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CONFLICTS
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Conflicts

The Security Challenge in Conflict-Prone Countries

Paul Collier, Lisa Chauvet and Haavard Hegre

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Overview

Definition of the Challenge:

Large-scale violent conflict takes several forms and our focus is far from comprehensive. Recent media attention has been dominated by Iraq and it is important to acknowledge at the outset that this type of situation is not covered in our analysis. Iraq is in many respects highly atypical of modern conflict. It began as an international war, yet over time international conflict has tended to become far less common. Most warfare in low-income countries is internal. The situation in Iraq has indeed evolved into what is currently probably best described as an ongoing civil war. While our focus is indeed civil war, the interventions that we evaluate here are designed to prevent rather than arrest such wars. In the first challenge paper on conflict for the Copenhagen Consensus, Collier and Hoeffler (2004) also chose to focus on civil wars. However, within this remit they were more ambitious than the present paper, including the ‘deep prevention’ of civil war, the ending of on-going conflicts, and the prevention of the recurrence of violence in post-conflict situations. In this paper our ambition is more limited. We focus predominantly on the prevention of the recurrence of violence in post-conflict societies. This restricted focus enables us to consider post-conflict instruments in more depth, and it is also the core of the problem of violent conflict. Around half of all civil wars are post-conflict relapses. From this core we range a little more widely, taking in the prevention of coups d’etats and some limited ‘deep prevention’.

Benefits

Civil war is often horrific. Unlike the American Civil War of the nineteenth century, modern civil wars in low-income countries overwhelmingly affect the civilian population. They target the most vulnerable members of societies that are already the most impoverished on earth. Their consequences are often highly persistent: child soldiers who have been taught to kill are not only appalling victims, they are a menace to their society for many years to come. It is inevitably difficult to place a meaningful value on the avoidance of such phenomena.

The benefits of a reduction in the global incidence of civil war are common to all successful deployments of instruments for conflict reduction. They accrue at three levels: national, regional and global. The benefits at the national level are partly economic and partly social. As in Collier and Hoeffler (2004) we build a lower-bound estimate based on the effects of civil war on economic growth. The mortality effects are more difficult, but we follow their estimate in terms of DALYs. Where we depart most radically from that previous study is in allowing for the possibility of a much wider range of costs. These are by their nature far less quantifiable than the direct consequences for GDP and mortality, but they are likely to be large. We therefore work with both the economic costs

considered by Collier and Hoeffler, treating this as a lower-bound, and a more speculative figure which we suggest may nevertheless better illustrate the centre-of-the-range of likely full costs. We hope that subsequent research, perhaps drawing on pertinent analogies with other catastrophic phenomena, will begin to place better bounds on these effects.

Stabilizing post-conflict situations

Within the challenge of reducing the global incidence of civil war, we focus on post-conflict situations. As noted above, post-conflict relapses into renewed violence account for around half of all global civil wars, and so they provide an opportunity for highly focused interventions. Further, since 2000 there have been many settlements of civil wars, some of which look fragile and would probably already have relapsed into violence but for international intervention, examples being the Democratic Republic of Congo, and Timor Leste, where as of 2008 a thousand Australian troops have been sent in response to violent unrest.

Discouraging coups

A second opportunity is the drastic reduction of coups d'états. Coups have been getting less common but they still threaten many governments of low-income countries. Unfortunately, as we show, democracy does not provide protection against them so that the rapid spread of democracy in recent years has actually increased the danger that democratically elected governments will be overthrown by their own militaries. While coups do not have anything like the high costs of civil wars, they are usually undesirable and we consider feasible ways in which, in the weakest societies, they could be discouraged.

Instruments

The major innovation of the present study is to focus on international military interventions as instruments for conflict reduction. Given the experience in Iraq this is inevitably controversial. However, as we argue below, for precisely this reason it is important to have a dispassionate assessment of the instrument. Iraq is likely to be an extremely poor guide to the utility of military instruments. To benchmark the military instruments we compare them to the post-conflict aid. Such aid is most probably the most effective use of aid *for conflict reduction*, though of course not necessarily the most effective use of aid overall. It is also politically far less controversial than military interventions. Hence, if post-conflict aid is as cost-effective in conflict reduction as the military interventions then the latter are redundant.

Benchmark Instrument: Post-conflict aid

Since Collier and Hoeffler (2004) much new research has enabled a better-grounded estimate of the benefits of post-conflict aid for reducing the risks of conflict recurrence. We show that there is indeed now a good case that such aid significantly and substantially reduces the risk of further violence. However, once we subject the instrument to a cost-benefit analysis its overall performance is good but not spectacular. Aid is unfortunately quite expensive relative to what it achieves unless huge values are placed upon the maintenance of peace.

We then use this benchmark intervention to compare three much more politically controversial instruments of military intervention.

International peacekeeping

United Nations peacekeeping interventions have increased enormously since the end of the Cold War and are now a massive claim on both money and manpower. Our study is the first attempt to provide a cost-benefit analysis of their deployment and considering the scale and controversy surrounding this deployment of resources such an analysis is surely overdue. As with most first attempts, our estimates need to be treated with due caution. However, our figures suggest that international peacekeeping is highly cost-effective in securing peace.

Over-the-horizon security guarantees

An important variant on international peacekeeping is the strategy of over-the-horizon guarantees. This is the strategy currently being adopted in Sierra Leone where an in-country military force of only 80 international troops is supported by a credible logistical commitment to fly troops in should they be needed. A similar, though less explicit arrangement appears to be in place between Timor Leste and Australia.

Caps on Military Spending

Our third military-related instrument is for a donor cap on military spending by governments of post-conflict societies. While this is evidently controversial, we show that because aid inadvertently leaks into such military spending, at present the lack of a cap inflates spending. More importantly, we show that in the post-conflict context military spending sharply increases the risk of further conflict so that discouraging it not only restores aid to its intended uses but directly reduces the risk of conflict. Such limits on spending are an example of a wider family of interventions aimed at reducing armaments in conflict-prone regions, another such instrument being limits on the arms trade. While there is indeed now some evidence that cheap guns increase the risk of violence in these societies, there is less evidence that instruments that try to limit

armaments such as trade embargoes are actually effective. We have selected the spending cap as being the instrument which may be the most straightforward to implement effectively.

I. Introduction

The need for security from political violence is fundamental to human society. The great archaeological legacies of antiquity, such as the Great Wall of China, and the massive barrier constructed by the ancient Jutes against the Germanic tribes, stand as an enduring testimony to the overwhelming priority afforded to defence. This priority continued until very recently: for forty years the richest society on earth, America, devoted 10% of its national income to defence spending to meet the security threat from the Soviet Union. However, with the collapse of the Soviet Union an era is over. Political violence has not passed into history, but it now happens ‘elsewhere’. Rich countries no longer fight each other, and they no longer fight themselves. Among the middle-income countries war has virtually disappeared. Even the big poor countries are now pretty safe: China and India have massive armies, but they haven’t used them against each other for over forty years.

But some places are still dangerous. Usually, the violence is internal: the country tears itself apart while the rest of the world watches. Sometimes the violence draws others in, mostly the neighbours, and sometimes the local regional power. Occasionally the international powers intervene: to prevent internal mayhem, as in the Democratic Republic of the Congo, to expel an invader, as in Iraq 1, or to force regime change, as in Iraq 2. The uncomfortable fact is that a large group of impoverished little countries are *structurally* dangerous. Quite where the violence erupts is usually unpredictable, but its incidence is predictable. Just as the security problem for the previous generation was the containment of the Soviet Union, so for our generation the problem is the curtailment of violence in these societies. We begin with a review of global trends in armed conflict, based on recent research at the International Peace Research Institute of Oslo (PRIO) and the Uppsala Conflict Data Program, Uppsala University.

Global Trends in Armed Conflict

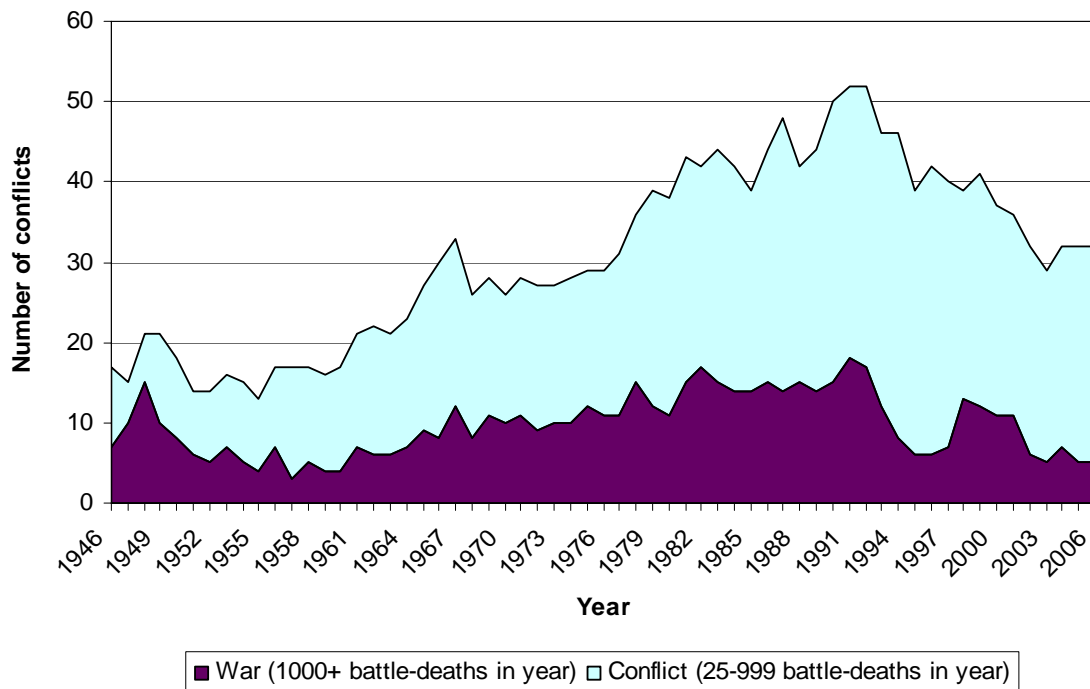
We rely upon the Uppsala Conflict Data Program (UCDP) definition of an armed conflict:

a contested incompatibility that concerns government and/or territory over which the use of armed force between the military forces of two parties, of which at least one is the government of a state, has resulted in at least 25 battle-related deaths each year.

As noted in the overview, intrastate (civil) conflict is the most common form of armed conflict, and this has been so since the end of the Second World War. Despite the current prominence of Iraq, interstate wars (fought between at least two countries) have been relatively rare events. Until 1991 the number of armed conflicts trended upward but since then there has been a general trend towards peace. Correspondingly, the number of very large wars has diminished, but there are now more small wars. However, ‘small’ may be

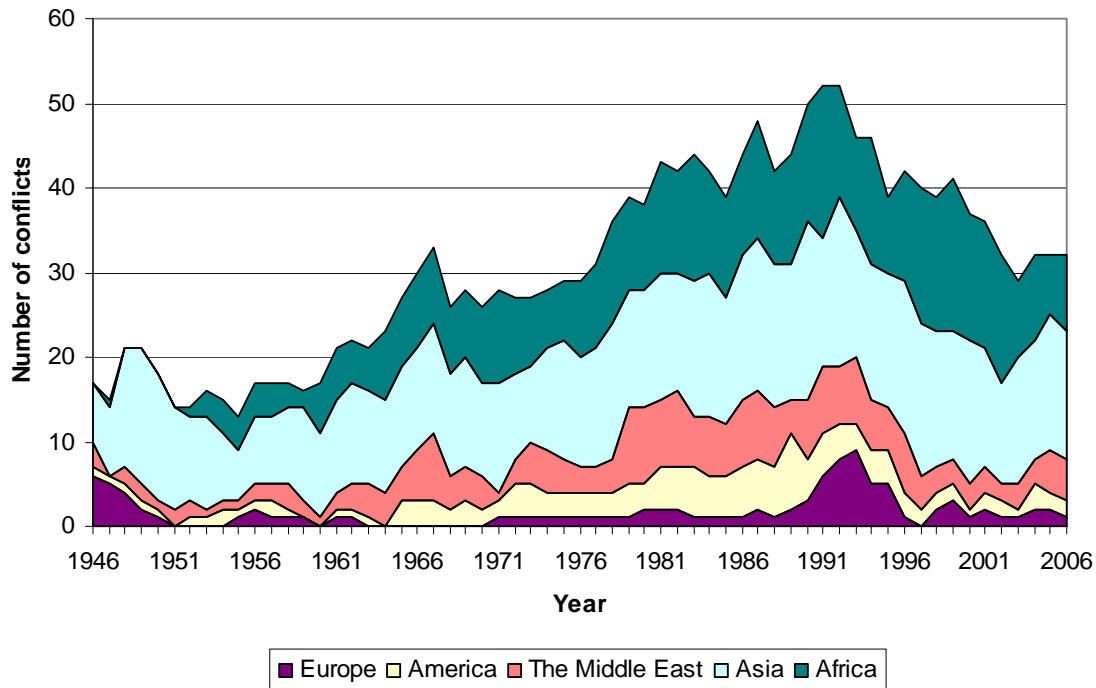
deceptive. The measure used is *battle-related deaths*. However, the increasing involvement of civilians, and indeed the blurring of the distinction between civilians and combatants, implies that even ‘small’ wars measured in battle deaths can have highly adverse consequences for the societies in which they occur.

Armed conflicts by intensity, 1946-2006



Source: Gleditsch et al., 2002; Harbom & Wallensteen, 2007.

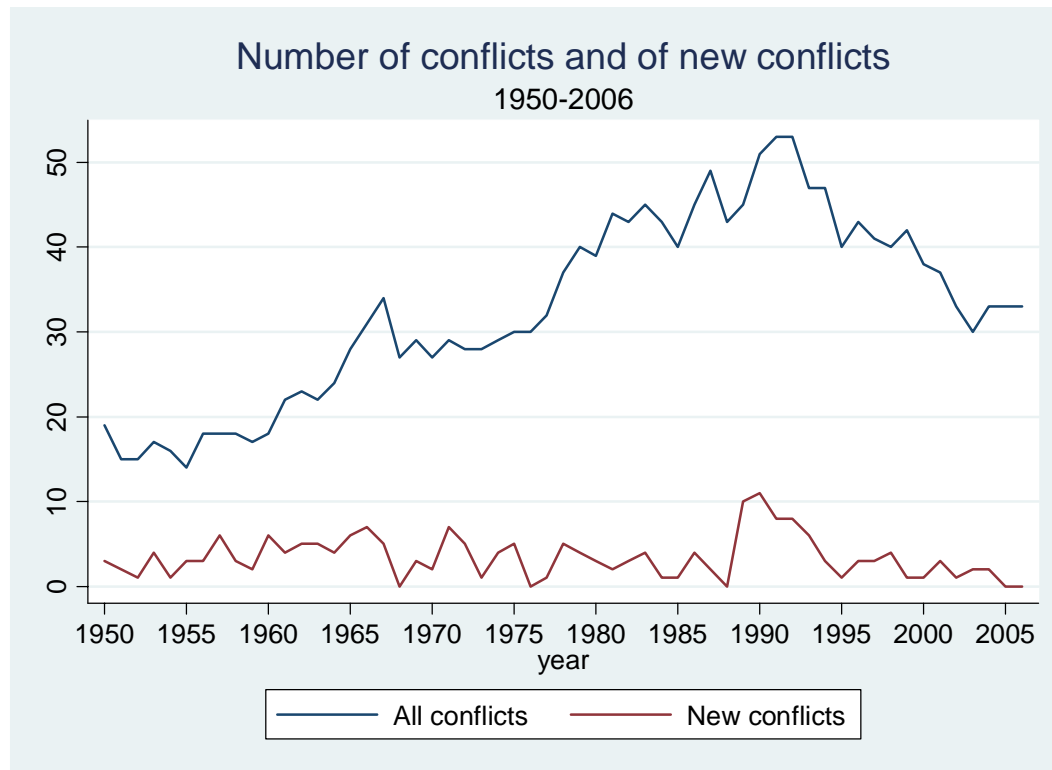
Armed conflicts by region, 1946-2006



Source: Gleditsch et al., 2002; Harbom & Wallensteen, 2007.

The geographic distribution of conflict has also changed. There has been a substantial decline in the number of conflicts fought in Europe and the Americas. Conflict in the Middle East has been relatively stable. In recent years most conflicts have been fought in Asia and Africa. Indeed, most conflicts are geographically grouped: two ellipses, one stretching from Turkey to the Philippines and the other in the Great Lakes and Horn of Africa, illustrate the transnational nature of civil conflict.

While the total number of active conflicts has been fairly constant in recent years, this does not imply that new conflicts have not broken out: new conflicts have replaced the ones that have ended. Nine of the 32 conflicts active in 2005 had been inactive in the previous year, and in 2006 there were a further four new active conflicts. All of these new conflicts in 2005 and 2006 were conflict relapses, further emphasizing the importance of our focus on post-conflict situations.



Source: Gleditsch et al., 2002; Harbom & Wallensteen, 2007.

The increase in the number of conflicts up to 1992 was not due to an increase in new conflicts, but rather to a gradual accumulation: few conflicts ended. It is this that has changed since 1992. The main reason for the encouraging reversal of trend is a considerable increase in successful termination of conflicts. It is thus timely to concentrate, as is done in this paper, on interventions which attempt to prevent these many recent terminations from reigniting.

