Charting a Course for Applied Research in Peace Building
(concluding chapter to USIP e-book)
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Introduction: The Importance of Evidence Reviews to Programming

A thorough review of existing evidence on high-priority learning can strengthen programming in important ways. And, thus, we begin this concluding chapter by describing how such reviews can provide value. Although this list is not comprehensive, it highlights several of the greatest contributions to be drawn from the book's main chapters.

1. On a basic level, evidence reviews can ensure that both practitioners and researchers are aware of the evidence that already exists, thereby preventing the duplication of research efforts and allowing new research to build on (rather than parallel to) previous findings. This often involves breaking down research silos by compiling evidence from different disciplines. For example, the chapter on peace mediation draws extensively on evidence from social psychology to consider how psychological characteristics such as trust might affect mediation efforts. This effort is combined with insights from more traditional research that considers how factors such as process design and style affect mediation outcomes. The chapter concludes that measuring levels of interpersonal trust between parties may serve as a reliable indicator of progress in mediation efforts.

2. Evidence reviews can also identify the critical gaps in available evidence that would be especially useful for practitioners. For example, the chapter on peace negotiations points to a lack of research on how the characteristics of negotiating parties might shape the outcome of negotiations. The best instances of this type of review can actively shape future research that works to fill such gaps. The chapter on peace negotiations draws on interviews and case studies to posit testable hypotheses, as well as proposes specific quantitative indicators that could be used to measure critical concepts and test these hypotheses. This exercise provides a clear path for USIP to commission research that would advance program design. Similarly, evidence reviews can identify high-impact findings from less formal practitioner research, qualitative scholarship, or quantitative studies that rely on purely observational data that should be tested using alternative methods. For example, the chapter on strategic religious engagement identifies a large set of findings in existing work (which should be subject to more rigorous testing), as well as points to examples from a new "third wave" of empirical research that is beginning to tackle this critical task.

3. Finally, evidence reviews provide an opportunity to think through the assumptions that underpin practitioners' theories of change (TOCs) and to map these assumptions onto

the available evidence. Identifying unrealistic assumptions, or assumptions that are not supported by research, can clarify why conventional programming has failed to achieve the desired impact on outcomes. Similarly, such comparisons may reveal that TOCs are too convoluted or complex to be investigated empirically, suggesting a need for more simple, tractable models of conflict and its underlying causes.

While the benefits of evidence reviews are potentially quite large, the teams conducting such reviews face significant challenges.

1. First, an effective review must agree about what rigorous evidence looks like. Making recommendations based on evidence requires claims about empirical relationships that exist in the world or about the impact of specific policies and programs on important outcomes. However, the authors of the chapter on evidence for peace building argue that, both within and across organizations, there is no consensus on what constitutes rigorous evidence in peace building. The effective use of evidence requires organizations to define standards for evidence in their decision-making.

   Contemporary social science relies largely on the application of statistics to quantitative data to make such inferences about relationships and impacts. At the same time, qualitative data and contextual knowledge play essential roles in interpreting quantitative evidence and recommending ways to incorporate evidence into the design of policies and programs that improve outcomes in the real world.

   As is described in the chapter on evidence for peace building, such an approach allows for a pluralistic understanding of the types of evidence that are useful. However, strong recommendations based on claims about the evidence for an empirical relationship or the impact of a specific intervention should require evidence based on rigorous research methods.

2. Second, an informative review requires authors not only to review existing research, but also to recognize the strengths and weaknesses of different types of evidence in consistent, principled ways. Drawing on contemporary social science research to inform programming requires a basic understanding of the research methods being used. Social science research varies tremendously in the precision with which important concepts are measured, the extent to which the samples used for analysis are representative of populations of interest, and the ability of research designs to credibly isolate a causal effect.

   Often, researchers face constraints in easily observing and measuring concepts, in collecting data from certain populations, and in ethically manipulating causes of interest in the real world. Reviewers of evidence must look beyond basic considerations when determining how well the research design approximates the "gold standard" of a randomized control trial (RCT): for example, they must consider (1) whether important behaviors were measured by observing real-world behavior or by collecting self-reports about behavior through surveys; (2) the extent to which the primary sources used to generate measures of important concepts, such as news articles or administrative records, were biased in their coverage of events or groups or were subject to manipulation; (3) the extent to which a "treatment" in experimental or quasi-experimental studies resembled the "treatment" deployed in an intervention; and (4) whether studies included the population of interest or some other populations or contexts.

Evidence reviews must take these considerations into account when assessing the strength of research findings. This matters for individual studies (where research using different methods can produce conflicting findings) and for entire bodies of literature (where certain areas of investigation are more difficult to study with the most rigorous methods). Where research using different methods yield substantively different findings, studies that use better research designs, more realistic treatments or relevant populations, and sounder measurement strategies should receive more weight when synthesizing findings to inform program design or policy recommendations. Alternatively, some causal relationships are nearly impossible to study using credible research designs, and some important behaviors and events are nearly impossible to measure directly. When many studies produce the same findings but they all suffer from similar deficiencies in design or measurement, there may still be significant uncertainty about the validity of the conclusions. For example, the chapter on strategic religious engagement identifies a common set of methodological weaknesses that severely limit the ability to derive strong policy recommendations from the available research. In these instances, communicating the extent of uncertainty about findings is essential.

Without some degree of training in the contemporary tools used by social scientists, it is extremely difficult to understand how much confidence is merited by a specific study or even an entire body of research. However, practical experience and contextual knowledge are often necessary for mapping evidence from formal research to program and policy design. For this reason, the best evidence reviews will include input from both trained social scientists, who can assess the strength of different streams of evidence, and from experienced practitioners, who can translate research findings into actionable recommendations. Through an iterative process, this book's authors' worked to solicit and incorporate the perspectives of social scientists and practitioners in each chapter.

3. Third, generating actionable recommendations from an evidence review requires linking social science evidence with practitioner theories of change. However, practitioner TOCs

often posit complex, conditional relationships where positive outcomes are contingent on multiple conditions and assumptions. These TOCs are often informed by deep substantive experience to describe the conditions most likely to yield success for a specific intervention, but they are difficult to test empirically. Alternatively, social science evidence is often designed to test simplified hypotheses that strip away complexity. To facilitate clear recommendations based on rigorous evidence, practitioner TOCs and social science hypothesis testing must speak to one another. Specifically, practitioner TOCs could often benefit from being broken down into simple, testable hypotheses, while academic research often needs to grapple more explicitly with the ways that local conditions and implementation can shape variation in empirical relationships observed across contexts.

