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## **STUPID AND EXPENSIVE ? A CRITIQUE OF THE COSTS-OF-VIOLENCE LITERATURE**

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## **Stupid and Expensive? A critique of the costs-of-violence literature**

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### **Introduction**

A recent and justly celebrated book asserts that ‘civil war is not a stupid thing’ (Cramer 2006). But is this true? Armed conflict causes so much human suffering and material destruction that Cramer’s claim runs the risk of being dismissed without further discussion. In fact, the rapid development of the ‘costs-of-violence literature’ into a cottage industry of sorts is due in good measure to the conviction that civil war is not only a stupid, but also an awfully expensive thing.

That war is a costly undertaking is beyond reasonable doubt. In order to survive armed organisations at the very least need to feed, dress and arm their members, as well as to build up a logistic infrastructure and a system of communications. Their explicit objective is to destroy the enemy’s will to fight, in the canonical Clausewitzian rendering, but this implies killing and maiming people, blowing up infrastructure – in short, the wholesale destruction of human and physical capital. The case for the stupid and expensive character of war thus seems to be overwhelming. But is this really so? Can ‘stupid’ be translated into ‘anti-developmental’? Would we want to make a general pacifist statement out of a belief that any kind of violence (or war) is stupid? If not, then it is necessary to specify why civil, or any other specific type of war, is more (or indeed uniquely) stupid in contrast with other modalities of the use of organised violence. Until now, nobody has come up with such an argument. If, on the other hand, we do want to make such a general statement, then several theoretical issues appear. Some of the best extant explanations of state strength are based on the proposition that modern states are the children of the armed build-up of territorialised entities. According to this perspective, increasing internal and external violent challenges to their control over a given territory forced them to increase the number of people under arms, which buttressed both taxation and bureaucratic differentiation, leading to an enhancement of the initial expansionist drives each of those entities had, and so on. In sum, the model is one of a positive feedback process where war plays the role of a necessary condition for organisational development: ‘War made the state, and the state made war’ (Tilly 1975). The model has been formalised by political economists (Olson 2000) and appears to be supported by social historical research (Tilly 1992 and 2003; an early version of all this is Schumpeter’s fiscal state, 1951). “Tillian wars”, then, although terrible, are not a stupid thing.

But can today’s conflicts be considered ‘Tillian’? An influential set of authors has suggested that in today’s globalised world Tillian wars are no longer possible (Kaldor 1999). Indeed, Tilly did not pretend that the conclusions he had crafted for Western Europe were universal;

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and in some concrete contexts, such as Latin America, they have been challenged (Centeno 2003). Additionally, the policy implications of the findings of social historians, concerning the progressive role that war can play in state formation, may seem to be highly anti-intuitive and resistant to any sensible translation into the realm of policy action (though this is not necessarily true, see below). But they do pose a serious methodological problem for the costs-of-violence literature: how can the consequences of violent events be accounted for in the long run<sup>2</sup>? How can we show that wars are bad and costly, indeed stupid, for society (or how can we distinguish the stupid ones)?

This paper evaluates the costs-of-violence literature in light of these questions. The main effects of conflict can be divided into two categories: efficiency and distribution. With respect to the former, the main claim is that civil war (or any other form of massive violence) moves the economy of a given country away from the frontier of optimality. It does this in several ways: firstly, by the very fact that war is destructive – lives, infrastructure and communications are wiped out in the maelstrom of conflict; secondly, by forcing the state to invest in military resources that it could have used otherwise, resulting in social and long term priorities falling to the bottom while the sheer survival of the state apparatus rises to the top; thirdly, by preventing fundamental reforms that the country may need, but that in an ambience of divisiveness and strife may become difficult or impossible; and finally, by generating opportunistic violence and illegality – while the state is occupied fighting rebels, other illegal actors may find space to develop their activities; Justice is overwhelmed by the situation of conflict; and the borders are increasingly porous, and contraband thrives. Thus, in many cases, there is a positive feedback loop between internal conflict and other forms of illegality, all of which converge on putting more and more stress on the state. With respect to distribution, the basic idea is that civil conflicts hit the most vulnerable social sectors worst. Firstly, the rich can defend themselves (pressuring the state, establishing private collective action or simply fleeing) much better than the poor. The cost of looting the latter is much lower (they hold considerably fewer assets, but they are more numerous, have little possibility of fighting back or of acceding to private security and state security may be biased against them). Second, precisely because the state is diverted from social policy, crucial egalitarian investment (for example, in education) may disappear. Third, the disruption of social institutions and the implantation of the law of the strongest – a typical result of the disaggregation of the state – affects everybody, but differentially: those who have fewer resources to defend themselves individually, or through collective action, suffer the most. Obviously this is applicable not only to socio-economic cleavages, but also to other ones (for example, children tend to be brutally hit by conflict, as a result of direct aggression, destruction of families, recruitment, etc).

