



Development in the Shadow of Violence: A Knowledge Agenda for Policy

Report on the Future Direction of
Investment in Evidence on Issues of
Fragility, Security and Conflict
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Approximately twenty participants representing nine organizations met at Canada's Permanent Mission to the Office of the United Nations in Geneva to discuss:

- a. the state of evidence on issues of fragility, security and conflict;
- b. a future agenda for building the evidence base; and
- c. strategies to bridge the knowledge to policy/practice gap.

This baseline paper was commissioned by IDRC to stimulate dialogue among funders of research on fragile states, both about the current state of knowledge and about how to increase the impact of research on policy.

The organizers of this conference were: John de Boer (Governance, Security and Justice Program, IDRC); David Schwartz (Partnership Division, IDRC); Emily Bishop (Governance, Security and Justice Program IDRC); Joanna Macrae (Governance, Conflict and Social Development, Research and Evidence Division, DFID); Tom Wingfield (Governance, Conflict and Social Development, Research and Evidence Division, DFID); Olivier Ray (Direction de la prospective, Ministère des Affaires Etrangères et Européennes); and Virginie Diaz Pedregal (Recherche économique et sociale - Département Recherche, Agence Française de Développement).

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Development in the Shadow of Violence

Executive Summary

Fragile states are beset and defined by recurring cycles of violence, and violence feeds their chronic underdevelopment. Violent and impoverished countries have poverty rates that are, on average, 20 points higher than their impoverished but peaceful counterparts.

Past work on conflict has often sought a simple, one-cause explanation (ethnicity, “greed”, etc.) – and the accompanying silver bullet to solve the problem of fragility. We now know with confidence that the causes of conflict are multiple and complex, and generally occur in combination. We know that it is difficult to sustain an exit from conflict, but not impossible; that inclusive political settlements are important to peace; that building trust and confidence around the political settlement and in reformed institutions is vital to its success. We also know this takes time – often decades.

Despite the complexity of causes, the overarching reasoning behind these conflicts is straightforward: to put it plainly, violence occurs in contexts where institutional alternatives to violence are weak or nonexistent; weak institutions combined with a range of political, security and economic motivations (and external pressures) creates the conditions for conflict and violence.

There are important gaps in our knowledge though. Among the most important are: understanding the dynamics of political settlements; of confidence and risk-taking; and of legitimacy. In policy terms, key gaps exist in understanding the role of external instruments in overcoming trust deficits (“commitment technologies”); on job creation; on judicial institutions; and on sub-regional capacity. We also need more knowledge about organized crime, terrorism, and their links to civil conflict.

More disturbingly, what we do know rarely translates into policy and practice. Much of what is “known” by the research community is not “known” by the policy community in any real sense. The reasons behind this divergence are many, but three pieces, in particular, are key to understanding this gap: academic research tends not to focus on policy tools, limiting its relevance to decision-makers; donor research is rarely rigorous enough to reliably translate into implementation; and we have lacked (until now) a baseline framework against which knowledge can accumulate. To make progress on the stability and development of fragile states will require new approaches.

Well within reach of donors are modest changes. These include adopting *minimum standards in research design*, and using the ‘multiple causes, weak institutions’ framework reinforced in the 2011 World Development Report as a *baseline framework* that can be tested and revised and against which new findings can accumulate. A more ambitious agenda would comprise of *jointly funded initiatives*, *coordinated research*, and a *division of labor* to ensure that knowledge is cumulative and relevant.

Achieving deeper impact will require more radical changes. Some ideas include:

- joint investment in centers of excellence, including mechanisms to allow participation of researchers from the global South
- sustained and critical accumulation of evidence from practitioners
- investment in baseline and tracking data on progress, focusing on violence reduction, confidence, trust and legitimacy – and eventually, institutional reform.

More profoundly, existing evidence calls for a fundamental shift in the policy approach to fragile states: away from a development orientation towards a venture-capital style acceptance of political risk. That is not a function of a research gap, but a political one – but a sustained, rigorous and cumulative base of knowledge can help inform a more effective policy approach to fragile states.

Development in the Shadow of Violence: A Knowledge Agenda for Policy

By Dr. Bruce Jones and Molly Elgin-Cossart, with Kaysie Brown

Introduction: The purpose (and limits) of this paper

This paper was prepared at the request of Canada’s International Development Research Centre and the UK Department for International Development. Its purpose is to stimulate dialogue among funders of research on fragile states, both about the current state of knowledge and about how to increase the impact of research on policy.

It is intended to be brief and (hopefully!) reader-friendly. As such, it is necessarily non-comprehensive: rather, it points to the most important conclusions and gaps in knowledge, drawing from far larger studies, especially recent work by the OECD, several recent, major multi-scholar studies of violence and fragility, and the 2011 World Development Report. It focuses on fragility and what has been the most common form of violence in those states – namely civil war – while pointing to research agendas on evolving forms of violence including organized crime and terrorism.

The paper proceeds as follows. Section A provides a brief account of the current state of knowledge, with a focus on findings in which there is a high level of confidence. It continues by noting (Section A.4) that much of what is “known” in research terms is not “known” by the policy community, and suggests some reasons why this is so. Section B then sets out some of the priority areas where there are gaps in knowledge. There are obviously more gaps than this paper can cover – the ones selected here are those where lack of knowledge is most likely to cause fundamental misjudgments in policy, i.e. those areas that should be a priority for research *seen from a policy perspective*. Section C turns to the question of the research-policy nexus, and sets out basic, modest and ambitious proposals for how donors can increase the impact of research on policy-making.

Section A – What do we know? And do we really know it?

A.1. Causes

Fragility: a function of violence, not poverty

Violence has emerged as one of the central development challenges of our time. Virtually all “fragile states” have experience repeated episodes of violence, and the large majority of the world’s poorest people live in states affected by violence – over 1.5 billion people. The tight relationship between violence and underdevelopment is reflected in this stark statistic: no low-income fragile or conflict-affected state has yet to achieve a single MDG. Many of these seem stuck in a conflict trap: 90 percent of the last decade’s civil wars occurred in countries that had experienced a civil war in the last 30 years.¹

For too long, the study of violence and the study of development were fields that operated in distinct silos. More recently, development practitioners and scholars have begun to recognize the importance of political and security issues to the challenge of development and statebuilding,

¹ Conflict, Security, and Development: The World Development Report 2011.

especially in fragile states, and security studies have placed greater emphasis on the kinds of violence – especially civil wars – most likely to be experienced in low-income fragile states. Understanding the connection between violence, underdevelopment and recovery is the central research challenge for responses to fragile states.

Development and security take one: a wrong turn

The first major effort to connect the study of underdevelopment in fragile states to that of violence was the work undertaken by the World Bank in the late 1990s under the direction of Paul Collier.

Collier was not the first to write about civil wars. The decade had already witnessed large-scale international attention to civil wars in Central and East Africa and the Balkans. From a distance, these wars appeared to be motivated by long-standing ethnic rivalries – seemingly the source of potential quagmires and failures like Somalia and Rwanda. Political scientists who studied these wars found that these were not ethnic wars in a narrow sense: rather, a complex mix of political, security, identity, institutional and leadership factors were at play.² Security studies of civil war had little purchase on development policy though.

Collier's work was different. Emanating from the World Bank, which had a large, built-in audience, Collier's studies gained real policy traction, as the development community in particular took to the finding that “greed”, more than “grievances”, caused civil wars. (Collier's conclusions were actually more nuanced than this; but a distilled, simplified version of those conclusions were what entered the policy stream.) Economic motivations could be measured, and addressed through existing mechanisms, such as aid.

The development community was not the only one to embrace the findings on economic causes of violence. The political science field was in the midst of a turn away from comparative or qualitative work, towards an embrace of quantitative methods. Collier was among the first to employ quantitative methods to study the onset of civil wars. With the enthusiasm of the development community and the embrace of political scientists, Collier's work shaped an entire decade of research on economic motivations and causes of civil wars- natural resources,³ low per-capita income,⁴ income inequality,⁵etc. – and reinforced the aid community's focus on poverty reduction as a strategy for dealing with fragile states.

There was only one problem: the “greed” thesis was flawed.

The onset of violence: A complex problem

Collier is first and foremost a scholar. And like the best scholars, he constantly questions his own findings. In his further research, Collier began to modify the “greed” story line. In his most recent book on civil wars, Collier places little emphasis on economic factors at all, focusing on security concerns, political factors and institutional questions – some of the very factors the early political scientists were examining before the “greed” hypothesis focused attention elsewhere. Nor is he alone: several new major studies have found that “greed” explanations are not sufficient to explain civil wars or patterns of under-development.⁶

In a parallel development, the OECD's Development Assistance Committee (DAC) had begun to investigate a new line of thinking, related to statebuilding. This work tapped into not the usual

² Stedman 1995; Brown 1995; Stedman & Rothchild 1996.

³ Collier 2003; de Soysa 2000.

⁴ Fearon 2010; Keefer 2008.

⁵ This relationship is measured by homicide rates: Fajnzylber, Lederman, Loayza 2002; Messner, Raffalovich and Shrock 2002.

⁶ Fearon 2010; Goldstone 2010.

stream of development economics literature, but rather the political science and historical literature on the construction and development of states. From this work emerged a more political focus: a concern with legitimacy, with political settlements, and with the relationship between various statebuilding enterprises – building institutions, meeting social needs, fostering national identity.⁷

In 2011, the World Development Report undertook a comprehensive review of the economics, political science, and policy-relevant literature on fragile states and violence. It drew on research in multiple languages, especially French, Spanish and Arabic, in addition to the voluminous literature in English. It retested every major quantitative study, reviewed the qualitative and comparative literature, tested the OECD findings, and commissioned exhaustive new work on both single cases and cross-national factors. It held consultations with policy makers and local scholars, in South Asia, the Middle East, the Gulf, Latin America, Central, West and North Africa, and the Balkans. It made data about institutional factors available to social scientists. One of the eye-opening findings about the review of earlier quantitative studies is that many of them simply did not have data on political or institutional factors; it is easier to access and measure economic variables. Correctly read they did not say “economic factors matter more”, but “*given available data*, economic factors appear to matter more.” The WDR 2011 for the first time made the World Bank’s own massive data bank of institutional variables for every client country available for study and testing.

In essence, the WDR’s findings take us back to the point that comparative political scientists reached in the late 1990s and that the OECD had started to point to: fragility and war are complex problems, with complex causes. Collier’s new book, new research by Fearon, and a major, multi-author study on political violence reach similar conclusions.⁸ The fact that these findings have been replicated in several separate studies solidifies confidence in the findings.

Put simply, the storyline on causes of civil wars is this. First, and critically, violence should not assumed to be an irrational or emotive response – for many actors in many settings, violence is a *rational* choice of strategy. Second, in a given setting, any one of a number of factors can motivate political leaders/entrepreneurs to pursue a violent strategy. Those factors can be political in basis: the exclusion of an ethnic or religious or territorial group from the trappings of power can create powerful motivations to challenge the existing order, including through violence. They can be security oriented: minority groups can fear (or actually experience) persecution or oppression, and turn to arms to redress the situation. External security factors – including the threat of invasion and spillover factors like cross-border militants, equipment, resources, criminal networks, finances, and refugees – can also trigger violence.⁹ Motives can also be economic: if marginal groups are blocked from accessing a state’s budget and the private sector is limited and controlled, powerful economic incentives exist to challenge the existing order – amplified when the state has significant natural resources.¹⁰ These motives are primarily internal, but they can be amplified, manipulated, supported or restrained by regional and external dynamics.¹¹

⁷ Jones and Chandran 2008; Papagianni 2009.

⁸ Ibid.