Relatedly, normative commitments that underpin TOCs must be interrogated. To the extent that organizations have a normative commitment to the validity of an empirical claim, these empirical claims should be prioritized for rigorous evaluation by an independent research team. For example, the chapter on youth, peace, and security criticizes quantitative research for its findings about youth participation in violence. However, understanding these descriptive facts about the world are essential for developing effective interventions. For example, the authors criticize “demography, civil war, and security studies” on the grounds that they “bring attention to youth participation in violence but rarely put forward arguments for fostering youth political participation in peace-related decision-making processes.” They go on to criticize this work for contributing to stereotypes that drive further exclusion of youth from politics. While we agree that research should also explore arguments for fostering youth participation, we strongly disagree that rigorous research from demography and other fields should be discouraged or ignored because it seeks to describe empirical facts about the way the world is rather than the way it ought to be. To the contrary, this research has played a fundamental role in informing the design of future research that focuses on understanding low levels of youth engagement and designing interventions to increase it.

Throughout the remainder of this concluding chapter, we reflect on the contributions made by each section. For each of the book’s four thematic groups, we briefly describe the constituent chapters—including their approach to reviewing evidence, any important gaps in the review, and ways that the review can guide future applied research to build a stronger evidence base. We conclude each section by synthesizing the insights from the constituent chapters.

Peace Processes: The Core Tools

The section on track 1 peace processes provides a compelling overview of the four core tools used in the peace-building field: negotiation, mediation, dialogue, and reconciliation. Although there is a great deal of overlap in the tools under consideration, including the actors that take part in these processes and the importance of social factors such as social cohesion and trust, these chapters take very different approaches to the review of evidence.

Negotiation

The chapter on negotiation provides an excellent model for evidence review. It combines a thorough review of existing research and practitioner knowledge to chart a clear path for high-impact applied research. In their review of evidence, the authors comprehensively evaluate the most rigorous academic work on conflict negotiation and identify a major gap in research that, if filled, could yield actionable insights for program design. Specifically, the authors find that although practitioners see the characteristics of parties involved in negotiations as a critical factor shaping negotiation outcomes, academic research has yet to theorize about the influence of party characteristics or investigate these matters empirically.

To facilitate future research on the topic, the authors use practitioner interviews to gather common knowledge about the ways in which party characteristics matter for outcomes, as well as use case studies to illustrate how these insights have been applied in practice. Through this process, the authors develop a TOC describing the specific party characteristics that appear to be linked to successful negotiations. Finally, they carefully define specific indicators that could be used to measure these characteristics in future applied research and propose specific research questions that can be investigated empirically.

To carry this research to fruition, specific means of collecting data and testing the TOC should be proposed. For example, quantitative data on negotiations and their outcomes has been used extensively in academic research. Augmenting these existing datasets by coding participating parties according to the indicators proposed in the chapter on negotiation could generate descriptive findings that support the authors' TOC or suggest more complicated relationships that require further investigation. Recent methodological research in political science has found that artificial intelligence tools are often as good as humans at performing such coding tasks[1], which would dramatically reduce the cost of generating such data.

While such coding would allow for an observational analysis that reveals correlations between the authors' "viability" factors and the success or failure of negotiations, it is unlikely that such a dataset could contribute causal evidence for the TOC. However, survey experiments involving

[1] Heseltine, Michael, and Bernhard Clemm von Hohenberg. "Large language models as a substitute for human experts in annotating political text." Research & Politics 11.1 (2024): 20531680241236239.

members of political parties that have been, or may be involved in, peace negotiations could be a relatively low-cost means of isolating the causal effect of viability factors on negotiation outcomes. For example, presenting respondents with hypothetical negotiation partners; randomly varying the extent to which these partners exhibit the viability factors (such as authority, legitimacy, capacity, necessity, and confidence); and soliciting respondents' perceptions of whether and why negotiations are likely to be successful would provide more direct evidence of the relative importance of individual viability factors for success. Combining insights from observational data on the correlation between real-world negotiating party characteristics and outcomes with experimental evidence from surveys of party members would provide extremely rigorous evidence for or against the TOC developed in the chapter on negotiation.

Mediation

The chapter on mediation takes a different approach but also yields insights that could guide future applied research to improve mediation programming. The authors conducted expert interviews and group meetings with a large sample of academic researchers and mediation practitioners to "take stock of contemporary perspectives on trust in peace mediation." Through this process, they conclude that although cognitive trust has played a central role in much of the research on mediation, there has been insufficient attention paid to the role of affective trust between mediation parties in shaping outcomes.

To make a case for the importance of affective trust in mediation, the authors draw on evidence from other disciplines, such as psychology and management, where affective trust has been studied extensively. They discuss in-depth how trust is built across key relationships, what the most relevant objects of trust are in mediation efforts, and how signs of trust could be used as indicators of progress in ongoing mediation efforts. The authors also draw on case studies to emphasize the gap between the importance of affective trust in mediation outcomes and the attention it has received in past research.

By mapping theory and evidence from psychology and management onto our understanding of mediation, the authors propose a framework for studying which types of affective trust and methods of trust-building developed by these disciplines would be most effective for overcoming specific mediation challenges at different phase of mediation. However, although they establish the importance of affective trust for other social phenomenon, they largely assume that affective trust has a significant influence on outcomes in mediation. While a great deal of research is cited, future work would benefit from deeper engagement with the strengths and weaknesses of available evidence. Most importantly, it is important to acknowledge that findings from experiments conducted in psychology labs involving dyadic relationships between ordinary citizens may not fully apply to high-stakes mediation efforts involving representatives of political groups, each of which have their own internal dynamics.

Before importing methods and approaches from psychology and business negotiation, it is necessary to establish whether affective trust plays a similar role for the type of groups involved in conflict mediation.

To establish the importance of affective trust, researchers and practitioners should work together to specify how the concepts identified in the chapter on mediation—such as trust in mediators, trust between conflict parties, and trust within conflict parties—could be turned into indicators and measured in rigorous research. This effort may also help in establishing specific research questions that should be prioritized in applied research.