This is the general landscape, and it should be noted that it combines a number of different types of assertion. Some are pure ‘statements of facts’ – internal conflict forces states to pay less attention to social policies – while others are evaluations of ‘states of the world’ – the economies of countries affected by conflict perform worse than others. Though both seem very intuitive, this paper will demonstrate the difficulty in proving them, due to the problems involved in comparing very different contexts and outcomes in order to reach a general conclusion. I argue that there are in fact three ways of raising effectiveness and distributive issues: ‘bullionist’, that is by calculating the costs and benefits caused by armed conflict; ‘deadweight’, in which an evaluation is made in terms of, for example, the loss in GNP growth that the society as a whole has suffered due to the conflict; and ‘distributive’, showing

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<sup>2</sup> It is true that in the long run all of us are dead, but not entities like organisations and states; this is precisely the point.

how assets and income have passed from some hands to others. I will claim that each of these entails very different methodological challenges, and that collapsing them into a single notion is a major, if quite frequent, mistake that tends to produce meaningless results.

The paper is divided into five sections. In the first section, I present some conceptual nuances which I believe are quite basic for a proper understanding of the problem, and which have been frequently disregarded even by technically sophisticated scholars. I also develop ideas around the bullionist-deadweight-distributive distinction. The second section discusses an assortment of some of the best examples from the costs-of-violence literature: internationally, and then applied to the conflicts of Colombia and Peru. With respect to the international literature, I focus on a small number of key texts. The literature on costs of violence is already bulky, and I limit myself to discuss the main trends and ideas. Based on the literature review and conceptual discussion, in the third section I propose some adjustments to the general approach offered in the costs-of-violence literature. I follow this with a separate treatment of the distributional issues, before drawing some general conclusions.

## **A reflection on conceptual and methodological standards**

### ***The basic meanings***

Expressions such as ‘costs of conflict’, ‘costs of violence’ and ‘costs of war’ can have at least three interrelated, but distinct, meanings. The first one is concomitant with the bookkeeping of the outlays, which both the state and its contenders have to incur to wage war. These ‘bullionist’ exercises characteristically ask questions such as how much did this or that armed organisation invest in weapons, food or logistics, or how big were the rents that they captured through looting, ransom or expropriation. The second one is the measurement of the hypothesised economic ‘deadweight’ effect that violence and/or conflict have had on a given society: how much more, for example, would a given country have grown had it not been for its internal conflict? Please note that the deadweight perspective must be based on a counterfactual: if there had not been conflict, then what would have happened; or how many social goods did a given society forego for indulging in violent struggle? The third one focuses on the differential effects of war over key social sectors. As with the other two meanings, this impinges not only on the evaluation of the social impact of conflict, but also on the probability of arriving at a peaceful settlement.<sup>3</sup>

The first, bullionist perspective appears at first rather uninteresting – a problem for an accountant. Actually, it is not as banal as it appears. On the one hand, it requires careful attention to the quality of the data, a dimension frequently obviated in the application of econometric techniques to social problems (Ball 1996 and 2001). On the other, it has a direct linkage, unfortunately seldom highlighted, to Fearon’s rationalist puzzle. Fearon’s basic idea (1984) is that if information about the relative strengths of the contenders is public and these are rational, wars should not take place, independently of the magnitude of the gains and losses implied. The underlying argument is both very simple and strong. Rational agents will be able to deduce what the *ex-post* distribution will be, so the equilibrium outcome is that they are able to strike a bargain without incurring the costs of war. The puzzle, then, is why we observe armed conflicts anyway. To solve this by arguing that agents are not rational in some specific sense is not very interesting: first, because we are not dealing with the soldiers, but with leaders whose very business is strategising; and second, because we know that frequently the type of settlements Fearon hints should *always* take place are in effect

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<sup>3</sup> For example, according to Wood (2000) peace in El Salvador and South Africa was a product of the calculation by economic elites –it was cheaper for them to negotiate than to persist in the fighting.