⁹ Brown 1996; Puerto Gomez and Christensen 2010; Gleditsch 2007; Fearon and Laitin 2003; Saleyhan 2006, 2007; Collier 2003; Craft 2002.

¹⁰ For example, Auty 2001; de Soysa 2000; and Karl 1997.

¹¹ See, for example, Fearon 2004; Auty 2001; de Soysa 2000; Ross 2004; Svensson 2000.

Table 1: Internal and External Stresses Triggering Violence¹²

Stresses	Internal	External
Security	<ul style="list-style-type: none"> • Legacies of violence and trauma 	<ul style="list-style-type: none"> • Invasion, occupation • External support to rebels
Economic	<ul style="list-style-type: none"> • Low income levels, low opportunity cost of rebellion • Youth unemployment • Natural resource wealth • Severe corruption • Rapid urbanization 	<ul style="list-style-type: none"> • Price shocks • Climate change
Political	<ul style="list-style-type: none"> • Ethnic, religious, or regional competition • Real or perceived discrimination • Human rights abuses 	<ul style="list-style-type: none"> • Perceived global inequity and injustice in the treatment of ethnic or religious groups.

Third, such motives exist in many more countries than actually experience civil wars. Another critical factor helps explain why some states experience war, while others do not: weak institutions. Where political and accountability institutions are strong, challenges to the existing order can often be accommodated through political dialogue, legal action, non-violent civil strife, or similar. It is when institutions are weak that the incentives to violence grow. Indeed, weak financial, administrative and coercive capacities are a better predictor of the onset of civil war and extreme violence than other aspects, including economic predation, political grievances, and ethnic inclusion.¹³

And then, fourth, there are the “x” factors: leadership and legitimacy. To say what we do and don’t know about leadership is to enter the morass of human psychology, group dynamics, and other fields of social study that are in their infancy. But the fact that we know little about it does not mean that we fail to notice that Libya is a different place than it would have been without Qaddafi; that Zimbabwe is weaker for the personality of Mugabe than it need have been; that South Africa’s recovery from apartheid was deeply shaped by Mandela; and so on.¹⁴ Leadership may not be susceptible to policy design, but it is profoundly important nevertheless. Legitimacy is also complex, and we will turn to it in discussions of building the knowledge base moving forward.

The causes of any specific war emerge from this complex mix of motivations, regional/international dynamics, institutional arrangements and leadership and legitimacy issues. In one way, this is an unsatisfying outcome: far more re-assuring to find a single cause (Inequality! Ethnicity! Natural resources! Rain!) to explain fragility and war. Common sense, though, tells us that as complex a phenomenon as war and fragility should have complex causes, and that these should vary

¹² World Development Report 2011.

¹³ Fearon 2010.

¹⁴ There is an important political science literature about the conditions under which such leaders emerge. It is not at a stage where one could predict with reasonable confidence that conditions x are likely to lead to leadership style y, or similar; but it does cause us to remember that benign leadership is more likely under benign conditions than hostile ones – the Mandela’s of this world are a historical accident that we can be thankful for and take advantage of but neither predict nor plan for. Planning for weak or mixed leadership is a more realistic strategy.

considerably from case to case – and that is what the science tells us too, now with a substantial degree of confidence.¹⁵

The theory of change: why do these factors cause violence and inhibit recovery?

What is the logic behind the multiple causes of violence and war?

Let us start with this point: violence is a rational tool for forcing political change. In the economics literature, war is treated as intrinsically irrational; and in the development community, violence and war are treated as explicit bads. In most western policy discourse, violence and war are negatives (except when waged by the West...). Throughout history, though, violence has been a tool for social change. The most important theorist of contemporary state formation, Charles Tilly, is justly famous for his conclusion that “states make war, and war makes the state” – a neat summing up of the process of western European and later North American state formation.¹⁶ In the modern period, violent resistance was an important part of the process of ending imperial occupation – in Algeria, Kenya, Bolivia, Zimbabwe, etc. – and in ending apartheid in South Africa. It’s politically incorrect to note the point, but terrorist campaigns have several times succeeded in bringing an end to external occupation. In many parts of the world, violence has been the handmaiden of progressive social change. Violence is now a major obstacle to development, but it is not intrinsically irrational.

It is, for example, perfectly rational to adopt a violent posture if you believe that you or your group may be subject to violence by an opposing group. Decision-making studies show that risk-avoidance drives much human behavior.¹⁷ For minority groups or groups that fear they may be targeted by state security services, risk calculations involving violence are skewed by the fact that death is an absolute risk – so people often take substantial risks to avoid that outcome. Moreover, every sub-group is aware that every other sub-group is making similar calculations – and so risk-avoidance may suggest moving first to pick up weapons and organize for self-defense, a move that will seem threatening to another group, causing them also to organize for war. History is replete with examples of wars that start unintentionally as one group moves to defend themselves from a possible attack by the state or another group, causing the state or the opposing group in turn to move towards violence. This kind of “security dilemma” is a powerful explanation of the outbreak of war, and of the recurrence of war.¹⁸

(It is important to point out that political/community leaders making the decision to go to war are often less exposed to violent death once involved in a warring campaign than they were as individual citizens within the state. The calculation may be different for individual combatants, but once a group mobilizes to war, individuals may have only bad choices to join or be excluded. Similar differences in the calculations between the leaders of terrorist organizations and their foot soldiers help explain why it is the case that terrorism is most commonly associated with middle class actors within middle income or lower middle income countries – not with poverty, as is commonly assumed.)¹⁹

Similar logic applies to political/justice motivations. The sustained injustice of apartheid or occupation is certainly reason enough to take risks to generate social and/or political change. In

¹⁵ Examples include Fearon 2003; Vreeland 2008; Paul and Collier 2009; Hegre, Ostby and Raleigh 2009.

¹⁶ Tilly 1982.

¹⁷ de Figueiredo and Weingast 1999.

¹⁸ Walter 1999, 2002.

¹⁹ Krueger and Maleckova 2002.

most instances, moreover, violent resistance follows years or decades of failed civil or legal action to produce change. In the absence of positive change, political leaders of oppressed groups may feel that they have little to lose by adopting violence.

Equally, for a group that is denied access to the economic resources of the state, or to the market, using violence to create market access, or to dominate control of a natural resource, can be a perfectly rational act. Predatory groups often get quite rich during wars, which also creates economic incentives to sustain war rather than accepting peace.²⁰ (Collier is often at pains to point out that in his early work on greed, he did not argue that greed often caused war; he argued that political grievances and injustice, sometimes combined with economic factors, caused wars; but that economic interests created motivations to continue wars.) The truth is more nuanced – it has been pointed out, for example, that conflict profiteers could make more money by investing in the stock market, but overconfidence, human biases, path dependency, and a focus on short-term horizons all work to push rebels towards war and away from the stock exchange. For example, Real Sinn Fein in Northern Ireland had substantial interests in organized crime and sustained these long after the Good Friday Accords).

We know less about the motivations for terrorism and other forms of large-scale violence. Surveys of “foot soldiers” of rebel groups, gangs, and terrorist organizations show that there is a higher prevalence of injustice as a motivating factor among foot soldiers in terrorist organizations than the other two categories.²¹ However, this finding does not capture the motivations of the key decision-makers in each of these groups. Throughout the study of violence it is critical to distinguish between the motives and calculations of leaders and wider populations/groups.

Efficient institutions can provide solutions to the security, political and economic motivations described above by serving as tools for bargaining across groups. This requires the groups to have faith in the institutions and in the possibility of redress if they don't like the first outcomes. Where bargaining institutions are weak, groups are unlikely to have this confidence. Weak institutions create the conditions for the rational adoption of violent strategies.

Together, these factors underlie the logic that inclusive political settlements are the best approach to sustaining an end to violence. If a group is excluded from political settlement— all of the security, political or economic logics that drove them to war in the first place will recur. Inclusive political settlements are a substitute for efficient formal institutions – because they are inclusive, every group and sub-group can have some degree of confidence about their ability to achieve security, economic and political claims within the settlement, thus diminishing the logic of violence. (For game theory aficionados: opponents in a civil war are in a form of single-iteration ‘prisoners’ dilemma’ game, for which there’s no reward for cooperation; the challenge is to move them into a repeated prisoners dilemma game, for which there is at least a potential reward for cooperation – this is the function of political settlements, in the short term, and institutions, in the long term.)

Now, there’s a sting in that tail: by the same logic, efficient *oppression* can produce the same outcome. Most low-income states with weak institutions aren’t capable of efficient oppression – it requires too large an economic investment in internal security services, intelligence, security apparatuses, etc. But those states that are efficient at oppression can, at least for a time, avoid political challenge to the settlement – especially if the state is also willing to share some of the economic spoils.

²⁰ Lujala 2010; de Soysa 2000.

²¹ World Development Report 2011.

Enduring fragility: A ‘simple’ problem

If the onset of war is a fairly complex problem, the fact that wars continue, that they recur, that fragility is endemic – this is a rather simple problem. Simple analytically, that is – not at all simple to solve.

Here, the story line is in three parts. First, whatever set of factors engendered conflict in the first place are likely to remain and intensify with violence. If security fears triggered violence, the reality of conflict exacerbates and confirms those fears. If political or ethnic discrimination motivated violence, acts of war intensify a sense of difference and enmity. If economic factors are to blame, they are likely to worsen – violence often provides opportunities for predatory entrepreneurship, and it can be hard to convince people that they can profit as much from peace as from predation (though they often can).

Second, whatever else has happened, at least two groups that command sizeable portions of the population will have proven to one another, beyond all doubt, that they are willing to use violence to deny the other their claims or demands or rights. Rational analysis by the other side tells them that it's possible – not certain, not necessarily even likely, but *possible* – that their former opponents could at any point return to violence. (Irrational views will also have been amplified by violence, and often by acts of brutality that reinforce perceptions of ancient enmity, atavistic hatreds, etc.) Each side knows this about the other. The rational decision for both sides is thus to retain the option to return to violence – and in many cases, the rational decision is to take violent action preemptively.²² This “commitment gap” or “trust deficit” is a powerful explanation for why many ceasefires or peace agreements fail – roughly 3 out of 4 – and wars that end often resume.

Third, whatever the state of institutions before violence, they are likely to be worse after it. Institutions fail to maintain political relations; security institutions palpably fail to provide security at a national level, and prey on the populace. Leaders and officials flee or are killed, reducing human capacity. Physical damage may also be extensive. Since we know that bad institutions are the strongest indicator of conflict onset, the deleterious effect of violence on institutions becomes a negative cycle – indeed, a violence trap.²³

Thus, once a country with weak institutions has gone down a route of internal violence, sustained exit is hard. For this reason (and others) states often go through repeated cycles of violence.

A.2. Recovery

Inclusive political settlements, and confidence-building

Over the long run, the most successful economic states have developed “open access” orders – i.e. formal institutions that limit the role of informal agreements among elites in regulating the affairs of the state, and provide for rule-based access to administration for all.²⁴ These institutions are not a guarantee of conflict-avoidance: Yugoslavia had fairly developed institutions before the unraveling of the Soviet Union created centrifugal forces stronger than the centripetal forces of the state institutions, ripping it apart with extreme violence. Stronger institutions, though, mean the ability to withstand stronger political, economic, and security pressures, or stresses, on the state, both internal and external.

²² Kaufman 1996, 1998; Walter 1999, 2004; de Figueireda and Weingast 1999; Posen 1993.

²³ Walter 2010.

²⁴ North 2009; North, Weingast and Wallis 2009.

Here, of course, we encounter a Catch-22: for when states have weak institutions and have experienced violence, they encounter a powerful trust deficit – and that trust deficit is a major obstacle to building institutions.