Another future priority for progress in the mediation field should be a deeper engagement with the extensive literature on cognitive trust. For example, a substantial body of work in political science uses formal theory to explore the conditions under which mediators can build cognitive trust and effectively convey information between parties.[1] Integrating affective trust into the dominant formal theoretical models of mediation could offer valuable insights into how affective and cognitive trust might interact under different conditions. Ideally, the insights from these models would be backed up with empirical evidence from observational data that extend existing data on mediation efforts with information on levels of affective trust within and between parties and between parties and mediators.[2]

Unfortunately, it may be difficult or impossible to code historical mediations for levels of affective trust. The primary sources available are unlikely to contain enough information to code it with confidence for a large number of cases. A more feasible path to generating empirical support for the importance of affective trust may involve a survey of mediation parties and mediators. For example, a survey experiment could present party representatives with hypothetical pairs of mediators and opposing parties, randomly vary the extent of trust across parties, and solicit respondents' perceptions of the likelihood that mediation will be successful. Such an exercise could afford compelling evidence to inform the ways in which trust-building is emphasized in real-world mediation efforts. Alternatively, if respondents tend to place much greater importance on cognitive rather than affective trust, this should raise doubts about the returns to investing heavily in building affective trust during mediation.

Dialogue

The chapter on track 2 dialogue focuses on the development of a contingency model to guide practitioners in determining "what form of dialogue might be useful under what circumstances and with what participants." The authors begin with a typology of dialogue methods, identifying three types of dialogue distinguished by their goals, focus, participants, and the stage of conflict for which they are best suited. In contrast to other chapters, this contingency model is focused more on organizing models according to "what the three types of dialogues claim to achieve" than on assessing "empirical evidence on what each has achieved." The "meta-synthesis" used in this review draws on research from a wide range of disciplines using diverse methodologies, thereby yielding an extremely useful resource for practitioners looking to deploy dialogue interventions in conflict settings.

In addition to providing the base for the contingency model, the meta-synthesis identifies several important gaps in evidence. Two gaps are most apparent. First, the amount and nature of evidence on the dialogues' impact on desired outcomes is extremely varied across the three forms. Pure (or relationship-focused) dialogue has been subject to numerous RCTs exploring the conditions under which it is successful and the mechanisms through which dialogue impacts outcomes such as prejudice reduction. Because interventions involve ordinary citizens and outcomes can be measured at the individual level, such interventions are relatively easy to implement and evaluate using rigorous quantitative social science methods. Problem-solving (or outcome-focused) dialogue also has a relatively extensive evidence base, drawn from qualitative case studies. Unlike pure dialogue, problem-solving dialogue involves more influential participants and is focused on high-level outcomes, including group agreement on potential solutions to conflict that could be adopted by policy makers. The focus on outcomes that cannot be measured at the individual level is a barrier to more rigorous evaluation methods. Alternatively, little effort has been made to evaluate the efficacy of agonistic dialogue, which has less clearly defined objectives.

While the authors note that there is "adequate evidence on the effectiveness of pure and problem-solving dialogue," they acknowledge a need to provide evidence on the details of the contingency model.[3] To address this gap, they suggest pilot-testing through case studies and comparative analysis to ascertain how the different types of dialogue may support various forms of peace building. However, researchers should also aspire for more rigorous evidence on these matters. For example, the contingency model recommends specific stages of conflict during which different types of conflict are most likely to be effective. Pure dialogue is believed to be effective at the lowest stage of conflict escalation. Yet recent rigorous evidence suggests that pure dialogue can also be effective at higher levels of escalation.[4] Future research should continue to explore the extent to which these tools are effective outside of the conditions in which they were designed to be applied.

Similarly, the contingency model specifies the participants that each type of dialogue is designed to incorporate. However, testing the impact of pure dialogue on the attitudes and behavior of more influential figures or the incorporation of grassroots participants in problem-solving or agonistic dialogue could stretch our understanding of their utility.

Furthermore, there is ample room for future research to refine our understanding of how and when dialogue approaches are most effective. For example, RCTs should be used to determine how to select dialogue participants in a way that encourages the spread of attitudinal and behavioral change beyond direct participants through participants' broader social networks.[5]

Similar methods can and should also be applied to problem-solving dialogue. Convening multiple groups of influential participants, randomly varying their structure (such as the ethnic diversity or homogeneity of their participants), and assessing their ability to generate viable solutions to intergroup conflict could produce extremely valuable insights. Finally, although there are not clear outcomes traditionally associated with the agonistic method, researchers interested in understanding agonistic dialogue on individual-level outcomes could easily test the impact using rigorous randomized trials.

Reconciliation

The chapter on reconciliation relies on synthesizing insights from practitioner interviews to identify practitioner TOCs, expert interviews to identify recommended processes and skills, and document analysis and a survey of practitioners to identify institutional arrangements that enhance the durability of reconciliation efforts. This process resulted in a typology of approaches to reconciliation, from which a reconciliation matrix was developed. The matrix specifies the drivers of reconciliation (each driver corresponds with one of the four prongs), the level of society at which the driver applies, the TOC that informs the drivers connection to desired outcomes, the processes involved in effective implementation, and the institutional support needed for sustainability.

This matrix resembles the contingency model developed in the chapter on dialogue and is an extremely useful resource for practitioners looking to facilitate reconciliation after conflict. However, while the author's collection and synthesis of original data from practitioners makes a valuable contribution, the recommendations from the matrix would benefit from rigorous social science research (as is also the case with the contingency model). Much of the evidence from research on topics such as contact theory, social cohesion, and prejudice reduction could be brought to bear on the reconciliation matrix's contributions. Furthermore, significant literature in political science offers theory, data, and rigorous empirical evidence on a wide range of questions surrounding transitional justice. This body of work would provide fertile ground for deeper collaboration between practitioners and academics.

In particular, the University of Chicago's Transitional Justice and Democratic Stability Lab, led by Professor Monika Nalepa and featuring a Global Transitional Justice Dataset, is actively building an evidence base around fundamental elements of the reconciliation matrix and the TOCs that it invokes. The dataset provides cross-national data on the use of important reconciliation tools, such as truth commissions, after conflict. Through collaboration, these data could be augmented—with more fine-grained coding of different types of truth commissions as well as data on institutional arrangements—to test whether the institutional support mechanisms stipulated in the reconciliation matrix are actually associated with more durable peace.

In addition to using existing research and data from academia to reflect on the claims made in the reconciliation matrix, other social science tools could be useful in deepening the evidence base on reconciliation. For example, survey experiments have measured the extent to which civilian collaborators are seen as culpable for violence and have tested ways to mitigate the negative impact of these perceptions on support for violence and retribution.[6] Similar approaches have been used to study the types of peace agreements that civilians are willing to support and the characteristics of national dialogues that make citizens more or less likely to support them. These methods not only carry important implications for the design of reconciliation programs, but they could also serve as a model of rigorous research that can lend critical evidence to the claims made by the reconciliation matrix.