*frequently* arrived at (for example, Snyder 2004). It is possible that the pattern of costs and rents is related to the probability of the onset and termination of the conflict (as in Snyder 2004). Be that as it may, the existence of huge rents to capture does not explain automatically either the onset or the continuation of war. A careful scrutiny of the assets and liabilities of state challengers might help understand the initiation and persistence of conflict.

Unfortunately, an empirical approach to the second, deadweight effect is quite difficult; indeed, it is terribly destructive. War and violence are tags attached to sets of thousands of correlated events that produce a multiplicity of effects. But as seen above, in the social history literature it has been found time and again not only that war can have developmental and pro-growth side effects, but also that an anti-growth status quo can have worse long term effects than war itself, independently of the outcome of the latter – and this holds not only for Europe (Tilly 1992) but also for late developers (Moore 1993). Unaware of this, many contributions to the costs-of-violence literature take for granted the inexistence of positive externalities. Nevertheless, some of them are quite easily identified: Keynesian policies attached to military build-ups, road building, technological change triggered by military challenges, the strengthening of the state and the transformation of women's position in the work force and in the public sphere, are some of the obvious examples. It might be argued that: a) none of these effects are observed in today's conflicts; or b) all of them introduce long-term distortions that are much more harmful than their supposedly positive short-term impacts. But that this is the case ought to be demonstrated, not assumed – though this is difficult to achieve<sup>4</sup>. In particular, it is very important not to abuse the *ceteris paribus* postulate. War is such a brutal and massive phenomenon that it changes nearly everything and each of these changes alters the future state of the economy. As happens in other contexts, though much more radically (Przeworski 2003; North 2005), there is no way of summing up the aggregated results of such processes: a) because some of them develop in the very long term, and are beyond, and below, our capacity of observation; b) because there is no reasonable way of making an exhaustive list of them; and especially c) because the number of potentially relevant control variables is infinite, such that we cannot specify by direct observation the state of the world had the conflict not occurred. For all these reasons, the only way to capture the deadweight effect is through counterfactuals (see, for example, Knight, Loayza and Villanueva 1999). This implies defining the status quo, and then creating tools to extrapolate it into the future<sup>5</sup>. The difference between the extrapolated status quo and what actually happened is the magnitude of the deadweight. In sum, the deadweight question boils down to how plausible and well built the counterfactual is. Researchers can make a long list of the costs of the conflict, but if they are not able to build a viable counterfactual that exhibits the foregone growth, or social good that was not created, this would simply mean that in their observation of reality they skipped some relevant variables, thus failing to identify multiple side effects, some of them potentially pro-growth, that the conflict produced. For example, in Colombia economists of the National Department of Planning have correctly stated that the internal conflict offers multiple opportunities for the illegal economy (basically coca) to develop, putting a substantial stress on the state (Sánchez 2003). But they have conspicuously omitted taking into account that the cash crop contributes substantially to the economy – between 3

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<sup>4</sup> Besides, this introduces several very serious additional issues. How can we compare outcome X for generation t with outcome Y for generation t+1? This is not only a problem of temporal discounting. Suppose that in apartheid South Africa a broad sector of the population decides that it will allow a decline in its living standards provided this is useful to guarantee political rights for future generations of Blacks. How can this be put in terms of streams of income? CVL analysts seem not to have noted that when the impacts, positive or negative, of a conflict unfold over a long period, we are speaking about different sets of people.

<sup>5</sup> There are several ways of doing this, each one with its strengths and weaknesses.

and 5 percent of GNP, according to what appear to be the most reasonable evaluations (Cárdenas 2007) – and that several legal sectors benefit from its spillovers.