The Consequences of Violent Internal Conflict

One contribution of this paper is to attempt to measure the cost of violent internal conflict. Since we focus on the prevention of repeat conflicts we consider the typical cost in a small, low-income country that has already had a war. Even if we include only the direct loss of income and ignore mortality and morbidity effects, the cost is around \$43bn. Including mortality and morbidity the costs rise to close to \$60bn. However, these estimates, though large, grossly understate the true cost because they make no allowance for five important considerations. These omissions reflect limitations both of our own work and of the economic methodologies currently available. It is therefore important that we should state them right at the outset of our study.

The first omission is that the people affected by violent internal conflict tend disproportionately to be among the poorest and most disadvantaged people in the world. This is because low income is itself a risk factor in political violence. Modern economic methodology indeed allows such considerations to be included in a global comparative analysis: in effect, a dollar added to the income of a person on very low income should be valued more highly than a dollar added to the income of a person at a higher level of income. Conventional economic estimates of the diminishing utility of income, derived from observed behaviour towards risk, suggest that the required adjustment is large. However, it may be best made once different proposals are compared: in effect, the income level of the beneficiaries of each proposal should be taken into account. The income level of the beneficiaries of the maintenance of post-conflict peace is, however, likely to be lower than that from other proposals. Thus, comparisons based on the implicit assumption that they have the same income as the beneficiaries of other proposals should be revised accordingly. The income differential between the typical citizen in the countries of the ‘bottom billion’ and a typical citizen of the other developing countries is already around one-to-five (Collier, 2007).¹ Even within the ‘bottom billion’ there is a wide range of incomes with those countries that have recently been in conflict grouped right at the bottom. Further, interventions need to be guided not just by current poverty but by reasonable estimates of future poverty (Wood, 2006). For example, an intervention which this year permanently alleviates the poverty of one thousand people in China is simply less valuable than an intervention with the same effect in the Democratic Republic of Congo, not because Chinese people should be less valued than the Congolese but because the prospects for the Chinese are manifestly more promising. As Wood argues, in some sense our interventions should have the objective of alleviating future misery, appropriately discounted. This makes successful interventions in environments that otherwise offer little hope much more valuable than those in societies that are already succeeding. He shows formally that within the Collier and Dollar (2002) analytic framework for optimal aid allocation, these dynamic considerations have large consequences: far more aid should go towards the slowest-growing countries. Since 1980 the income of the typical citizen of the bottom billion has been diverging from the typical citizen of the next four billion by around 5% per year. As we show below, slow growth is itself a significant risk factor in violent conflict, so that the most violence-prone countries are systematically among the slowest growing.

The second omission is that because peace is fundamental to development, its absence frustrates all other potential interventions. For example, the vaccination of children or the reliable provision of anti-retroviral drugs is virtually impossible in wartime conditions. Sometimes this creates ‘weakest link’ problems in the provision of global public goods. For example, smallpox was eliminated globally in a country-by-country campaign which was evidently a race against time: until it is eliminated everywhere there is a risk that it will break back out as a global disease. The last country on earth where it was eliminated was Somalia during the 1970s. Had it not been eliminated in Somalia before the society collapsed into civil war, smallpox would still be a global disease. From 1993 to 2007 the

absence of security in Somalia made similar global health advances infeasible. The maintenance of peace is thus a logically prior investment which opens the possibility of all the other interventions. Conceptually, within the formal analysis of modern economics, the opened possibilities should be thought of as having an ‘option value’. The concept of an option value was developed in financial economics to show that the true return on liquid assets was greater than merely the interest earned on them because they enabled other investment opportunities to be taken as they arose, but the idea generalizes to any action that is necessary for other possibilities. Hence, the discovery of other promising uses for global resources is more likely to enhance the case for investment in post-conflict peace than to compete with it: few interventions are not dependent upon peace.

The third omission is that the costs of civil war are concentrated within a society. The ‘methodological individualism’ inherent in economic analysis essentially treats the costs as being the same whether the losers are spatially and socially dispersed across a continent, or constitute a distinct political and social unit. Yet in some sense the collapse of a society is far more disturbing than were the same individuals to experience the same suffering as isolated individuals in other societies. In effect, a well-functioning nation and its society generate public goods both for its members and for the world that are disrupted if it implodes into sustained internal violence. A useful thought experiment that reveals this idea is to ask what would be lost if all the citizens of, say, Ghana, were to move to the USA and Europe. Each individual ex-Ghanaian might be happier yet the world might reasonably consider that it had lost something of value. Just as species of animals and plants are now recognized to have an ‘existence value’ so do societies. The convulsions that have occurred in Somalia and Rwanda may well have gone beyond destroying the ‘existence value’ of their societies. These societies were not erased from global consciousness, which is the counterfactual implied by existence value, but rather they became nightmares. An analogy here is the consequences of terrorism. The direct damage done by terrorist attacks is manifestly trivial relative to the resources devoted to countering them. Yet the deployment of large resources to counter-terrorist measures need not be mistaken if a value is assigned to the horror and revulsion which people who are neither victims nor potential victims feel once an attack occurs.

The fourth omission is that the costs of internal conflict are highly persistent. This persistence raises two issues for quantification, just how long do the adverse effects last, and how should the future be valued relative to the present? We base our cost estimates on the duration of a typical civil war being seven years and the recovery back to normal taking around fourteen years. While these figures are based on evidence rather than being assumptions, we have deliberately been conservative in adopting figures at the lower end of the reasonable range. Even the duration of internal conflict is surprisingly difficult to estimate and one distinguished study puts it at fifteen years as opposed to the seven we use here. Similarly, a new study on the path of exports after civil war finds that even after a quarter of a century, following a large war there is still no sign of recovery from the

collapse that occurs during the war. It has also been shown that the collapse in health conditions that occurs during war is highly persistent. The valuation of the future relative to the present is standard in economics, by means of discounting. However, as the Stern Report has recently argued in connection with the costs of climate change, the valuation of the future becomes more problematic when the effects are inter-generational and their magnitude large. Stern argues that in such circumstances it is appropriate to discount the future less heavily than is appropriate for short-horizon, marginal investments. Both of these qualifications apply to the costs of violent internal conflict. The degree to which the after-effects of a civil war are discounted is important for an overall measure of costs because some of the costs are two decades or more into the future. In our estimates for this study we have discounted future effects at 3% and 6% per year, but these may both be higher than is warranted once the effects highlighted by Stern are considered. Hence, both due to the potential for highly persistent costs, and to the case for a less severe discount on the future, we are likely to have under-estimated the cost of violence.

Our final omission is of three global spill-over effects: crime, disease and terrorism. Large-scale political violence and the resulting breakdown of the state create territories that have a comparative advantage in international criminality. They provide safe havens both for criminals themselves and for their material activities such as the storage of illegal commodities, notably drugs. Some 95% of hard drug production is concentrated in civil war or post-conflict environments. They socialize young men into violence and provide powerful 'push' incentives for them to emigrate to societies in which they are then liable to be predatory. We omit these costs from our core quantification partly because the link between violence and criminality is not one-to-one: some peaceful societies can nevertheless be quasi-criminal states, as for example Guinea-Bissau. A second reason is that the costs are highly speculative because the counterfactual is unclear: to what extent would criminality merely shift and to what extent would it be curtailed. The third reason is that the cost of international criminality is enormous, so that any reasonable number would be both speculative and large. Civil wars also create the conditions for the spread of disease: the breakdown in public health systems and the mass movement of refugees. Some of this spread of disease affects neighbours, and potentially it can also affect the entire world. One of the explanations for the origin of AIDS for which there is some evidence is that it originated during an African civil war. Again, we exclude the risk of such a pandemic from our core estimate of costs for the same reasons as crime: there is again no one-to-one connection, and since the damage of a pandemic is enormous our results would be dominated by a highly speculative number. Finally, civil wars appear to assist terrorism. Al Qaida based its training camps in Afghanistan because the absence of a recognized government was convenient. Similarly, the American government finally decided that leaving Somalia without a recognized government was too dangerous, once evidence built up that Al Qaida was relocating there. We exclude the risk that civil war will assist international terrorism: there is no one-to-one link, and we would again have a huge but speculative number driving our calculations.

Should all five of these factors simply be ignored when estimating the benefits of peace? It seems to us unreasonable to do so. If on average each factor implied that the true costs were double the number generated by neglecting it, then cumulatively the five would imply a 32-fold increase in the valuation of the cost of conflict! If the average adjustment was 50% it would imply a 7.5-fold increase, and if it were only 20% a 2.5-fold increase. Can we make any judgment on the magnitude of the likely adjustment? The first omission, that conflict affected people are the poorest and least hopeful people on earth, surely warrants a large adjustment. Both the evidence from estimates of typical utility functions and the simulations of Wood (2006) suggest that the required adjustment is more likely to involve doubling than alterations by a few percentage points. For the other factors there is even less guidance, but doubling would seem to be unwarranted. Were we, for example, to allow a 20% adjustment for each of the other four omissions, and to double our estimate as an adjustment to the first omission, the overall adjustment would be a 4.2-fold increase, implying a cost of the typical civil war of the order of \$250bn.

Evidently, the leap from the estimate of \$60bn to that of \$250bn is speculative. We therefore leave it to the reader to judge between them. The \$60bn estimate essentially treats a prolonged civil war as broadly analogous to an economic recession with some added illness. In our view the \$250bn figure better approximates to how ordinary people view civil war. It is, for example, more commensurate with the scale of resources the governments of developed countries are periodically willing to deploy to attempt to change political regimes for the better. In our subsequent analysis we will use the figure of \$60bn as being the likely lower bound, and that of \$250bn as an illustrative figure which is probably closer to the centre of the range.

International Policy Options: Abstention versus Intervention

What, if anything, should be done internationally to address this security problem? Some very able scholars argue that within these societies autonomous processes will correct it, so that external strategies are unnecessary (Weinstein, 2005). Others argue to the same effect that external interventions are illegitimate, ineffective, or even counter-productive. However, benign neglect can itself backfire. When Somalia collapsed into anarchy in 1993 it was allowed to remain without a government for fourteen years. Only once there was evidence that Al Qaida had moved in to the resulting safe haven was international action organized to impose a new government. Perhaps, if Somalia had been left on its own for a century, it would have developed a viable government without external assistance. But, perhaps not: little very recognizable as a national government had emerged in mainland Africa in the centuries prior to external intervention. Small, impoverished societies tend to be structurally insecure: insecurity is a trap from which it is difficult to escape without assistance from beyond the society, although the intervention may come from neighbours, as in the end it did with Somalia, rather than from the developed world.

There are two reasons why external security intervention should be afforded a high priority: compassion and self-interest. The argument from compassion is that the insecure societies are the poorest and most desperate environments on earth, and that the provision of security is fundamental both to personal wellbeing and to economic development. Without security a society can hardly get started. The benefits of security provision are thus both particularly well-targeted on the most needy societies, and have good prospects of being geared up by making other interventions feasible. The argument from compassion is complemented by the argument from self-interest. Insecurity spills over across borders. As we show, most of the costs of insecurity accrue to neighbours rather than to the society directly effected, and so neighbourhoods have both an interest and a right to be involved. And some of the costs spill over to rich societies. Broken societies are havens for illegality, whether this is trafficking in drugs or training of terrorists. The argument from self-interest is compatible with the argument from compassion: most commonly enlightened self-interest is a vital spur to effective solutions. However, on its own, the self-interest of the rich societies risks distorting our perception of effective solutions. The high profile of the terror risk facing rich countries has tended to crowd out the rather different security challenges facing the failing societies. A discourse on international security that does not address these challenges is one-sided and less likely to gain acceptance. In fact, the security challenge facing rich countries is in large part a side-effect of the much larger security challenge facing the failing societies. A sustained strategy of strengthening these societies against structural insecurity is likely to be more worthwhile than that of neglect interspersed with invasions if ‘they’ appear to threaten ‘us’.

Although the core of this paper focuses on a few specific interventions we should stress that we do not see these as the only means of enhancing security in these societies. Rather, they are one part of a wide array of desirable interventions. Each gains both effectiveness and acceptability by being part of the whole. Precisely because the military interventions highlighted here are inevitably going to be regarded as controversial, we wish to make clear that we are not advocating military-only approaches. What would a comprehensive strategy of addressing structural insecurity look like? We can get some insights from the successful sustained security strategy of countering the threat posed by the Soviet Union. That threat was taken seriously and infused a wide array of policies. Not only was 10% of US income devoted to security for over forty years. The pre-war policy of isolationism was torn up: America created NATO, the system of mutual security guarantees, and placed over one hundred thousand troops in Europe. The serious response went well beyond security measures: America reversed its protectionist trade policy, integrating a fragile post-war Europe into its own powerhouse economy by dismantling trade barriers. Nor did getting serious stop with trade: America launched a giant aid program to Europe: Marshall Aid. American responses to the security challenge even extended to how its allies governed themselves. No viable strategy was neglected. Even with this massive response it took over forty years of sustained effort. Is the challenge facing our generation greater or less than that? It is clearly less stark: The

Democratic Republic of the Congo is not pointing missiles at Washington. The amorphous nature of the threat is closer to the security challenge at the end of the First World War. As Macmillan (2006) notes, this was one reason why in 1919 the peacemakers failed to resolve the core security challenge. The amorphous nature of the current security risk has led to wild swings in Western response. Somalia was an instance of total neglect whereas Iraq 2 is at the opposite end of the spectrum: pre-emptive total intervention. The lesson of how America saw off the threat from the Soviet Union is that for challenges of this scale to be ‘winnable’ the major nations of the international community need to apply a consistent set of policies for a long time. Security and compassion are not rivals here, they can coalesce into a sense of common purpose.

The Quantitative Analysis of Security

This paper does not provide a comprehensive menu of policies that might effectively address structural insecurity, but it provides a quantitative analysis of some salient possibilities. Quantitative analysis is vital in order to move discussion beyond political posturing. Inevitably, issues of security are difficult to get into a quantitative framework because they are about averting relatively low-probability events. Take the Cold War. Our instinct is to say that the immense expenditures on countering the threat posed by the Soviet Union proved to be well-justified, but that sense depends upon a counterfactual: what would have happened without this expenditure, and in particular what probability to place upon the more dire among the possible scenarios. Compared to the security expenditure during the Cold War, the security expenditures considered in this paper are far more amenable to quantification. This is because dire scenarios are not hypothetical: the forms of insecurity of concern to failing states occur with sufficient frequency to be analyzable as statistical phenomena. We are thus able to quantify the factors which make societies more or less prone to these forms of political violence. Some of the factors that make a society prone to violence do not directly provide fruitful opportunities for policy intervention. For example, societies in countries with a lot of mountainous terrain face a much higher risk of rebellion, most probably because such terrain facilitates the organization of rebellion. Yet levelling mountains is not a sensible strategy. However, other risk factors are more amenable to policy and these provide the foundation for estimating the costs and benefits of interventions that marginally reduce risks.

Marginal analysis may not seem very exciting, but it is the essence of policy analysis. Security is all about risks, and so the pay-offs to effective interventions are indeed going to be measured as reductions in risks. These changes in risk will then be multiplied by the consequences of counterfactual scenarios that become more and less likely. Analytically, our approach will be equivalent to the way the costs and benefits of any expenditure on risk-reduction, such as safety belts in vehicles. The benefits of fitting the belt are the reduced chance of severe injury times some cost in terms of loss in the quality of life should such an injury occur. These benefits then have to be weighed against the cost. Such calculations, involving both the estimation of changes in risk and the valuation of

counterfactual scenarios, are a routine part of cost-benefit analysis. The application of this approach to the structural insecurity of failing states involves no departure of principle, but it indisputably gives rise to a culture shock among both political scientists and economists. Political scientists, who are familiar with thinking about the issues of failing states, often grow uncomfortable when faced with quantification. There is, of course, good reason for concern that quantification necessarily sets aside the myriad of factors that cannot be quantified. Economists, for whom quantification is entirely normal, often grow uncomfortable for a different reason: failing states are a phenomenon for which data are poor and so they worry that there is insufficient basis for quantitative analysis. Both of these concerns are reasonable. A cost-benefit analysis of security strategies should be seen as one approach that can help to supplement the way in which decisions are made rather than being the only basis for those decisions. Nevertheless, it can hardly be claimed that the record of decisions on insecurity interventions in failing states is so impressive that not such supplement is warranted. Purely qualitative analysis has its own catastrophic pitfalls. The precise numbers generated in this paper are fragile, but where possible we indicate the likely range.

The key security challenges facing poor countries are civil wars and coups. Currently, the governments that face these risks respond to them by military spending. Both the risks and the response are highly costly. If there are cost-effective international interventions that would substantially reduce the risks of wars and coups, and reduce military spending, the payoff to poor countries would be enormous. Yet even among the international interventions designed to help such countries, security has received less policy attention than the ‘photogenic’ topics like health and education. International security interventions, though numerous and expensive, have not been guided by cost-benefit analysis. Especially with the establishment in September 2005 of a permanent *UN Peacebuilding Commission*, there is a real opportunity for more informed and coherent international action. There is the potential to build on recent advances in the quantitative study of security issues in poor countries as exemplified by the contributions to the new *Handbook of Defence Economics*, (Hartley and Sandler, 2007).