Conclusions

As the discussions of each chapter make clear, the diverse units within USIP that completed these evidence reviews took very different approaches. However, each makes an important contribution that can improve the design and implementation of programming that utilizes the four core tools of peace building. Furthermore, the substantive overlap across these chapters suggests several common areas of investigation and approaches to research where evidence could have an outsized impact on how and where to deploy peace-building core tools. Most importantly, a single survey that includes a large sample of respondents that represent conflict parties could be designed to include four modules focused on each core tool. These modules could include survey experiments that provide new, rigorous evidence on how best to structure the application of these tools in the real world. Given USIP's deep experience working with conflict parties, constructing a large list of members/representatives of conflict parties across several countries and inviting these individuals to participate in a survey could be the basis for a collaboration with academics willing to co-design and manage data collection.

Peace Processes: Tools for Promoting Inclusion

This section of the book considers the benefits of increased participation of women, youth, and religious actors in peace processes, as well as the proper role for external support in nonviolent social movements. As with the previous section, despite the substantive overlap across these topics, the chapters take very different approaches to evidence review. This results in big differences in the extent to which they engage with the rigorous evidence that is currently available. However, these different approaches highlight the importance of diverse perspectives on complex questions that demand both rigorous evidence and deep substantive and contextual knowledge.

Women, Peace, and Security

The chapter on women, peace, and security (WPS) identifies factors that facilitate or impede "the meaningful participation of women" in peace building, with a particular attention to contexts of insecurity and natural resource competition. The authors make a compelling case that insecurity and resource competition adds formidable barriers to women's participation while also making women's participation even more important than in other contexts. In doing so, the authors grapple with many of the same challenges as the succeeding chapter on youth participation, including the need to resist the characterization of women (youth) as victims, the importance of heterogeneity in the needs and motivations of women (youth), and a need to reduce barriers that make participation disproportionately hard for women (youth).

The authors begin the chapter with a clear description of their TOC and the research question guiding the evidence review. The TOC links the meaningful inclusion of women in peace building with improved gender relations and security outcomes, promising that a deeper understanding of the factors that allow for meaningful inclusion would allow for better policies and programming in the field. From this TOC, the authors set out to uncover "what factors facilitate or impede the meaningful participation of a diversity of women in peacebuilding at the local level in contexts of pervasive insecurity and natural resource competition."

The authors also present a helpful review of key terms, which makes the chapter much more accessible to readers from outside the immediate community of practitioners. Despite their declaration that the chapter "takes as given that the meaningful participation of diverse women . . . is integral to positive and sustainable outcomes," the chapter includes citations to rigorous research demonstrating an empirical basis for these claims.

The review consists of two primary components. The first includes an examination of writings on women's participation in relation to the United Nations WPS agenda, of policy and practitioner reports, and of a sample of primarily qualitative academic research. The review suggests that WPS research has focused on women's representation in formal processes and the ways in which women's participation contributes to peace outcomes. The authors conclude that the literature is only beginning to attend to the link between environmental factors, conflict, and peace building; they cite a variety of studies, including rigorous work linking climate change and various forms of conflict as well as work linking climate change with increases in gender-based violence (GBV).

The second component includes original case studies on Colombia, Honduras, and South Sudan. The cases highlight the experiences of women leading peace-building efforts in these countries and reveal common lessons about the most important challenges faced by peace builders and the critical factors shaping their success. The authors use three themes to organize common factors that facilitate and impede meaningful participation in local peace building: leveraging collective power and building capacity, navigating intersecting identities, and responding to state fragility and structural violence. The authors draw on these themes to make insightful recommendations for policy and programming on women's participation in contexts of conflict and resource competition.

While the original data collection to inform these case studies yields valuable insights, the authors could have usefully engaged with existing rigorous research that also has implications for women's participation. For example, the authors cite research linking climate change with GBV and the ways in which pervasive insecurity increases GBV through the availability of weapons and the breakdown of institutions of justice. Their recommendations identify several ways in which cultural change may help to address this daunting problem. However, they do not engage with the large body of evidence from RCTs that effectively reduced GBV across a variety of security contexts.[2]

There are several opportunities for rigorous evidence to build on the chapter's recommendations for women's engagement programming. First, the authors recommend investing in networks and coalitions of women's organizations that can facilitate collective action. Their case studies provide compelling examples of how such networks allowed diverse groups of women to create political pressure for change.

The importance of both individual and organizational networks for collective action has received extensive attention in the social sciences. At the organizational level, a recent RCT tested the impact of a capacity-building intervention among local nongovernmental organizations (NGOs) in Cambodia, investigating the ability of the intervention to strengthen NGO networks and improve the capacity of individual NGOs. However, despite a baseline of relatively weak networks between NGOs, the intervention was not successful at expanding them, which suggests an urgent need to study the most effective means to support civil society networks.[7] Interventions that can successfully strengthen civil society networks could be leveraged to analyze how stronger ones impact NGO behavior and outcomes.

The authors also recommend continued investment in capacity building among women activists and community leaders, including training and skill-building workshops to increase feelings of self-efficacy and self-confidence. Social scientists have developed and tested a large number of interventions designed to encourage women's participation in politics, including methods that boost feelings of self-efficacy.[8] While training and skill-building workshops may accomplish these objectives, it is possible that lower-cost, more easily scalable interventions might be deployed to boost women's engagement. Recent research also suggests that providing direct experience with political participation might be a highly effective way of encouraging engagement among populations that have been traditionally excluded from formal politics.

Mengsteab and others built on these findings to design an intervention that connected Ethiopian university students with formal civil society institutions focused on peace building.[9] An RCT impact evaluation found that the program caused a large increase in both self-reported measures of civic engagement as well as behavioral measures of engagement collected from NGO volunteer records. Importantly, this increase in engagement was evident for both female and male students and for students from dominant and minority ethnic groups. Such

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[2] https://blogs.worldbank.org/investinpeople/evidence-action-how-prevent-and-respond-gender-based-violence

interventions enable direct engagement with members of civil society and have the added benefit of building individual networks that can also encourage political engagement.

Yet, while these approaches may increase women's likelihood of seeking out opportunities for political engagement, they cannot ameliorate discrimination that seeks to exclude women. Changing prevailing norms around the inclusion of women in these initiatives—for example, by providing rigorous empirical evidence to gatekeepers that the inclusion of women in peace-building initiatives can lead to more lasting peace, or by changing prevailing norms around the role of women in society—may attack these deeper barriers.[10] All of these approaches are backed by existing rigorous work and can be paired with context-specific innovations and rigorous evaluations to track their impact on the most important outcomes.