States that have successfully exited violence and begun a process of development have often started with strong political settlements, or elite pacts.²⁵ These sometimes taken the form of peace agreements; sometimes electoral coalitions; sometimes they are simply informal understandings between elites about the rules of the political game and the division of economic spoils. They are, in short, informal proto-institutions. They are, by definition, primarily about elites, and thus dependent on elite relationships, and only secondarily address the needs of broader populations.

The evidence demonstrates with a relatively high degree of confidence that failure to focus on political settlements can increase the risk of failed peace agreements, contested power sharing arrangements, and often, conflict relapse.²⁶ Quantitative research provides preliminary evidence of the positive effects of power-sharing regimes on the duration of peace²⁷, but empirical evidence on the potential adverse effects of such agreements in ethnically divided societies²⁸, or when power is given to former armed groups²⁹, is still limited to case studies of varying rigor. That political settlements matter is clear; but we are only beginning to know much of detail about the *shape* of political settlements that do or don't endure. We have partial knowledge here, which pushes in seemingly contradictory directions.

We also know that forging a political settlement is not enough: it's also necessary to create *confidence* in that settlement – confidence that it will endure and deliver, confidence in various parties intent, to overcome the trust deficit discussed above. The 2011 WDR was among the first studies to ask the question, how do proponents of a political settlement (or peace agreement) foster *confidence* in that agreement? Peacekeeping is a part of the answer, but far from a full one. The WDR found that pro-peace leaders have to send commitment signals to generate confidence in the settlement –they have to make public, costly, hard-to-reverse decisions that demonstrate to other parties that they are serious about cooperating for peace. They have to use what the financial literature calls “commitment technologies” – and external actors can help. But little research exists on commitment technologies – we are only beginning to understand what set of factors do or don't generate confidence in political settlements over time.

The long process of building institutions: especially for justice, security and jobs

When we move from political settlements to building institutions, we encounter an important lacuna in knowledge. Development actors and scholars have studied institution building, but have largely neglected political and security institutions. Political and security actors and scholars have long engaged in providing or understanding short-term security gaps (wars, peacekeeping operations, etc), but have largely neglected true institution-building. Only a small body of political science has studied the development of political and security institutions, mostly in comparative and historical perspective.³⁰ Yet the development of political (accountable, representative), justice and security institutions is fundamental to long-term, sustained recovery from war and the prevention of relapse. This is the core finding of the WDR's rework of all of the quantitative models. It echoes earlier

²⁵ OECD 2011; Licklider 1995; Hartzell 2003.

²⁶ Walter 2004, 2009; Licklider 1995; Wagner 1993.

²⁷ Hartzell and Hoddie 2003.

²⁸ Simonsen 2005, Samuels and von Einsiedel 2004; Horowitz 1985; on the opposite side, Lopez-Pintor 2006.

²⁹ Tull and Mehler 2005, De Zeeuw 2008.

³⁰ Moore 1958, 1966; Huntington 1965; Moe 1990

qualitative findings about the importance of institutions as tools for managing the sources of tension that otherwise end up in violence.

There simply is not good enough historical data covering the last twenty years to know with any degree of certainty the most effective sequence prioritization for states seeking to recover from violence. Reviewing comparative case evidence, the WDR reaches the *tentative* conclusion that a focus on justice institutions, security institutions and – in the short to medium term – on jobs, are likely to be important priorities. These, however, are only interim conclusions.

There is also powerful data on the timelines for institution building.³¹ The following table illustrates the timelines needed to take *one* step forward in institutional performance, as measured by a series of empirical scales. It shows the historical range of timings that reformers in the 20th century took to achieve basic governance transformations. The first column shows how long it would take for today's average fragile state to develop if its institutions improved at the average pace of the fastest 20 transformers; the second shows how long it would take if it matched the pace of the single fastest transformer.

Table 3³²

Indicator	Years to threshold at pace of:	
	Fastest 20	Fastest over threshold
Bureaucratic quality	20	12
Corruption	27	14
Military in politics	17	10
Government effectiveness	36	13
Control of corruption	27	16
Rule of law	41	17
Regulatory quality	22	25
Voice and accountability	31	37
Political stability	18	26

This data needs further elaboration and testing; but it is at the very least cause for substantial modesty about the speed of impact of external engagement in fragile states.

We also know from comparative experience that the process of institution building and political recovery often comes with setbacks. Very rarely does a state make a steady, linear progression from one set of political arrangements, or violence, to another. There are winners and losers from any set of regime transitions, and the losers will often resist change. Confidence problems mean that many people will bet on the failure of reform, creating a self-fulfilling prophecy. Leaders come and go, and with them, vision, ideas, human strengths and weaknesses. A major policy challenge is recognizing that the path any given state will take from violence to development is unpredictable, contingent and fluid – and adapting policy tools to accommodate that fluidity.

³¹Pritchett and de Weijer 2010.

³² Calculations taken from Pritchett and de Weijer 2010. Calculated according to ICRG and WGI.

A.3. Tools

Beyond updating policy to reflect realistic timelines, practitioners must have a better idea of the actual impact of specific policy instruments. Until very recently, academic study has focused on the causes of conflict rather than policy interventions. Donors themselves, and international institutions, have spent more time on policy, but rarely with good research design or method. Thus only a smattering of instruments has been studied systematically.

Peacekeeping

An important exception is peacekeeping. Repeated, credible studies have shown that peacekeeping has made a critical difference in helping to sustain peace and reduce the risks of relapse³³ – roughly by 70-75%.³⁴ The evidence indicates that success is due at least in part to the role that external actors can play in creating the leverage and confidence necessary for various actors to come together to take risks for peace.³⁵

Of course, even here major challenges exist. Peace agreements and ceasefires fail more frequently than they succeed, roughly at a rate of four to one.³⁶ Failures point to the fact that not all peacekeeping operations provide credible guarantees against security dilemmas – either because the force is too small, too slow to arrive, or too unwilling to use force. The challenge in fragile states, therefore, is to provide peacekeeping operations that constitute a credible commitment or third party guarantees for internal actors.³⁷

Aid

Foreign aid is one of the most widely employed instruments used to ensure peace, yet studies on its effectiveness are inconclusive at best. Some studies show that foreign aid may be an important determinant of economic growth, particularly after peace is reached, though it shows diminishing returns.³⁸ Alternatively, empirical evidence supports the idea that aid flows relax government budget constraints, with 40% of foreign aid directed towards the military. This may, in turn, help to centralize power with the domestic government and promote peace.³⁹ In contrast, other literature argues that foreign aid often has a counterproductive effect on the target society. Aid can become destabilizing through three separate pathways: supporting inefficiency by providing too great a margin for error;⁴⁰ rent-seeking by providing unearned income;⁴¹ or overburdening by creating too many small projects for a weak government to effectively manage.⁴²

Statistical evaluation of the impact of foreign aid (for instance to rebuild infrastructures) is challenging,⁴³ but some scholars have begun using innovative strategies to isolate the effects of foreign aid,⁴⁴ in particular through the use of randomized or experimental evaluation.⁴⁵ Researchers

³³ See Walter 2004; Fortna 2004, 2008; Gilligan and Sergenti 2008; Paris 2004; Barnett 2006; Ghani and Lockhart 2008.

³⁴ Fortna 2008.

³⁵ Hartzell et al. 2001.

³⁶ Werner and Yuen 2005

³⁷ Johnston 2010.

³⁸ Elbadawi, Kaltani and Schmidt-Hebbel 2008; Dalgaard and Hansen 2001; Lesink and White 2001.

³⁹ Collier and Hoeffler 2007.

⁴⁰ Casella and Eichengreen 1996.

⁴¹ Svensson 2000.

⁴² Brautigam and Knack 2004; ODI 2008.

⁴³ Data may be missing for the most destructive cases or when the recovery was not successful, while countries with successful postwar economic recovery are more likely to collect systematic economic data.

⁴⁴ De Ree and Nillesen 2006.

have used this method to evaluate effects of community-driven reconstruction programs⁴⁶, voter education projects or election monitoring⁴⁷, and reconciliation activities between rival groups⁴⁸. While offering robust evidence of impact to donors, such experiments also provide a powerful tool for testing causal arguments. As yet, however, not enough of this kind of evaluation has been undertaken to provide a critical mass of evidence about specific instruments or tools.

Elections are another prominent policy instrument.⁴⁹ While some consider elections to be a turning point in the transition from war to peace, competitive elections may reignite existing tensions and cause new violence.⁵⁰ Research on electoral-related violence is not abundant, often theoretical⁵¹ and case-specific.⁵²

Reform of police has been identified numerous times as a major challenge to post-conflict and post-authoritarian states.⁵³ However, relatively modest work has been done considering the role of police reform in authoritarian transition and post-conflict stabilization; Call's comparative work in an important exception.⁵⁴ Similarly, relatively little good comparative or cross-national work has tested the links between disarmament, demobilization, and reintegration (DDR) processes and the stability of peace processes, and initial studies show mixed evidence.^{55,56}

Sector specific – method problems

Across the board, the study of specific instruments or tools has suffered from a two-part problem. In the academic world, a lack of interest in policy issues has meant that most major studies have been about the onset of war, and much less about the policy impact on recovery or prevention. The world of governments and international organizations is of course more focused on policy impact, but here we encounter a series of methodological problems. A scan of much of the donor-funder literature shows a huge emphasis on short comparative studies, often limited to a single donor's own programming, very little of which uses sound research methodology or is subject to peer-review.

The prevailing tool is for a given donor to commission a study (usually using its own research centers which may or may not have world-class expertise or relevant regional knowledge) of the impact of its contribution to DDR, in (say) four countries. The fact that the donor's contribution is roughly 10% of one sector, further divided into 3-4 regions, is not really taken into account. The four countries chosen for study are four where the donor has a specific interest or large program and do not include control cases. (Quality comparative research methodology stipulates that to study the impact of a policy intervention on several cases, we should also examine 'control' cases where that policy was not applied, to see whether those cases experienced the same or different outcomes.) The resulting study will not be subject to peer review, frequently won't even be published or made available to other researchers or donors, but just circulated within the sponsoring donor.

⁴⁵ Involving communities that are randomly assigned to the intervention or the control group, making possible to hold all factors constant when comparing the performances of the two groups.

⁴⁶ Fearon, Humphreys, and Weinstein 2009.

⁴⁷ Collier and Vicente 2008, Ichino and Schündeln 2011.

⁴⁸ Paluck 2009.

⁴⁹ In the Statebuilding and Development Landscape document prepared for this conference by IDRC's Donor Partnership Division, 8 of 20 donors had an entire section of their organization committed to democratization.

⁵⁰ Jarstrand & Sisk 2008, Kumar 1998, Paris 2004.

⁵¹ Chaturvedi 2005, Collier and Vicente 2008, Ellmann & Wantchekon 2000.

⁵² For instance, Wilkinson 2004.

⁵³ Weitzer, 1995; del Frate 1998; Mishler and Rose, 1998; Reisigl et al. 2004.

⁵⁴ Call 2001.

⁵⁵ Humphreys and Weinstein 2007.

⁵⁶ Pugel 2007.

There are four deep problems here. First, it is all but impossible to isolate the impact of a small program on the broader political/security dynamics that will lead a state to remain stable or relapse into war – the program itself is almost certain to be far too diffused to have that kind of broader impact, even if you can isolate the localized effects. Second, because most donor-sponsored studies employ questionable methodology and fail to submit research to peer review, many such studies make frankly specious causal claims, or contain findings that may be accurate but are of only narrow utility. Third, as noted, many studies are not published or circulated. Fourth, and most important, because there has been no common, complex model of the basic story line of war and recovery (other than briefly the flawed “greed” model), individual studies do not add to a common bank of knowledge – they are random data points that appear and then fade, leaving almost no impact on knowledge, or on policy.

This brings us to the wider question. How much of what we know about fragility and violence is actually “known” by those who make decisions?