We first estimate the costs of these phenomena in poor countries and then investigate four possible ways of ameliorating them: increasing aid in post-conflict situations; imposing limits on the military spending of post-conflict governments; expanding peacekeeping forces; and guaranteeing security from ‘over-the-horizon’. The results presented here are new rather than a repetition of the previous challenge paper for the *Copenhagen Consensus*, although they build upon that work (Collier and Hoeffler, 2004). Since that paper was written there have been substantial advances in the quantitative analysis of conflict, and of the efficacy of potential interventions. Specifically, there have been quantitative analyses of how military spending, aid, coups, the risk of war and peacekeeping forces are interlinked using more recent data. These new studies provide the foundations for estimates of the costs and benefits of the four proposed interventions. Since these are the first such estimates they have a dual function. Most directly, they can

inform policy: even ballpark figures of costs and benefits provide guidance as to the absolute and relative merits of interventions that are already sometimes deployed. Beyond this direct effect, we hope that our estimates open up a new area for future researchers so that over time this further research will reveal the credible range of answers.

II: The scourges: civil wars and coups

Structural insecurity has various manifestations and we do not attempt to address them all. The major forms of insecurity are international wars, such as that between Ethiopia and Eritrea; civil wars, such as that within Sudan; pogroms perpetrated by the government against its own civilians, such as happened in Rwanda; community-level violence between different ethnic, religious or political groups, such as recently occurred in Northern Nigeria; and coups d'états, such as occurred in Cote d'Ivoire. All these forms occur from time to time in the countries of 'the bottom billion', while having receded into history in the developed societies. We will focus only on civil wars and coups. There are two reasons for this restriction of focus. First, each of these five phenomena is quite distinct and requires a separate analysis. It is inappropriate as far as quantitative and policy analysis are concerned either to amalgamate them into some amorphous aggregate of 'insecurity', or to assume that what is true of one category will extend to the others. Our research has concentrated upon civil wars and coups, and it would require major further research to extend to the other forms of violence. Second, while all these forms are periodically of importance, civil wars are unambiguously the most costly: they are highly disruptive and last a long time. Hence, this is the place to start. Coups are potentially of importance far beyond their direct disruption, because they either usher in or prolong military rule. Hence, the costs of coups are, at least in part, the obverse of the gains from democracy. Not all coups oust democratic governments, and some even lead to democracy. But on the whole, coups empower the military over civilian government. The fear of a coup can also generate costs: as we will see, governments sometimes increase military spending in order to pre-empt a coup attempt. Thus, coups are costly and the quantification of these various costs poses a challenging array of problems.

Civil wars

For the purposes of undertaking a cost-benefit analysis of strategies to reduce the incidence of civil war, we need two components. One is the cost of the typical civil war, and the other is a quantitative analysis of the factors that cause civil war.

The costs of civil war

The full consequences of civil war are so various that it is not practical to quantify them all. We therefore focus on the salient costs while considering whether those factors we omit are likely to be so important as to nullify our analysis.

The most readily measurable costs are the economic damage done to the country during the war. Even this depends upon some hypothesized counterfactual of how the country's economy would have evolved in the absence of war. Such counterfactuals are generated from multi-country growth regressions. The approach is to introduce into such standard growth regressions a dummy variable which picks up the typical effect of civil war. This

was the approach taken in Collier (1998), who estimated that civil war typically reduced a country's growth rate by around 2.2 percentage points for the duration of the conflict. Since that study there have been various other estimates. One genuine difficulty is whether to attribute some of the costs not to the war itself but to the damaging economic policies that governments commonly adopt during war: while these policies are usual in wartime they can also occur during peace and so potentially the pure costs of war could be seen as distinct from the costs of bad policies. Collier (1998) treats the costs of war-related policies as part of the costs of war and that will be our approach here. The figure of 2.2% remains around the centre of the range of estimates. Note that it implies that in most countries even during a civil war the economy keeps growing, so that simple before-and-after comparisons would radically under-state the costs. Since the typical civil war lasts around seven years, by its end the economy is typically around 15% poorer than it would have been with peace.

Evidently, the next issue is what happens once the war is over. If the economy were to persist in being 15% poorer than it would have been then almost all the costs of war would occur after it was over. In fact, the typical post-conflict economy gradually recovers to its pre-war growth path. The recovery has been estimated by Collier and Hoeffler (2004b) and by Elbadawi et al (forthcoming). As a rule of thumb, the recovery takes about twice as long as the war itself: destruction is more rapid than reconstruction. Hence, for the typical economy, a civil war shrinks the economy for seven years by 2.2% per year relative to counterfactual, followed by a recovery at around 1.1% for fourteen years. New research on more detailed aspects of post-conflict recovery broadly supports this pattern. For example, Martin and Mayer (forthcoming) investigates the recovery in the share of exports in GDP which unsurprisingly collapses during civil war. They find that for the majority of civil wars, namely those where combat-related deaths are moderate, it takes around 18 years to get back to normal. Similarly, Adam et al. (2008) investigate the recovery in the demand for domestic currency which also declines during civil war as governments resort to desperate measures of financing which undermines confidence. They find that typically it takes up to two decades fully to restore confidence in the currency. This pattern of economic collapse during war followed by slow recovery creates a V-shaped path for income relative to counterfactual. If both this path and the counterfactual path are depicted graphically, the difference between them is a triangle showing the losses due to the war. By discounting the area of this triangle we arrive at our first component of the cost of civil war: the NPV of the cost of a war to the country affected.

However, countries have neighbours and in the modern global economy national economies are sufficiently interconnected that wars are likely to create spill-over effects. Potentially, these effects could be positive. For example, the recent troubles in Zimbabwe have shifted tourist business for the Victoria Falls to the Zambian side of the river. However, the more likely effects are negative. We have estimated the effects to neighbours and find that they are indeed predominantly negative (Chauvet, Collier and

Hoeffler, 2006). The approach we used was again standard, although care has to be taken to distinguish those neighbourhood effects which have nothing to do with war from war itself. For example, a neighbourhood might be affected in common by a drought as in Southern Africa during the mid-1990s. We find, unsurprisingly, that the costs to any particular neighbour are considerably less than the costs to the country itself. Typically, a country might lose around 0.9 percentage points off its growth rate if one of its neighbours is at war. However, the typical civil war country has three or more neighbours and further the economies of the neighbouring countries are usually larger than that of the civil war country itself. This is because, as we will see, being small and poor are both risk factors. In our subsequent analysis we include only costs to immediate neighbours. However, this omits demonstrated adverse spill-over effects across a wider area. Murdoch and Sandler (2002) show that the adverse effects on growth permeate the entire sub-region, not just neighbours, perhaps due to reputation and trade effects. Even with the restriction to immediate neighbours, the numbers imply that the costs to the neighbours as a group are likely to be even larger than the costs to the country at war. This has wide-ranging implications. Evidently, if most of the costs of a civil war accrue to neighbours then they have a strong interest in peace and may be seen as having legitimate rights in actions that encourage both conflict avoidance and the settlement of wars. Untrammelled national sovereignty may not be appropriate in these conditions.

While a rethinking of sovereignty evidently lies beyond the scope of the present paper, the issue bears sufficiently on international strategies for limiting civil war that it warrants some discussion. The delineation of national sovereignty limits the set of strategies that are potentially available. Our key proposal, which is for a package of international interventions, rests on a view of what concept of sovereignty would be appropriate, and this in turn is based on the research reported here. Historically, the entire concept of national sovereignty arose out of the Thirty Years War, largely because of a perception that whatever wrongs a government perpetrated on its own population they had little effect upon the wellbeing of other countries. At the time the concept was developed in the seventeenth century there were reasonable grounds for such a proposition: economies and societies were not highly integrated. Nowadays, however, at least in the case of a civil war in a small country with many neighbours, the externalities are too large to be dismissed in this way. The United Nations has recently formulated a proposition known as the 'responsibility to protect' which in effect suggests that the international community has the right to intervene to protect the citizens from their own government. Compared with that proposition, the notion that the neighbours of a country at risk of civil war have a right to protect *their own citizens* is surely modest. If the neighbourhood externalities are approximately as large as the effects on the country itself, which is one of our results, then for sovereignty to be shared by the neighbourhood would simply reflect a standard economic solution to the problem of how externalities should be internalized into the decision process.

While the costs of civil war are large and widespread, are they offset by gains to the society itself after the conflict is over? In particular, are there future political gains? Rebellion is often portrayed by its perpetrators as an investment in political liberation. At least implicitly, the costs are judged to be outweighed by the subsequent political benefits. Some civil wars indeed have such beneficial consequences that they surely outweigh the costs. However, the political legacy of civil war is on average depressing. Measured by political rights, the post-war period is usually worse than the pre-war period. Indeed, one troubling legacy that will particularly concern us is that the country becomes more rather than less prone to further civil war. Thus, wars do not usually resolve political conflicts, but rather intensify them. It is thus hard to view civil wars as on average generating benefits anywhere commensurate with their costs. This may raise the question as to why, in that case, they occur. Are rebellions mistakes? In some cases this indeed seems quite likely. For example, the terrible conflict raging in Western Sudan was triggered by a local rebellion. Regardless of the rights and wrongs of this rebellion, it is unthinkable that it has generated net benefits for the population on whose behalf the rebellion was ostensibly launched. In other cases there may be a substantial divergence between those who launch the war and the mass of the population. Overwhelmingly, the costs of conflict accrue to those who have no influence over the decision to fight, including neighbouring countries and those who are children at the time of the war. It is quite possible that the perpetrators of violence benefit from it, or have reasonable prospects of doing so, even if the overall net costs of conflict are enormous.

We now turn to the construction of an estimate for the cost of a civil war. Our starting point is the loss of growth to the country and its neighbours, as discussed above. Using the estimated reduction in growth of 2.2%, the average duration of conflict of seven years, and the gradual recovery post-conflict of 1.1%, we arrive at a present value of the cost of conflict to the country itself of 105% of one year's GDP, measured at the point of conflict onset. Similarly, the cost to each neighbour is 42% of one year's GDP.

In the study undertaken for the first Copenhagen Consensus on conflict, (Collier and Hoeffler, 2004), these numbers became the underpinnings of the estimate of the cost of conflict. For the present study we have access to better information. In particular, the data on post-conflict countries is greatly improved on that available four years ago. However, although there is now more evidence on the consequences of conflict, the growth effects used in that study continue to look reasonable. Our revision of those estimates is therefore confined to using new data on GDP, and on constructing GDP figures that pertain specifically to post-conflict countries and their neighbours. We thus multiply the percentage losses of GDP by the GDP of the typical post-conflict country at the onset of peace, and the typical sum of the GDP of the neighbours of post-conflict countries. The former cost, which is the cost to the country directly affected, is around \$20bn at PPP prices. The cost to neighbours is in aggregate around \$23bn. It is superficially surprising that the cost to neighbours is of the same order of magnitude as that to the country itself. However, despite the smaller proportionate impact upon their economies, the neighbours

are more numerous, each country having several neighbours, and they are also typically larger and richer. Recall that conflict occurs disproportionately in small economies with very low incomes, so that it is to be expected these differences are systematic.

The combined cost of \$43bn is a little lower than the estimate of Collier and Hoeffler (2004) which was for the expected cost of an initial civil war. The cost of war recurrence in post-conflict settings is lower than that estimate because the economies of post-conflict countries are systematically smaller than average. Collier and Hoeffler then added an estimate for the costs of loss of life and disability-adjusted life years (DALYs). In our estimates we adopt two values for DALYs, namely \$1,000 and \$5,000. With a discount rate of 3%, these yield an additional cost of a civil war of \$13bn and \$33bn respectively. We also estimate the figures using a 6% discount rate, yielding total war costs of \$46bn and \$66 bn.

In addition to the loss of income and loss of life, civil wars divert expenditure into socially unproductive uses, notably military spending. During civil war military spending by the government increases by around 1.8 percentage points of GDP. This is evidently a lower bound since by its nature a civil war also involves military spending on the part of the rebel organization. Since there are usually no data on such spending it is not included. However, to give an indication of its likely scale, one middle-sized rebel group, the Tamil Tigers, are estimated to be spending around \$350m per year, almost exclusively on military activities. The typical civil war lasts for around seven years, so that the increase in military spending during the war amounts to around 10 percentage points of GDP allowing for discounting. Nor is this the end of the excess military spending resulting from civil war. During the post-conflict decade, domestic military spending typically remains high. Although it is reduced from its level during the war the reduction is typically only around fifth of the war-induced increase. Hence, spending at around 1.5% of GDP in excess of normal during this decade is a further cost of around 8%. The total diversion of spending into military purposes resulting from the war is thus around 18% of a year's GDP, even if it is assumed that thereafter spending reverts to its initial level. Given our GDP figure of around \$20bn this cost is around \$3.6bn.

Thus, the total cost of a typical civil war in a low-income country that has already experienced a war is around \$58.6 bn. Over the entire period since 1960 there have been on average around two outbreaks of civil war each year, implying a running total cost of the order of \$123bn per year. To put this in perspective, the costs of civil wars are thus of the same order of magnitude as the global total of development aid. In recent years the number of outbreaks has been lower, but as discussed above, it is probably premature to see this as a new pattern.

Recall from Section I that these quantified costs omit severe important considerations that are less readily quantified but may collectively make the quantified costs merely the tip of an iceberg. We suggested that for illustrative purposes a figure of \$250bn per war

might be closer to full costs and decided to work with both figures, the \$59bn figure being a lower bound.

The causes of civil war

Historians are still arguing about the cause of the First World War, and the typical civil war is likely to be no less complicated. Our aspirations in analysing causality are more modest. We abstract completely from the immediate political antecedents to a civil war: the mistakes made by the government, the charisma of an opposition leader, the inflammatory statements and provocative actions on both sides. These are indeed in some sense the ‘causes’ of the war. Instead, our focus is on the underlying conditions which make countries more or less prone to civil war. In some societies these conditions are so ripe that relatively small errors on the part of leaders ignite large scale violence, whereas in others the conditions make civil war impossible regardless of what individual leaders do. In the last five years several economists and political scientists have applied statistical analysis, usually logit regressions, to establish these underlying risk factors and the results published in reputable, peer-reviewed journals (Hegre, 2001; Fearon and Laitin, 2003; Miguel *et al.*, 2004; Collier and Hoeffler, 2004a; Urdal, 2005). These studies largely agree although around the margins there are, of course, many interesting differences and room for dispute. There are defensible alternative choices that can be made as to statistical method, data sources, the variables to be included, and the definitions of variables. In this paper our analysis is based on the new work of Collier, Hoeffler and Rohner (forthcoming) which is a major update of the Collier and Hoeffler (2004), including a comprehensive coverage of wars and an array of robustness checks.

While in this paper we only need to know about those risk factors that are potentially amenable to policy, it is nevertheless sensible to ground our analysis in a brief overview of the risk factors that this approach generates. This is especially the case since many people presume that they understand the causes of civil war simply from the accumulated accounts of them given in newspapers. Such accounts generally amplify the discourse of ethnic grievance disseminated by the parties to the conflict, each seeking to justify its actions to an international audience. Rebellion is usually justified by its perpetrators in terms both of historic wrongs and current atrocities. These claims often have sufficient basis in fact to be credible, but they fall far short of actually explaining the occurrence of the conflict.

We find that three economic characteristics make a country particularly prone to civil war: a low level of income, a low rate of growth, and a structure of income in which primary commodity exports constitute a substantial, but not overwhelming, proportion. Hence, those countries with all three of these characteristics: low-income, slow-growing, and dependent upon primary commodity exports, are decidedly at risk. In addition to these economic factors, social factors also matter. Societies that have a small population, those with many ethnic divisions, those that are mountainous, and those with a high

proportion of youth, are at risk. 'Small is dangerous' only in the sense that if the same territory is split up into more and more countries the overall risk that somewhere on the territory there will be a civil war goes up sharply. It is not the case that a small society literally has a higher risk than a big society. These are not the only factors that are important. Others that are pertinent for policy will be discussed in the next section.

Collier, Hoeffler and Rohner conclude from their results that the most reasonable interpretation of the risks of civil war is the 'feasibility hypothesis'. This is that in the relatively unusual conditions under which civil war is militarily and financially feasible there is a very high probability that it will occur, although the specific agenda of the rebel group (or often groups) is indeterminate.

The quantitative analysis of proneness to civil war, which can be thought of as a diagnosis, provides some basis for both prognosis and prescription. The latter is the focus of this paper: what international interventions look to have promise as strategies to reduce the incidence of civil war, and how do their likely prospective benefits compared to their costs. This is the subject of Part III where we focus upon a particular set of strategies for reducing conflict, namely international military intervention. Our reason for this choice of focus is primarily that it is currently the policy area where popular opinion is most in need of being informed by fresh evidence. Clearly, the experience of Iraq 2 has massively reduced both the willingness of citizens of developed countries to countenance military intervention in developing countries, and reduced the legitimacy of such interventions in the eyes of the governments of developing countries. In effect, though for different reasons, international opinion resembles its state in the wake of the withdrawal from Somalia: never intervene. Yet our research suggests that it is possible to delineate situations in which military intervention would be beneficial. Our contribution is thus intended in part to rehabilitate the concept of international military intervention from the highly particular circumstances of Iraq 2.