Youth, Peace, and Security

The chapter on youth, peace, and security begins with the assumption that "current power dynamics" are the primary factors that limit the meaningful participation of youth in leadership and decision-making spaces in peace building. The chapter then sets out to "identify the limiting factors" that maintain these power dynamics. Based on interviews with youth leaders and a review of "policy documents, program reports, and scholarly literature," the authors present youth perspectives on the barriers to participation that they encounter, criticize mainstream social science research, and contrast this work with the types of research that they believe properly "build on youth experience to advocate meaningful youth engagement."

The authors take a critical perspective that largely dismisses the ability of mainstream social science to make meaningful contributions to peace building. In fact, they go further in blaming research from fields like demography for reifying the oppressive institutions that suppress meaningful youth engagement. This critical perspective is extremely useful for shedding light on the complex reasons that meaningful inclusion of youth in peace building is rare. At the same time, however, the track record of quantitative social science research demonstrates a litany of meaningful contributions to development, including in regard to the barriers to youth participation that the authors identify.

For example, youth perspectives on the ways in which exclusive power dynamics are "hardwired" into existing institutions and often channel youth toward informal or community-level modes of political participation provide greater depth into findings familiar to mainstream quantitative research. In another example, Holbein and others use a deep reserve of qualitative and quantitative data to investigate the causes of low youth election turnout in the United States—a descriptive finding mirrored in quantitative research on youth engagement with formal institutions around the world.[11]

In the US context, Holbein and others also find that even in very high-information environments, young people are disproportionately deterred by several barriers to participation, including a lack of experience with participation and a lack of confidence in their political knowledge. This and other research across several countries pointed to an important role for youth social networks in encouraging political participation. Again, the findings of Mengsteab and others are relevant to both women's and youth participation.[12] The collaboration between Ethiopian civil society groups and academic researchers from Ethiopia and the United States provides one example of how insights from quantitative research on the low involvement of youth participation informed research that successfully integrated youth into formal institutions. Importantly, this intervention was not effective at increasing youth engagement with political parties or government representatives.

While these approaches reduce certain barriers to youth inclusion, they are unlikely to address youth engagement with institutions that are more intentionally exclusive, such as those that support formal negotiations between conflict parties or national dialogue platforms. Changing prevailing norms around the inclusion of youth in these initiatives—for example, by correcting misperceptions of the prevalence of youth participation in unrest, providing rigorous empirical evidence to participants that the inclusion of youth in formal peace-building initiatives can lead to more lasting peace, or changing prevailing norms around the role of youth—is one potential route to addressing structures of exclusion.[13]

Strategic Religious Engagement

The chapter on strategic religious engagement provides an extremely thorough review of existing evidence from relevant social science literature, careful consideration of the strength of evidence based on research design and measurement strategies, and an honest accounting of the shortcomings of existing research. While the author concludes that the available evidence does not dispel skepticism about the causal impact of engaging with religious actors on peace-building outcomes, he proposes a strategic approach that can guide research and program design. He also points to a growing body of research that is subjecting to rigorous evaluation important questions around the impact of engaging religious actors in peace building.

The author begins the chapter with a careful discussion of the beliefs and assumptions that motivate recent interest in the participation of faith-based actors in peace building. Specifically, he notes the widespread belief that faith-based actors' participation will contribute to sustainable peace. He then points to two assumptions that have been used to justify this belief:

1. Faith leaders are influential and embedded in local communities because most people are religious and religion is a strong source of identity.

2. Religious actors are "high-yield" partners because they are driven by convictions and have extensive networks.

To understand the strength of evidence behind these beliefs and assumptions, the author reviews more than 100 academic products as well as dozens of practitioner reports.

Through this review, the author finds that although these beliefs and assumptions are not entirely unfounded, the evidence base is quite weak. We note that this evidence gap is especially problematic given recent evidence that pro-peace messages from prominent religious leaders can cause backlash and increase intolerance.[14]

Particularly noteworthy is the heavy reliance of the literature on case studies of highly successful peace-building campaigns without an attempt to understand how these cases are different from those where religious engagement is not effective and where outcomes are too broad to measure and usually impossible to attribute specifically to religious engagement.

To improve both research and practice, the author recommends that policy makers and practitioners think through critical strategic considerations that should offer clues about when and how engagement with religious actors is most likely to benefit peace-building efforts. Specifically, those seeking to engage religious actors in peace building are encouraged to think clearly about, for each unique context and intervention, which religious actors are relevant,[3] what the interests (material vs spiritual) of those actors are, how the environment shapes their influence (including competition from other religions in a country and whether religious divides cut across other social cleavages), and which peace outcomes they can influence. Importantly, the questions posed by these strategic considerations should be tested with field and survey experiments focused on more narrowly defined outcomes (including intergroup tensions) and with quasi-experiments and observational studies that draw on a broader range of both successful and unsuccessful cases.

Nonviolent Action by Social Movements

The chapter on nonviolent action (NVA) by social movements draws on vast literature to generate insights into the ways that external support may contribute to the success of these movements. The authors' massive review effort was accompanied by a convening of experts along with a survey of these experts to identify especially useful research on the topic.

The authors begin by explaining the theory of change that underlies most attempts by external actors to support NVA. Specifically, external actors generally aim to support NVA movements by (1) fostering conditions that will allow the movements to emerge and operate (for example, through pressuring governments to respect free speech); (2) protecting nascent movements

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[3] The author notes that this will require greater conceptual and definitional clarity around what constitutes a religious actor, the differences and similarities between violent and nonviolent religious movements, and the diversity of religious actors that are included.

from repression (for example, through the threat of sanctions); and (3) assisting movements in achieving their short-term goals (for example, through securing political reform).

Despite the breadth of work on NVA generally, the authors find that the amount of research focusing specifically on the impact of external support is extremely small. Furthermore, the available evidence is based on observational data that do not provide strong causal evidence for the impact of external support on the success of NVA campaigns. Based largely on case studies, existing analyses are often focused only on cases where NVA movements were successful; and they come to opposite conclusions about whether external support contributes meaningfully to campaign success, with some studies even finding that external support backfires.

The large amount of mixed evidence is likely driven by the importance of contextual factors in shaping the suitability of external support. This suggests that researchers and practitioners need to think strategically about how external support is likely to affect NVA movements in each specific context and at each stage of a movement. To this end, the authors pull several clear lessons from existing work and present recommendations for external actors looking to provide support while minimizing the potential for this support to cause harm.

