

## Civil War: Academic Research and the Policy Community\*

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This article argues that the academic conflict research community has far less impact on the policy community than the importance of its work deserves. This is so for a number of reasons. First, the scholarly and policy communities communicate badly – the former rarely seeking to make their work more accessible to the latter. This is particularly true of the work of the econometricians, which few in the policy community understand. Second, the still-dominant realist academic security studies community continues to focus on interstate wars, while tending to ignore the 90% plus of armed conflicts that take place within, not between, states. Realist theories are, moreover, largely irrelevant to the task of explaining civil wars. Third, few policymakers recognize that probabilistic theories cannot be refuted by one or several counter-examples, leading them to reject important findings for the wrong reasons. Fourth, the conflict datasets used by quantitative researchers have no official standing, are often incommensurate, are unavoidably inaccurate and ignore key measures of violent conflict. Fifth, while there is some consensus with respect to findings on the causes of civil war, there are also fundamental disagreements. Little effort appears to have been made to resolve the differences. Policymakers have neither the time nor the expertise to choose between competing explanations themselves. Sixth, while there is growing consensus that the causes of civil strife are to be found in the interrelationships between development, governance and security, divisions of labour between academic disciplines and between departments in both governments and international institutions constrain both interdisciplinary and interdepartmental collaboration. The article concludes with a number of recommendations to improve the policy impact of conflict research.

In the World Bank, the UN, the OECD's Development Assistance Committee and even the G-8, conflict prevention is currently accorded a priority that would have been inconceivable even five years ago. Donor governments now are regularly enjoined to view their aid policies through 'the conflict prevention lens'.

What this means in practice is not clear. Understanding violent conflict is a necessary condition for preventing it. But the international community's approach to

prevention is too often akin to physicians prescribing treatment without prior diagnosis. Policymakers confront political imperatives to 'do something' about violent conflict, but often have to act without really knowing what will or will not work.

Policy is often determined by mandates, past practice and politics, with little reference to the findings and prescriptions of academic researchers, much of whose work tends to be viewed with scepticism. This is particularly true at the UN which, unlike the World Bank, has few resources with which to undertake in-house research and lacks a research-oriented culture.

There are a number of reasons for the

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failure of academic research on armed conflict to have much impact on the policy community.

First, scholarly and policy communities do not communicate well. Academic rewards derive from publishing theoretically informed studies in university presses and scholarly journals, not from producing succinct policy briefs for busy officials, or even for writing for policy-oriented journals like *Foreign Affairs*, *Foreign Policy* or *International Affairs*. Policymakers tend to view academic research as being unrelated to their practical concerns, abstruse and often difficult to understand. Academic publications almost never have executive summaries; overworked policymakers rarely have time to read anything much longer. The more senior the policymaker, the less time she has to read.

Second, the mainstream academic security studies community in the USA (where most of the world's academic security analysts live and work) continues to focus most of its attention on the causes of war in the *interstate* system, even though interstate wars now constitute less than 10% of the total number of armed conflicts. In *International Security* and *Security Studies*, the leading security studies journals, articles on interstate war vastly outnumber those on intrastate war.

This is no accident. Realism, the mainstream security studies theoretical paradigm, is largely irrelevant to the task of explaining the causes of civil wars. Realism's focus is the international system. Its core assumption is that relations within this system are characterized by 'anarchy' – by which is meant the absence of over-arching global government. Since security is assumed to be achieved through relative strength, the strategic logic of anarchy leads inexorably to power struggles between individual states. But the pursuit of power in the name of security also produces 'security dilemmas' which are themselves a potent cause of armed conflict.

The tragedy of anarchy is that states acting both rationally and defensively in pursuit of security often achieve its opposite.