Even so, we wish to emphasize that we see such intervention very much as a 'last resort' solution. To date much greater emphasis has been placed upon the prevention of violent conflict by means of creating the conditions for political legitimacy. In practice this has meant the promotion of democracy. Clearly, if the establishment of democratic forms of government substantially reduces the risk of political violence then it is both a more attractive and a less costly solution than any military-based strategy. However, so attractive is the democratic approach that its efficacy has, perhaps, been *assumed*, or 'derived' from some equally attractive foundation assumptions as to the basis for citizen loyalty to government. Nevertheless, that democracy reduces the incidence of political violence remains, in the end, a proposition the validity of which depends upon empirical evidence. The proposition has recently been tested in Hegre (2003), and Collier and Rohner (2008). They find that systematically, across a wide range of measures of political violence, and across different structural models of the risk of civil war, democracy has an ambiguous effect. In particular, they find that below a threshold level of income democracy significantly *increases* the risk of violence whereas above the

threshold it has the opposite effect. Thus, in low-income countries democracy seems not to be the key solution to political violence, and indeed seems likely to intensify the problem of maintaining peace. Collier and Rohner suggest that the explanation for these results may be that democracy has two opposing effects. Its legitimacy effect, whereby citizens accept the authority of an elected government, may be stronger in societies that are more educated or have other characteristics commonly associated with higher levels of income. Offsetting this, democracy makes government repression more difficult, and while this is in itself an attractive consequence of democracy, it might weaken a strategy of maintaining the peace that governments in low-income countries find particularly effective. This form of security, sometimes referred to as ‘the peace of the zoo’, might nevertheless have some attractions if the alternative is civil war. There is some evidence that it is effective at least against some forms of political violence, and that democracy curtails resort to repression. However, these are not exhaustive explanations. Neither we nor Collier and Rohner mean to imply by these results that democracy is inappropriate for low-income countries. Rather, our argument is that the comfortable belief that the genuinely high security risks facing small, low-income countries with particular social and geographic characteristics can be resolved purely by political means is probably not well-founded. The evidence points to the need for international interventions to address these security concerns.

Even if this is accepted it does not imply that international military intervention is an appropriate form of international assistance. Recent quantitative research suggests that two types of international intervention have promise: economic and military. The key economic interventions are aid, supportive trade policies, and the promulgation of standards and codes pertinent for the specific characteristics of risk-prone countries such as the management of natural resource revenues. These were the main focus of our previous paper for the Copenhagen Consensus and we continue to regard them as important.

Prognosis

While prescription is our goal, prognosis is also pertinent. If other developments in the global economy and polity are in any case gradually reducing the incidence of civil war then the problem may not require the scarce commodity of global cooperation. Unfortunately, three major recent international developments have raised the risk of new outbreaks of civil war.

One development is the commodity booms and the consequent discovery of valuable resources in fragile states. Both the analysis of Collier, Hoeffler and Rohner (CHR) and that of Fearon and Laitin suggest that this will tend to increase risks of violence. A recent example is the extraction of oil from Chad, which came on stream in 2004 and can credibly be linked to both rebellion and coup attempts in the following year. Oil discoveries are proliferating in ‘fragile’ states, because high prices have boosted

exploration and politically difficult territories are the major remaining unexploited areas. For example, in 2007 oil was discovered beneath the lake between Uganda and the Democratic Republic of the Congo, an area that has already suffered substantial violence and where the border is ill-defined. In addition to the consequences of new discoveries, there are also concerns over the long term consequences of high commodity prices for economic development. If there is a 'resource curse' then the damage done to the economy will itself increase the risk of conflict. Collier and Goderis (2007, 2007a)) use a co-integration approach to investigate the short and long run effects of high commodity prices on the growth of commodity exporters. They base their analysis on global data for the period 1970-2003. They find that for the first few years high commodity prices significantly boost the growth of constant price GDP so that income is augmented both directly by terms of trade improvement and indirectly through the growth of output. However, after this initial boost most countries experience sustained and substantial economic contraction: there is indeed a powerful 'resource curse'. Simulating the present commodity booms in Africa they project an eventual decline in constant price GDP relative to counterfactual by around 25%. The decline is not inevitable but is contingent upon initial levels of governance. Governance is a slippery concept to measure, but they use data from the International Country Risk Guide. An advantage of this measure is that it is a long-standing commercial service which, since companies continue to buy it, can be presumed to have some informational content. So measured, a commodity exporter with good governance, such as Norway, completely avoids the resource curse and grows as a result of the boom both in the short term and the long term. The critical level below which governance is not adequate to harness the long term growth potential of commodity rents is unfortunately rather high - around that of Portugal in the mid-1980s. Most of the low-income countries currently experiencing commodity booms have standards of governance well below this level. Both the scale of the current commodity booms and these research results on the resource curse vindicate the proposal made in the previous Copenhagen Consensus paper on conflict to support the *Extractive Industries Transparency Initiative (EITI)*, an international attempt to improve the standards of governance of resource revenues. While the proposal was not prioritized by the panel of judges, the international community subsequently indeed decisively scaled up EITI. In 2007 it turned what had begun as a small NGO campaign into an official international organization headquartered in Oslo. Our concern here, however, is not to revive this former proposal but simply to note the likely consequences of the commodity booms for the global incidence of conflict and hence guide the prioritization of conflict among the other competing claims on international resources.

The second development is the large number of recently negotiated peace settlements. While these are at least in the short term a triumph of international diplomacy, research, both quantitative and qualitative, finds that negotiated settlements have historically faced a high risk of relapse (Walter, 2001; Nilsson, 2008). This implication of the recent spate of international settlements of disputes should, however, be qualified: while a pessimistic prognosis is entirely reasonable on the historical data, an alternative optimistic

interpretation would be that the new international political will to prevent civil war has decisively changed behaviour.

The third development is the proliferation of democracy across low-income states since the 1990s. Most low-income countries are now at least partially democratic. As discussed, Collier and Rohner (2008) find that this seems likely to increase rather than reduce the incidence of civil war, and similar concerns about democracy in low-income countries is much more general in the political science literature. The specific results of Collier and Rohner are consistent with previous research by Hegre, while other political scientists propose that the problem arises because intermediate levels of democracy ('anocracy') are more dangerous than autocracy. Collier, Hoeffler and Soderbom (2008) examine the consequences of elections in post-conflict situations for the continuing maintenance of peace. Their general analysis of post-conflict risks is discussed more fully in a subsequent section, so here we focus purely on this particular result. They find that post-conflict elections have systematic effects on risk, but they are not particularly encouraging. Although in the year prior to the election the risk of reversion to conflict falls sharply, in the year after the election it increases even more sharply, so that the net effect is risk-increasing. Recent research has also investigated whether elections in low-income countries with very poor economic policies and governance accelerate or retard the process of reform (Chauvet and Collier, 2007, 2008). It finds that the overall effects are ambiguous, but that echoing the specifically conflict-related concerns about 'anocracy', semi-democracy retards reform even relative to its slow pace under autocracy. There is also both micro and macro evidence that commodity booms and democracy interact adversely. Vicente (2006) compares Sao Tome and Cap Verde, two similar islands off the West African coast, both formerly Portuguese. The discovery of oil in Sao Tome creates a 'natural experiment'. He shows that following the oil discovery government corruption rapidly increased in Sao Tome relative to that in Cap Verde. Collier and Hoeffler (2006a) investigate the macro-statistical interaction of democracy and natural resource rents using global data. They find that whereas in the absence of resource rents democracy improves economic performance, with large resource rents democracy substantially worsens it. In effect, instead of democracy disciplining the use of resource rents, the rents undermine the functioning of democracy. The critical level of resource rents above which democracy has adverse effects is around 8% of GDP, so that the current resource booms have lifted many low-income resource exporters well above this threshold.

These three major recent developments, the commodity boom, negotiated settlements, and democratization, are between them likely to change the global incidence of conflict. In the short term, the evidence on the ground is that there has been a change for the better, but it would probably be unreasonably optimistic to conclude from this that the problem of conflict was no longer a priority. As we have seen, on the whole the analytic literature suggests that each of these three developments is liable to increase the incidence of conflict rather than reduce it, although their short term effects may be

benign. CHR test whether recent favourable developments mark a significant sea-change in the incidence of global conflict. Their model of civil war investigates the entire period 1965-2004, and they introduce a dummy variable for the sub-period 2000-2004 into their analysis of risk. They find that it is insignificant: their results suggest that the improvements during the sub-period were explicable in terms of the variables included in the model, rather than reflecting some omitted political development. This suggests that without more effective international intervention, internal violent conflict is likely to continue to be a substantial problem for low-income societies in coming decades and thus warrants our concern and attention.

Coups

There have been over 200 coup attempts just in Africa in the past 30 years. Coups continue to plague the region: for example, recent successful coups have occurred in Mauritania (2005) and the Central African Republic (2003), a failed coup led to the present civil war in Cote d'Ivoire, and in 2006 there was another failed coup against the democratic government of Madagascar. The phenomenon has recently become researchable by quantitative techniques thanks to a comprehensive dataset compiled by McGowan (2003).

The costs of coups

Although coups are nothing like as costly as civil wars, they do generate costs in various forms.

One cost is the direct loss of income in the year of the coup due to the political disruption. Collier, Goderis and Hoeffler (2006) investigated these costs through a co-integration analysis of growth using global data from the period 1970-2003. CGH found that a coup significantly and substantially reduced growth in the year following the coup, by around 3 percentage points. It was not feasible to instrument for coups in this analysis and there is some potential risk of endogeneity: an exogenous growth collapse might indeed increase the risk of a coup. However, this effect would need to be very powerful to account for such a large apparent growth loss and the interpretation we here place on the result seems to us to be reasonable. They did not find the effects to be highly persistent: if the dependent variable was changed to growth over a decade a coup during the decade had no significant effect. Since the average coup would occur in the middle of the decade, this gives some guide to the likely evolution of the economy. In the first year following the coup (i.e. year 6) the economy would lose 3% relative to counterfactual, but during years 7-10 it would fully recover this lost growth. Assuming the recovery to be linear and reached in year 10, on average over the four years following a coup GDP would be 1.5% below its counterfactual, implying a cumulative cost of around 6% of annual GDP.

A different cost of a coup is its consequences for military spending. There are two components to this cost. One, investigated by Collier and Hoeffler (2007), is that a successful coup leads to a sharp increase in military spending. This is unsurprising: the coup leaders reward the army for its loyalty by expanding its budget. It is reasonable to think of this increase as a waste of public revenues. The increase is persistent: military regimes tend to spend considerably more on the military than civilian regimes, even controlling for the risks of war and other factors which account for military spending. Were a coup to result in the maximum shift in regime type, from a fully democratic government to a severe autocracy, military spending would be persistently higher by around 2 percentage points of GDP. Coups never result in such sharp swings although that in Burma may have come quite close depending on how the counterfactual evolution of the country is seen. Many coups simply replace one military dictator with another and so a more reasonable assessment of the persistent impact upon military spending is around 0.5 percentage points of GDP. If this loss persists for a decade, which is again within the reasonable range, then the cost after discounting would be around 4 percentage points of GDP. Note that this treats the excess military spending resulting from rule by the military as an indulgence. Since Collier and Hoeffler estimate the effect on military spending controlling for the risk of conflict this is probably reasonable, although we should recognize the possibility that a higher level of military spending than that chosen by a civilian government could conceivably not be a social waste.

The other potential link between military spending and coups arises because where the risk of a coup is high governments try to reduce it by pre-emptively raising military spending. This is investigated in Collier and Hoeffler (2007a) who build a simultaneous model to disentangle the two-way causation between coups and military spending. They find that controlling for endogeneity, a high level of coup risk indeed significantly increases spending. However, the size of the effect is quite small and we omit it from our calculations.

Potentially, the risk of a coup has a chilling effect on economic performance, an effect that would be consistent with the adverse effects of 'political instability' that are commonly found in the growth literature. However, CGH used the estimated coup risks generated by the above model to investigate the possibility and did not find coup risk to be significant in the growth process once the significant adverse effects of actual coups were included.

Like civil wars, coups could potentially be politically beneficial. Evidently, coup leaders routinely justify their actions in such terms. Besley and Kudamatsu (2007) have recently investigated what determines whether an autocratic government performs well or badly and has highlighted the importance of the ability to remove a leader in response to disaffection among the 'selectariat'. While it is indeed possible to find examples of coups which led to improved governance, this does not appear to be the general pattern. Collier and Hoeffler (2007a) find that a coup leads to a significantly higher risk of further coups.

This is hardly surprising, since for the very process which attempts to legitimate a coup inadvertently legitimates a counter-coup. In unpublished work they also find that coups significantly increase the risk of civil war. This was, for example, the sequence in Cote d'Ivoire, until then 'the jewel of Francophone Africa'. On the basis of these statistical results there is some basis for concluding that coups, like civil wars, generally erode the political system. However, for our purposes we merely need to assume that the costs delineated above are not offset by political benefits to the society.

In summary, a successful coup d'etat typically generates economic costs of the order of 10% of one year's GDP. In part this is due to losses in output, and in part due to the diversion of output to useless military spending. For the typical small, coup-prone developing country these costs amount to around \$2bn per successful coup. In Africa over the period 1956-2003 there were 83 successful coups, implying a cost of the order of \$166bn spread of the period, before discounting, or an annual average of around \$4bn. Costs of this order of magnitude are not large, measured on the scale of global concerns.

As with civil wars, before accepting this figure we should consider whether our quantification omits other important costs. The first omitted consideration in our quantification of civil wars was that the citizens of war-prone countries are the poorest and least hopeful on earth. As we will see, this consideration carries over to coups because, like civil wars, they are more likely the lower is income and the slower is growth. In general, the other considerations that apply to civil wars do not carry over to coups: their effects are not usually large and highly persistent, and they do not threaten the existence value of a society. They do, however, often have one further cost: almost by definition a coup ushers in a military government. Sometimes the government that is replaced was itself a military government, but sometimes a coup indeed replaces a democratic regime by the military. As argued by Azam (1994), democracy should surely be seen as having some value over-and-above its consequences for macroeconomic performance. Hence, such coups are intrinsically undesirable. Should the international community attach any negative value to regime change from democratic to military rule? Manifestly, part of the justification for the hugely expensive intervention in Iraq was to induce regime change in the opposite direction and there was also a huge international effort to support the transition to democracy in Eastern Europe. Hence, the international community certainly behaves in a manner consistent with an assignment of some positive value to democracy as opposed to military rule. We thus have two large omitted considerations, the atypically poor circumstances of coup-prone countries, and the non-economic costs generated by the replacement of democracy by military rule. Again, one approach is to ignore these considerations and treat the figure of \$2bn per coup as the lower bound. Alternatively, we can scale up this lower-bound by an adjustment factor. We previously suggested that the order of magnitude adjustment for the fact that citizens of countries prone to civil war were particularly poor was to double the lower bound and these arguments carry over to coups. What, if any, allowance should be made for the intrinsic value of democracy? Again, we propose that a reasonable adjustment is to

double the estimate. This is because the economic costs of a coup are modest so that even doubling does not place a very high intrinsic value on democracy. As an example of how the transition between democracy and military dictatorship is now valued consider Nigeria. When the military regime that had ruled Nigeria for fifteen years was replaced by President Obasanjo the event was treated within Nigeria as a national triumph and globally as a giant step forward for Africa. Allowing for these two adjustments would produce a cost of a typical successful coup at around \$8bn, or an average cost to Africa over the period 1965-2003 of around \$16bn. One way of 'truth-testing' this figure is to pose the direct question 'what would it have been worth, both to Africa itself and to the international community, for Africa to have been free of coups? For us, the figure of \$16bn is at least within the sensible range of answers. A figure double this would imply that being coup-free would have been broadly commensurate with the entire international aid program to Africa which seems excessive, whereas a figure half of it would begin to marginalize the problem that Africa's main form of political change has been military. As with civil wars, we thus have a reasonably hard lower bound figure, namely \$4bn per year, and a highly approximate attempt at a figure more representative of the centre of the range, \$16bn per year. As with civil wars the reader is left with the choice between them.

Even if the cost of coups has been of the order of \$16bn per year, the cost is not large relative to some other global problems. However, they have one potentially interesting feature for the international community. Those governments subject to coup risk naturally consider the threat of a coup to be of vital concern. In Africa, setting aside death in office from natural causes, coups have been by far the most frequent reason for changes of regime, dwarfing the number of changes due to either elections or rebellions. Hence, governments have a strong interest in cooperating with strategies that significantly reduce this risk. We will suggest that this may open up interesting possibilities for packaging international interventions.

Causes of coups

What makes a government particularly prone to a coup d'état. Collier and Hoeffler (2007a) find that low per capita income significantly and substantially increases risks. At the mean of other variables, halving income increases the risk by 35%. This has the important implication that exposure to coups is concentrated among the poorest countries. Growth also affects coup risk, but although the effect is statistically significant it is small. As noted, coups beget further coups. This result is not merely picking up a fixed effect since the heightened coup risk fades with time.

Since coups are political events it would seem likely that they are determined primarily by the political context. In particular, we might expect that autocratic regimes are more prone to coups than democratic regimes. Unfortunately, the data do not bear this out. Collier and Hoeffler search for a significant break point along the range of the Polity IV classification of political regimes, the range being from -10 (severe autocracy) to +10

(impeccable democracy). They find that there is only one such break point, namely at -5, this demarcating highly repressive regimes as opposed to less repressive regimes. Unfortunately, far from the extreme autocracies facing a higher risk of coups, they face a markedly lower risk: repression works. The effect is large, repression halving the risk at the means of other variables. An implication of this is that governments cannot defend themselves from coups simply by democratizing: democratic governments face higher risks.

