promote healing through discussion in community groups in Rwanda found similar positive effects on orientation toward outgroup members but positive effects also on trauma symptoms (Staub et al. 2005). In Colombia, an experiment on discussion groups between community members and ex-combatants randomized perspective-giving (“the opportunity to share one’s own perspective and personal history with antagonists”), a debate, or no treatment, and found the first treatment led to increased empathy and cooperative attitudes (Ugarriza and Nussio 2017).

Even less direct programs promoted reconciliation within communities, or other characteristics potentially useful to coexistence, with just a few notable exceptions, although still with few test ties to conflict recurrence or peace. A field experiment that randomized visits to a museum that memorializes victims in Chile found increased support of restorative transitional justice programs and inclusive institutions (Balcells, Palanza and Voytas 2018). In Rwanda, a field experiment that randomized radio programming found little effect on attitudes toward other groups, unlike those above, but increased willingness to express dissent and engage in productive community deliberation (Paluck 2009; Paluck and Green 2009). Later perspective-giving by a popular character on that radio program also improved group relations and empathy (Bilali and Vollhardt 2013). However, in a study in the Democratic Republic of the Congo, amidst ongoing conflict, adding a talk show to a similar radio program to encourage consideration of other opinions actually had negative effects on attitudes and behavior toward the outgroup (Paluck 2010). The outcomes across these studies vary based on the particular aims of the program, from inclusion to critical thinking, but they again do not tie back to overall conflict.

Contact between members of different groups, which vary on duration and how scripted they are, also represent a potential mechanism for diminishing prejudice and potentially fostering cooperation in communities. However, while an early review found positive effects of contact on prejudice (Pettigrew and Tropp 2006), a more recent review of experiments found little assessment of racial or ethnic relations and, when studied, little effect (Paluck, Green and Green 2018). Most studies were in developed countries, though (Paluck, Green and Green 2018). A handful were conducted during active fighting or otherwise contentious contexts, showing generally positive effects of contact, even indirect contact, although some deepening of ethnic identities and even feelings of discrimination (Dawop et al. 2019; Donno, Psaltis and Zarpli 2019; Lowe 2019; Paler, Marshall and Atallah forthcoming; Scacco and Warren 2018; Svensson and Brounéus 2013; Yablon 2012), but rare cases feature classic post-conflict contexts, meaning ten or fewer years after a civil conflict in the state. One such study, focused on ethnically-based soccer teams after a localized victory against rebels in part of Iraq, found positive effects on interactions among teammates but no change in generalized prejudice (Mousa 2018). Meanwhile, another study focused dialogue among university students in Sri Lanka found little effect on social identity measured through an index of survey measures, which the program had sought to change, based on an underpinning theory being that during a conflict or atrocity

individuals cannot identify peaceful pathways to meet their needs, feel victimized, and resort to extreme prejudice and exclusion (Lonergan 2016). Again, however, not many studies theorize and test potential links to conflict, beyond only cooperation or prejudice within a community.

Finally, a related set of studies, often using survey or lab-in-the-field experiments, test whether leaders' appeals or persuasion can foster cross-cutting interactions, cohesion, or similar outcomes, but find mixed results. For example, a study in Beirut of Shia and Sunni participants that randomized an elite appeal and cross-sectarian discussions that explored the conflict found more friendliness, or unconditional cooperation, between sects but no increased resistance to sectarian appeals that offered financial incentives for co-ethnic voting (Chang and Peisakhin 2019). Likewise, a study in Cote d'Ivoire, also couched in terms of contemporaneous tensions, that randomized video-based information found that Christians responded with less bias after political but not religious leaders' appeals for peace, whereas Muslims did so with theological rather than economic arguments in either leader's appeal (McCauley 2014). In Burundi, a persuasion treatment that randomized counter arguments to the position that individuals choose on forgiveness versus punishment made individuals *more* likely to maintain especially extreme opinions (Samii 2013). Even these programs emphasizing outgroup attitudes and behavior, and showing mixed results of intervention, do not directly address when conflict is likely to recur.