But, unlike the international system, the *intrastate* system is not anarchic; nation-states *do* have governments; the alleged logic of anarchy does *not* prevail within them. From this it follows that realism is of little relevance to the task of explaining violent intrastate conflicts. When states fail, when functioning government ceases to exist, intrastate group politics *may* resemble international anarchy. In this case, realist theory *may* help explain escalation dynamics in any subsequent civil war that breaks out. But realism cannot explain why states fail in the first place; and state failure is, in itself, a cause of civil war.<sup>1</sup> So, whether or not civil wars are associated with state failure, realism is largely irrelevant to their explanation.

Third, some of the most innovative research on civil wars, particularly in the past decade, has come from scholars using comprehensive databases on the incidence and duration of armed conflicts and multivariate statistical analyses to determine the salience of causal factors that generate these conflicts. This research, which builds on the earlier work of the Correlates of War project, has been closely, but not exclusively, associated with the World Bank's Economics of Civil War Crime and Violence programme and with the peace and conflict research community whose research findings are often published in such journals as *Journal of Peace Research* and *Journal of Conflict Resolution*.

Econometric research poses a different difficulty for policymakers. For if realism, while readily understood, is largely irrelevant to the task of explaining civil wars, then multivariate statistical analyses, while certainly relevant, are largely incomprehensible to the

<sup>1</sup> For a less compressed and simplistic discussion of this issue, see Posen (1993), Steven (1997) and Walter & Snyder (1999).

policy community. The virtues of Bayesian Heteroskedastic Probit models are unlikely to engage the attention of busy officials. Moreover, few if any policymakers have any idea what coefficients are, so even those who skip the technicalities in quantitatively oriented academic articles may still not understand what the results mean.

Much of the difficulty here arises because very few researchers in this growing field – the World Bank's Paul Collier is a notable exception – bother to 'translate' their work in such a way as to make it accessible to policymakers. Realist scholars do not have the same communication problem, since their theoretical paradigm underpins the 'common-sense' discourse of the security policy community. Thus it is no accident that security studies scholars from Harvard's Kennedy School are interviewed more often in the media, and get to write more OpEds in prestigious newspapers, than do quantitative researchers working on security issues.

Fourth, the fact that the causal models used in quantitative research are probabilistic means that they cannot be refuted by claims to the effect that, 'It's not like that in Burkina Faso' (or wherever). Yet the Popperian notion that a single counter-example falsifies causal claims remains extraordinarily pervasive in the policy community – and is an impediment to the wider acceptance of probabilistic theories.

Critics of econometric approaches to explaining armed conflicts often argue that detailed analyses of individual conflicts provide explanations that are richer, closer to the truth, and more relevant for the policy community than generalizations derived from macro-quantitative models by researchers with little or no area expertise. This objection misses the point. The two approaches are complementary, not contradictory. It is certainly true that econometric studies cannot provide detailed explanations of individual cases; they do not claim to do

so. But nor can individual case studies, however rich, produce generalizations.

The econometric approach to understanding armed conflict is analogous to that of epidemiologists who seek to identify those environmental and behavioural risk factors that impact on public health. Such findings provide both public health departments and individual physicians with useful general prescriptive advice – e.g. avoid diets high in saturated fats, do not smoke, exercise regularly, etc. Econometric analyses of the causes of armed conflict can, in principle, provide similarly useful broad prescriptive advice for policymakers.

Epidemiological research on the ecology of diseases does not, of course, substitute for detailed diagnoses of individual patients by their physicians. Similarly, econometric analyses of large samples of armed conflicts in no sense substitute for the analysis of individual conflicts by researchers with country and area expertise.

The need for both approaches may seem self-evident to researchers. It does not always seem so to policymakers.

Fifth, the quantitative conflict research community's approach to datasets has created another problem for the policy community. Researchers have created more than a dozen different armed conflict datasets, but have signally failed to provide policymakers with any guidance on which is the most appropriate for their needs. In June 2001, at a major conference in Uppsala dealing with data issues, calls for researchers to identify (or create) one dataset that the policy community could rely on were rejected in the name of pluralism. Different research designs, it was argued, required different datasets. This may well be the case, but, as Sambanis argues in a recent paper (2002a), many of the disagreements about the causes of civil war between researchers in the field (which are discussed in detail below) are a function of different coding rules. Gates

(2002) makes a similar point. Sambanis (2002a: 32) also points to a 'remarkably low correlation between some pairs of databases' – less than 50% in some cases.