Finally, the authors call for more rigorous research designs, better data, and improved evaluation methods. Several of these recommendations would be relatively easy to implement. For example, survey experiments could be used to understand how external support shapes public perception of critical factors such as NVA legitimacy. Although this promising method has inherent weaknesses, it would allow researchers to test expectations about when external support is likely to create certain forms of backlash that could undermine NVA movements. Similar methods could also be used to explore the conditions under which NVA movement leaders expect that external support would be more likely to strengthen or undermine their efforts.

Although they require much greater investments in time and resources, RCTs provide an even more promising avenue for research on certain types of external support (such as nonviolence discipline training)—and the authors' discussion of USIP's groundbreaking work on this front is an intriguing contribution. For forms of external support that are less amenable to these experimental research designs, such as the withdrawal of support from governments opposing NVA movements, the authors offer clear ways for researchers to expand existing datasets, trace causal mechanisms, and interrogate the temporal dynamics of the interaction between external support and NVA success.

Conclusions

As with the book's first section, the wide range of approaches and perspectives taken by these chapters allows for a refreshing diversity of viewpoints but makes synthesis challenging. The chapters on women's and youth inclusion provide extremely valuable perspectives on the deep reserves of experience and context knowledge that shape practitioner approaches in these fields. However, future reviews would benefit from deeper engagement with mainstream social science research that could help generate rigorous evidence for the TOCs that guide peace-building activities.

By contrast, the chapters on religious actors and NVA movements both focus heavily on reviewing rigorous research and find common ground in their recommendations about the importance of both applying a strategic lens to the design of interventions and subjecting widely believed TOCs to careful empirical testing. Jason Klocek's proposed strategic approach is especially successful in offering a common framework to guide more evidence-based program design, more rigorous program evaluation, and more useful program-related research.

The Role of Governance in Peace Building

The chapters in the book's third section are notable for their deep dive into very large bodies of evidence on complex issues of extreme importance. Both chapters synthesize available evidence with original analysis to (1) advise practitioners on the design of programming and (2) pose specific research priorities to maximize the impact of learning. Furthermore, these chapters are unique in their use of quantitative data to establish basic empirical patterns or assess the quality of the research base.

Preventing and Countering Violent Extremism

The chapter on preventing and countering violent extremism (P/CVE) focuses on an evolving approach to programming that centers on community resilience to violent extremism. This approach rejects earlier efforts at P/CVE that both implicitly blamed specific communities for "attracting extremist networks" and targeted these "threatening" communities with counterterrorism efforts that often contributed to underlying drivers of extremist violence. Instead, the approach assumes that communities have a strong desire to resist violent extremism and have varying levels of capacity to resist extremist movements.

The authors conducted an extremely thorough review of (1) existing evidence on P/CVE and community resilience, including quantitative descriptions of the quality of evidence based on expert coding of extant research products; (2) key informant interviews with P/CVE experts on the role of youth, gender, and religion in community resilience; (3) available evidence on the importance of social cohesion for both thwarting and promoting violent extremist networks; and (4) in-depth case studies of the evolution and effectiveness of hybrid governance and local peace committees as tools to address the core drivers of violent extremism.

This intensive review yields a clear picture of the state of evidence on P/CVE to date, how well current policy and practice draw on this knowledge, and the most important evidence gaps that remain. For example, the authors effectively synthesize research into core drivers of extremism, including the exclusion of certain groups by existing governance institutions and tendency for this exclusion to create bonding rather than bridging social ties that undermine social cohesion. However, their review suggests that most P/CVE interventions focus on resolving the symptoms of this exclusion rather than the root causes.

Overall, the review strongly supports the authors' claim that P/CVE researchers should see their task not as identifying risk factors that make specific communities vulnerable to violent extremism, but rather as understanding which resources allow communities to resist extremism, why some communities lack these resources, and how to effectively build them where they are lacking. However, we caution against a strong assumption that all communities have a desire to resist violent extremism. In cases where state abuse and exclusion has been prolonged and severe, this may not always be the case. Identifying such cases could help avoid building social capital in contexts where it is vulnerable to co-optation by extremist elements. Importantly, we are not arguing that certain communities are intrinsically prone to extremism—only that entrenched grievances against the state may preclude effective hybrid governance and render local peace committees vulnerable to co-optation.

Although the authors conclude that "community resilience research has not evolved to a level where it can inform practice," they provide clear recommendations for high-impact research to address this deficiency. Most critically, they urge greater attention to the governance of community resilience networks, the configurations and enactment of social capital, and the ability of external actors to support the community practices and institutions that promote resilience. We strongly endorse these recommendations. We also echo the authors' call for "small experiments" that can be integrated into interventions at a relatively low cost to provide rigorous feedback on impact. And we recommend that practitioners partner with researchers to utilize monitoring and evaluation surveys as an opportunity to conduct survey experiments that measure the likely impact of external support on the legitimacy of local institutions or to collect data on existing social networks.

Security Sector Governance

The chapter on security sector governance focuses on contemporary approaches that, during the postconflict phase, aim to make improvements through increased transparency, accountability, and inclusion. The evidence review begins by examining the history of security sector reform (SSR) efforts and describing the contemporary state of policy and practice. The author then sketches the theory of change that has dominated SSR efforts in recent decades, which places state legitimacy at the beginning of a virtuous cycle of public support and capacity. Specifically, the author identifies nine security reform approaches focused on transparency, accountability, and inclusion and examines in detail the practitioners' justifications for each approach. This discussion of SSR history and the informal theories that shape its current form is followed by an evidence review of the impact of these approaches, which is based largely on case studies and suggests mixed results.

The review clearly lays out the most frequently used components of reform efforts and the most important contextual factors that vary across the countries where these reforms have been tried. As a result, the author was able to produce a helpful typology of the ways in which reform efforts differ from one another and the ways in which local contexts may mediate their impact. Furthermore, the author discusses in detail how and why the contemporary emphasis on state legitimacy has remained dominant despite a thoroughly mixed record of success.

In the final section, the author draws on observational data measuring important security outcomes to generate new evidence on whether specific combinations of SSR approaches are more likely to yield sustained improvement in security provision. Ultimately, the author describes the conditions that appear necessary for reforms to succeed (such as the level of democracy and the combination of multiple reform elements) and the conditions that appear to make the success of ongoing reforms most likely (such as the timing of reforms after a major political transition).

Throughout the chapter, the author is very careful to define key concepts and outcomes that underpin her analysis, gives examples that make these concepts and outcomes clear to readers from outside of their focus area, and carefully defends her analytical choices. The principled comparisons made in both the qualitative and quantitative sections are well-reasoned, and the use of independently collected data to measure the impact of reforms provides an excellent model for future evidence reviews.