Prognosis

Again we briefly turn to prognosis. How important are coups likely to be in the future? The higher risk due to democracy suggests that coups may continue to occur. However, unlike civil wars, there is a favourable time trend: coups have gradually been going out of fashion. As noted, they have by no means disappeared. But their gradually declining incidence suggests that there may be a moment when ‘eradication’ is possible, at least at the regional level.

III. Four military-related instruments

To summarize so far, over the past forty years civil wars have been on average costing a lower bound estimate of around \$117bn per year, with a centre-of-the-range illustrative estimate of around \$500bn per year; while the corresponding costs for coups have been around a further \$4bn per year (lower bound) to \$16bn (illustrative centre-of-range). These costs have fallen systematically upon the poorest and slowest growing countries in the world: the bottom billion. The costs considerably exceed the aid flows that these countries have received over this period: even the lower bounds are broadly commensurate with total aid flows, but a considerable proportion of this aid has accrued to countries that are at income levels above this bottom group. The prognosis for coups is encouraging: they are becoming less common, most probably because they are increasingly seen as illegitimate by the international community. However, we should note that this very process implies that the cost of those coups that still occur is greater since the lost democracy is valued more highly. The prognosis for civil wars is more confused: the short term evidence on the ground is encouraging, but three important longer term influences are all adverse.

The case for concern about political violence is threefold. Partly, as the above numbers show, the economic costs are substantial. Second, as our discussion in the Introduction emphasized, the lower bound numbers are probably far below the true costs. In particular, the costs are concentrated upon the poorest people on earth, and without secure peace it is difficult to help them in other ways. Further, global peace is increasingly seen by many people as having an intrinsic value over-and-above its measurable economic benefits. While it is important that such vague sentiments are not allowed to override well-founded quantification, it may suggest that large scale violence has some negative ‘existence

value' that while not infinite is substantial. This was, indeed, to an extent the sentiment of the previous Copenhagen Consensus panel: they described the eradication of violent conflict as being of fundamental value, if only instruments could be found that were demonstrably effective.

We now turn from the objectives to the instruments. In the paper on conflict by Collier and Hoeffler for the first Copenhagen Consensus, the main instruments considered were development aid and the promulgation of international standards for the governance of natural resource revenues. The panel was generally sceptical of the efficacy of aid. The promulgation of governance standards incurred very little cost and its benefits were at that time highly speculative, so that the strategy did not lend itself to the calculation of a cost-benefit ratio.

In the present paper, we have narrowed the focus to two types of violent conflict that we see as preventable, namely the recurrence of civil war in post-conflict situations, and the prevention of coups in countries that are at least partially democratic. We have also narrowed the range of instruments, focusing in particular upon various military strategies. Our focus on military strategies is partly because in one sense these are the most 'natural' strategies with which to counter large-scale political violence. If civil wars and coups are to be discouraged, quite possibly, force needs to be opposed by force. The other reason for our focus on military strategies is, as we have discussed, that the war in Iraq and the immense attention that it has attracted have come to dominate thinking on military intervention. Yet the invasion of Iraq and the subsequent military engagement have both been highly peculiar. Although Iraq is often described as a post-conflict situation, it is not analogous to the post-conflict period following a civil war in a low-income country which is both the core phenomenon addressed in this paper and the most common conflict risk, globally. The 2003 war in Iraq was manifestly not a civil war but an international war. The situation since April 2004 has had combat-related mortality rates arising out of organized internal violence against an incumbent government that are well above the threshold for classification as a civil war, albeit one that is partially internationalized. Hence, citizen opinion as to the likely efficacy of international military intervention has been powerfully shaped by an experience that is irrelevant to the core security problem of small, low-income societies. Whether or not such intervention is sometimes a useful, cost-effective instrument, is a matter for analysis. This paper attempts such analysis. We are not concerned to *advocate* military intervention. However, the need for a dispassionate analysis of its potential efficacy seems to us to be much stronger now than at the planning stage of the first Copenhagen Consensus.

We are going to investigate three international strategies that focus on military provision. One is the attempt to change the size of the domestic military establishment chosen by a government. We will focus particularly on post-conflict governments. The second strategy is to provide military services internationally, generally but not exclusively as

peacekeepers under the auspices of the United Nations. The third strategy is to offer guarantees of external military provision should circumstances require it.

International Curtailment of Domestic Military Spending

The level of military spending chosen by the government of a country facing a risk of a civil war or a coup d'état is sometimes either higher or lower than international actors would prefer. For example, in Uganda during the 1990s the major aid agencies tried to induce the government to reduce its military spending. Conversely, foreign governments sometimes provide either arms or finance to bolster of government's capacity to fight an insurrection, an example being USA military support to the government of Colombia. These attempts at changing the level of spending chosen by a government raise two important questions: is such a change desirable, and if so, is external influence effective? Here we consider the first of these questions. The efficacy of intervention is deferred until Part IV.

Evidently, the case for curtailment or support must be made on a case-by-case basis, but does research give any guide as to the likely effect of military spending by a government on the risk of internal conflict? Since our interest is specifically in post-conflict situations, this is the context on which we focus. Military spending in low-income countries has recently been the subject of some substantial published quantitative studies (Dunne and Perlo-Freeman, 2003; Collier and Hoeffler, 2007). They find that the level of military spending chosen by a government is systematically explicable in terms of the threats that it faces, the domestic pressure groups, and the ability of the government to finance public spending. Collier and Hoeffler are able to show that governments indeed significantly and substantially increase their military spending in response to the risk of civil war, objectively measured. This is consistent with the pattern we discussed above when estimating the cost of a civil war: post-conflict governments usually fail to reduce their military spending back to peacetime levels. Thus, the high level of post-conflict military spending is at least in part a response to the recognition that post-conflict risk of conflict reversion is high.

In effect, post-conflict governments are operating on the hypothesis that high military spending in this situation is risk-reducing. While this is *a priori* plausible, it is no more than a hypothesis. Collier and Hoeffler set out a counter-hypothesis, rooted in a game-theoretic analysis of why in post-conflict situations military spending might have an unintended adverse effect. Their key point is to consider the decision as to whether to revert to violence from the perspective of those outside the post-conflict government. Typically, as part of the peace settlement, this group will have been given various undertakings as to post-conflict policies: in effect, they will have been promised some share in government revenues. Sometimes this will be highly explicit, as in the settlement of the conflict in southern Sudan, at other times it will be largely implicit. In either case, there is a potential time consistency problem. As the peace persists, the capacity of the

rebel party to maintain its military forces gradually declines. The rebels may then reasonably fear that there will come a point at which the government revokes its commitments and uses its now-superior military force to crush its opponents. Fearing this, the rebels may decide to pre-empt this risk by returning to conflict. A recent example of such a sequence is the post-election shoot-out between the forces of the opposition and the government in the DRC in November 2006. In this instance the rebels left the return to violence too late for success and their leader had to go into exile. Thus, the key rebel decision problem is to determine the intentions of the government: will it renege on its commitments? The rebels thus face a ‘screening’ problem of trying to distinguish genuine commitment from lies. In turn, if the government is indeed genuinely committed to maintaining its promises, it faces a problem of how to establish this fact, given that anything it *says* could also be said by a government that was trying to dupe the rebels, and that the environment is one of intense hatreds and suspicions. In other words, the genuinely-committed government faces a ‘signalling problem’: what action can it take that would reveal its type? As in the standard theory of signalling, such a government needs to find an action that, were it not genuinely committed, it would simply refuse to do. Only such an action cannot be imitated and so it reveals the government’s true type. A substantial reduction in government military spending may well be such a signal. It has two advantages. One is that it directly bears upon rebel fears of rule by oppression. The other is that it is difficult to reverse: once the military is scaled down it would take time to rebuild it. Hence, a deep cut in military spending might well signal that the government is committed to maintain an inclusive style of government and so reassure opponents sufficiently that they do not return to violence.

In principle either of these hypotheses could be correct. They are tested in Collier and Hoeffler (2006). The evident problem with such an investigation is that because military spending is endogenous to risk, an apparent relationship from military spending to risk is likely to be spurious. High levels of spending may appear to cause high levels of risk when in fact causality is the other way round. In order to control for endogeneity and establish a clear causal relationship it is therefore necessary to instrument for military spending. Fortunately, because military spending is systematically predictable by a range of variables, it is possible to find valid instruments that strongly influence chosen levels of spending but do not influence the risk of civil war. Once military spending is instrumented, Collier and Hoeffler find that its effect on the risk of conflict is distinctive in post-conflict situations. Their key result is that in post-conflict situations, but only those situations, government military spending is significantly and substantially counter-productive. This is consistent with the prediction of the screening-signalling theory: it is only in the post-conflict situation that the opposition has an established military capacity which is in decline, and some explicit or implicit understanding with the government that helped to conclude the civil war.

If it is correct this is evidently an important result. An implication would be that post-conflict governments are operating on precisely the wrong theory of risk reduction and

inadvertently aggravating rather than reducing it. In turn, it would suggest that international actors would in general be right to discourage spending to the extent that they have the scope to do so and so open the question of the efficacy of such interventions.

To investigate the robustness of the CH2006 results we undertook an entirely fresh analysis, using both an improved methodology and expanded data. Whereas CH2006 relied upon a logit analysis of risk during five-year periods, the subsequent paper on post-conflict risks by Collier, Hoeffler and Soderbom (CHS), (forthcoming), had switched to the more continuous approach of hazard functions. As discussed further below, in our re-analysis of the CHS model for the present study we therefore included domestic military spending, instrumented as in CH2006 as an explanatory variable. This was potentially quite a severe test both due to the change in approach and the expansion of the data set. In fact, domestic military spending, as instrumented, continued to be both statistically significant and adverse, substantially increasing the risk of further conflict.

We now use these results to quantify the costs of high post-conflict military spending by the government. The costs are normally measured simply at face value: that is, if the government spends \$100m extra on the military, this is typically seen by external actors as a waste that has an opportunity cost of \$100m. Our point here is to demonstrate that if both the present results and the CH2006 results are approximately right, then this is a gross under-estimate of their true costs. For the typical post-conflict country an additional \$100m of military spending would increase the military budget by 0.5 percentage points of GDP. This in turn would increase the risk of conflict reversion by 2.56 percentage points. Applying our previous estimates of the costs of a civil war to the post-conflict country and to its neighbours, this additional risk incurs lower-bound costs of \$1.5bn, and centre-of-the-range costs of around \$6.3bn. Thus, the government spending of \$100m generates additional costs of the order of \$6.3bn: the concealed and inadvertent costs are of the order of sixty times the apparent costs.

Thus, in post-conflict situations, domestic military spending has very high and largely inadvertent costs. This creates some basis for international actors to attempt to curtail it to the extent that they have instruments that are efficacious. We will investigate this in Part IV.

While the ‘true’ costs may seem very high, we have actually chosen not to include a further layer of costs which come from neighbourhood ‘arms races’. Collier and Hoeffler (2007) establish that typically one influence upon military spending is the amount spent by neighbours, although the motive for emulation need not be that of a perceived threat. For example, it could simply be that the military in each country uses the spending increases of neighbours as an effective means of lobbying its ministry of finance. The resulting interdependence of military spending generates an arms race multiplier. In effect, the excessive military spending of post-conflict countries induces increases in the

military spending of their neighbours which diverts public spending from productive uses. As long these neighbours are not themselves post-conflict, there is no adverse effect on the risk of conflict: the aggravation of risk is confined to post-conflict situations. Nevertheless, in effect excess military spending by one country is a neighbourhood public bad in that it induces this expenditure diversion.

Expanding the role of peacekeeping forces

International peacekeeping in post-conflict situations is now a major activity with a high political profile both in developed and developing countries. This is precisely the type of policy for which a quantitative analysis can add value to decisions because, in its absence, decisions are liable to be highly politicized. In particular, following the experience of post-conflict peacekeeping in Iraq, in developed countries there is little appetite for sending troops into post-conflict situations, and little belief in their likely success. Conversely, in part of Africa the Department of Peacekeeping Operations (DPKO) of the UN is increasingly seen as the ‘new IMF’, that is, a challenge to untrammelled domestic sovereignty. Yet Iraq may be a very misleading basis for understanding post-conflict peacekeeping. Recall, Iraq was not a peacekeeping operation following a civil war, but rather an international war which, after a brief interval, has triggered an ongoing civil war. Similarly, the notion of untrammelled national sovereignty in the face of high risks of further conflict may need to be challenged in view of the high costs that conflicts inflict upon neighbourhoods.

Peacekeeping interventions have become very much more common since the end of the Cold War. Currently there are 100,000 uniformed personnel serving in UN peacekeeping operations in 16 countries.

Guaranteeing security from ‘over the horizon’

The supply of effective peacekeeping troops is limited. A simple way of economizing on them is to base them in their home countries but to provide ‘over the horizon’ guarantees of rapid intervention should this be necessary. The initial British military intervention in Sierra Leone has evolved over the last seven years into a variant which is potentially a particularly interesting security technology, namely an ‘over-the-horizon’ guarantee. The British government has withdrawn all but a token military force in Sierra Leone, but has made a ten-year commitment to fly troops back into the country should there be any security need. Sierra Leone has continued to be peaceful under this guarantee, but evidently, in any particular instance it is not possible to establish the likely counterfactual: possibly peace would have been maintained even without any commitment.

The British over-the-horizon guarantee reinvents and refines a much older strategy of the French government which until the late-1990s provided a less explicit security safeguard

for the whole of Francophone Africa. For over thirty years, from the early 1960s until the late 1990s, the French government provided security cover to Francophone governments in Africa which, while less than a guarantee, was nevertheless credible because it was backed by a chain of French military bases around the region. The ending of the French policy can be clearly dated. It began with the onset of the genocide in Rwanda. French troops had been stationed in the country to protect the government, and this presence came dangerously close to propping up the regime as it began the genocide, only just being terminated in time to avoid accusations of complicity. Following this, French government policy was rethought, its first important manifestation being the reaction to the coup de'état in Cote d'Ivoire in December 1999. The 'old guard' within the French government advised the president to use French military forces to put down the coup as would previously have been standard. However, the president decided that this would be inconsistent with France's new policy and so decided not to authorize intervention. This then marks the clear end to the French informal security quasi-guarantee.

The subsequent history of Cote d'Ivoire suggests that the French withdrawal of a security guarantee was very costly. The country descended into prolonged civil war which necessitated French military intervention to police a security zone between the warring sides. Paradoxically, the British government reinvention of the policy of a security guarantee occurred shortly after the French had abandoned it.

A priori, an international security guarantee may be either more or less effective than actually maintaining peacekeeping troops on the ground, country-by-country. An over-the-horizon guarantee has two major cost advantages over country-by-country peacekeeping. First, for most of the time most of the troops can be kept in their home country which is far cheaper than maintaining them in-country. For example, the British have only 80 troops actually in Sierra Leone but could fly in a large force overnight and did so during the onset of Operation Palliser. Second, the same force can guarantee security in several different countries. Since these risks are unlikely to be called simultaneously, a pooled central force can provide a far larger potential presence when it is actually needed. This is, incidentally, consistent with the result discussed previously that the risk of civil war within a given geographic area is reduced as the number of countries is reduced: there are security economies of scale. CHS find that it is the absolute scale of the force that matters, as opposed to troops per head of population, and so pooling can actually be more effective in deterring the recurrence of conflict than individual country-by-country forces that cannot be rapidly reinforced because neither the politics nor the logistics have been sorted out in advance. Thus, potentially, an over-the-horizon guarantee might be a considerably superior technology to conventional peacekeeping.

IV: A Cost-Benefit Analysis of Interventions to Reduce the Risk of Civil Wars and Coups

We now consider how military interventions and aid might be deployed in post-conflict situations so as to reduce risk, and how the costs of these interventions compare with the benefits. The analysis is complicated because several distinct interactions must be considered. We begin with aid provided in post-conflict situations. We choose this intervention for three reasons. First, compared to other forms of aid it is far more effective *in reducing the risk of violent conflict*. This qualification is important: aid to post-conflict societies may or may not be more effective in poverty reduction than aid to other low-income societies. In general, it is likely to be the case that aid for the express purposes of poverty reduction is best highly-targeted within a society. Similarly, aid to reduce mortality may best be targeted on activities such as the vaccination of infants. If, however, the objective is to reduce the risk of violent conflict, then post-conflict societies are the most appropriate focus because of their abnormally high risk of conflict. Recall that the typical post-conflict society has a risk of conflict reversion during the first decade of 40%. Other societies generally have risks far less than this. Nor is it possible to make actionable forecasts of differences in risks between such societies: the forecasts themselves would be highly uncertain and in any case could not be publicized to aid agencies. Further, as we will see, aid is more effective in the growth process post-conflict than in other situations. Thus, the case for aid as an instrument for security is at its most powerful in the post-conflict context. Our second reason for focusing on post-conflict aid is that it is the least controversial type of security intervention. In contrast, military intervention in any form is now highly controversial. Because of this, aid for post-conflict situations constitutes a useful benchmark. If it is more cost-effective than military interventions then it dominates them both in economic terms and politically. If it is less cost-effective then potentially there is a trade-off between effectiveness and political acceptability. Our third reason is that, as we will argue, aid can complement other interventions, forming a more effective package.

The Benchmark Intervention: The Instrument of Post-Conflict Aid

Post-conflict aid is the main non-military instrument of maintaining peace available to the international community. It was indeed the initial rationale for aid, being the founding purpose for the World Bank, which was initially going to be called the International Bank for Reconstruction. The potential importance of post-conflict aid is increased by the evidence that political interventions such as democratic elections does not appear to be effective solutions to the post-conflict security challenge.