A.4. Do “we” really “know” this?

How much of the above is actually “known”? The academic literature on civil war causes and recovery has limited circulation, especially at decision-making levels. Very little of the work on specific instruments has had sustained policy impact. Why?

Put simply, the field has been characterized by the following weaknesses:

- academic research has been rigorous, but has given short shrift to policy instruments, meaning that there is much better knowledge about general level causes/dynamics, and little cumulative knowledge about the impacts of specific policy instruments or tools;
- donor-sponsored research, which is policy oriented, has rarely adopted solid research design or good methodology (e.g. proper design of comparative case study work); such studies have minimal impact on the accumulation of knowledge, and rarely have sustained impact on policy;
- with the exception of the “greed” hypothesis, there has been no central framework against which research is tested, meaning that knowledge has not accumulated: we’ve had thousands and thousands of studies, but limited advance in knowledge.

In addition, there are further barriers to research, and to policy uptake.

Barriers to research

Fragile states are difficult environments in which to conduct research. Lack of security is a very real barrier, and discourages work outside capital cities. Access to rebels, warlords, and separatists is extremely limited, constraining the ability to fully understand motivations and strategies. Because of the cost and risk of sending people into conflict areas, pollsters often rely upon telephones, ignoring the portion of the population without access.⁵⁷ (More recently, with the sudden and massive penetration of cell-phones in Africa, SMS-based studies have become possible, capturing a far wider population.) Other strategies have included drawing on secondary sources or interviewing members of a group of interest outside of the conflict zone.⁵⁸

⁵⁷ OECD 2008; some enterprising researchers have made progress, see, for example, Bundervoet, Verwimp, and Akresh, forthcoming.

⁵⁸ DFID 2010.

Even when data are collected, linguistic and cultural barriers create selection biases that distort the data. Current data collection methods often disregard reluctance to speak with foreigners or someone of a different gender, misinterpret terms, or ignore news reports published in local languages.⁵⁹ Normative biases influence which data are recorded and ignore valuable information, such as the activities of non-state actors.⁶⁰

Quantitative research on fragility can be especially frustrating. Many of the factors influencing civil war are highly correlated with one another, making it difficult to unpack the divergent effects through statistical analysis.⁶¹ This leads to inconsistencies across quantitative studies, and makes it nearly impossible to determine causality.⁶² Furthermore, without a full picture of the causes, it is common to omit variables that may influence conflict, which can bias the results, a problem that can be overcome by collecting more and better data on theoretically relevant factors, such as unemployment⁶³, institutional capacity, and rebel structures. The lack of consistent, granular data on political variables and on institutions constitutes a major barrier to effective research. This is even truer if we include the question of sub-national institutions – for which comparative data is almost entirely lacking.

Political factors can hamper research, as well. National bias drives governments to select their own research entities, creating a sort of “research protectionism” that ignores comparative advantages and may reduce the overall pool of knowledge.⁶⁴ These studies often use very weak methodology, are not subject to peer review, and are frequently not published. Also, official institutions often protect their knowledge; for example, the Bank’s data-base on multiple institutional features of fragile states is vital research resource that has not been available to outside researchers. Other institutions have different problems: the UN, for example, collects very little data about its own operations or work, and subjects very little of it to rigorous evaluation.

Barriers to uptake

There are also multiple barriers to the uptake of research by policy and decision-makers.

One major factor is a lack of expertise within institutions. Until very recently, most policy managers on fragile states within development institutions were experts on neither fragility nor violence, and thus not necessarily familiar with the latest developments in the literature.⁶⁵ As a result, policy managers tend to focus on a few studies with large impacts, ignoring a host of other studies, but also neglecting revisions and critiques of the biggest studies. This is compounded by high levels of staff turnover, especially in the field.

At the institutional level individual lack of expertise is sometimes matched by the absence of a learning culture, or constraining features of that culture. This differs radically from institution to institution. The World Bank, for example, has a very strong learning culture but a huge bias to quantitative research and limited understanding/tolerance for political research – with important exceptions. The UN’s peacekeeping and political departments are far more open to political topics, but have a very weak research/learning culture – with important exceptions. The UN’s member

⁵⁹ DFID 2011; Free 2010.

⁶⁰ OECD 2010.

⁶¹ For instance poverty, ethnic or social divisions, dependence on natural resources, accountable institutions.

⁶² Some quantitative studies have overcome this problem through the use of exogenous instruments. See Miguel, Satyanath&Sergenti 2004, Ciccone 2008, Besley and Persson 2008, Dube and Vargas 2008.

⁶³ World Development Report 2011

⁶⁴ Ibid.

⁶⁵ Paris 2011.

states, especially those from the global south, are often suspicious of research-based policy development, given how massively the west dominates the production of research.

A further problem is high staff turnover. Particularly in-country, high staff turnover tends to reduce the knowledge base of organizations working in fragile states.⁶⁶ As a result, the lessons of prior engagements are often lost and external research discounted. Then we wonder why we keep repeating the same mistakes.

A lack of coordination among donors also limits the extent to which findings are implemented. The multitude of programs from multilaterals, regional, bilateral and private donors often work at cross-purposes, with each donor's programs determined by separate strategic interests, election timelines or Board member pressure, and the latest trends in the field.⁶⁷ Added to this, donors' accountability to their own constituencies can create artificially short time horizons in which to deliver results. Though research tells us that the path from fragility to resilience is a long one, most funders continue to sponsor one-year programs and push for results within unrealistically short time periods.⁶⁸

All of this is compounded by the lack of a common framework or analytical baseline. In the absence of a common framework that is used as a reference point, individual studies neither confirm nor deny nor modify generalizable conclusions or hypothesis. Thus the knowledge they generate, even when it is methodologically sound, does not accumulate – it is lost after use in a very narrow window of time by a single or small group of actors. And because this is so, there is no strong basis in accepted research to push policy or decision-makers on the basis of that research. For research to have an impact on policy, a minimum condition is that it be sound, credible, widely shared, and accessible. To date, most fragile states research has exhibited none of these conditions.

Of course, for research to have an impact on policy, these conditions are the starting point, not the end point. Many, many non-research factors impact policy and decision-making. Research findings matter only marginally to Security Council or member state decision-making; national concerns about cost and risk trump research-inspired policy findings. The world's best research agenda can't wish away these realities – but it can constrain them.

And then there's this rather critical point: development ministries aren't the main decision-makers on many of the policy instruments that respond to fragile states. And for all of the progress that's been made on "joined up government" or "whole of government", there are still profound differences between the worldviews and strategic orientations of foreign, defense and aid ministries.

Section B: Future research agenda

The lack of accumulation of evidence is a key constraint on the quality and policy impact of fragile states research in the previous fifteen years. The 2011 WDR, which builds on the OECD's studies, and reinforces findings from an earlier scholarship about the multiple causes of war, provides an important opportunity to make a decisive break with this unproductive pattern. Whatever the flaws, gaps or weaknesses of the WDR, a common starting point and basis for accumulation of findings is the *sine qua non* of developing effective research strategy on fragile states in the coming period.

⁶⁶ DFID 2010.

⁶⁷ See Van de Walle 2001, Knack and Rahman 2007, Patrick and Brown 2007.

⁶⁸ McGillivray 2006.

Success requires a three-pronged strategy: (i) test, refine, build on and disseminate the ‘multiple causes, weak institutions, multiple transitions’ framework; (ii) adopt rigorous methods for research on the application to policy tools; and (iii) invest in new systems for measuring progress (and therefore also baseline indicators)

B.1. Test, refine and build on WDR findings

Making the ‘multiple causes, weak institutions, multiple transitions’ framework a shared point of departure.

Left to its own devices, the academic response to the 2011 WDR will be “let a thousand flowers bloom.” Some scholars will individually test specific components of the model; it will inspire some PhDs; other, ongoing studies of various features of fragile states/civil war will indirectly comment on, test, or refine the WDR framework. Similarly, left to ‘business as usual’, the donor community will focus attention on the WDR for a short period, but then fall back into a pattern of disparate, uncoordinated, and non-cumulative studies that do not advance a genuine research/policy agenda.

There are important areas where recent work by the OECD, the academic community and the WDR constitutes not just a collection of research findings but rather, a research agenda: areas where its conclusions require substantial addition investigation and/or refinement. The five most important are: political settlements; legitimacy; confidence and risk-taking; donor-driven diffusion of effort; and organized crime. The first four are tightly inter-linked; and the last one may be as well, but we do not know enough to say.

Political settlements

As noted in the previous section, a growing body of evidence points to the nature of the political settlement in determining fragility or stability. There are substantial research gaps here, however.

First, settlements that lead to democratic systems are *not* the only stable option. Indeed, many studies show that early movement to democratic systems is risky: young, immature democracies are the *most* conflict-vulnerable system out there.⁶⁹ This may be true for some decades: Singapore and Vietnam and China are powerful exemplars of the option of combining oligarchic political systems with market economic systems. Recent political science work⁷⁰ suggests that these systems are among the most stable in the world, at a certain stage of development.

Second, the above point being said, the fact is that since the end of the Cold War, countries that have emerged from violence and stayed out of violence have done so by adopting an *inclusive* political settlement. (With one exception, Angola.) Strikingly, this is even true of countries where one side has won a civil war outright: when victors try to exclude their former opponents from political arrangements, they tend to slide back into war in due course. Military victors that have extended an olive branch to former opponents have done better.⁷¹

Third, authoritarian systems have a shelf-life. A sustained inability to address the political demands of a population or to administer justice in an accessible, even-handed way, ultimately stokes up popular pressures that are difficult or impossible to contain. This is surely a large part of what’s underway in the “Arab spring”, with amplifying factors of joblessness and food price rises creating economic motivations for change on top of pre-existing political ones. What we don’t know is how long these types of regimes can reasonably endure.

⁶⁹ Hegre, Ellingsen, Gates and Gleditsch 2001; Eubank and Weinberg 2001.

⁷⁰ Buena de Mesquita; Buena de Mesquita and Downs.

⁷¹ World Development Report 2011.

For low-income states with weak institutions trying to exit from violence, an inclusive political settlement looks like the most stable path in the short to medium term. How, when, and to what degree to democratize is a far more complex question. Below we address research gaps at the operational level – i.e., how to help shape positive, inclusive, sustained political settlements. There are also more basic gaps in our understanding of how settlements evolve and endure, namely:

- Are some forms of political settlement more/less resistant to the temptation to violence?
- What degree of inclusion is sufficient to reduce the risks of future episodes of violence?
- Under what circumstances do elites abandon an existing settlement? What combination of security fears, power competition, economic interest, regional/global opportunities/pressures produce ‘stay’ or ‘go’ decisions among elites?

This is of course a variation on basic causes research but in more policy-actionable terms.

Legitimacy

Questions of political settlement are closely related to issues of legitimacy, for which the base of research knowledge is exceptionally weak, though it is central to historical studies of state formation.⁷² There is no international consensus around the dimensions of legitimacy, making it difficult to measure. Yet absent an account of legitimacy we lack an essential ingredient in understanding conflict and state-building decisions by elites and broader populations. Important lingering research questions include:

- How do different features/versions of legitimacy (process; performance; historical/embedded) relate to one another? To what extent can a state (or proto-state) with strong embedded/historical legitimacy ignore performance/process pressure, or vice versa?
- How is legitimacy formed/fostered after internal violence? What role does violence play in forging perceptions of legitimacy, versus political dynamics or other facets?
- How does service delivery reinforce or fail to reinforce legitimacy? Meeting the basic needs of a population is one important dimension of legitimacy (though far from the only one); how can donor support to state service reinforce perceptions that the state (or the managers of the political settlement) are seeking to meet social needs? Similarly, does humanitarian assistance, by bypassing the state, undermine legitimacy and the political settlement and thus increase the risk of relapse?