The quantitative analysis relies on observational data from countries with major SSR efforts and focuses on detecting informal patterns in long-term results. This inherently leaves a great deal of uncertainty about the inferences that are made. However, the transparency and clarity of the exercise is worthy of emulation in future evidence reviews. The analysis could be strengthened by including countries that did not experience SSR efforts; this would allow comparisons between the trends observed in SSR countries with broader trends in violence around the world. Such an analysis could also benefit from more sophisticated methods that attempt to compare SSR outcomes with a counterfactual, such as an interrupted time series or synthetic control methods.

In addition to greater investment in the quantitative analysis of SSR efforts, we also recommend more thinking about ways in which rigorous evaluations can be built into future reform efforts. Some reforms could be rolled out in a staggered manner across different units within a country's security forces, allowing for analysis of how behavior of these treated units changes in response. Furthermore, survey experiments on public opinion surveys could be used to anticipate how certain reforms are likely to affect security force legitimacy.

Conclusions

These chapters serve as excellent examples of how important concepts in peace-building policy and practice can be linked with foundational theories in social science research—theories such as the social contract between citizens and states and the potential for virtuous cycles between state legitimacy and capacity. By building these conceptual bridges, evidence reviews can encourage stronger TOCs and draw clear links across practitioner experience, existing scholarly work, and ideas for high-impact, rigorous research to fill remaining evidence gaps.

These chapters also illustrate how simple descriptive analyses can strengthen evidence reviews. Notably, the authors pair their quantitative analyses with clear statements of the questions at hand, with careful attention to the concepts and outcomes that they seek to understand, and with transparency about the analytic choices being made. The pairing of these novel approaches with more traditional reviews of published research and qualitative case studies results in compelling findings that could guide both current practice and future investigations of impact.

General Approaches to Gathering and Applying Evidence in Peace Building

The chapters in the book's final section provide an exciting endpoint. The chapter on effective use of evidence emphasizes the newness of rigorous research designs in the social sciences and reminds readers that we are only just beginning to see the ways that these tools can be adapted and improved to accomplish the goals of peace building. The chapter on systems science similarly emphasizes the proliferation of systems-aware research methods in other areas of practice and presents compelling examples of how these tools could shape peace-building activities. Together, the two evidence reviews highlight the essential role of collaboration across the research, practice, and policy communities in advancing peace-building efforts.

Effective Use of Evidence across the Peace-building Project Cycle

This chapter begins with the important observation that as the quality of social science evidence has improved, practitioners' demand for such evidence has grown apace. To provide recommendations for organizations interested in strengthening the use of evidence, the chapter considers how evidence from conflict analysis is currently used to inform the design and adaptation of peace-building activities. To this end, the authors conduct a meta-synthesis of published sources and a series of interviews and roundtable discussions with expert practitioners both within and outside USIP.

The authors find, both across and within organizations, that there is no standard practice for incorporating available evidence into program design and no consensus on what constitutes rigorous evidence. Importantly, they emphasize the centrality of organizational culture and capacity for progress on this front. The effective use of evidence requires organizations to develop cultures wherein the integration of evidence is encouraged by leadership, institutionalized into decision-making processes, and supported with training to build capacity on evidence-informed practices.

This review provides an excellent road map for organizations looking to improve their utilization of evidence. However, the chapter's focus on a broad definition of conflict analysis seems to underemphasize the role of rigorous research. The authors consider the role of different types of evidence ranging from RCTs to the soliciting of "informal input from partners." This pluralistic approach is important and allows the contextual knowledge of practitioners to be integrated alongside insights from quantitative research. To provide more guidance to practitioners, future reviews might complement this pluralistic approach with a typology of the types of knowledge imparted by different forms of evidence and the amount of confidence we should have in different forms of evidence.

The authors also seem to overemphasize programming that is evidence-based rather than programming that embeds evidence about impact. We need to distinguish projects that are "evidence-based" at the design or adaptation stages from those that have evidence supporting their impact. For example, a "homegrown" practitioner-developed approach can be evidence-based if there has been iteration or learning from observing other approaches. However, this does not by itself constitute evidence of impact, which is still needed. Frameworks and practices cannot substitute for true evidence on specific activities in specific places at specific times.

While the authors' reluctance to provide a single definition of "evidence" makes sense given the diversity of information that practitioners must weigh in program design and implementation, categorizing the types of evidence available and their potential contributions seems worthwhile. For example, we see three types of evidence that are useful to peace-building practitioners, each of which are best positioned to play separate roles in informing practice:

1. **Model-based knowledge:** Evidence for general causal relationships, causal mechanisms, and theories of conflict processes resulting from a body of rigorous social science research. This type of knowledge is most useful for informing the design of specific interventions. However, note that the availability of rigorous evidence is extremely unevenly distributed across the subfields within peace building. While there is a large body of rigorous research on intergroup contact and dialogue, there is far less of this research on other subjects, including women's participation in peace building.

2. **Evidence of impact:** Evidence of impact is associated with rigorous evaluations of the impact of specific interventions on important outcomes. This is the most direct type of evidence, and it comes from research designed to identify a causal relationship between

an intervention and outcomes. This evidence is particularly useful for deciding whether existing approaches to programming are having their desired impact (or unintended impacts); and if not, whether programming can be tweaked or an entirely new approach has become necessary.

3. **Contextual knowledge:** Contextual knowledge includes things such as "informal input from partners," knowledge of specific social contexts, or past experience deploying a specific type of intervention. This type of knowledge is particularly useful for thinking about how to translate model-based knowledge into program design and adaptation, or for thinking through how evidence of the impact for a specific intervention in one context might translate to a similar intervention in another context. Finally, contextual knowledge may also be extremely useful for developing TOCs that can inform the design of interventions that are then subject to rigorous evaluations or to research that contributes to model-based knowledge.

Because evidence comes in many forms, integrating available evidence into program design requires different types of expertise. While authors with formal training in contemporary quantitative social science are best positioned to "weight" evidence of impact and model-based knowledge according to the quality of measurement and strength of research designs, practitioners with contextual knowledge are essential to translating this knowledge into action. We see collaboration as a means both to improve the use of evidence in peace building and to overcome some of the biggest challenges raised by the authors.