The absence of official statistics on armed conflict, or of a single authoritative non-official source of data that has the backing of the research community, means that the policymakers not only have no guidance as to what data source to use, they often have little idea of trends in armed conflict either. Many UN officials, for example, simply could not believe that there had been a decline of more than 35% in the number of armed conflicts in the 1990s – the decade of Srebrenica, Somalia and Rwanda. Ironically, as both Gurr (2000) and Wallensteen (2002) have argued, part of the reason for that decline was the sharp increase in peace agreements in the 1990s – many of which had been brokered by the UN.

Sixth, the scope of the existing datasets means that some important measures of armed conflict and other forms of violence are not recorded.

One of the great virtues of the dataset produced by researchers at the University of Uppsala is that it is updated annually with results published (in somewhat different ways) in the *SIPRI Yearbook* and the *Journal of Peace Research*.<sup>2</sup>

But while the Uppsala researchers count the numbers of armed conflicts that cross particular casualty thresholds each year, they do not record the *total* number of battle-related deaths each conflict has generated.<sup>3</sup> So a war that kills 1,001 people is counted the same as one that kills a million. This

means that the number of armed conflicts in the world could in principle decline while the total global death toll rose. Nor are data on violent inter-communal conflicts – i.e. those in which a state is not a party – collected by Uppsala. Finally, the definition of armed conflict excludes the unopposed slaughter of innocents – so the Rwandan genocide, for example, is not recorded.

Funding is currently being sought to enable Uppsala to collect these additional data as part of a project to create an annual *Human Security Report* modelled in part on the UN's *Human Development Report*.<sup>4</sup>

Seventh, there is now widespread agreement that the underlying causes of intrastate conflicts are to be found in the interrelationships between development, security and governance, and that understanding these interrelationships requires an interdisciplinary approach. This, incidentally, is yet another reason why realism, an essentially uncausal theoretical paradigm, is inadequate for the task of explaining today's most common wars. No serious analyst believes that civil wars have a single cause. However, divisions of labour within both the research and policy communities mean that interdisciplinarity and interdepartmental collaboration have been easier to advocate than to achieve.

Integrating political, sociological and economic theories of armed conflict is not easy, not least because the assumptions that underpin different disciplines are sometimes incommensurate. Achieving interdepartmental collaboration within governments and international organizations is fraught

<sup>2</sup> Uppsala's Department of Peace and Conflict Research and the International Peace Research Institute, Oslo (PRIO) collaborated to produce the *Armed Conflict 1946–2001: A New Dataset*, which extends the Uppsala dataset backwards to 1946. See Gleditsch et al. (2001, 2002).

<sup>3</sup> There is a good reason for this. Researchers can be far more confident that a conflict has crossed a particular threshold – 25 or 1,000 deaths – than they can be in estimating total numbers of war deaths.

<sup>4</sup> The *Human Security Report* will provide an annual mapping of the incidence and severity of armed conflicts and criminal violence around the world and policy responses to it – from preventive diplomacy to structural prevention strategies. It will examine some of the socio-economic consequences of this violence and review research findings on its causes. The first issue of the *Report*, which is being produced by the Centre for Human Security at the University of British Columbia, is due to be published in 2003.

with greater difficulties. This is due in part to 'turf' disputes, but also to the fact that bureaucratic departments that have a responsibility for security rarely know much about development and governance, while those with the responsibility for the latter do not normally think about them in security terms.