Confidence and risk-taking

Confidence and risk-taking are essential not only to the issues above, but also to the application of policy.

This is the area where the WDR progresses the furthest in the direction of making new propositions. Here, the WDR builds on earlier conclusions from Stedman, Fearon and Walters, and reaches similar conclusions to Collier in his recent work.⁷³ It also incorporates findings from game theory and the literature on financial crises to deepen our understanding of the role played by gaps in confidence and/or trust following violence. The argument is that a lack of confidence in the future action of other parties constitutes a major barrier to cooperation, and thus to institution-building – and that real institution building can only begin *after* some basic restoration of confidence or the use of commitment technologies to overcome trust dilemmas.

⁷² OECD 2006.

⁷³ Collier 2010; Stedman 1995; Walter 2010; Fearon 2010.

If the WDR's conclusions on confidence and risk-taking are right, they should constitute a major new starting point for how to think about policy intervention in fragile states. But first, the conclusions must be challenged, tested, and refined. Essential research questions include:

- If violence produces a confidence barrier to cooperation/development, how long does that effect last? Does it have different lengths of endurance in different sectors (i.e. does economic risk-taking recover more quickly than political/security risk-taking?)
- What set of factors drive elite/popular decision-making to rebalance between risk-avoidance (because of a confidence gap) and risk-taking? What roles do economic opportunity/need play? Family/household pressures? Regional/global factors? Security guarantees?
- How much does national mobilization matter? The WDR's contention is that a sharply limited number of achievable objectives can rally domestic risk-taking/participation – this is an important claim and requires further testing/refinement.

Organized crime and transnational terrorism

Patterns of war and violence are changing. There is initial evidence of a rise in the impact (and level) of organized crime in several regions, notably Latin America and West Africa, and there is some evidence of a direct relationship between the nature of post-conflict recovery and the onset of organized criminal violence. Micro-studies reinforce these conclusions, for example in Colombia and Guatemala. Broadly speaking, though, there is both (a) less knowledge than warranted on the dynamics of organized crime and (b) less knowledge than warranted about the connections/relations to civil war or post-war dynamics.⁷⁴ Similarly, we are only beginning to understand the ways in which transnational terrorist organizations prey on and/or amplify local conflicts for their own purposes.⁷⁵ The research agenda includes:

- Further – but comparative, cumulating – research on the impact of specific social structures on patterns of mobilization and demobilization, and the impact in turn on the likelihood that former combatants will re-organize for organized crime
- What is the relationship between democratization processes and organized crime risks; some initial literature suggests that just as nascent democracies are at greater risk of civil conflict, they are also at greater risk of organized crime, but this needs to be tested
- What is the relationship between organized crime and de-legitimation – does a state lose legitimacy in the eyes of elites/population through association with organized crime? At what level/point?
- How do the leaders of transnational terrorist organizations and local civil conflicts interact? Are the leaders of civil conflict willingly co-opted by transnational terrorist organizations, or the victims of a form of 'hostile takeover'? What set of factors drives alternative relationships? Under what conditions is a civil conflict ripe for infiltration by a transnational movement?

B.2. Applied research on the application to policy tools

Perfect understanding of the dynamics of fragility and conflict is insufficient unless there is a further translation of that understanding into the implications for policy tools. This is the area where

⁷⁴ Skaperdas 2001; Bilingslea 2004.

⁷⁵ Kenney 2007.

methodology issues are especially important, given the poor research design applied in much policy-relevant work; this is elaborated in the next section.

Far too much research on policy tools focuses on specific sectors, as if it were the case that people within fragile states compartmentalized their interactions or decisions in the same way that external agencies do. This is at odds with everything that we know about human decision-making outside of bureaucratic contexts, and especially decision-making in high-risk environments. Rather, a more appropriate way of thinking about engagement is to orient around the decision-making process of elites and broader populations: their risk calculations; their decision-making calculations. In violent or recently violent contexts, risk-avoidance and calculations about risk-taking may be of paramount importance. To understand the impacts of external policy action, we should be asking: how can external policy tools shape the choice sets confronted by local actors in ways that increase the odds that they feel confidence to take positive risks for peace/cooperation, rather than to either avoid taking those risks or, worse, acting on mistrust and insecurity?

Commitment technologies

We know a reasonable amount about the manner in which *security interventions* constitute an important commitment tool for parties who do not trust one another not to return to the use of violence.⁷⁶ We know far less about *political commitments*. American political science discounts politics *per se* as a matter of methodology, and is only beginning to analyze and understand the ways in which political commitments by leaders or other actors can influence decision-making and behavior.⁷⁷ Other traditions are more appreciative of political intervention, but less committed to rigorous methods. The combined result is that there is little in the literature that gives us credible guidance as to when, where, or how political commitments, political guarantees, or the like, actually matter in fragile states and conflict response.

Political commitments matter – national diplomats and international officials place a great deal of stock in the business of hosting international conferences, making explicit political commitments, signing peace agreements, etc. Anyone involved in such efforts knows that a certain portion of this behavior is driven by domestic political and career ambitions. Despite differential motivations, though, these actions might actually help: we simply don't know. It is important to understand the conditions under which external political guarantees actually matter, and how.

We also need to know more about *external legal or judicial commitments*. For example, the role of mixed courts as a source of confidence to parties in the credibility/independence of court action is taken as a given, but has not been the subject of systematic study. Experiences of mixed economic tools (e.g. the dual-key system used in Liberia for control over the sale of natural resources, the mixed national/international system established (but then eroded) for the control over Chad-Cameroon pipeline) are too few as yet to be a basis for truly rigorous comparative assessment, but case-specific assessment of the influence of external participation on national decision-making is warranted and needed.

In all likelihood, we need substantially to extend the use of external commitment technologies in the economic sphere. Tools such as reinsurance, small loan guarantees, low-interest rate loans, etc., either directly to the private sector or through state investment entities, could affect economic risk-taking by citizens. Tools such as pooled investment instruments for high-risk FDI in fragile states could create new resources and new risk-taking opportunities. Simply put, we need to know much

⁷⁶ Fortna 2008.

⁷⁷ Fortna 2003; Walter 1999, 2002; Horowitz 2007; Benomar 2004.

more about the decision-making and risk calculation of market actors and potential market actors (governments; government agents; private businesses; wealthy individuals; household heads with savings; regional firms) in fragile states settings.

Jobs

Better understanding market decision-making in fragile states is closely related to a second major theme for policy-specific research: jobs. For much of the past three decades, mainstream macro-economists, including at the World Bank, have deliberately not focused on the question of jobs as a first part of economic reform. This may constitute sound macro-economic policy in a politically stable setting, but as part of a recovery process or in post-authoritarian transitions, it's simply daft. Even advanced industrial economies are learning the political perils of job-less growth or sustained high-levels of unemployment; for politically fragile states, joblessness can be – figuratively and literally – fatal.⁷⁸

A refocus on labor economics would be an important corrective. Just as important, though, would be to undertake a systematic review of senior political and policy figures in recovering states that have successfully implemented job-based recoveries. An understanding of their political calculations, of their assessment of market conditions in periods of instability, their strategies for balancing competing interests – these would be vital compliments to a more traditional analysis of job creation under conditions of fragility and conflict risk.

Justice and accountability

The fragile states policy community has increasingly focused on the importance of justice and accountability issues both as part of what causes instability, and as important elements of recovery strategy. As noted in Section A, there is reasonable technical understanding of certain aspects of the judicial, human rights and accountability process.

What is deeply lacking in policy terms is a solid understanding of the relationship between political settlements and judicial/accountability processes.⁷⁹ Under what conditions do tentative political settlements support the development of accountability/justice mechanisms? Under what conditions can justice/accountability mechanisms survive even in the absence of strong political support? Under what conditions can justice/accountability mechanisms actually drive political reform? Deeper understanding of such questions is essential to understanding where and when policy engagement aimed at support to justice/accountability mechanisms can actually succeed.

Regional administrative capacity

Justice and accountability mechanisms might also operate at sub-regional or (for high-order issues) even regional level. There are obvious nationalist impediments to this. But states with very weak institutional capabilities and a low base of financial and human capital with which to build, may want to consider vesting some administrative capabilities in sub-regional institutions or with shared sub-regional tools. The West Indies central court mechanism is one example; the African Court on Human and Peoples' Rights another.

Little is understood about the political conditions that make such decisions feasible; the nature of regional or sub-regional cooperation that does or does not have to pre-exist for such arrangements to come into being; or how effective they are at influencing national reforms or national policy.

B.3. Measuring progress (and evaluating policy performance)

⁷⁸ Dixon 2009.

⁷⁹ Fearon 2010; Kalvyas 2006; Collier 1999.

For the most part, donors wanting to evaluate progress look to measures of institutional development. What we are beginning to learn, though, is that the timelines for real institutional change, as opposed to mere formal mimicry of change, takes decades, even generations. Measuring progress based on institutional change or economic growth or the MDGs is thereby deeply problematic – it simply takes too long to register within the timelines relevant for donor accountability.

This is a major issue and has been taken up as part of the OECD's Monrovia action plan, so this paper need only touch briefly on it. The essential points are these:

- Institutional development takes too long a time to show results against existing donor timeframes
- other factors can show results: reduction in levels of violence; establishment of a political settlement; confidence (among elites, among the general population) in the political settlement; trust in government or in specific institutions (police, army, etc.)
- to measure such results, donors will need to invest in baseline data: as of now, there is not good, reliable, cross-national data on deaths from war; deaths from organized crime; incidental deaths from violence; trust in institutions; perceptions of legitimacy; or similar – this is a necessary step to develop an effective system for measuring progress.

Two additional points – which have not yet been adequately captured by the OECD process – should be made here. First, it is vital that measures of confidence/trust/legitimacy capture both elite perceptions and popular perceptions. Everything that we know about the dynamics of war and violence tells us that there are differences between the motives of leaders and broader groups. For the most part, the decisions to start war, end war, or return to war lie in the hands of elites. Household or confidence surveys or similar that capture popular attitudes but not elite attitudes will tell us little about the risks of onset or relapse into violence or war.

Second, to illustrate how such progress studies will work, it would be useful – right away – to conduct some illustrative studies of cases where there has been substantial progress, and where even deeper institutional progress is beginning to be seen. Obvious cases would be Mozambique, where there has been 20 years of progress; and Sierra Leone, where there have been 10. Historical studies of this progress would illustrate the pattern we are likely to see elsewhere: a reduction of violence; the establishment of a political settlement; the fostering of trust in that settlement; the emergence of confidence, and then of economic activity; the beginnings of institutional reforms; the deepening of institutions and of the legitimacy of the state. Illustrating the contours of that positive story line will be essential to demonstrate how external assistance can help.

Section C – Strategies for Bridging the Knowledge to Practice Gap

To bridge the gap between knowledge and practice will require more than good intentions, but a hardheaded dose of realism. There are substantial barriers to the production and uptake of research, and some of these are immutable. Some, however, can be eliminated with the concerted effort of the donor community. The challenge is in identifying these barriers, as we have begun to do here, and then to critically evaluate each barrier to identify those that can be reduced or removed, and those that we must work with or around.

C.1. Minimum steps

If donor-funded research is to add value, it must adopt a minimum level of research design and methodological rigor. This is not an argument for large-scale quantitative research – indeed, there are good reasons to believe that quantitative research is going to be limited in what it can tell us about the role of policy tools in addressing violence. Rather, it is simply to note that, for example, good design of comparative case studies and the use of peer review can substantially increase the long-term impact of studies.

Not all research managers in donor entities have training in research design. A University with a sound track record in methodology and applied work on fragile states could be asked to develop a ‘crash course’ in applied methodology for research managers; it could also be made available for researchers undertaking new studies in fragile states work. The box below gives a preliminary overview of some of the basics of case study design and development, with which researchers and donors should be familiar.