The third, distributional problem is, at least from an abstract point of view, much easier to answer. The transfer of assets can be tracked down, and the global impacts over specific social sectors evaluated. The departure point that makes this question interesting is that the exercise of organised violence is prone to hit the most vulnerable groups much harder (Collier 2003), and many examples show that this is precisely the case. But at least regarding the purely economic dimensions of conflict this is not the only possible scenario. Conceptually, the main challenge is not to ignore: a) the important ‘positive’ transfers, mainly the possibility that violence is associated with an egalitarian outcome or that it has egalitarian side effects by way of social incorporation (as in Latin American independence wars, or in the classical accounts of European state building); and b) the important ‘neutral’ transfers (that is, from rich to rich or from poor to poor).

The differential effects can be of two types. First, repertoire-driven effects: depending on the type of actions carried out by the state and its challengers, some social sectors will be more or less affected. In Peru, according to the Peruvian Truth Commission the fact that the Shining Path (*Sendero Luminoso*) built its strategic base in Ayacucho and neighbouring provinces led to an extreme over-representation of victims of a given ethnicity (*cholos*), a region (the *Sierra*) and a social class (peasants)<sup>6</sup>. In Colombia, the insurgency decision to fund its fight by kidnapping, hit the cattle ranchers very hard. In the costs-of-violence literature it is frequently stressed that in a conflict the state will earmark resources for war that otherwise would have been concentrated on, for instance, social policies. Looting and stealing by all actors are common place. More generally the destruction of assets – for example of the electricity infrastructure – can have distributional consequences, beginning with the obvious fact that they are regionally differentiated. Second, distributionally motivated effects: during the conflict (or as result of its termination), wealth is consciously transferred from some agents to others. For example, in Colombia there is strong evidence that the two waves of violence that occurred during the twentieth century produced an inverse agrarian reform and that some powerful actors consciously pushed for such an outcome. The conflict in South Africa and its negotiated end had distributional consequences, although less profound than the aspirations of those who fought for the abolition of apartheid.

In sum, each of the three meanings is related to different questions, and entails identifying diverse methodological minima. The bullionist question can only be solved through careful accounting – here the main difficulty is to find reliable data and to be able to identify the key cash flows for each actor and for society as a whole. The deadweight question can only be dealt with through a counter-factual: what would have happened if there had not been conflict? Finally, the distributional problem implies identifying at least the main transfers of assets and flows of income that a given conflict has produced. Each of these methodological minima implies its own challenges, of which researchers should be aware. The three questions are interconnected in the following way. The deadweight exercise, if feasible, will give investigators an evaluation of what the society has lost by being at war. This should guide bullionist researchers to refine their balance sheets; if they have identified a massive imbalance that is not captured by the deadweight, then they know that they have missed a set of important variables. Finally, deadweights and accounting for them should improve

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<sup>6</sup> Peruvian Truth Commission, at <http://www.cverdad.org.pe/>

substantially our understanding of both the impacts of the war over different sectors of society, and the pattern of rent generation and capture associated to war.

If all this is true, no sensible conclusion can be arrived at when: a) the three questions are mixed up; b) the bullionist question is answered through unreliable or one-sided data; c) the deadweight question is not addressed through an appropriate and carefully constructed counter-factual; d) the critical base lines have not been identified; and e) the distributional effects are discussed without having identified previously the broad distributional landscape that frames the conflict.

### *The signs of the externalities of war*

The notion that war can lead to positive change, or produce positive externalities, will no doubt sound contrived to policy and opinion makers, not to mention the overwhelming majority of Colombians and Peruvians. This is not an idea that lends itself to direct policy translation, and any attempt to do so would be suspect or whimsical (Luttwak 1999; Leander 2004)<sup>7</sup>. However, this does not mean that the underlying notion is unsound or purely academic in the perjorative sense, although implementing it seems, on the surface of things, to be monstrously absurd, not only because of the ethical issues involved but also because the majority of the potentially pro-growth externalities of conflict are, as many other things in life, basically sub-products (Elster 1985) that can be attained only through indirect strategies. One cannot indulge in violence with the objective of building the state or boosting growth via the war economy, just as one cannot plan to be spontaneous<sup>8</sup>. It is probable that such a discussion will only reveal the severe limits of thinking of war in terms of a cost-benefit analysis.