This pattern is beginning to change. The World Bank certainly recognizes the importance of an interdisciplinary approach and, among governments, Britain's Department for International Development (DfID) leads the field in its push to ensure that defence, foreign ministry and development departments work more effectively together to promote prevention. DfID has also sponsored the Conflict, Security and Development Group at King's College in London that is associated with the new interdisciplinary (but non-quantitative) *Journal of Conflict, Security and Development*. As the mission statement of the group notes,

Traditional policy thinking and research have failed to connect the areas of conflict, security and development and much of the related academic work has tended to be subject specific (such as human rights, gender, peacekeeping and micro-disarmament) . . . However, there is growing recognition of the need for a holistic approach if development assistance is to have a long-term, meaningful impact on the lives of the world's poorest people.<sup>5</sup>

Eighth, there are major disagreements among leading scholars about the causes of civil wars. If at some stage policymakers become more interested in seeking policy advice from quantitative researchers, these disagreements will pose a major dilemma – namely, which findings and related explanations and prescriptions to accept. Some of these disagreements are considered in more depth below.

## Consensus – and Contradiction

There is considerable consensus on research findings within the new literature, as well as major disagreements.<sup>6</sup>

Some of the findings are both striking and have obvious policy implications. Fearon & Laitin (2002a: 3), for example, note that the steady increase in the numbers of armed conflicts around the world from the end of WWII until at least the early 1990s arose because more civil wars (2.3) start on average each year than end (1.7). This suggests that the international community ought to be putting at the very least as many resources into ending wars as preventing them. Currently, prevention gets the lion's share of resources – political as well as economic.

In a recent survey of the literature on the causes of civil war, Gates (2002) notes that there is a 'consensus of sorts' among researchers working in the field that the following factors increase the risks of armed conflict:

- poverty, lack of economic opportunities, and a low level of economic development;
- a previous history of armed conflict – the more recent the conflict, the greater the risk;
- the dominance of one ethnic community over another; and
- political instability (Gates, 2002: 9).

But there are also a number of striking disagreements. Perhaps the most controversial relates to the relationship – or absence thereof – between democracy and civil war.<sup>7</sup> Both Fearon & Laitin (2002b) and Collier & Hoeffler (2001) argue that, once income is controlled for, whether or not a country is

<sup>6</sup> For further examples, see Gates (2002) and Sambanis (2002b).

<sup>7</sup> There is little dissent from the proposition that, in the international system, inclusive democracies almost never go to war against each other.

<sup>5</sup> See <http://csdg.kcl.ac.uk/Profile/html/profile.html>.

democratic has no impact on its propensity to become embroiled in a civil war.<sup>8</sup>

For many political scientists, this finding has been troubling – democracy is, after all, a form of nonviolent conflict management. All things being equal, political scientists would therefore expect inclusive democracies to have a lower level of conflict than less democratic states. The exhaustive research of Rudolph Rummel, over a much longer time period than most of the current quantitative research, would appear to bear this out. Rummel states flatly that ‘Democracies have, by far, the least internal violence.’<sup>9</sup> Moreover, two of Collier’s colleagues, Elbadawi & Sambanis (2000), have argued that not only does political liberalization reduce the risk of war, but it may be the single most effective strategy for achieving this end. In this context, it is worth noting that *in every region of the developing world* the incidence of civil war declined quite dramatically from the early to the late 1990s while levels of democratization increased. This pattern was even true in sub-Saharan Africa.<sup>10</sup>

Hegre et al. (2001) argue that there is a parabolic relationship between degrees of democracy and the incidence of civil war. Repressive authoritarian states and inclusive democracies have relatively low levels of civil violence, although for quite different reasons. ‘Transitional’ or ‘middling’ states with political institutions halfway between repressive autocracy and inclusive democracy have the highest level of violence. This finding is

borne out by a number of other studies, some of which also support the claim that winner-take-all, majoritarian democracies that do not protect minority rights and interests are more violence-prone than inclusive democracies (Reynal-Querol, 2002).

Given such flatly contradictory findings, what lessons are policymakers, who have neither the time nor expertise to make informed judgements in this field, supposed to draw from these studies?