Whether aid is effective in reducing the risk of conflict reversion depends upon three steps. First, aid should be effective in the post-conflict growth process. Second, growth

should be effective in bringing down post-conflict risks. Third, there should be no offsetting effect of aid that directly increases the risk of conflict.

We previously considered the instrument of post-conflict aid in the first Copenhagen Consensus. We now summarize the results of that work and discuss how those results need to be qualified in the light of more recent work.

Aid and growth post-conflict

The effect of aid on growth has become considerably more contested since the original Collier and Hoeffler paper. In general there is increased skepticism as to the efficacy of aid although there have also been some high-quality research papers that have found evidence that aid-for-growth is effective. A useful ‘meta-study’ by Doucouliagos and Paldam (2006) reviews some seventy underlying studies of the effect of aid and from this the modal study finds small but positive effects. However, few studies focus specifically on aid in the context of post-conflict. There are good reasons to expect that aid might be less problematic in such situations. Unlike the normal aid relationship, there is no serious ‘moral hazard’ problem. That is, no society is likely to be tempted into civil war by the prospect of post-conflict aid: the costs are simply too high relative to the likely returns. Further, the devastation of infrastructure during war opens obvious opportunities for high-return aid investments. For example, in Uganda the restoration of rural roads by World Bank projects shortly after the end of the civil war in the late 1980s is estimated to have had an annual rate of return of 40%.

At the time of the first Copenhagen Consensus the only quantitative study of aid in the post-conflict growth process was that of Collier and Hoeffler, (2004b). The detailed results of that paper were summarized in Appendix 3 of their Copenhagen Consensus chapter and will not be repeated. They found that during the first post-conflict decade growth is typically faster than normal. That is, there is some economic recovery. Indeed, this result has been fundamental to subsequent estimates of the cost of civil war because the recovery limits the post-conflict costs: eventually the economy reverts to the level of economic activity without the conflict. While the result that there is a growth recovery massively reduces the estimated cost of a civil war, it is also the foundation for the evidence that post-conflict aid is effective. Collier and Hoeffler find that the pace of post-conflict growth is determined partly by the quality of economic policies and governance, and partly by the volume of aid. In particular, they find that compared with aid in societies that are not post-conflict, aid is considerably more effective in raising growth.

Although this paper was reputedly published, in view of the considerable skepticism relating to aid and the small sample available for that study it is important to consider whether there is further statistical evidence. Three studies specifically focused on aid and economic performance in post-conflict situations have subsequently been published. Elbadawi et al. (forthcoming), incorporate effects of aid via the real exchange rate and

use a much larger sample of post-conflict countries. Their focus on the real exchange rate reflects recent concerns that aid might inadvertently kill the growth process through generating Dutch disease: destroying the competitiveness of exports. An influential new paper by Rajan and Subramanian (2005) provides some evidence for these concerns. Incorporating these real exchange rate effects, Elbadawi *et al.* indeed find that there is an adverse effect of aid via real appreciation. However, crucially for present purposes, they also find that this effect is much weaker in post-conflict situations so that aid is substantially and significantly more effective than normal. In part this may be because exporting is in any case likely to be limited in post-conflict economies. Typically the recovery of exports is confined to mineral extraction which is not very sensitive to concerns about the real exchange rate. Adam, Collier and Davies (forthcoming) focus on the consequences of conflict for inflationary financing. Unsurprisingly, they find that during civil wars governments resort to money printing and heavy implicit taxation of the financial system. During civil war the typical government has its back to the wall and is desperate for revenue, even from sources that are so damaging in the long term that normally it would not adopt them. Further, these sources of finance may be the only options because, far more than in the case of international war, external sources of borrowing dry up. There is abundant evidence that inflation and severe financial repression are indeed both damaging to long term growth. In effect, during conflict economic policy is in crisis mode. During the post-conflict period governments are faced with hard choices. There are large demands on spending, while tax revenues have been reduced both by economic decline and the retreat of activity into subsistence. They show that in the absence of aid governments resolve this dilemma by persisting with inflationary financing, thereby delaying the recovery of confidence in the currency and the restoration of the financial system. They find that aid is used by governments in post-conflict situations distinctively: it resolves this dilemma and is used by governments to abandon the deficit financing strategy. Finally, Davies (forthcoming) investigates capital flight during and after civil war. Capital flight is a major macroeconomic phenomenon in the countries most prone to conflict. McIndoe (2007) updates the published estimates of capital flight by Collier, Hoeffler and Pattillo (2001) and estimates that as of 2004 around 36% of Africa's private wealth was outside the region. Consistent with other studies, Davies finds that capital flight increases both during civil war and during the post-conflict period. His important contribution is to analyze what determines capital flight during the post-conflict period, and in particular to what extent this is distinctive. He finds that in the post-conflict period capital flight is significantly and substantially more sensitive to inflation than in other superficially similar but peaceful situations. Possibly the explanation for this heightened sensitivity is that asset holders are aware of the risks of conflict reversion and are looking for signals from their government of whether it is continuing to function in crisis mode or is taking a longer term view. The government's choice of the inflation rate is thus a powerful, if inadvertent, signal of whether it is taking a long term view. This result is important for present purposes because it evidently relates closely to that of Adam *et al.* on the distinctively anti-inflationary effects of aid in post-conflict situations. In currently unpublished work, Adam, Collier and Davies combine

these results and show that in the post-conflict context aid is substantially reinforced by the induced reduction in capital flight. This is a further macroeconomic reason why aid might have distinctively powerful growth effects in the post-conflict context.

Hence, in the post-conflict context aid has an important, distinctive and positive macroeconomic role which is not offset by the usual concerns about appreciation of the real exchange rate. While the evidence that aid raises growth in developing countries is generally somewhat weaker now than at the time of the first Copenhagen Consensus, the evidence that *in the post-conflict context* it raises growth has become stronger. Not only are there supporting econometric results, but the distinctive macroeconomic channels by which they might be explained are now much better understood. The coefficient on post-conflict aid estimated in Collier and Hoeffler (2004b) implies that the gain in the growth rate from additional aid worth one percentage point of GDP sustained over the decade would be around 0.5 percentage points, and in the light of subsequent evidence this still seems a reasonable figure.

Growth and risk post-conflict

We now turn to the second step that must hold before aid can be regarded as even potentially cost-effective as an instrument for reducing post-conflict risks. This is that growth must be shown to reduce risks.

At the time of the first Copenhagen Consensus the main evidence for this was the paper on the causes of conflict by Collier and Hoeffler (2004a). This had found general evidence that growth reduced risks, but the endogeneity problem inherent in the relationship was only addressed by means of a five-year lag. Since then an important study specifically on growth and the risk of civil war has been published as the lead article in the *Journal of Political Economy* (Miguel et al., 2004). This uses the ingenious and robust instrument of rainfall shocks for growth, an approach which requires them to confine their analysis to Africa. They show that so instrumented, growth significantly and powerfully reduces risk. The robustness of the general growth-risk relationship is further confirmed in the study by Collier, Hoeffler and Rohner (forthcoming) which, as discussed, is a major update of the Collier and Hoeffler (2004a) analysis, doubling the data set so as to be comprehensive and applying many robustness checks.

The general relationship that faster growth reduces the risk of civil war is thus far more well-founded than at the time of the former study. The next issue is whether there is any evidence relating specifically to the post-conflict context. After all, much of our argument rests on the distinctive features of this period and so it is entirely possible that the growth-risk relationship is different. For example, a pessimistic view would be that the post-conflict situation is typically so fragile that even though growth has the potential to benefit everyone, it might so disturb delicately balanced relationships that it opens up arenas for violent dispute.

At the time of the former Copenhagen Consensus paper there was only one study specifically on risks during the post-conflict decade, namely that of Bigombe et al. (2000). This simply introduced a post-conflict dummy variable into the Collier-Hoeffler (2004a) model, using the same data as their original paper. It found that growth was slightly but significantly more effective in reducing the risk of conflict during the first post-conflict decade than in normal situations. Clearly, this evidence was limited in that its derivation was so closely related to the more general Collier-Hoeffler result on the effect of growth on risk: to the extent that the general result could be doubted, so could this refinement.

Since the former Copenhagen Consensus paper there has been a substantial advance in the analysis of post-conflict risk as a result of the model in Collier, Hoeffler and Soderbom (forthcoming). As discussed further below, this paper takes a completely fresh approach to the data, analyzing 68 post-conflict situations, and uses a methodology much better suited to the question of how post-conflict risk evolves, namely the estimation of hazard functions as opposed to five-year logits. That study found that growth both significantly and substantially reduced the risk of reversion to conflict. As discussed, in work for the present study, we have revised and re-estimated the CHS study, incorporating domestic military spending as an explanatory variable. The addition of this variable does not affect the significance of the growth variable but somewhat increases the size of the coefficient.

In addition to its direct contribution to risk reduction, faster growth cumulates to a higher level of income. Potentially, the level of income affects risk over-and-above its effect on the growth rate. Again, at the time of the former Copenhagen Consensus paper the main statistical evidence for a direct effect of the level of income came from the Collier-Hoeffler model. Since the evident endogeneity of income to conflict was only addressed by means of a lag, there were reasonable lingering concerns that the result was not robust. The proposition that low income causes an increased risk of conflict is now more widely accepted in the literature. It is also supported by the analysis in Collier, Hoeffler and Rohner (forthcoming) which addressed the potential endogeneity of income by instrumenting it using some standard geographic variables. They found that this increased the magnitude of the effect compared to using the lagged income variable. There is also now evidence for the effect of the level of income specifically in post-conflict situations. CHS include the level of income, lagged by two periods, in their analysis of post-conflict risks in addition to the lagged growth rate, and find it to be significant.

Hence, since the previous study for the Copenhagen Consensus, the evidence that growth reduces risk in post-conflict situations, both directly and indirectly via the level of income, has become considerably stronger. General endogeneity problems have been decisively addressed and the evidence specifically pertaining to the post-conflict situation is far more reliable.

CHS provide a useful simulation that gives a sense of the magnitude of the overall effect of growth on risk. They take a post-conflict country otherwise at the mean of characteristics for post-conflict societies and vary the growth rate, cumulating the effects on the level of income. If the society is stagnant the risk of conflict reversion during the decade is 42.1%. If it sustains 10% growth through the decade, which some post-conflict societies such as Mozambique indeed manage to achieve, the risk falls to 26.9%. The reduction in risk comes roughly equally from the direct growth effect and the cumulating effect of a higher level of income. Hence, on average, each additional percentage point of growth brings down the decade risk of reversion to conflict by around 1.5 percentage points.

Finally, we need to consider the possibility that while aid might raise growth, and growth might reduce risk, aid might inadvertently directly increase the risk of conflict thereby nullifying its favorable indirect effect via the growth process. At the time of the first Copenhagen Consensus there was some speculation that aid might have such adverse effects, there being a discussion around the concept that aid should aim to ‘do no harm’. However, the only quantitative evidence was from Collier and Hoeffler (2002) which had introduced aid directly into their model of conflict risk. They found that aid controlling for growth, aid had no significant effect on the risk of conflict. However, this study predated the advance made by Tavares (2003) so that aid was not instrumented. Since aid could be presumed to be allocated with some view of conflict risk, the results were potentially spurious. Since the innovation of Tavares and the first Copenhagen Consensus, in unpublished work Collier and Hoeffler have revisited this question, instrumenting aid, and find the same result: aid does not have a significant effect on the risk of conflict. This result is not specific to post-conflict, but it is evidently a step towards reassurance that a post-conflict aid program would not inadvertently generate effects which undermined the intended benefits via growth.

Costs and Benefits

So far we have shown that there is now quite strong evidence that post-conflict aid is effective in bringing down the risks of conflict recurrence. This does not imply that the intervention is *cost*-effective, but it is a necessary condition for cost-effectiveness. In most cases other possible global interventions are known to be effective, and so the only issue is whether their known benefits justify their costs. In effect, our analysis so far has simply brought post-conflict aid up to this starting line.

We have now assembled the building blocks needed to calculate the costs and benefits of aid to post-conflict societies. First, consider the costs. To achieve the above risk reduction in the risk of conflict recurrence of 1.5 percentage points over the post-conflict decade would require an additional one percentage point of growth sustained over the decade. This in turn could be achieved by aid worth 2 percentage points of GDP sustained for a

decade. For the typical post-conflict society this would cost around \$400m per year, or \$4bn before discounting.

Now consider the benefits. Recall that our lower-bound estimate of the cost of conflict reversion is around \$60bn. At this lower bound the gross gain purely in terms of the reduction in the risk of recurrence of civil war of 1.5 percentage points is of the order of \$900m. Note that although the figure of \$60bn is discounted so as to be a present value in the year that the conflict might start, the reduced risk is spread over the entire decade, in effect being a flow of gains of around \$90m accruing annually through the decade. Hence, for this to be shown as present value at the beginning of the post-conflict decade this flow would itself need to be discounted. It is thus commensurate with the undiscounted figure for costs of \$4bn. Evidently, at this lower bound post-conflict aid is not cost-effective when the only gains that are counted are the security gains. When the lower bound estimate is replaced by the centre-of-the-range illustration of \$250bn, the gain rises to \$4.25bn and so modestly exceeds its costs. This, of course, depends upon estimates of benefits which are speculative. However, we should note that when we have presented the figures on the degree of effectiveness of aid in bringing down risks to the international donor community they are seen as constituting a powerful case for post-conflict aid. In effect, with these responses international donors are revealing a value on peace rather in excess of our illustration of \$250bn and there is no particular basis for seeing such a high valuation as misplaced.

There is, of course, a further reason to value post-conflict aid, namely that it also generates additional income during the decade and so alleviates poverty. The additional one percentage point of growth augments GDP during the decade by \$10bn before discounting and before valuing the benefits beyond that decade. Hence, again using the centre-of-the-range illustration, the gross gains are around \$13.8bn from costs of \$4bn, implying a ratio of benefits to costs of around 3.5:1.

Post-conflict aid thus looks to be a good use for development aid, but not so spectacular that it would trump most other calls on scarce international public resources. To an extent donors are already taking this opportunity. However, although aid is provided in post-conflict settings it is not usually sustained. Over the entire course of the first post-conflict decade aid is no higher than were the society not post-conflict, while during the first couple of years of peace there is a flood of aid. Hence, aid typically tapers out just as it should be tapering in.

Military Intervention Opportunity 1: a conditionality requirement limiting military spending

One concern about aid is that it may inadvertently finance military spending. Donors do not permit their development assistance to be used for this purpose, but there are various ways in which aid might nevertheless have this effect. One is as a result of the well-

understood concept of fungibility: aid given ostensibly for one purpose releases government money that would otherwise have been used for this purpose and the money so released can be spent on anything. Another mechanism is that aid is foreign exchange and so augments the capacity of the economy to import. Typically governments levy substantial taxes on imports and so, indirectly, aid is taxed, the resulting revenue being available for the government to use for any purpose.

Typically, the governments of developing countries choose to spend about 10-20% of their revenues on the military. It would not, therefore, be surprising if through one or other of these routes aid augmented military spending by some such proportion. Collier and Hoeffler (2007) investigated this empirically, by including aid as an explanatory variable in their analysis of the determinants of military spending. To allow for evident problems of endogeneity they instrumented aid, following the approach pioneered by Tavares (2003). This approach utilises the fact that a considerable proportion of aid provided by the bilateral aid agencies is determined partly by the domestic budget cycle within the aid-providing country, and partly by historic ties to particular recipient countries, both of which are independent of the current circumstances of recipients. With aid so instrumented, Collier and Hoeffler indeed find that it significantly increases military spending, the proportion of aid that leaks into military spending being around 11%. Collier and Hoeffler do not analyze whether aid in post-conflict situations leaks by a larger or smaller factor than in other contexts. In what follows we will assume that the rate of leakage is average, namely 11%. This is probably a conservative assumption since we know that in post-conflict situations governments place a much higher priority on military spending than in normal peacetime situations, manifested by military spending being significantly higher.

This raises two issues, one being the true costs of these leakages viewed from the perspective of the donor, and the other being whether any actions on the part of the donor could potentially reduce leakage.

One cost of the leakage is evidently the opportunity cost in terms of the intended uses of aid. If 11% of aid leaks into an activity which is unproductive the all the beneficial effects of aid are attributable to the remaining 89%. Were it possible to prevent leakage, the amount of aid available for beneficial uses would thus be augmented by around 12% ($11/89$). Hence, just through this route, an effective conditionality clause combined with post-conflict aid would raise the benefits of post-conflict aid in the benchmark example from \$13.8bn to \$15.5, implying a ratio of costs to benefits of 1:4.

However, the main cost of the leakage is that in augmenting military spending it is inadvertently generating adverse effects, since as discussed, extra military spending in the post-conflict context is significantly counter-productive. Recall that the adverse effects of military spending in post-conflict situations are between 15 and 60 times its direct cost depending upon whether lower-bound or centre-of-range estimates are taken.

Take a typical post-conflict situation in which GDP is around \$20bn. Then the aid package of two percentage points of GDP considered above would amount to \$400m per year. Thus, an 11% leakage of aid into military spending would augment the military budget by around \$44m. This leakage would generate inadvertent costs in terms of a heightened risk of conflict of between \$660m and \$2.6bn. We should stress that this is not the net effect of post-conflict aid, since this adverse effect is already implicitly included in the net benefits estimated above. Nevertheless, it is a major negative effect. The contribution of aid to peace by means of faster growth is inadvertently partially offset by its tendency to undermine peace by augmenting the domestic military budget which, uniquely in post-conflict situations, aggravates the situation.