The aims of programs designed to reconcile communities and build social capital also often do not directly engage with a related set of studies indicating that ex-combatants and others affected by conflict tend to be more pro-social at least in some settings. In simulated contexts, such as respondents in a laboratory treated with a bargaining game meant to represent conflict, the control group is more prosocial (Abbink 2012). However, recent work also examines lab-in-the-field experiments in which former combatants, victims, and others in conflict-affected communities play games, often with randomized assignment of their partners, and at times with randomized emotional priming as well (e.g., although during conflict, Callen et al. 2014; Zeitzoff 2018). While some of these treatments are partially experimental, in terms of matching partners, for instance, most of these studies also rely on some sort of "as if" randomization. For example, a set of studies match communities or fighters to compared based on the idea that the spread of violence in Nepal or the rebels' recruitment through abduction in Uganda is random (drawing on Blattman 2009). These post-conflict games that fighting and even being exposed to conflict generally *increases* pro-sociality for those involved (the fighters: Bauer, Fiala and Lively 2017 in Uganda; and, others exposed: Gilligan, Pasquale and Samii 2014 in Nepal; Voors et al. 2012 in Burundi).⁹ Even those studies that indicate lower levels of trust, for example (notably Cassar, Grosjean and Whitt 2013 in Tajikistan), also indicate more participation (for a review, see Bauer et al. 2016). Some studies suggest altruism or other prosocial behavior is limited to co-ethnics (e.g., in a set of studies in the Balkans, Mironova and Whitt 2018; Whitt 2014; Whitt and Wilson 2007), although respondents' behavior is also shaped by other factors such as their age during the conflict (e.g. Bauer et al. 2014), and, ultimately, there is not strong evidence of prejudice against outgroups (again, Bauer et al. 2016). There are also mixed results in these studies about whether there is more discrimination by communities against fighters (e.g. Bauer, Fiala and Lively 2017 find little bias, but Osborne, D'Exelle and Verschoor 2018, also on Uganda, find significant

⁹ Being exposed to conflict also may have an effect on risk preferences, but the results vary dramatically across studies: for example, Gilligan, Pasquale and Samii (2014) finds no effect in Nepal; Voors et al. (2012) finds increased risk-seeking and impatience in Burundi.

bias). Overall, then, while post-conflict programs focused on communities may improve outcomes like cohesion, and we do not know their effect on peace more broadly, it is also possible that these communities may already feature high rates of pro-sociality and social capital.

Individuals: Finally, a set of experiments assesses whether programs in post-conflict contexts improve the well-being of those affected by conflict, at times with an aim of consolidating peace as well. Consider post-conflict employment programs: experiments on these programs found significant economic benefits across the board, especially with cash transfers (Blattman and Ralston 2015), but generally they did not “directly engage with the social fissures that make conflict or other antisocial behaviors a risk in these environments” (Brück et al. 2019, p. 19). One of the studies that most directly addresses these ties is a field experiment in Liberia that randomized training, counseling, and capital inputs for farming to high risk men, and it found self-report shifts away from illegal mining and interest in working as mercenaries in an ongoing war (Blattman and Annan 2016). The results from this study, though, were potentially not driven by attitudinal change but by conditional cash transfers that were not made available immediately but promised to those who remained in their villages. Moreover, many men did not exit these activities entirely (as noted in a review by Nolan, Knox and Kenny 2019, p. 22). Field experiments in Uganda that randomized two different programs to start businesses found significant improvements in economic outcomes such as income, but they found basically no effect on social outcomes such as antisocial behavior, collective action, or even support for the government (Blattman, Fiala and Martinez 2014; Blattman et al. 2016). One of these experiments found more community participation with the program but also increased resentment, although self-help groups helped mitigate the negative effects and amplify the positive (Blattman et al. 2016). Some of these programs are more removed from any tie to peace consolidation (Alfonsi et al. 2017; Blattman, Emeriau and Fiala 2018; Blattman, Fiala and Martinez 2014). During conflict, however, other studies on jobs or related programs have assessed outcomes like support for the government, and they have found mixed results, although the mechanisms may be specific to the context, such as beneficiaries in the Philippines who likely used their conditional cash transfers to pay revolutionary taxes (Crost, Felter and Johnston 2016; Khadka 2019; Lyall, Zhou and Imai 2019). Considering the reviews of this literature, most studies do not actually address peace consolidation, but rather assume a link between the development and conflict (Brück et al. 2019, p. 20); importantly, however, recent work theorizes that some forms of violence do not respond to employment, especially without therapy as well, and so these links remain very tentative in addition to being largely untested (Blattman and Ralston 2015; Gilligan 2016). Programs that combine resources and therapy in post-conflict contexts are often DDR programs, which have not been very frequently assessed experimentally, as discussed above.