But precisely because pinpointing such limits is extremely important, it is necessary to understand what I will call from now on the ‘anti-intuitive externalities’ of violent conflict. In other words, if the name of the game is cost-benefit analysis, then we should thoroughly respect the established rules, lest the exercise lose all its meaning; and the rules imply that the departure point is that it is possible that benefits for society as a whole, or for some specific sectors, do exist. No self-respecting firm would consider analysing its economic perspectives without a carefully constructed balance sheet that considers the basic scenarios. When speaking in the name of the state, society or humanity, failing to do this seems a tad careless.

The anti-intuitive externalities of armed conflict can be both pro-growth and distributional, and can combine in several ways. Keynes (1987), for example, suggested that War World II had opened unprecedented opportunities for growth, due to both the opening of distributional possibilities and a pro-consumption re-education of economic agents. Nobody would claim that in 1939 Keynes would have advocated going to war to produce those results, because the extra-economic ramifications were so incommensurably horrible. The importance of Keynes’ remark is methodological: wars do not only produce costs and destruction, but also change the whole baseline, something that any sensible evaluation should take into account (see Table 1). Similarly, with regards to other violent incidents common in the contemporary world, such as

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<sup>7</sup> Indeed, Edward Luttwak’s paper (1999) was a pledge for “giving war a chance”, but though it did cause a stir it had no policy impact. The notion that Tilly’s studies of state formation are directly applicable to the contemporary world also do not hold (rather obviously). See Leander, 2004

<sup>8</sup> Though for slightly different reasons. In the case of planning to be spontaneous, there is a logical contradiction. In the case of creating violence to build the state, the means are not accessible (nobody would expose his life for such an abstract motive) and the outcome too uncertain (the probability of winning and surviving is much less than one).

invasions, researchers and policy makers typically focus on the trade-offs involved in the operation: for example, death and destruction against democratisation.

**Table 1: The other side of the balance sheet: a list of some key anti-intuitive externalities**

<b>Externality</b>	<b>Example</b>
Technical change	Military industry, spill over into other branches
Achievement of egalitarian political objectives through a repertoire of methods that includes violence	South Africa, de-colonization
Other forms of political inclusion	Female enfranchisement
Strengthening of the state	Strengthened armies and police, more taxation
Keynesian effects of military build-up	State investment in public works through the army

**A panoramic review of the literature**

*International*

The departure point of the best papers in the international literature is a clear, though with few exceptions (for example, World Bank 2007) implicit<sup>9</sup>, understanding of the different hues of the expression ‘costs of violence’ (or conflict), and the methodological implications this has. The majority of the papers focus on the deadweight and distributional meanings, and somewhat less on the bullionist perspective.

Currently, there are two main ways of attacking the problem of deadweight. Firstly, by comparing the trajectories of countries in conflict with those that most resemble them, before, during and in the aftermath of war<sup>10</sup>. The great advantage of such an approach is that it is simple, easily understandable and comparative. The counterfactual is that country Y would have roughly behaved like its similar counterparts X1, X2..., Xn, if Y had not had war. On the other hand, there are two main shortcomings. The first one is that, using this tool, the deadweight has been extraordinarily difficult to capture. For example, Ammons (1996) finds no effect. The second one is that ‘similar’ is a dangerous expression: similarities can hide great differences with respect to important variables. For example, Collier and collaborators have announced that armed conflict reduces the GNP growth by 2.2 percent (2003). This result is taken from Collier (1999), where per capita GNP growth between 1960 and 1989 is utilised as a dependent variable, while the independent variables are how many months a country spent in war during a decade, how many months were spent in reconstruction and how many months preceded the initiation of war. A few control variables were also included (dummies for period, education, income per capita, ethnolinguistic fractionalisation, and for being landlocked). Collier’s findings are suspect for various reasons, which underscore neatly the hidden dangers that the notion of similarity contains. First, when comparing countries with or without conflict, they do not control for such fundamental variables as the weight of agriculture (or export-agriculture production) in the GNP<sup>11</sup>. They also do not include dummies for region, which seems fundamental, and they collapse into the same category

<sup>9</sup> For a good explicit discussion of the different possible approaches, see World Bank, 2007.