Consider another example. One of Collier & Hoeffler’s most striking research findings is that a typical developing country whose exports are heavily dependent on primary commodities is *twenty times* more likely to experience violent conflict than one that has no primary commodity exports (Collier, 2001: 3). Yet Fearon & Laitin, using ‘a dataset with broader coverage’, found ‘slight or no evidence’ linking commodity exports with the propensity for war (cited in Fearon, 2001).

The policymaker’s dilemma is again obvious. If the Collier & Hoeffler findings are accepted, then economic and export diversification becomes an obvious prevention strategy, and opening commodity markets in the North becomes an important security strategy for the South. If Fearon & Laitin are correct, such strategies will not decrease the probability of war at all.

Part of the problem here is that researchers, not unnaturally, follow their own research agendas rather than seeking to resolve differences with other scholars. But this does not help the policy community – nor does it really help push research agendas forward.

Finally, both Fearon & Laitin and Collier & Hoeffler, as well as most other researchers in this field, concur that GDP per capita and other surrogate variables for modernization/economic development are important predictors of the risk of armed conflict. All other things being equal, as GDP rises, the

<sup>8</sup> Gates suggests that the reason Fearon & Laitin find no relationship between war and democracy is that they count colonial wars involving democracies – e.g. the Algerian war – as civil wars *in* democracies. But even if we believe that this move is justified with respect to the past, it makes little sense with respect to seeking to predict the outcome of future wars, since colonial wars have ceased to exist. Gates also argues that the manner in which Collier & Hoeffler code democracy accounts for their failure to find a relationship supporting the democratic civil peace thesis. See Gates (2002: 14–15).

<sup>9</sup> See <http://www.hawaii.edu/powerkills/DPCLOCK.HTM>.

<sup>10</sup> See graphs in appendices of Elbadawi & Sambanis (2000).

incidence of war falls. Rich countries are relatively peaceful; poor countries suffer the most wars. Development, it seems, is the best form of conflict prevention.

The rising income/declining violence finding can be interpreted in different ways. Fearon & Laitin stress the importance of state capacity for which GDP per capita is a proxy. The greater capacity a state has, the more effectively it can repress rebellion – or buy off grievances. Collier & Hoeffler focus more on insurgents' opportunities for rebellion, which relate in turn to their access to resources. But the central point is that both agree that as incomes rise the propensity for armed conflict decreases.

Yet, these claims confront an apparent paradox. Per capita income has increased just about everywhere in the developing world over the past 50 years, but the number of armed conflicts – at least until the early 1990s – has also increased steadily. Should we not have expected the reverse to be the case, given the importance that both Collier & Hoeffler and Fearon & Laitin attach to rising incomes as an antidote to war?

Looked at from a policymaker's perspective, the Fearon & Laitin and Collier & Hoeffler findings are puzzling. Their thesis appears to be borne out by the cross-sectional data, but is apparently contradicted by the time-series data.

There is no *necessary* contradiction here, of course. Economic growth could well have the violence-inhibiting effect that has been claimed, but the effect may be too weak to offset other factors that *increase* the risk of violence. As Collier notes (2001: 3), 'We cannot rely on global growth to remedy the problem of civil war. Over the last forty years, despite unprecedented global growth, the incidence of civil war has been rising, not falling.'

What, then, caused the 40-odd years of rising violence? Collier & Hoeffler suggest that the main economic risk factor with

respect to the onset of war is reliance on primary commodity exports. But, as noted above, their finding that there is a very strong relationship between primary commodity reliance and war is contradicted by Fearon & Laitin.

Other researchers take a middle position, suggesting that there is a curvilinear relationship between economic development and the propensity for war. Economic growth generates political instability and an increased risk of war in very poor economies, but decreases the risk of war in richer economies (Gates, 2002: 10).

Macartan Humphreys makes a similar point, noting that new wars have been taking place in poorer and poorer countries. In 1969, peaceful countries were on average about twice as rich as those at war; by 1999, the peaceful countries were three times richer. Countries that grew had fewer conflicts; those that did worst economically had a higher propensity than before to be engaged in war.<sup>11</sup>

### The Rejection of Grievance as an Explanation of Civil War

Many political scientists have been uncomfortable with the rejection of grievance as a determinant of violent conflict by most of the econometricians working in this area.