There are dozens of experimental studies of individual-level programs that range from cash transfers and skills training, to cognitive behavioral therapy (CBT), to general education, to broader early childhood programs, and to informational campaigns. While the space available constrains this chapter from covering each of topic, some are much more closely linked to the possibility an individual or group will return to fighting, such as therapy to treat trauma or post-traumatic stress disorders, while others are less so. However, many of these studies focus on outcomes related to general well-being. Perhaps not surprisingly, given the diversity of programs studied, these studies showed mixed effects. These programs seek to change the lives of

individuals, including at times by also changing social norms, and they are conducted in post-conflict contexts, and so they do not speak to peace specifically.

A final important note: many of these programs are very similar to those implemented outside post-conflict settings. For example, many jobs programs are conducted in developing states without conflict and many seeking to prevent violent extremism are run in states with current conflicts. Comparing across contexts that are and are not post-conflict can help researchers understand if the same experiments could be run in more stable contexts – or during active conflicts where other resources may be available (see Wolfe 2019 on how funding priorities also shapes this). There are, however, reasons to believe that these programs may not work the same way in post-conflict contexts, necessitating this testing. These reasons include potential distributional effects that may affect the power dynamics of the sides, particular patterns of interactions with others, logistical challenges such as population displacement, different concerns about side effects such as retraumatization, problems due to clustered nature of conflict’s challenges including violence but also weak institutions, low development, and low trust, increased international involvement, and choosing time horizons since short-term harm can disrupt a broader peace process even if it would otherwise result in long-term benefit.¹⁰ And, of course, outcomes most directly related to conflict recurrence like whether political entrepreneurs reignite old tensions may operate in particular ways in post-conflict contexts.

Overall, then, considering potential directions forward, peace consolidation programs could benefit from more experimental work going forward, but an emphasis on bringing together a diverse set of programs and studies into theoretical frameworks can advance our understanding of what works and what does not work to reduce the risk of conflict recurrence. As one of these studies notes, “nearly every fragile state funds some form of public works scheme, training, or other employment intervention for young men” from the World Bank (Blattman and Annan 2016). And many of these donors are willing to work with scholars on experiments. This represents an area with potential. But studies that do not engage theory of conflict recurrence, even as they use peace to frame the project or assert its link to certain development outcomes, and test relevant outcomes are often not producing greater understanding of the topic. This literature covers a wide range of programs studied but the next important step is to tie them more to specific theories and test more steps of these theories. More individual experimental studies may show how their interventions and outcome measures fit with new or existing theory on conflict recurrence, including by specifying potential scope conditions, going forward. In addition, new meta-analyses or other reviews that seek to draw together experimental work in particular areas (e.g. Barron 2011; Blattman and Ralston 2015; Brück et al. 2019; Casey, 2018; David 2017; Dittmann, Samii and Zeitzoff 2017; Ellsberg et al. 2015; Gilligan 2016; King, 2014; Nolan, Knox and Kenny 2019, especially those that seek to tie experiments to theory) may provide more forward progress on this topic than new experiments at the moment. However, many of these experiments do not have common treatments – making comparison difficult – so this literature seems ripe for a Metaketa-type of approach in which a broader theory of conflict is tested through multiple experiments doing primarily the same thing in different locations, considered under a unified theory that they can test together (Dunning 2016; also see Blair and McClendon’s chapter in this volume). Given that these programs are funded and at times conducted by outside actors, this literature would also benefit from applying ideas from studies

¹⁰ I thank Graeme Blair for suggesting some of these potential differences.

on the effects of foreign aid and development programs that experimentally credit different providers and examine attitudes toward the state (e.g. Baldwin and Winters 2018; Brass 2016; Cammett and MacLean 2011; Dietrich, Hyde and Winters 2016). These are likely to be field experiments in many cases, especially given willing partners, but some of the conflict outcomes may necessitate designs that include surveys on attitudes because fighting may simply not be observable in these contexts. Finally, some states have received many experiments on the same type of program, while others have received none. For instance, Liberia has been the main site for projects on security sector reform. In general, regime type and governance quality may shape where studies are conducted (also on this, see Blair, Iyengar and Shapiro 2013). But Metaketa-type coordination could also ensure that tests take into account types of post-conflict contexts, either by selecting for similarity or difference depending on the aims of the particular effort.