Most importantly, there should be continuous dialogue between researchers and those commissioning the research to ensure the use of proper design and methodology. Checking in on progress and building in time for iterative consultation in the design and hypothesis-forming stages is key to the production of quality research products. This process must remain open and candid; academic researchers frequently present their work in its early stages, benefiting from the feedback and criticisms of others in the field. Because of the dynamic of donor-driven research, there is less opportunity to engage and incorporate feedback, and this deficiency leads to under-developed theories and hypotheses, which then cannot be properly investigated.

Closely tied to this idea is that of an iterative process. As a study progresses, researchers will be constantly learning, and they should have room to incorporate these findings, even if it means altering the original ToRs. Barring researchers from reacting to the realities on the ground, or to shifting events, will only lead to stilted and outdated lessons. Another important lesson is taking small risks upfront: before funding a huge, multi-year study on an unproven theory or hypothesis, try it out in smaller form. This encourages new ideas, but also limits the downside consequences.

Another possibility to ensure rigor is a “good research funder” pledge. Such a pledge would encourage the adoption of rigor in research design – regardless of methodology – in the donor community. In doing so, it would allow for the accumulation of research, providing a solid foundation of knowledge that facilitates action, and especially joint action, as the pledge is taken up across the donor community. Additional steps to implement such a plan would be to formulate a reference sheet of best practices in methodology, particularly qualitative methodology (the box above is a start at such an effort), an open-source set of methodologies, and a shared data platform to which researchers are required to submit their findings.

It should be stressed: an emphasis on method and research design is not a case for excluding other inputs. Among those that can be valuable are anthropological work, local work, work in local languages, single case studies, and inputs from practitioners. All of these can add substantial value. The challenge is to understand how such inputs relate to broader findings. Too often, for example, practitioners will report that “in every case I’ve worked on, x or y factor has been essential” – without having the breadth of vision about the entire field to know that, by historical accident, they happened to work on two cases where x and y were important, whereas those factors were not present in other cases. Good research design, and a baseline framework, can allow donors to make use of all such methods but understand where the results fit into the broader scheme.

The primary benefit of the case study method lies in allowing deep historical, social, and political analysis. However, if we only look at one case, it becomes difficult to differentiate between factors that matter, and those that have no effect on the situation. The function of the *comparative* case method is to construct a counterfactual to test a hypothesis (or set of hypotheses) – e.g., if we say a particular outcome emerged because of international presence, we need to show what that case might look like without international presence.

In real life, this is difficult to do. There are a plethora of variables and contexts, and it is hard to control for the particular variables in question. Knowledge of the particulars of the context, therefore, is crucial. A good knowledge of historical, cultural, and political contexts allows the elimination of unlikely explanations and the development of a sound research question and hypothesis.

Practitioner knowledge, individual case studies, and quantitative studies of specific factors (rainfall; change in food prices; etc.) have to be incorporated into a wider framework by research professionals knowledgeable about the entire field.

It is thus essential to have a baseline framework against which new studies can be tested. Given the alternatives, the most feasible option is to adopt the ‘multiple causes, weak institutions, multiple transitions’ framework (summarized in the 2011 WDR) as a baseline, and use it as a backdrop against which new research can be added. This does not mean adopting the framework as if it were perfect, or had no flaws or gaps. But whatever the flaws of the framework, having a common reference point that can be challenged, refined, and revised, is absolutely essential to generating accumulated knowledge.

One approach to start this would be to create a bi-annual conference of donors that takes the ‘multiple causes, weak institutions, multiple transitions’ framework as a starting point, and updates the findings on a regular basis. Such a conference would bring together academic researchers, donors from bilateral, multilateral, and private organizations, and practitioners from the field to present, review, and provide feedback on research. Emerging from these conferences would be a review of the state of knowledge on key fragile states issues, and an agenda for research to focus on in the period ahead. The conferences would serve three purposes: they would create an informal ‘clearing house’ where high-quality, rigorous research can be aired and reviewed; foster a network of academic researchers, policy researchers, and practitioners with a shared interest in improving both the knowledge base and policy application on fragile states; and – if major research funders participated – create incentives for continued research by the academic community. This forum builds the base of knowledge but also provides an impetus to policy action. The in-person element is key; many attempts to bring donors together and share research have been tried, but because these often rely upon web platforms and impersonal communication, there is no mechanism to ensure engagement and upkeep.

Donors make frequent recourse to consultants and evaluation teams to generate knowledge about the impact or effectiveness of their policy tools. The current base of case studies suffers from two main problems: the cases selected and the method in which the study was completed. Too often, cases are selected either because a researcher has experience or interest in that particular set of countries, or because donors have dictated cases in which they have a particular investment. This results in exclude cases that could help determine whether the outcome in the countries are due to consistent policy effects, or merely characteristics of that set of countries that make them more likely to succeed/fail. These studies confuse correlation (things happening at the same time) and causation (one thing causing the other).

A simple five-step process can make an enormous difference in the quality of work produced.

- (1) **What are we testing?** Focus first on variables and the hypothesis. What are we trying to test? We need a hypothesis that can be tested – e.g. “the deployment of peacekeepers creates a sense of security that allows economic actors to start re-investment, leading to an increase in economic activity.” Hypotheses should be testable: they can be found valid, or not. Findings that show failures are incredibly important, and should be shared rather than hidden. As with medical studies, it’s just as important to know where a given treatment won’t work as where it will. A study with narrow but testable parameters (“our aid supplement to this or that Ministry increased its capacity to contribute to confidence building measures in this or that issue area”) is more likely to succeed than one with broad parameters or no hypothesis (“was our aid effective?”).
- (2) **What are we trying to learn?** Be clear about what the consultancy/case study is trying to do. The primary benefit of the case study lies in allowing deep historical, social, and political analysis. Single-country studies can do several things: provide country-specific evidence that illustrates a policy dilemma or policy proposal (that could be the subject to further testing); test the relevance of a causal theory in a specific case; provide a ‘truth check’ against new theories or causal arguments; observe previously unstudied phenomena; or provide raw data as a building block for deeper explanations. Comparative case studies, involving multiple countries, can also differentiate among the factors involved in producing divergent outcomes.
- (3) **What method is useful?** Once you determine the purpose, select an appropriate method to focus your study. There are a number of specific methods researchers can use to ensure better rigor: controlled comparison of two or more instances of a phenomenon that recurs in two cases that are extremely similar; process-tracing, where the goal is to understand the impact of policy intervention on the decision-making of a defined set of actors; and theory testing – where the study starts with a hypothesis derived from the literature or prior knowledge, testing its validity in a set of cases.
- (4) **Select cases carefully.** Avoiding selection bias is critically important to knowledge generation, as is including ‘control’ cases. The limits on donor resources and the political imperative to examine ‘priority’ cases add to the case for donors to work together to share studies, share terms of reference, and pool resources – four donors, each wanting to study the 2-3 cases that matter most to them, could come up with a joint study that would cover the 6-8 cases plus 2-3 control cases that would give more rigor.
- (5) **Peer review before and after.** Submitting the research design or inception report to peer review will help avoid some of the recurrent problems of selection bias and lack of rigor. Submitting draft research to peer review will help catch important mistakes in conflating correlation with causation.

Another approach would be for research funders to adopt the practice of adding into Terms of Reference, on a routine basis, reference to the current base of knowledge (accumulated in the WDR 2011) and the importance of researchers explicitly commenting on how/whether their findings reinforce/refine/contradict WDR conclusions. (This must be twinned with solid methodology.) Ideally, research funders would provide a small amount of supplemental funding to most studies to enable researchers to write separate research notes specifically on the way their findings relate to the literature, especially the WDR.

C. 2. A modestly ambitious approach

A more ambitious approach involves joint funding of initiatives, coordination of studies, and division of labor. Joint funding is one way to lessen the negative impact of national bias, and improve coordination across donors. It creates a ‘tripod’ of accountability, such that donors are accountable to their own constituencies, the beneficiaries of the work, but also to each other. Formulated with an understood focus on policy impact, these arrangements could increase the incentives for impact.

Coordination is simply a matter of efficiency, but also trust (aided if all major donors have adopted the methodology and joint starting point ideas above.) If the UK is to commission a study of the effectiveness of DDR programming, say, and that study is methodologically rigorous, why should that study not inform Norway’s programming, or Holland’s? And if Holland is commissioning a study of the impact of protection standards on child welfare, why can DfID not use the results? And if this is the case, then each donor can commission fewer studies, and coordination between donors can give broader coverage, and greater impact. A division of labor can emerge.

All this is easier said than done. The same arguments have been made about financial reporting standards and audits, for years, with minimal effect. Still, no harm in trying. And if donors had adopted a set of ‘good research donor’ commitments, or similar, the ability of one donor to sell the results of another donor’s study would rise, as the commitments would provide a standard of rigor and credibility that would make it harder for Parliaments or others to reject.

C.3. Taking the policy-research nexus seriously

If the research donor community on fragile states were genuinely serious about making a step change in the way they go about funding research, to maximize policy impact, then more substantial shifts could be made.

A comprehensive approach would start with investment in the baseline and tracking data described in Section B. This would require joint investments by donors and jointly operated research programs by a range of multilateral institutions, research centers and NGOs.

The second step would be adoption of basic standards in research design and methods by research funders, and an agreement to either (a) use institutions familiar with basic research method and/or (b) provide basic methods training to other researchers to be used for specific projects. The third step would build on the “Good Research Funder Pledge”, creating a core group of donors to pursue coordinated investment in various elements of the research agenda outlined above – on legitimacy, confidence, tools, etc. This would entail a sharing of information about what studies various donors have planned or intend, and a willingness to co-invest, avoid duplication, and make use of each other’s findings. The group should adopt a basic framework, using the ‘multiple causes’ framework as a starting point. Importantly, this group must not merely share knowledge, but ensure its critical review. They should invite a group of qualified scholars and practitioners to join them as reviewers

of proposals and reports, offering suggestions, comments, and criticism to be incorporated into findings and recommendations. Only work that has been through this process should be allowed for submission at the bi-annual conference.

A further step would be the creation of joint investment in centers of excellence in various aspects of the fragile states research agenda. (It is essential that these be joint investments: for bureaucratic reasons, donors pay more attention to research that they pay for than that which is available for free.) It would be important to ensure that nascent or developing research centers in the global South participate in such efforts, perhaps building on an expanded version of IDRC's Think Tank Initiative. In truth, though, much of the existing capacity to drive a genuinely impactful research agenda exists in western universities. One option to manage the politics of this would be to twin funding for core research in such centers with funding to host scholars from the global South for sustained periods of residence within those centers. Donors should refrain from insisting that their funding be used for scholars of their own nationality.

Quite apart from scholarly research, an important part of generating policy-relevant knowledge is to sustain the process of garnering lessons from policy-makers in fragile states. This has been done episodically and without rigor at various points in time, but rarely done systematically. A sustained process of engagement on lessons learned from policy makers, but submitting those lessons to testing and questions about comparability, would have real value.

There would be value, as well, in investing in a genuine research capacity at the UN Secretariat. The absence of a genuine research capacity close to the political decision-making of peacekeeping operations and political/mediation efforts denies the international system with the kind of evidence-based analysis of interventions that has characterized the best of the World Bank's work in the economic realm. There are substantial North/South barriers to this, however, as well as new reticence on the part of western donors to fund capacity at the UN. One option in that regard would be to twin this proposal with a cut to funding to the innumerable UN-based research centers on various topics of marginal relevance or for which there is far better research work in the academic community.