For example, the authors report a feeling among peace-building actors that international donors are pressuring local organizations to adopt specific evidence-based interventions. Academics and practitioners can work together to propose program evaluations that adapt well-regarded evidence-based approaches to local contexts and to demonstrate the importance of adapting programs to the local context. Alternatively, such collaborative teams can work with practitioners to develop TOCs based on contextual knowledge, refine these TOCs into testable hypotheses, and propose alternative interventions that can evaluate and contribute to the arsenal of evidence-based programs. In this way, collaborations between practitioners and researchers can support innovation in peace building while satisfying demands for evidence-based programming.

The authors also note practical and ethical concerns around who gets to decide what counts as credible evidence. To assuage such concerns about political and power dynamics, USIP and other peace-building organizations can facilitate collaborations involving scholars from universities in both developed and developing countries. These collaborations can foster the transfer of technical capacity between the universities, allowing researchers from developing-country institutions to take increasingly central roles in adjudicating claims about evidence.

The Application of Systems Science to Peace Building

The chapter on systems science begins by describing the characteristics that make peace building well suited to the application of systems science. In particular, the focus of much peace-building work on the attitudes and behavior of individuals situated in interconnected social systems means that system dynamics are of fundamental importance. For this reason, interventions to reduce conflict and foster peace must deal with complex system characteristics, such as interdependence and heterogeneity across units, system adaptivity, and feedback loops.

The authors then investigate whether systems science could help practitioners to understand and anticipate complex dynamics, identify leverage points to effect change, and anticipate and manage unpredictability during program implementation. In doing so, the authors thoroughly review how systems science has sought to accomplish these objectives in other fields. They discuss in detail the tools used, the practical impact of the work, the barriers to implementation, and the potential for using similar tools in peace building.

The insights from their overview and case studies furnish practitioners with clear, compelling examples of the ways in which systems science tools can be deployed to inform programming. However, the deficiencies of systems science tools deserve some additional emphasis for practitioners hoping to benefit from these tools. Most importantly, several of the tools discussed rely on theoretical models that are calibrated to explain or even predict patterns in data; while these models can be extremely useful, it is often difficult or impossible to validate the insights gained from them. Specifically, it is rarely feasible to subject the causal relationships specified in these models to proper testing. For this reason, we must be cautious about assuming that a model's ability to predict changes in data means that it properly identifies a causal relationship that can be used to develop an effective intervention.

To encourage the integration of systems science tools into peace-building programming, we see several promising paths forward. As the authors note, use of these tools requires significant technical training and computational resources, but fortunately, these skills and resources are becoming much more common in both universities and private sector organizations. This suggests that more collaboration with universities or private sector partners would be highly beneficial.

Further study of how best to encourage practitioners to use the tools would also be beneficial. Comparing the performance of practitioner teams that are trained to use group model building (GMB) with the performance of those that are not would provide the most compelling evidence that the significant investment required to use these tools are worth the expense. For example, if teams that employ GMB in their work develop interventions that are more highly rated by communities during routine evaluations, such methods may be seen as more credible.

Conclusions

The two chapters in this section provide useful guidance for peace-building practitioners and organizations to better integrate evidence and research tools into their work. To improve the impact of peace-building interventions, such improvements will be necessary. Fortunately, the importance of applied research is being increasingly recognized, and the barriers to conducting rigorous applied research are falling.

One key takeaway from these chapters is that collaboration across peace-building organizations and social scientists working at universities and in the private sector plays a vital role. As social science tools become increasingly sophisticated, they can add more value to program design and evaluation. Of course, they will require more training to deploy and the evidence they generate will require more training to understand and synthesize. But the number of individuals that possess this training has increased dramatically, and the costs of using advanced tools have declined.

In addition to the specific pieces of applied research suggested in the paragraphs above, another promising avenue for such collaboration is to study how practitioners understand and assess evidence generated by the types of rigorous social science research we describe. In recent years, social scientists have made large advances in understanding how policy-makers assess evidence and the ways evidence can be better communicated to inform policy-makers.[15] Using survey experiments—to understand (1) the amount of credibility peace-building practitioners assign to different types of evidence, (2) when and why practitioners believe results from systems science models or impact evaluations are applicable to the contexts in which they work, or (3) the ways of communicating to practitioners findings that make evidence more accessible—could significantly improve the prospects for evidence-based programming. While the validity of specific TOCs and impact of specific interventions can only be established by rigorous research, the same is true for understanding how decision-makers use such evidence in practice.

Looking at the Big Picture

Evidence reviews can and should serve as a map to guide an applied learning agenda in the peace-building field. To that end, here are several big picture takeaways from the book's thematic studies:

- Some interventions described in the book's chapters lend themselves extremely well to rigorous evaluation and adaptation based on high-quality evidence. For example, rigorous evaluations of dialogue interventions are well documented, even though many unanswered questions remain. Other interventions lend themselves well to evaluation but haven't received such attention by researchers or practitioners, such as the contribution of externally supported training on nonviolence discipline to the success of NVA movements. Thankfully, USIP is working to fill this notable gap. By comparison, other important peace-building activities do not lend themselves well to such evaluations. For example, it would likely be infeasible to randomize the characteristics of negotiating parties in high-stakes peace negotiations. However, for most of these kinds of interventions, the rigor of evidence can still be improved beyond its current state.

- Theories of change should pay more attention to issues of heterogenous treatment effects. The presence of conflicting findings from case studies and qualitative research in many of these peace-building fields suggests that elements of context and circumstance shape outcomes in ways that we do not currently understand. Similarly, many chapters point to the need to identify which particular groups would benefit most from the intervention, increasing the likelihood of success. Several chapters call for a richer discussion of the heterogeneous contexts in which interventions, such as protests and civil action, take place. This is a clear frontier for future research.

- In some instances, it was difficult for the evidence review to produce an unbiased summary of the evidence. A number of the book's chapters indicate a normative and subjective bend to some outcomes—and some academic reviewers therefore encourage the USIP team to conduct more balanced assessments and reviews. To the extent that organizations have a normative commitment to the validity of an empirical claim, these empirical claims should be prioritized for rigorous evaluation by an independent research team.

- Theories of change, including their underlying assumptions, should be interrogated. Many TOCs would benefit from a detailed description of the intervention under consideration and clear linkages between the outputs, mechanisms, and outcomes of interest. Important assumptions should be articulated as part of the TOC. Improved TOCs, inter alia, would help clarify weaknesses in program design, facilitate a rigorous analysis of the effectiveness of specific interventions in the literature, and support the development of a rigorous monitoring and evaluation framework. If the focus is indeed on several types of interventions for a particular thematic area, then we recommend structuring the evidence review in a way that takes this broad focus into account while at the same time discussing separately each type of intervention and its respective goal; this is important to do given the varied potential outcomes of different interventions and the conditions for their success.

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