Peace consolidation programs potentially represent an area that could benefit from more theoretical work to accompany the experimental work going forward, as well as from organized sets of experimental studies across contexts.

III. The challenges of experiments in post-conflict contexts

Although the peace stabilization and peace consolidation literatures each face their own challenges, as described, studies on both topics also share some obstacles. Both peace stabilization and peace consolidation studies face logistical and ethical challenges inherent to unstable contexts, and, as civil conflict ends, both experience the rush to improve these situations undertaken by many different actors. The experiments described above, across these two perspectives, drive home these challenges. Both would benefit from communities of researchers advising on these projects, as well as from more work defining the contexts and programs through simple tools like typologies.

From an ethical perspective, the possible risks are extreme – including a potential return to conflict – but the benefits also extreme. The magnitude of the cost-benefit assessment is almost incomparable to many other contexts because of a very real possibility that a program could produce conflict recurrence. This, however, also means that the benefits can often be compelling because peace is so important – and yet something we know relatively little about how to produce reliably over time although aggregating results of past work is essential to identify any negative consequences. Experimentation in these contexts is therefore often justifiable *because* we do not know whether a particular treatment helps, so more benefit may result from providing *or* withholding it (a condition of uncertainty called “equipose”; see Djulbegovic 2009), and because scarcity means even treatments expected to work cannot be applied to all areas in a state or to all states recovering from conflict. Despite the rush of aid to these post-conflict contexts, resources for any particular project are often limited. Challenges remain nonetheless (and some of these are discussed in the expanding literature on research in post-conflict contexts generally, including Campbell 2017; Cronin-Furman and Lake 2018). Subjects are especially sensitive due to an often deeply traumatic conflict-time experience, but also because of the continued economic hardships resulting from destructive fighting. In addition, many studies do not allow informed consent or reliable individual debriefing in the traditional sense. In cases where patrols randomize which villages they visit, for example, obtaining individual or even collective consent can be difficult, if not impossible. Afterward, in some of these scenarios, individuals can be told the results of studies through national conferences or media programs, assuming these are widely

broadcast. But some, especially when the outcomes of interest are occurrences violent attacks or community-level voting behavior over time, informing at a more individual level is impractical (for more on this decision, see Teele's chapter in this volume). In addition, of course, there are special risks to research teams in these contexts given the instability.

From a logistical perspective, significant challenges arise in post-conflict contexts due to instability, producing measurement issues and frequent change, which can present both internal and external validity challenges. First, a key outcome in many of these cases is propensity for violence, fighting, and, in the aggregate, conflict. Measuring individual propensity and even participation in this type of activity is often only feasible through surveys, but researchers have adopted innovative question designs to overcome issues such as faulty recall and social desirability (e.g. Zeira 2019). Beyond surveys, recent studies have also innovated in tracking broader fighting and even aggregate conflict, including through crowd-seeding and remote sensing (e.g. Van der Windt and Humphreys 2016; Witmer 2015), as well as combinations of these mechanisms (e.g. Arriola et al. 2019). Second, deeply unstable contexts also mean that individuals, and even program providers, are more difficult to identify and track. The changes also mean that subjects adapt, learn, and grow over time. This presents a set of challenges that affects internal validity in significant ways, including maturation and performance effects, which can be difficult to assess (McDermott 2002). Donors and implementers drive some aspects of this challenge because they often switch programs midway through based on their perceptions of or evidence on program effectiveness. But recovery from conflict that has upended communities also produces change. In addition, this feature of post-conflict contexts also means that those who can be recruited in these contexts may not be representative of the broader population due to displacement and other shifts, in addition to the normal challenge of subjects who are reachable potentially being more prone to alter their responses because they are being watched and even becoming "professional subjects" over time. Given these changes, some aspects of the treatment assignment or uptake may not be effectively randomized, and the measures that can be obtained may also be unreliable (also McDermott 2002).