Supposing that the leakage could be prevented by effective conditionality, what would the benefits be? Obviously, the benefits of conditionality have to be assessed in combination with the aid itself: the conditionality simply augments the effectiveness of the aid. However, taking the centre-of-the-range estimate, the benefits of the aid would increase from \$15.5bn to around \$18.1bn, thus raising the benefit-to-cost ratio to around 4.5:1.

This raises the evident question as to whether anything can be done to curtail the leakage of aid into military spending. There are various possible approaches, which may be reinforcing.

One is to improve the information available to post-conflict governments on the typical effects of high domestic military spending. Governments may be deluding themselves in believing that it resolves their security problem, or more realistically, ministers of finance who wish to constrain military spending lack persuasive evidence to counter pressures from the large and powerful military lobby that is inevitably resistant to reductions in its budget.

A second approach is for the donors to tighten conditions on the provision of aid. Any conditions must recognize the limits resulting from both the fungibility of project funding and the freedom attached to the alternative aid channel of budget support. It is infeasible to establish how their money is being spent. However, if the concern is that money should not go into military spending, then the donor has the analytically more feasible task of monitoring this part of the budget. In effect, the military spending of the post-conflict government would need to be subject to an explicit cap as a condition for the receipt of post-conflict aid. This in turn is liable to encounter two obstacles, one political and the other technical. The political obstacle is the argument that such a condition would breach the sovereignty of the post-conflict government. This argument can be countered by the reasonable position that the sovereignty of the government does not extend to the right to spend donor money on purposes that flout donor conditions agreed to by the government. The donor cap would be designed to prevent aid being diverted rather than

to induce the government to spend less of its own money on the military. Indeed, if the government did not want to constrain its military spending, it would simply reject the aid. The technical obstacle to a budget cap is that it can be evaded by concealed misclassification of spending items. For example, it eventually came to light that following an attempt by the British aid agency DFID to curtail the military spending of the Ugandan government, some Ugandan military spending had been mis-classified under the education budget. To overcome this technical obstacle would require a verified system of scrutiny of budget data, a function which could potentially be supplied by one of the international financial institutions. Again, governments which wished to avoid budgetary scrutiny could simply decline the aid.

A third approach is to address the security needs of post-conflict governments by effective provision of external peacekeeping, perhaps by means of an over-the-horizon guarantee as in Sierra Leone.

These approaches complement each other and can potentially be packaged together. A commitment to the external provision of security from international actors would balance a commitment from the post-conflict government to limit its own military spending and to permit sufficient scrutiny of its budgetary system to make this commitment credible. These matching commitments reinforce each other: the effectiveness of external peacekeeping as measured by risk reduction would be substantially increased if domestic military spending is reduced. Conversely, the perceived need for military spending would be reduced if there was an international commitment to the maintenance of security. Finally, the credibility of that commitment would be enhanced by the fact that the government had, at the insistence of the international community, renounced its option of self-defence.

However, a package would need some authorizing environment which could orchestrate it. The package would have four components. One would be a commitment on the part of the donors to provide post-conflict aid. A second would be a commitment on the part of the Security Council to external military peacekeeping or an over-the-horizon guarantee. A third would be a commitment by the government to cap its military spending for the post-conflict decade, and to accept an international system of budgetary verification. A fourth would be the provision of such a system of verification. While no single entity is in a position directly to provide this package, there is now an entity which could negotiate and authorize its provision, namely the Peace Building Commission of the United Nations. This reports both to the Security Council and the General Assembly and was established in 2005 precisely because it was recognized that there was a missing link in the international governance architecture.

Whether such a packaged approach would be politically feasible depends in particular upon the willingness of conflict-affected regions to accept it: the combined neighbourhoods of conflict-affected countries constitute a large and influential group.

Given that the main costs of conflict accrue to these neighbourhoods, it would be in their interest to do so. It would also be in the interest of the developed countries that provide the finance both for post-conflict aid and for post-conflict peacekeeping. Their clear interest is that in combination this assistance should be far more successful than in the past.

Military Intervention Opportunity 2: Peacekeeping in Post-Conflict Situations

We now turn to a cost-benefit analysis of international peacekeeping. The first and critical step is evidently to investigate whether peacekeeping is effective in reducing the risk of conflict.

The Effect of Peacekeeping on Risk

At the time of the first Copenhagen Consensus there was no quantitative study available to determine whether peacekeeping is effective in reducing post-conflict risk. Although the chapter by Collier and Hoeffler included peacekeeping as a possible intervention, the analysis was based entirely upon the particular case of Sierra Leone and the contribution of peacekeeping was inferred from the peace in that country that had prevailed while peacekeepers were present, compared with an imputed counterfactual risk of conflict generated from the Collier-Hoeffler (2004a) model. Evidently, this was not a very satisfactory basis for assessing the more general impact of peacekeeping. Since then the analysis of peacekeeping has advanced considerably, with a major evaluation by Doyle and Sambanis (2006) and the CHS model of post-conflict risks which introduces peacekeeping as an explanatory variable.

Our analysis is based upon a re-estimation of the CHS model. That model had a sample of 68 post-conflict situations, this being a comprehensive set of countries with post-conflict experience and adequate data. We should note however, that the very notion of 'post-conflict' is controversial. Civil wars do not always have a clear ending. Some researchers, though not CHS, consider that a civil war has only ended once there has been no fighting for two years, so that early relapses are not even analyzed. CHS then analyze the persistence of peace through a hazard function. Each year is treated separately, and the model focuses on the first ten years after the end of the civil war.

Before turning to the role of military spending, it is useful to get a broader sense of the model through its selection of other explanatory variables and its results. Variables are selected through a process of stepwise deletion of a wide range of variables that have been proposed by the pertinent literatures. Evidently, one such literature is the quantitative studies on the causes of civil war, although those studies use logit analysis as opposed to hazard functions, and do not focus in detail upon post-conflict situations. These studies do, however, show that post-conflict situations are distinctively risky for reasons other than fixed effects. Hence, the high rate of reversion to conflict is not simply

because these countries are inherently at high risk even prior to their initial war (Collier and Hoeffler, 2004a; Collier, Hoeffler and Rohner, forthcoming). The other pertinent strand of literature is the largely qualitative studies of post-conflict situations.

CHS investigate temporal, economic, social and political influences on risk reversion as well as the presence of peacekeeping forces. Their analysis of temporal effects produces the somewhat disappointing result that during the post-conflict decade there is no 'safe period'. It is known from the quantitative studies of general risks of civil war that the risk of reversion to conflict gradually diminishes over time. However, this risk reduction is slow, occurring decade-by-decade rather than year-by-year. During the first decade the risk indeed appears to decline, but the effect is not statistically significant nor is it substantial. The inference is that the entire first decade is at risk of further conflict. Recent delayed reversions to serious violence in Timor Leste and the DRC are consistent with the statistical pattern.

Consistent with the influences on the overall risk of civil war, they find that two economic variables significantly affect the risk of reversion: the level of income and its growth rate. We have already discussed these effects in our analysis of aid. Despite the high profile given to social factors such as ethnic divisions, CHS do not find ethnic or religious structure to be significant in post-conflict risks. The more general analysis of the risk of civil war does find such effects to be significant. However, the lack of a specifically post-conflict effect should make us wary of dismissing peacekeeping and economic interventions as 'missing the essence of the problem'. Just as ethnic and religious divisions between communities are usually treated as the 'explanation' for conflict, the most prominent 'solution' is assumed to be political, namely democracy and elections. Here the results of CHS are perhaps salutatory. They find that in post-conflict societies democracies are considerably more at risk of reversion to conflict than are severe autocracies, and that post-conflict elections appear to shift risk rather than reduce it. Risk falls sharply in the year prior to an election but increases even more in the year following it. Presumably, in the year before an election all the major actors hope to win it, whereas after the election a loser emerges whose best chance of a continuing share in power may then be to revert to conflict. A recent example of just this sequence, which is indeed too recent to be included in the CHS analysis, is the Democratic Republic of the Congo, whose election of October 2006 was followed by large-scale fighting between the government army and that of the losing opponent. In that situation it seems reasonable to attribute the eventual restoration of post-election peace at least in part to the presence of 17,000 international peacekeepers. If so, their presence has had a very substantial pay-off. In contrast, the \$500m spent by the international community on promoting the elections may not have been a particularly good investment in peace.

CHS were able to get comprehensive and detailed financial data from the United Nations of peacekeeping expenditures and so were able to introduce them as an explanatory variable in the analysis. An immediate problem is to address the potential endogeneity of

the provision of peacekeeping. Clearly, there is some decision process that determines whether and how many peacekeepers are sent and for how long. The risk of conflict could impinge on these decisions in either direction: troop provision could be directed to the situations judged to be most at risk, or, if troop providing countries seek to avoid danger, high risk situations may systematically be avoided. Evidently, the appropriate way of addressing these concerns would be to find a good instrument for military deployment in post-conflict situations. Unfortunately, although CHS explore a number of potential instruments, they are not able to find one that is satisfactory. This is consistent with the major recent study by Doyle and Sambanis (XXX) who conclude that the decision process by which troops are allocated is so complex, involving repeat-play bargaining between a large number of countries, that it is not possible to find a satisfactory instrument for the scale of deployment. CHS therefore investigate the likely bias from failing to instrument, by initially introducing the assignment of peacekeepers as a dummy variable rather than as a continuous variable measuring expenditure. The dummy variable is significant and positive, indicating either that the decision to send troops increases the risk of conflict reversion, or, more credibly, that the UN decides to send troops to those situations which are systematically more at risk than average. This suggests that if there is an endogeneity problem it is most likely to bias any favourable effects of peacekeeping downwards: troops are being sent where risks tend to be higher. Thus, a result which shows troops to reduce risk is understating the true benefits since the true counterfactual risk is higher than that being assumed by the model.

With this caveat, CHS indeed find that expenditure on post-conflict UN peacekeeping significantly reduces the risk of conflict reversion. The variable is significant at 2%. In addition to being statistically reasonably significant, the effect is quite substantial. However, CHS do not attempt to move from this result to a cost-benefit of peacekeeping provision. This is the main specific value-added of our analysis here.

For the purposes of the present study we reanalyzed the CHS model. Our primary extension of the CHS model was to introduce domestic military spending, that is, military provision by the post-conflict government. This has already been discussed above. However, here we should note that while the effect of such spending on post-conflict risk had already been analyzed in the model of Collier and Hoeffler (2006), (including due allowance for endogeneity by means of instrumenting), this had not been integrated with an analysis of external peacekeeping. In effect, there were two published studies, one on peacekeeping and the other on domestic military spending, but no study that combined the two. The inclusion of domestic military spending which we undertook for the present study did not overturn any of the results already described. However the additional variable was itself statistically significant and, importantly for our present discussion, it approximately doubled the estimate of the effects of peacekeeping (while statistical significance was unaffected). Hence, these are the results on which we base our cost-benefit analysis.

We investigate the maintenance of a peacekeeping force at a constant scale for the entire post-conflict decade. Some international forces have indeed been maintained for such long periods, although shorter periods are more common. However, since CHS do not find that the first few years are significantly more dangerous, a long period of provision seems to be the right intervention to model. It would be possible to fine-tune provision, gradually scaling it down somewhat as risks fall, for example due to economic recovery, and this would doubtless increase the ratio of benefits to costs. But again it would presuppose a degree of sophistication in the decision process which seems unwarranted.

The reduction in risk achieved by peacekeeping forces depends upon their scale of deployment. Compared with no deployment, an annual expenditure of \$100m reduces the cumulative ten-year risk very substantially from around 38% to 16.5%. At \$200m per year the risk falls further to around 12.8%, and at \$500m it is down to 9%.

The Cost-Benefit Analysis of Peacekeeping

To convert this reduction in risk into a benefit commensurate with costs, we multiply the reduction in risk by the cost should a conflict occur. Recall that each percentage point of risk of conflict recurrent is valued at between \$586m at the lower bound and \$2.5bn at the centre-of-the-range illustration. Hence, peacekeeping at the level of \$100m per year for the decade generates an undiscounted benefit of between \$12.6bn and \$53.7bn. Since the undiscounted cost is \$1bn, the ratio of costs to benefits is between around 1:13 and 1:54.

While the ratio of costs to benefits is a useful guide to action, it is not in fact the appropriate criterion by which to choose an intervention. For example, as will be evident from the above figures, peacekeeping forces appear to be subject to diminishing returns and so there is at least potentially some optimal scale of intervention. Optimality requires that the benefits should diminish relative to the costs so that at some stage they become equal at the margin.

As part of our re-analysis of the CHS model we investigated the extent and nature of these diminishing returns. Such diminishing benefits can potentially come from three different ways. One, which we investigated, is whether the effect of peacekeeping is better described by a non-linear treatment of the explanatory variable, such as through the introduction of a quadratic. We could find no such effect. A second approach would have been to force diminishing returns by ranking all the post-conflict situations in their order of estimated expected pay-off to peacekeeping. We preferred not to adopt this approach since it presumed that the assignment of peacekeepers as between situations would be highly attuned to their relative benefits, when the evidence suggests that decision processes are not so characterized. The approach we took was to rely for diminishing returns on the curvature implicit in our modelling of risk: as risks are reduced, their further reduction becomes progressively more difficult. This is unlikely to be a mere

artefact but the precise degree of curvature is evidently not well-estimated. Hence, the results are approximate.

By construction, the optimal scale of an intervention is that scale which should be chosen to maximize the benefits, abstracting from considerations of risk and the shadow price of resources. However, it is important to recognize that at this point the ratio of benefits to costs will not be at its maximum. The criterion of choosing the scale of an intervention by whether the ratio of benefits to costs is maximized is thus generally inappropriate. Rather, it is better to determine the optimal intervention as that at which the marginal benefit equals the marginal cost, and at that point evaluate the ratio of the benefits to the costs. This is not merely pedantic. In many types of intervention there are diminishing returns to the intervention, so that sub-optimal interventions have much higher ratios of benefits to costs. In some ways a better criterion for selection of an intervention is the estimated net pay-off over and above its costs. However, a high ratio of benefits to costs is evidently not irrelevant since it gives some reassurance that even if the numbers are quite inaccurate there is little danger of doing net harm.

We find that the optimal scale of peacekeeping forces is around \$850m. At this stage the risk of conflict reversion is reduced to only 7.3%, the last percentage point reduction (i.e. from 8.3%) costing \$2.4bn over the decade to achieve benefits of \$2.5bn (taking our centre-of-the-range illustration). At this level, the risk of conflict has been reduced by a little over 30 percentage points and so has generated gross gains of around \$75bn (taking our centre-of-the-range figure) compared to its cost of \$8.5bn. While this leads to a much less impressive ratio of costs-to-benefits of around 1:9, it leads to an enormous overall gain per post-conflict situation of around \$63.5bn.

While these numbers might seem astoundingly large, this is because of the nature of the phenomenon. If, for example, we take the current practical example of the Democratic Republic of the Congo, the large UN force is costing around \$1bn per year, and so within the vicinity of what looks from our figures to be appropriate. During the presence of these forces peace has been maintained, albeit with periodic outbreaks of considerable violence, one of which, as noted, followed the elections. A counterfactual with a high probability of relapse into large scale violence is by no means unreasonable in this instance. Our analysis above estimates that in the typical case a deployment of peacekeepers at this scale would bring down the risk by 30 percentage points and this again seems entirely within the credible range for the DRC. Indeed, many commentators might regard the estimate as being overly conservative for the DRC, where the risk of large scale violence in the absence of international peacekeepers might have been rather higher than the 38% of the peacekeeping benchmark. Similarly, our analysis supposes that a collapse into civil war in the DRC would incur global costs of the order of \$250bn. Again, although an accurate figure is impossible, this does not seem to be absurd once all the potential neighbourhood and global repercussions are considered. If the international community could with certainty 'buy' the difference between sustained peace and

collapse in the DRC for around two years worth of the global aid budget it would quite possibly choose to do so.

In summary, international peacekeeping looks to be a good intervention. If the intervention is kept to a modest scale it has a very high cost-benefit ratio, and if it is set at an optimal scale it delivers enormous overall net gains.

Military Intervention Opportunity 3: Over-the-Horizon Guarantees

Now imagine that the instrument of a security guarantee could be elevated to a fully-fledged international instrument, whether under the auspices of the United Nations or a regional entity such as the African Union. To avoid the problems encountered by the French in Rwanda, the security guarantee would have to be circumscribed by clear limits: for example, governments in the process of genocide could not expect to be propped up by international military intervention. Here we propose three possible criteria for eligibility for the provision of protection. The three are not exclusive.

The first is the automatic provision of powerful peacekeeping forces to protect governments that came to power through certified democratic elections from the threat of rebellion during their period of office. The second is a similar protection against the threat of a coup d'état. The third is for an over-the-horizon guarantee in post-conflict societies. While the initial maintenance of peace cannot credibly be done without troops on the ground, the British experience to date in Sierra Leone suggests that it may be possible to phase the bulk of international troops out after say five years, replacing them with a guarantee made credible by appropriate logistics for the second five-year period.

Supposing that this cover was attempted, would it be effective, what would be its benefits, and what would it cost? This is the remit of the present analysis.