The idea that grievances cause wars fits with our commonsense understanding of the world. But, as Fearon & Laitin have pointed out, while discourses of grievance are certainly present in societies embroiled in civil wars, they are also present in societies that are not. Intense grievance, they suggest, may be an effect, rather than a cause, of war.

Accepting the finding that grievance is not a causal factor requires us to accept the validity of the proxy variables that are used

<sup>11</sup> Macartan Humphreys, personal communication with the author.

to measure it. But it is not at all clear that objective proxies (such as income inequality) in fact capture either the nature or impact of emotions that may impel people to act violently. It is worth noting that none of the emotions – rage, humiliation or despair, as well as felt grievances – that may affect the propensity of people to resort to violence are directly measured in the econometric literature.

The possibility that factors which cannot be adequately measured may nevertheless have an important causal impact should not be dismissed too lightly. Indeed, by no means do all quantitative researchers reject grievance as an explanation (Gurr, 1993).

This is a critically important issue for policymakers. If grievances have nothing to do with the onset of war, then seeking to assuage them via preventive diplomacy, conflict resolution and confidence-building strategies will do nothing to reduce the risk of armed conflicts. If Collier & Hoeffler and Fearon & Laitin are correct, and what counts is not grievance but the relative capabilities of rebels versus the state, then strategies of ‘peace through strength’, repression and deterrence would appear to be optimal prevention strategies.

### **Viewing Development Policy Through ‘the Conflict Prevention Lens’**

As noted at the beginning of this article, the injunction to view development policy ‘through the conflict prevention lens’ has now become conventional wisdom among donor states, the World Bank, the UN and the OECD’s Development Assistance Committee.

But, as Macartan Humphreys has observed, it is not clear why many of the prescriptions that have emerged from this literature should be of much interest to policymakers running development

assistance programmes in donor states. This is not because the prescriptions themselves are unimportant, quite the contrary. But economic growth, export diversification, democratization and other policies prescribed by conflict researchers are *already* being actively pursued as part of the development agenda and have been for many years.<sup>12</sup>

This is a powerful point, although it may still be politically useful for development ministers to be able to make a security case for more development funding. In some cases, however, conflict research findings *do* suggest that traditional approaches to development need to be changed if the risk of war is to be reduced. Frances Stewart, for example, argues that what she calls ‘horizontal inequality’, by which she means unequal *group* access to economic, political and social resources, is a major cause of political instability and armed conflict. She further argues that donor policies that are intended to promote economic growth may in some cases exacerbate ‘horizontal inequality’ and thus increase the risks of armed conflict. She argues for a range of interventionary ‘affirmative action’ policies to reduce ‘horizontal escalation’. Some of these are at odds with current development policy orthodoxy (Stewart, 2001).<sup>13</sup>

But the development community is not the only, perhaps not even the most important, constituency for the prescriptions of the conflict research community. The donor states of the North have clear *national* security interests, as well as long-term economic and political interests, in promoting security in the South. For defence communities in the North, the idea that security would be enhanced by what are traditionally seen as development policies, rather than by new weapons systems and more effective

<sup>12</sup> Macartan Humphreys, personal communication with the author.

<sup>13</sup> Stewart’s work is part of a major WIDER research project on the causes of complex humanitarian emergencies.

military training, may seem rather odd. In fact, it makes a great deal of sense.

When European, Canadian, Australian and New Zealand forces deploy outside their own borders these days, they are far more likely to be part of a peacekeeping mission than a conventional war-fighting operation. In the peacekeeping operations where states of the North do not deploy forces, they usually underwrite the costs of deployment for developing countries that do. Either way, the North confronts real costs – human, economic and political – when wars erupt in the South. When wars end, the North pays much of the economic cost of humanitarian aid and post-conflict peacebuilding operations.