In addition, as part of the effort to help, multiple actors often rush into these post-conflict contexts seeking to improve situations, causing them to interact with each other and with the context. Many actors are involved in these post-conflict contexts, at times without substantial coordination from weakened states or others, especially as these actors expand beyond the auspices of the U.N. (see Fortna and Howard 2008). This dynamic produces a proliferation of programs that often take very different approaches in the same places (e.g. Cutillo 2006). Despite the presence of some "standard operating procedures" on the part of the U.N., for instance, most cases receive only a few common features, since these donors and implementers seek to customize to the case and fulfill any terms in peace agreements, producing combinations of post-conflict elections, security sector reform, disarmament, demobilization, and reintegration, development programs, and transitional justice programs of many different types (for example, on just the last type of programs that have proliferated, see Knight 2010; Muggah 2010; Thoms, Ron and Paris 2010; Vinjamuri and Snyder 2004). Most of these treatments are also bundled, because the U.N., for example, deploys patrols and resources simultaneously (e.g. Blair et al. 2019b). Such bundling can obscure the theoretical mechanism behind any improvements identified.

These programs, but also the heterogeneity of conflict itself, produce challenges of randomization, external validity, as well as mechanism testing. Many of the related experimental challenges are due to the substantial but often unmapped variation in programs – but also to the contexts in which they operate. Post-conflict contexts are heterogeneous in terms of the particular goals and means of the sides, the duration and severity of conflict, how they ended, and even how neighbors and other outside actors were involved. For example, consider the effect of programs on areas in which death tolls were in the thousands versus areas that were barely affected, areas that have had long histories of conflict or other weakness versus those with previously capable state security institutions, and so forth. Then also consider the effect of programs in states with different features, including states where peace agreements end the conflict versus those featuring a victory, for instance. Post-conflict programs interact with – and shape – popular responses across in a multitude of ways given that recent experiences during the conflict were often traumatic and may have been different from other contexts in important ways (e.g. Autesserre 2014). The particular combination of factors to which subjects are exposed, not just the treatments chosen in the post-conflict period but also any “treatment” that have already been received during conflict, can make it more likely that an experiment will identify a spurious or an irrelevant effect. For example, if individuals in areas affected by longer-term violence are especially prejudiced and hard to move through messaging, but randomization strategy inadvertently fails to account for exposure to an older conflict – for instance by overlapping in community assignment with some prior boundaries – the inference is affected. In addition, these features also present potentially severe external validity challenges for experimental studies, especially given that many are conducted in a narrow range of contexts, such as Uganda, where the main conflict ended through a victory rather than a settlement (even though the latter is much more common in the current era).

For both types of projects, then, researchers must work closely with partners, adapting as others adapt, and being especially sensitive to these contexts in which so much is changing (for more on these partnerships, see Levine’s chapter in this volume). They also, however, need to work to unbundle treatments. In addition, researchers need to go beyond traditional institutional review boards in these contexts to balance costs and benefits in these contexts, consulting with colleagues, state review boards, partners, and local communities in which they work (also see Desposato 2015). Advisory committees of those working in these areas would benefit many of these researchers. Existing networks such as EGAP could provide the bases for these committees that are already at times informally convened. These advisory committees would do well to go beyond go or no-go decisions to inform researchers about best practices in what protections are available, perhaps even establishing consortiums to study the effectiveness of different possible protections (e.g. a recent project in Nigeria randomized project committees to oversee experimental programming; see Wolfe 2019). In addition, more work on developing typologies of conflict types and post-conflict programming will help these researchers better develop their randomization strategies and gain traction on the potential limits of their external validity.

IV. Directions forward

Suggestions about how to do more with experimental studies in post-conflict contexts emerge throughout this chapter. From the peace stabilization perspective, new studies should conduct work on elites and mechanisms of institutional change, which may be limited to survey or lab-in-the-field experiments but can still advance our knowledge. From the peace consolidation

perspective, new work should both draw on theoretical frameworks more explicitly, at times incorporating outcomes variables more explicitly related to conflict, and feature Metaketa-type sets of experiments explicitly using common treatments that are tied to arguments about mechanisms of change across contexts. Finally, any new experimental work in these contexts would also benefit from advisory communities of those working in these areas as well as the development of typologies that help define potential scope and theoretical conditions.

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