The over-the-horizon guarantee and the risk of conflict

The original Collier-Hoeffler study for the Copenhagen Consensus briefly discussed over-the-horizon guarantees, but there was no basis for quantification other than to assume that the Sierra Leone intervention was the model for all such interventions. Since the time of that study a new model of the risk of conflict, namely that of Collier, Hoeffler and Rohner (forthcoming) has specifically estimated the impact of the French over-the-horizon guarantee. As discussed above, this was not an explicit guarantee, but it was well-understood that French forces would be likely to intervene to support regimes in Francophone Africa and this was made credible logistically by a string of French military bases in the region. As discussed, the policy was operational from the time of independence of France's African colonies until the coup in Cote d'Ivoire of December 1999 revealed that as a result of the experience in Rwanda French policy had been rethought.

As with our assessment of peacekeeping, the first and critical step is to quantify the impact of the over-the-horizon guarantee on the risk of conflict. Because the French guarantee lasted for over thirty years and covered an entire group of countries it is amenable to statistical investigation. If the guarantee was effective we would expect these countries to have a significantly lower incidence of civil war than predicted by their other characteristics. This is tested by Collier, Hoeffler and Rohner (forthcoming). They introduce a dummy variable for the guarantee into a global analysis of the onset of civil war for the period 1965-2004. They find that the variable is significant and substantial: the risk of civil war in these countries during this period was only one third what would otherwise have been expected. They then investigate whether the dummy could have other explanations: in particular, whether all Francophone countries are systematically less at risk regardless of their location, and whether Francophone African countries are less at risk regardless of the period. Each of these alternative hypotheses can be rejected in favour of the interpretation that it was only Francophone African countries during this period that were significantly less at risk. Of course, this does not preclude that there was some other feature of these countries that was distinctive during this period. Nevertheless, the external security commitment seems a likely explanation. After all, the French government was committing significant sums to maintaining this military capability. In aggregate the risk of a civil war breaking out in one or other of the 13 countries of Francophone Africa was reduced by three quarters, the annual risk for the countries combined falling from around 10.2% to around 2.6%.

The risk reduction achieved by the French security guarantee will form our estimate of the likely efficacy of an international guarantee. However, our purpose is not to arrive at a cost-benefit analysis of the French guarantee itself, but to use the effects of the guarantee as a guide to other contexts in which such guarantees might be used. Evidently, the French guarantee was effective and CHR have quantified its contribution. However, in extrapolating from this result to post-conflict guarantees we face a choice. Such situations have far higher levels of risk than those prevailing in Francophone Africa and we could potentially assume either that the reduction in risk is of the same absolute or proportionate amount. Whereas the decade-risk facing the typical Francophone country in this period was around 10%, that facing the typical post-conflict country is around 40%. In effect, we must choose whether a guarantee provided in a post-conflict setting would bring the risk down by 7.6 percentage points, as in Francophone Africa, or by around 30 percentage points, which would be the same proportionate impact. Recall that a reduction of 30 percentage points would make the contribution commensurate with a large peacekeeping force in-country. We therefore prefer to choose the lower figure. Only in extrapolating the result to conflict prevention in low-income democracies, where underlying risk levels are broadly comparable to those in Francophone Africa, we will extrapolate by assuming that risks are reduced by three-quarters.

The cost-benefit analysis of a guarantee

We will consider the benefits from a guarantee of forces broadly equivalent to the scale and credibility of commitment provided by France for 30 years, but provided internationally to countries which met the criteria specified above.

First, consider the guarantee to support democratic governments militarily should they be threatened by a rebellion. According to UCDP/PRIO data, in the past decade four democratic low-income countries have suffered outbreaks of civil war and we take this as the likely rate over the next decade. If an international guarantee could reduce this by three-quarters, as did the French guarantee, then three of these wars would be averted. The annual benefit would be in the range \$18bn (lower bound) to \$75bn (centre-of-range).

We now attempt to get an equivalent benefit to the replacement of external peacekeepers in the second-half of the first decade of post-conflict situations by a guarantee. Of course, at present, most peacekeepers are withdrawn before this period but on our analysis this is a mistake. The counterfactual depends not just on whether peacekeepers are present, but on their scale and the scale of the withdrawal. We will construct the experiment so as to keep the level of risk in the post-conflict country constant. Thus, we assume that the over-the-horizon guarantee reduces risk by around seven percentage points and release peacekeeping troops in-country up to the point where the withdrawal creates an offsetting increase in the risk of seven percentage points. This clearly depends upon the size of forces. For example, if the initial forces were at the 'optimal' level of \$850m, then they could be scaled back to merely \$150m while leaving the overall risk level unaffected. In practice, this 'optimal' level will normally be excessive because it makes no allowance for aversion to risk. If the typical provision were \$500m then it could be scaled back to around \$100m. If provision were at \$250m it could be scaled back to \$70m.

The benefit of a centralized guarantee then depends upon how many situations to which it might apply. Normally, there are around six countries in the second half of the first decade post-conflict. Hence, the global gain in peacekeeping would range from \$4.2bn down to around \$1.1bn depending upon the scale of peacekeeping. In order to simplify we will take the central figure based on a force of \$500m, implying a saving of \$2.4bn annually.

The third and final possible coverage of a guarantee would be against coups d'états in democracies. In many respects this is the easiest task for a guarantee: coups in small, low-income countries can usually be made unviable by an adequate and prompt scale of external intervention. For example, the African Union was able to put down the coup d'état in the democratic mini-state of Sao Tome, Principe by the threat of military intervention from Nigeria, but it lacks the logistical power to reverse coups in larger countries. Unfortunately, there is no equivalent study to that of CHR in the external

deterrence of coups. Recall that the annual cost of successful coups to Africa over the past 40 years has been within the range of \$4bn to \$16bn. By not means all these coups threatened democracies and coups are getting less common, but recall that democracy does not provide protection against coups: even in 2006 there was a coup attempt against the democratic government of Madagascar which was fortunately unsuccessful. The best that we can do here is to assume that as with civil war a credible guarantee would reduce the risk by three-quarters, and that this would apply to the more democratic half of Africa's countries. This would imply savings within the range of \$1.5bn to \$6bn per year.

We now turn to the costs. The financial cost of the French over-the-horizon guarantee is not simple to determine. Partly, military budgets are invariably subject to a degree of secrecy and partly the counterfactual is obscure. However, for our estimate we relied upon an informal estimate from the French Treasury. It must be treated as subject to caveats. The estimate was that the cost was of the order of \$1bn per year. This is a high figure, equivalent to a super-force of peacekeepers in a single country, but this very scale presumably added to its credibility. Indeed, the guarantee force must evidently be at least as large as that needed in the largest envisaged operation. Since the French guarantee applied only to Francophone Africa, a more extensive guarantee such as we envisage would presumably be more expensive. In what follows we suppose that the cost of an international force would be double that of the French force at \$2bn.

Combining the benefits with the costs, the pay-off to the guarantee depends upon whether the same force can cover all three risks. If it can then the benefits are very large relative to the costs. The lower edge of the combined benefits is \$19bn per year and the centre-of-the-range estimate is around \$81bn. Since the assumed cost is only \$2bn per year, the implied ratio of costs to benefits is from 1:10 to 1:40.

**Packaging Interventions: Post-Conflict Aid+Military Spending
Limits+Peacekeeping+Guarantees**

As is apparent from our previous discussion, our four interventions complement each other. The effectiveness of post-conflict aid can be enhanced by limits on military spending. Those limits, and the curtailed military spending that they imply, can in turn be made more acceptable by the offer of credible external security provision. If this is provided in the form of an over-the-horizon guarantee, not only is it cheaper for those who provide it, but it is less intrusive and so more acceptable for those who receive it. In particular, while a guarantee against coups does not of itself generate particularly large global gains, it generates huge gains for the governments who benefit from them: coups are by far the biggest risk of regime change in low-income countries. Hence, such a guarantee might be sufficiently attractive to governments for them to be prepared to accept more limited military spending. Finally, the growth that substantial post-conflict aid would generate, and the strategy of phasing in over-the-horizon guarantees provides the long term credible 'exit strategies' that are now essential before credible international

military commitments are likely to be forthcoming. Thus, the package is more credible than any of its component parts viewed individually.

Further, each component of the package has clear precedents. Post-conflict aid is now standard, something that was not the case a decade ago. For example, the World Bank now has an explicit window for this need which lasts for the first seven post-conflict years. Military spending limits are applied by donors, but currently largely on an *ad hoc* basis. At the regional level, President Arias of Costa Rica is promoting an initiative for a coordinated mutual limitation on military spending, recognizing its properties as a regional public bad. Peacekeeping has grown enormously over the past decade, with DPKO the key agency within the UN system. Finally, over-the-horizon guarantees have been reintroduced by the British in Sierra Leone, and are being attempted by the African Union to guard against coups. Europe already has a ‘rapid reaction force’ which has the potential to enforce these guarantees. What has been missing to date is the vision to combine these interventions. However, the recent creation of the Peace-Building Commission of the UN, jointly under the Security Council and the General Assembly, provides an entity which could potentially coordinate such a vision. The Commission did not exist at the time of the first Copenhagen Consensus and so there was no potential to coordinate such a package of instruments.

We therefore conclude with a ‘grand vision’ of a security package that combines all four instruments. Since the linkages between the components are primarily political we have not attempted to quantify them. Hence, in terms of our estimated costs and benefits the package merely sums the components so that each can be accepted or rejected separately according to the threshold adopted. However, the package has political coherence and so it is worth also considering the four instruments together. We work with both our ‘centre-of-the-range’ illustration, and the lower-bound estimate which is around one quarter of this pay-off.

Post-conflict aid would be increased by 2% of GDP for the entire post-conflict decade, and on average cover twelve countries each with a GDP of \$20bn. It would thus cost \$4.8bn a year and add around one percentage point to the growth rate of post-conflict countries during this period. Combined with the ceiling aimed at preventing the leakage of this aid into military spending, this would yield a pay-off of between around \$12bn (lower bound) and \$21bn (centre-of-range).

Ceilings on military spending by post-conflict countries would aim to reduce such spending by 0.2 percentage points of GDP, this being the likely leakage from the additional aid. This would reduce spending by around \$480m per year in these countries, with a benefit in terms of reduced risk of conflict of the order of \$7bn (lower bound) and \$28bn (centre-of-range).

Peacekeeping forces would be provided in post-conflict settings at a scale of around \$500m per year in the typical country, being scaled down through the use of over-the-horizon guarantees in the second five years. Until allowance is made for double-counting due to the risk-reduction from the other components of the package, the ten-year risk per country would fall from around 38% to around 9%. With twelve such countries the annual cost would be around \$4bn and the annual gain between around \$21bn (lower bound) and \$84bn (centre-of-range).

Finally, an over-the-horizon guarantee would be provided at a cost of \$2bn per year. Again before allowing for double counting of risk reduction from other elements of the package, the gains from lower risks would be between around \$17bn (lower bound) and \$70bn (centre-of-range) per year.

The combined cost of the package would be around \$10.8bn per year. The benefits once double counting of security gains are eliminated, would be between around \$57bn (lower bound) and \$192bn (centre-of-range). Decomposing this, using the centre-of-range figures, it consists of security gains post-conflict of around \$100bn, increased GDP of around \$11bn, reduced coup risk of around \$6bn and fewer wars in democracies of around \$75bn. The ratio of costs to benefits is thus within the wide range 1:5 to 1:19.

V: Conclusion

Peace is a pre-condition for social and economic development. Civil war is development in reverse. Peace is an implicit assumption of those other challenge strategies that involve activities in the poorest countries. Further, internal violent conflict, both in the form of civil wars and coups is highly persistent: one manifestation of violence begets another. Such violence is concentrated among the poorest and least hopeful countries on earth. There is thus a powerful case for peace to be an objective for international intervention: it is both primary and well-targeted. The evident caveat to prioritizing peace is whether the international community can do anything to further it. After the intervention in Iraq many people might reasonably feel that the unintended consequences of security interventions are such that intervention in any form is too risky.

In this paper we have attempted to show that there is now considerable evidence that a combination of post-conflict aid and the provision of security through military interventions of various forms can fairly reliably and substantially bring risks down. Cumulatively this would radically lower the incidence of global conflict. That is, international intervention is effective.

The most difficult stage of our analysis has been to move from this demonstration of effectiveness to an analysis of cost-effectiveness. This is because assigning a value to the benefit of global peace is intrinsically problematic. We have therefore presented both a 'lower-bound' figure which has some reasonable basis in quantitative analysis, and a

‘centre-of-range’ figure which is much more speculative but which we feel may better capture true sentiments.

Table 1 provides a summary of the costs and benefits of the various measures. We use alternatively discount rates of 3% and 6%. Depending on which of these measures is taken, the ratio of costs of our package to its benefits varies from around 1:4 to 1:18. The individual components of the package range much more widely. Whether this is sufficient to warrant prioritization is a matter for the panel. However, it seems to us that the key step has been the demonstration of effectiveness. Peace is so fundamental that if interventions are effective and the absolute costs are not out of bounds, the interventions should be undertaken.

Table 1: A Summary of Costs and Benefits

Opportunity	Costs of intervention (over 10 years)	Benefits: Averted economic losses and DALYs over 10 years	Benefits: More averted losses of war when mil. Spending is lower	Benefits: Added growth due to aid	Benefits: Averting coups	Total benefits including all over 10 years	BCR
Discount rate 3%, DALY cost \$1,000. Cost of war is 58.6 bn							
Aid	4bn	880m		10 bn		10.9 bn	2.7
Conditionality	4bn	880 m	650m	11.7 bn		13.3 bn	3.3
Peace-keeping troops	1bn	12.6 bn				12.6 bn	12.6
Optimal level of PKOs	8.5 bn	18.0 bn				18 bn	2.1
Over-the-horizon guarantees (costs and benefits calculated for six post-conflict countries)	\$20bn	\$176bn			15 bn	191 bn	9.6
Combined 500m-PKO and over-the-horizon guarantee (costs and benefits calculated for six post-conflict countries plus countries at coup risk)	\$28 bn	\$103.6 bn			15 bn	118.7	4.2
Discount rate 6%, DALY cost \$1,000. Cost of war is 49.8 bn							
Aid	4bn	744m		10 bn		10.7bn	2.7
Conditionality	4bn	744m	550m	11.7 bn		13.2 bn	3.3
Peace-keeping troops	1bn	10.7bn				10.7 bn	10.7
Optimal level of PKOs	8.5 bn	15.3bn				15.3 bn	1.8
Over-the-horizon guarantees (costs and benefits calculated for all six post-conflict countries)	\$20bn	\$149bn			15 bn	164 bn	8.2
Combined 500m-PKO and over-the-horizon guarantee (costs and benefits calculated for all six post-conflict countries plus countries at coup risk)	\$28 bn	\$86.1 bn			15 bn	101.1 bn	3.6

Discount rate 3%, DALY cost \$5,000. Cost of war is 78.5 bn							
Aid	4bn	1.2 bn		10 bn		11.2 bn	2.8
Conditionality	4bn	1.2 bn	650m	11.7 bn		13.3 bn	3.3
Peace-keeping troops	1bn	16.9 bn				16.9 bn	16.9
Optimal level of PKOs	8.5 bn	24.1 bn				24.1 bn	2.8
Over-the-horizon guarantees (costs and benefits calculated for six post-conflict countries)	\$20bn	\$234 bn			15 bn	249 bn	12.5
Combined 500m-PKO and over-the-horizon guarantee (costs and benefits calculated for all six post-conflict countries plus countries at coup risk)	\$28 bn	\$135.6 bn			15 bn	163.6 bn	5.8
Discount rate 6%, DALY cost \$5,000. Cost of war is 69 bn							
Aid	4bn	1.0 bn		10 bn		11.0 bn	2.8
Conditionality	4bn	1.0 bn	550m	11.7 bn		13.3 bn	3.3
Peace-keeping troops	1bn	14.8 bn				14.8 bn	14.8
Optimal level of PKOs	8.5 bn	21.2 bn				21.2 bn	2.5
Over-the-horizon guarantees (costs and benefits calculated for six post-conflict countries)	\$20bn	\$207 bn			15 bn	227 bn	11.4
Combined 500m-PKO and over-the-horizon guarantee (costs and benefits calculated for all six post-conflict countries plus countries at coup risk)	\$28 bn	\$119 bn			15 bn	134 bn	4.8
Centre-range cost estimate: Cost of war is 250bn							
Aid	4bn	3.8 bn		10bn		13.8 bn	3.5
Conditionality	4bn	3.8 bn	2.6 bn	11.7 bn		18.1 bn	4.5
Peace-keeping troops	1bn	53.7bn				53.7 bn	53.7
Optimal level of PKOs	8.5 bn	310bn				310 bn	36.5
Over-the-horizon guarantees (costs and benefits calculated for six post-conflict countries)	20bn	750bn			60 bn	810 bn	40.5
Over-the-horizon guarantees (costs and benefits calculated for six post-conflict countries)	20bn	762 bn			15 bn	762 bn	38.1
Combined 500m-PKO and over-the-horizon	28 bn	432 bn			60 bn	492 bn	17.6

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guarantee (costs and benefits calculated for six post-conflict countries plus countries at coup risk)							
Over-the-horizon guarantees (costs and benefits calculated for six post-conflict countries plus countries at coup risk)	20 bn	\$780bn					39

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ⁱ Collier (2007) proposes the concept of the 'bottom billion', composed of 58 small, low-income countries in various development traps and with a combined population of around one billion people. These countries are distinctive in that they are both much poorer than other developing countries and diverging from them.