Policy will not be research driven, however, unless donors are willing to abandon a number of shibboleths. In the 1990s, the favorite of these was the notion, put forward in a UNICEF study, that "whereas in the past, most of those who died in wars were soldiers, in modern wars, roughly 90% of those killed in war are civilians." This research finding drove a lot of policy and investment in the humanitarian, civilian protection, and peacebuilding fields in the 1990s. However, not only was there no evidence for this finding, it was completely, profoundly, and importantly wrong, in several dimensions. (Many, many past wars had 90-99% civilian death rates; many contemporary wars see mostly soldiers killed – the range of civilian deaths in war has ranged through history, and still does.) The current shibboleth is "increasing the involvement of women in peacemaking increases the chances of sustained peace." This may well be true: but a scouring of the literature shows that there is literally no evidence whatsoever for the conclusion. There are myriad anecdotes to this effect, but few that would pass a control test. The most widely cited study on the impact of female peacemakers is a UNIFEM report that cites incomplete data. (The weak research culture at the UN is frequently responsible for these shibboleths entering the policy bloodstream.) If policy was truly research driven, we would still be agnostic on the question of women's involvement in peace. Rigorous research on this topic is sorely needed.

Donors working with a development orientation are also going to have to be willing to be responsive to the interests and concerns of foreign ministries (and to a lesser degree defense

ministries – they have their own research funds.) As noted above, many policy instruments that are relevant for the response to fragile states actually lie in the hands of foreign or foreign and defense ministries, not development ministries. Substantial gaps in worldview continue to characterize those divides. If development ministries wish to impact policy on mediation, political settlements, confidence building, peacekeeping, security guarantees, organized crime, and other, they will have to build bridges to these other parts of government, including by being willing to fund other department’s research needs (or see research funds divided more equitably across government.) This is bureaucratic heresy, perhaps, but necessary.

More fundamentally, the existing research already calls for a strategic shift in direction by donors: towards adopting a ‘venture capital’ approach to risk-taking in fragile states. In essence, the existing research findings tell us that external support will be most effective if it shifts some of the risk-taking from national decision-makers to external actors. As noted above, the recourse to violence is often a rational act; indeed, taking a peaceful strategy may be highly irrational, or at least, extremely risky. A critical element of success in moving forward in stability and development in fragile states will be for the international community to increase its willingness to absorb some portion of the risks faced by decision makers in fragile states. Donors and international organizations will have collectively to evaluate the upfront (and relatively small scale) risks that they are willing to absorb to prevent large-scale state failure and more positively to help fragile states move out of a destructive cycle of fragility and conflict. This could involve: providing close protection security for political elites, for a sustained basis (as NATO is doing in Afghanistan, and is currently being proposed for Libya); providing long-term, over-the-horizon security guarantees to the participants in a political settlement; providing low-cost insurance to the government, to private investors, to outside investors, for local or foreign investment in industry or other economic activity – basically, insuring losses; underwriting joint national/international justice mechanisms for a sustained period of time – a generation, not a project cycle.

Many such investments will fail: the point is to recognize that investments in fragile states need to be risk-taking, and are likely to succeed and fail roughly at the rate of venture capital investments, not at the rate of banking decisions – the logic of which currently governs World Bank financing and much of that from bilateral donors.

* * * *

This last shift is not a research one; it’s a profound policy and political shift. The connection to the research agenda is this: if western donors continue to operate in fragile states on the basis of a politics and policy that treats them either as slight extensions of a humanitarian operation, or slight modifications of normal development engagement, then overall policy engagement will fail, and the best research in the world won’t make a whit of difference. On the other hand, rigorous, cumulative research into the dynamics of violence and underdevelopment in fragile states, and the ways in which effective, risk-taking international policy can help overcome fundamental commitment problems, can help make the case for this strategic shift in approach.

Dos and Don'ts of Knowledge Generation on Fragile States

1. DO ensure familiarity with the current state of knowledge on the particular subject/theory that you are studying.
2. DO start from a theory or logic as to why a certain outcome may arise (case selection comes later).
3. DO identify a solid research question. The research question will guide your study, constrain it to a manageable scope, and situate the study's importance in the field.
4. DO clarify your assumptions. Are you assuming that elites are making rational calculations? Then say so. Thinking through the assumptions refines the underlying theory.
5. DO make sure that your hypothesis is testable – the design must be set up to confirm or call into question the hypothesis based on your study.
6. DO think through the counterfactual –what would happen in the absence of these factors?
7. DON'T pick your cases before you have fleshed out your theory and hypotheses.
8. DON'T limit your study to countries of particular interest or priority. This will bias your results.
9. DON'T limit yourself to researchers from your own country. This is complex research; you want the best out there, regardless of national origin.
10. DO invest in developing research capacity and cultivating new generations of researchers.
11. DON'T be fooled by fancy methods. A sound theory and knowledge of the field underlies all good research.

Levels of Confidence:

Breaking down the current base of knowledge

1. FINDING: *The quality of country governance or institutions is an important determinant of violent conflict.*
 - a. Degree of Confidence: MODERATELY HIGH.
In the 2011 WDR, this was an important finding of the work done to re-test existing qualitative studies. It reinforces a strong conclusion from earlier qualitative literature on comparative civil war. However, the finding still needs work in two areas: one is elucidating the specific elements of governance that matter – is it rule of law? Government effectiveness? We don't yet know the relative importance/influence (if there is a distinction) of the different dimensions of governance. Second, sub-national institutions: there's a dearth of data on sub-national institutions, so we simply don't know whether variation in the degree or quality of sub-national institutions helps explain the local variation in violence.
2. FINDING: *"Inclusive enough" political settlements are vital to avoiding relapse into violence.*
 - a. Degree of Confidence: HIGH. Comparative studies, theoretical studies, and quantitative studies align on this finding.
While we know that inclusive enough settlements are important, the meaning of inclusive enough is highly dependent upon context.
3. FINDING: *Confidence building and trust are vital components of a society's recovery from conflict.*
 - a. HIGH. Qualitative work, practitioner experience and comparative case study work all reinforce this finding.
Though confidence building is undoubtedly important, the hows of building confidence remain unclear. More is needed to explore the role of service delivery; whether certain kinds of measures are better at eliciting confidence, or whether it is only the act of meeting expectations that matters; and how confidence and trust affect a state's long-term legitimacy. This study should be undertaken along with the investigation of different dimensions of governance, mentioned above.
4. FINDING: *Justice is an important part of peaceful recovery.*
 - a. Degree of Confidence: MODERATE.
Many cases where there has been successful recovery have included some element of justice institutions reform or early confidence building steps in on justice as part of a package of measures. However, the lack of rigorous work on the justice components of recovery results in confusion regarding our current state of knowledge on this issue. One major barrier to conducting rigorous research is a lack of baseline data. It is difficult to assess the influence of justice when data are not indicative of identity groups, and few reliable perception surveys exist. Even those that are widely used, such as Afrobarometer, fail to differentiate by identity group, so it is difficult to say how just minority or vulnerable groups perceive their treatment to be.
5. FINDING: *Jobs are an important part of the post-conflict recovery story.*
 - a. Degree of Confidence: MODERATE – to – LOW.

Despite the fact that jobs are one of the most talked-about elements of conflict prevention and recovery (see the earlier discussion of the “Greed vs. Grievance” debate), there is little consensus around the role of labor. Part of the reason is that data on employment is limited; part of the reason is that the theoretical emphasis on the macro conditions for growth means economists dealing with development often fail to study employment. Employment data is notoriously poor because it is often used as a political tool, and so its measurements varies across countries, and is not comparable. Some research, notably Fearon’s WDR background paper, points to the importance of governance, instead of high unemployment itself (the two tend to be correlated), but more work needs to be done.

6. FINDING: *Peacekeeping is a critical instrument in the prevention of violent relapse.*
 - a. Degree of Confidence: HIGH. Repeated quantitative and qualitative findings align on this finding.

Studies of peacekeeping offer solid evidence that peacekeeping, when done properly, can provide a mechanism that allows parties to the conflict to believably give up arms, when peacekeepers are authorized to use force to ensure that all parties maintain their commitments.

7. FINDING: *Disarmament Demobilization Reintegration (DDR) is a key instrument in post-conflict recovery.*
 - a. Degree of Confidence: MODERATE.

Some recent work on DDR has started to shed light on its role in recovery, but it is still under-studied, and we cannot yet be sure that it plays a key role in preventing violent relapse, or how and why it does so.

8. FINDING: *Stable post-conflict recovery takes a long time.*
 - a. Degree of confidence: HIGH (but more work needed).

Based on historical evidence, the time it takes for countries to emerge from conflict and make steady progress towards the development of well-governed institutions, a growing economy, and peace is decades or more. We need more work on recent timelines – how long genuine transformation has taken in recent cases is an important indicator of credible timelines and progress goals. An important research agenda is historical research on cases that are now at the 20, 15, or 10 year mark following the intensive phase of post-conflict recovery: how well have they done; which transformations have occurred; which have not.

MUST READS:

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2. Stephen John Stedman, "Negotiation and Mediation in Internal Conflict," in Michael Brown (ed.) *The International Dimensions of Civil Conflict* (Cambridge University Press 1996): pp. 341-376.
3. James D. Fearon, "Why do Some Civil Wars Last So Much Longer than Others?" *Journal of Peace Research* 41, No. 3 (May 2004): pp. 275-301; James D. Fearon, "Governance and Civil War Onset," background paper for the *World Development Report 2011* (World Bank 2010).
4. *World Development Report 2011: Conflict, Security and Development* (World Bank 2011), available at <http://wdr2011.worldbank.org/>
5. *Concepts and Dilemmas of State Building in Fragile Situations: From Fragility to Resilience*. OECD/DAC Discussion Paper, 2008.
6. Charles T. Call, "Introduction," in Charles T. Call, (ed.) *Building States to Build Peace* (Lynne Rienner 2008): pp. 3-27.
7. Ghassan Salamé (ed.), *Foundations of the Arab State* (Routledge 2005).
8. Ashraf Ghani and Clare Lockhart, *Fixing Failed States: A Framework for Rebuilding a Fractured World* (Oxford University Press 2008).
9. Jeremy Weinstein, "Autonomous Recovery and International Intervention in Comparative Perspective," Working Paper, Center for Global Development (2005).
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11. Charles Tilly, "War making and state making as organized crime," in *Bringing the State Back In*, Evans P. Rueschemeyer D, Skocpol T (eds.), Cambridge University Press: Cambridge

In-depth Readings:

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- Francis Fukuyama, *The Origins of Political Order* (Macmillan 2011).
- *State Failure Task Force Report: Phase III Findings*, report by the Political Instability Task Force at George Mason University (September 2000), available at globalpolicy.gmu.edu/pitf/.
- Randall Collins, *Violence: A Micro-Sociological Theory* (Princeton University Press 2008).
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- Paul Collier, *Wars, Guns and Votes: Democracy in Dangerous Places* (Harper 2009)
- Bueno de Mesquita & George Downs, "Development and Democracy," *Foreign Affairs* (September/October 2005).
- Vanda Felbab-Brown, *Shooting Up: Counterinsurgency and the War on Drugs* (Brookings Institution Press 2009).
- Stewart Patrick, *Weak Links: Fragile States, Global Threats, and International Security* (Oxford University Press 2011).
- Mark Duffield, *Development, Security and Unending War: Governing the World of Peoples*, (Polity: Cambridge, 2007)
- Christopher Cramer, *Civil War is not a Stupid Thing: Accounting for Violence in Developing Countries* (Hurst & Co: London, 2006)

More Details on Case Study Design and Development⁸⁰

Properly performed, case studies can support existing theories, reveal new hypotheses, or identify causality in ways that large quantitative studies cannot. However, by definition the method is not all-inclusive, meaning that they can suffer from a bias in the cases that are selected for study (academics call this ‘selection bias’); cannot adequately control for the impact of a wide range of variables; and, if not grounded in the broader literature and a shared framework, can fail to add much to existing knowledge. The development of strong case studies requires a specific *research framework*, a clear *research strategy*, and consistent *case selection*.