There are other costs associated with inaction. In Africa, appalling poverty and social dislocation, which are both cause and effect of armed conflict, have provided a major impetus for illegal migration into Europe. Immigration has in turn become a volatile political issue in some European countries, one that has been linked by opportunistic rightist demagogues to unemployment, violent crime and, in some cases, international terrorism.

It is for these reasons that the security of countries of the South has become a national political/security interest for the North. The major powers continue to spend the lion's share of the \$800 billion that the world's states allocate to their defence budgets each year. Hundreds of billions of dollars are still being spent on major weapons systems intended to fight the sort of war that has effectively become obsolete in the industrialized North. Meanwhile, little more than \$50 billion gets spent each year on development aid – much of which contributes nothing to security in the recipient states.

In the North as well as the South, security would arguably be enhanced if rather less was spent on new weapons systems in the former, and rather more on development and

security policies designed to benefit the latter.

September 11 has made the idea of using aid to address the security and development problems of the South more compelling, even in the United States. In February 2002, the US Senate passed a bipartisan resolution that noted that 'poverty, hunger, political uncertainty, and social instability are the principal causes of violence and conflict around the world' and that 'the United States should lead coordinated international efforts to provide increased financial assistance to countries with impoverished and disadvantaged populations that are the breeding grounds for terrorism'.<sup>14</sup> Trent Lott was one of the signatories. In April, President Bush proposed a huge increase in US foreign aid, which, if approved, will reverse years of declining aid budgets. This initiative, noted one commentator, 'has only two parallels in modern U.S. history: John Kennedy's Alliance for Progress and Harry Truman's Marshall Plan' (Carothers, 2002).

## Conclusion

To build more a more fruitful relationship between the research and policy communities, the following initiatives could be helpful:

- Creating – or anointing – a conflict dataset that commands consensus in the research community. Policymakers lack the expertise to choose between different datasets. The PRIO/Uppsala dataset with the addition of data on intercommunal conflicts, absolute numbers of battle-related deaths and genocides/massacres would be an obvious candidate, though not the only one.<sup>15</sup>
- Securing more resources for data collection. The need in this area is huge. The

<sup>14</sup> See <http://www.usembassyjakarta.org/terrorism/senate204.html>.

<sup>15</sup> See Sambanis (2002a) for a wide-ranging discussion of this issue.

existing armed conflict datasets are based primarily on media reports, their accuracy varies inversely with the severity of the conflict, and they cannot be disaggregated on the basis of age or gender. Comparison with data collection in other areas is sobering. Tens of thousands (if not more) officials are involved in the global collection and collation of health, education and economic data. Under 100 are involved in the regular collection of data on armed conflicts.

- Convening a workshop to see if the disagreements between the different findings that the new econometric research has generated can be resolved, and to determine what findings command a robust enough consensus to be presented with confidence to the policy community.
- Indicating the limitations of macro-quantitative studies and emphasizing that their contribution is different from, but complementary to, that of country/area specialists.
- Ensuring that policymakers understand that one or more counter-examples do not constitute refutations of probabilistic theories.
- Ensuring that the findings of quantitative studies are presented in a form comprehensible to non-specialists. Few policymakers know what coefficients mean, so results should be stated in the form: 'a 20% increase in A is associated with a 5% to 25% rise in B at the 90% or 95% confidence level'. Appropriate caveats about uncertainty should always be included.
- Ensuring that any publications intended for the policy community include executive summaries and are written in clear, jargon-free language.
- Holding more regular meetings between the policy and research communities to address areas of common concern – and incomprehension.

- Making the case in the North that assisting equitable development policies in the South is not simply altruism, but in the national security interest of donor states.

The new research on the causes of civil war, associated in large part, but by no means exclusively, with the World Bank's Economics of Civil Wars, Crime and Violence project, is welcome for its thought-provoking findings, willingness to challenge conventional wisdoms, and stress on the importance of economic agendas in understanding civil wars.

But until some of the main issues noted above are addressed, policymakers will remain reluctant to embrace findings and prescriptions which they either do not understand or which appear to contradict one another.

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