Research Framework: A strong research framework for case studies can be derived from two broad questions: (1) *What is being explained?* and (2) *How can case studies be used to explore this issue?* A strong research question will guide the work, constrain it to a manageable scope, and highlight the study’s role in the field as a whole. A clear, solid research question is the *sine qua non* of a rigorous study.

Depending on the question, researchers select the appropriate type of case study for their research. Case work can do several things: it can provide historical evidence that illustrates a problem; it can test the relevance of a specific causal theory to a given case; it can use contemporary history to draw out new variables or hypotheses (which should then be further tested); it can provide a ‘truth check’ on untested theories; and it can provide building blocks for explanations. Understanding the role of a case study in relation to the overarching question guides further research steps and clarifies the relationship between theory and example.

Research Strategy: From the guiding question, researchers should determine how to isolate the specific issue being address (academics refer to this as identifying the ‘dependent and independent variables’.) Given the limits of case study methodology as a whole, these variables should be clear and limited in scope – case studies simply cannot address every possible set of relationships. From a sub-set of causal relationships, hypotheses can be drawn. A hypothesis must be testable – that is, observations drawn from case studies should be able to confirm or call into question its viability. The development of a research strategy that includes narrow parameters for variables under consideration and testable hypotheses ensures that, despite limitations, a case study will be rigorous enough to support its conclusions.

Case Selection: Case selection is where donor-funded research often falls down. Choice of cases should flow from the development of a research framework and strategy, not simply because they are of particular interest to the donor; this will result in selection bias and impugn the integrity of the study. Countries or situations under study should be chosen based on the presence or absence of the sets of variables identified as of interest in the research strategy, and to control for variables. (There’s a big difference between the application of a tool like mediation or peacekeeping or aid in a country with a high level of political interest, and one where political interest is low; there’s a difference between the application of aid or mediation in a country characterized by a long history of inter-ethnic tensions and one that’s got a unified population. Case studies that don’t control for the these factors will reach conclusions that can’t be generalized.)

⁸⁰ For a thorough explanation, see Alexander L. George and Andrew Bennett, *Case Studies and Theory Development in the Social Sciences*, Cambridge: MIT Press, 2005. Summarized by Jane Esberg.

There are a number of possible case study methods: controlled comparison uses two or more instances of a phenomenon that occurs in two scenarios that resemble each other in every way but one; process-tracing uses a series of related observations to explain an outcome; and congruence method starts with a theory and analyzes its ability to explain the outcome of a case or cases. Rather than focusing on specific cases and drawing from them potential hypotheses, in order to maintain clarity of purpose case selection should follow from the questions and hypotheses previously determined.

Annex A: Outcome Summary of the Geneva Roundtable on the future direction of investment in evidence on issues of fragility, security and conflict

On 22 September 2011, the U.K.'s Department of International Development (DFID) and Canada's International Development Research Centre (IDRC), in collaboration with Agence Française de Développement (AFD) and the Center on International Cooperation, hosted a donor roundtable discussion on *the future direction of investment in evidence on issues of fragility, security and conflict*.

Approximately twenty participants representing nine donor organizations met at Canada's Permanent Mission to the Office of the United Nations in Geneva to discuss:

- d. the state of evidence on issues of fragility, security and conflict;
- e. a future agenda for building the evidence base; and
- f. strategies to bridge the knowledge to policy/practice gap.

The following is a summary of issues discussed by these donors at the roundtable, which also drew upon the recommendations made in the baseline paper circulated before the Geneva meeting.

What do we know and how strong is the evidence base?

We know that fragile states are beset and defined by recurring cycles of violence, and that it is violence that feeds their chronic underdevelopment. Violent and impoverished countries have poverty rates that are, on average, 20 points higher than their impoverished but peaceful counterparts. Past work on conflict has often sought a simple, one-cause explanation (ethnicity, "greed", resource etc.) – and the accompanying silver bullet to solve the problem of war – we now know with a high degree of confidence that the causes of conflict are multiple and complex, and generally occur in combination. We know that it is difficult to sustain an exit from conflict, but not impossible; that inclusive political settlements are important to peace, and that building trust and confidence around the political settlement, especially among the elite, is vital to its success. We also know that this process takes time – often decades.

Yet, what we do know rarely translates into policy and practice. Much of what is "known" by the research community is not "known" by policy and practice communities in any real sense. The reasons behind this divergence are many, but three pieces, in particular, are key to understanding this gap: academic research tends not to focus on policy tools, limiting its relevance to decision-makers; donor funded evidence is rarely rigorous enough to reliably translate into implementation; and we have lacked (until now) a baseline framework against which knowledge can accumulate.

What are the major knowledge gaps and how can they be addressed?

Violence has emerged as one of the central development challenges of our time and developing a solid evidence base on the connection between violence, underdevelopment and recovery is more urgent than ever. There are important gaps in our existing knowledge base, these include understanding the:

- dynamics of transitions and the conditions for developing viable *political settlements*;
- conditions that generate public *confidence and risk-taking* to support state-building and development;
- dimensions and enablers of *legitimacy* in contexts of fragility, violence and insecurity;
- dynamics of *organized crime and terrorism* and their connection to local conflict and post-war dynamics; and
- role of external instruments in overcoming commitment/trust deficits; in job creation; on judicial institutions; and on sub-regional capacity in contexts of fragility, conflict and insecurity.

In order respond to the challenge of developing a solid evidence base on these issues, roundtable participants concluded that donors should adopt a four-pronged strategy:

1. test, refine and build on a common framework of analysis that will enable knowledge to accumulate in a coordinated manner;

2. apply rigorous methods to enhance the degree of confidence that we have in our findings;
3. invest in systems for measuring progress, such as the development and sharing of baseline indicators, assessments, and evaluations; and
4. create incentives to enable the inclusion of knowledge generated by non-Anglo Saxon and in particular Southern based institutions.

How can donors maximize the impact of new evidence on policy and practice?

Making progress on the stability and development of fragile states will require new approaches. Well within reach of donors are several modest changes. These include adopting *minimum standards in research design* and promoting the use of *comparative research methodology*. They also include using the 2011 World Development Report as a *baseline framework* that can be tested, refined, revised and against which new findings can accumulate. They also mean promoting *jointly funded initiatives*, developing a *joint architecture to help coordinate and improve the quality of evidence*, and adopting an informal *division of labour* to ensure that knowledge is shared, cumulative, and comprehensive.

Achieving deeper impact will require fostering and investing in a truly global network of academic researchers, policy researchers, and practitioners with a shared interest in improving both the knowledge base and policy application on issues of fragility, conflict and security.

More fundamentally, a strategic shift in direction by donors is warranted. A critical element of success in moving forward in stability and development in fragile and insecure contexts will be for the international community to increase its willingness to absorb risks faced by decision makers in fragile situations. Donors and international organizations will have to collectively evaluate the upfront (and relatively small scale) risks that they are willing to absorb to prevent large-scale state failure and violence, more positively to help move out of a destructive cycle of fragility and conflict. Many such investments will fail: the point is to recognize that investments in fragile contexts and situations need to be risk-taking and that investment in rigorously tested knowledge will help mitigate against these risks.

What are the next steps?

A number of immediate next steps emanated from the donor roundtable. They include:

1. Sharing ideas for jointly funded initiatives (i.e. on urban violence and political settlements);
2. Developing an informal joint architecture to share research plans and improve the quality of evidence;
3. A ToRs for a "Knowledge Investment Pledge" that promotes minimum standards in research design;
4. Linking up with related initiatives spearheaded by OECD-DAC's INCAF and the World Bank;
5. Finalizing the baseline paper circulated in Geneva with roundtable outcomes incorporated;
6. Inviting other donors investing in evidence to join in the Geneva process; and
7. Holding a follow-up meeting in Paris in December/January 2011, to action joint research.

Annex B: Roundtable Agenda

9:00 – 9:15 a.m.	<p>Introduction</p> <p>Welcome, overview, and objectives</p>	John de Boer (IDRC), Joanna Macrae (DFID) & Pascal Desbiens (Permanent Mission of Canada)
9:15 – 9:30 a.m.	<p>Session 1: Taking Stock of Past Investments</p> <p>What do we know and how strong is the knowledge base?</p>	Dr. Bruce D. Jones (NYU)
9:30 – 10:30 a.m.	<p>Roundtable Discussion on current Investments</p> <p>Objective: to gain an understanding of the scale and diversity of existing investment in research around fragility</p>	Chair: John de Boer (IDRC) Discussant: Eva Helen Østbye (NORAD)
10:30 – 11:00 a.m.	<p>Coffee Break</p>	
11:00 – 11:30 a.m.	<p>Session II: Identifying Priority Areas for Future Research</p> <p>What are the major knowledge gaps and how can these gaps be addressed?</p>	Dr. Bruce D. Jones (NYU)
11:30 – 1:00 p.m.	<p>Roundtable Discussion on Future Priorities</p> <p>Objective: To identify some key principles and issues to consider in selecting and designing the next generation of research</p>	Chair: Joanna Macrae (DFID) Discussant: Stephen Ndegwa (World Bank)
1:00 – 2:00 p.m.	<p>Lunch</p>	
2:00 – 2:30 p.m.	<p>Session III: Exploring Strategies to Bridge the Knowledge to Policy & Practice Gap</p> <p>How can research donors maximize the impact of new research on policy and practice?</p>	Dr. Bruce D. Jones (NYU)
2:30– 3:30p.m.	<p>Roundtable Discussion on Bridging the Knowledge to Policy and Practice Gap</p> <p>Objective: To identify the key barriers to the uptake and use of current knowledge on fragility</p>	Chair: Olivier Ray (AFD) Discussant: Tom Wingfield (DFID)
3:30-5:00 p.m.	<p>Conclusion</p> <p>What have we learned and what can we do about what we have learned?</p> <p>Objective: To identify practical ways in which we might work together to:</p> <ol style="list-style-type: none"> i. improve the efficiency of research investment ii. improve mechanisms to share knowledge and manage ‘noise’ and information overload 	Chair: Olivier Ray (AFD) Discussant: Joanna Macrae (DFID), John de Boer (IDRC)

Annex C: Roundtable Participants

Adam, Ségoléné	Swiss Agency for Development Cooperation (SDC)
Bishop, Emily	International Development Research Centre, Canada (IDRC)
de Boer, John	International Development Research Centre, Canada (IDRC)
Diaz, Virginie	Agence Française de Développement (AFD)
Elgin-Cossart, Molly	Center on International Cooperation, New York University
Hoyos, Christina	African Development Bank (AFDB)
Jones, Bruce	Center on International Cooperation, New York University
Lane, Sue	Department for International Development, United Kingdom (DFID)
Macrae, Joanna	Department for International Development, United Kingdom (DFID)
Marc, Alexandre	The World Bank
Ndegwa, Stephen	The World Bank
Østbye, Eva Helen	Norwegian Agency for Development Cooperation (NORAD)
Ray, Olivier	Agence Française de Développement (AFD)
Sangster, Rhett	Department of Foreign Affairs and International Trade, Canada (DFAIT)
Schwartz, David	International Development Research Centre, Canada (IDRC)
Vanveen, Erwin	Organization for Economic Cooperation and Development (OECD)
Wingfield, Tom	Department for International Development, United Kingdom (DFID)
Desbiens, Pascal	Permanent Mission of Canada to the Office of the United Nations, Geneva
Boyer-Meyerman, Michèle	Permanent Mission of Canada to the Office of the United Nations, Geneva