<sup>10</sup> Similar countries in all three periods.

<sup>11</sup> Which is kind of odd, as Collier had found previously that this variable was strongly associated with the onset of conflict.

high, medium and low intensity conflicts, something that should not be done. In sum, the model may suffer from mis-specification, because it drops variables that theoretically seem quite important both for defining base lines (or departure points) and causation. In this sense, Lila Ammons's (1996) exercise is more sophisticated. She establishes a comparison between 32 African countries, divided in two categories: with and without war. No deadweight was found through multivariate tests.<sup>12</sup> Her conclusion (Ammons 1996: 77) is that:

‘this analysis addressed the consequences of war on countries and the extent to which war accounts for the differences between non-warring and warring African countries’ development patterns. While these results are not conclusive and imply that wars do not have long range consequences on overall development in Africa, they do offer some suggestions about the areas in which war may negatively affect Africa in that the non-warring African countries seem to have experienced larger increases in the number of children who lived beyond the first year of birth as well as larger increases in GDP than the warring African countries.’

Nevertheless, multivariate tests did not yield any statistically significant difference between the two categories. Ammons suggests that this may be attributed to the fact that conflicts produce massive transnational migratory movements, which affect the neighbours of warring countries, pulling down the growth of the whole territorial cluster. This regional effect would make it more difficult to identify national deadweights.

The second approach is not to take the counterfactual from real life examples, but to build it. There are several ways of doing this. This approach makes it possible to take into account explicitly the problematic nature of the notion of similarity and the issue of baselines. The disadvantage is that the counterfactual can become increasingly artificial, and difficult to evaluate. Nevertheless, I believe this is the best way to try to identify the deadweight. For example, Knight et al (1999), while focusing not on the costs of conflict but of military investment, try to answer several questions, of which the main one is whether there is a peace dividend for cutting military expense. They start with a review of the costs-of-violence literature, acknowledging that until now it has failed to produce any clear evidence of the effects they are searching for. Then they define the ‘peace dividend’ as a:

‘percentage difference between the level of real capacity output per capita that would result from a given sustained reduction in the military spending ratio, and the baseline path of capacity output that would have prevailed in the absence of such a reduction’.

This is a typical deadweight question, and the authors came up with a very clever way to answer it. They found that there is a significant and negative association between the ratio of military spending to the GDP as independent variable, and growth and investment. They then fed these coefficients into a neoclassical, Solow model of growth to calculate what would have happened if there had been lower levels of military spending: finding that growth would have been substantially better. I find this is by far the most attractive exercise done in the costs-of-violence literature vein, but it leaves at least three unsolved predicaments. The first one is the model of growth itself. It assumes that technology is exogenous, as Solow models do, and thus disregards completely the contribution of military spending to technological

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<sup>12</sup> Ammons suggests that univariate tests might reveal the presence of the deadweight, but does not elaborate. At any rate to replace multivariate tests by several univariate ones is suspect, and should be done only with great caution. But Ammons's remark reveals a deeper problem, the instability of conclusions with respect to the technique that is used. If multivariate tests did not find any deadweight, but a list of univariate ones did, which conclusion should we choose?

change, which should be quite substantial<sup>13</sup>. This raises an additional redistributive question: do deadweights vanish or are they transferred? How much of the hypothesised 2 percent is a loss and how much is a transference from one nation to another?<sup>14</sup> Second, although the association between the independent variable and growth is statistically significant and negative, it is only marginally so. Third, due to missing data the authors present their results by decade, while an annual analysis might have yielded quite different results – something that is particularly relevant given the extremely small size of the association coefficient between military investment and growth.

### ***Colombia***

The present wave of Colombian internal conflict started in the late 1970s and continues today. Its main actors are the state, several guerrilla movements and paramilitary groups. From the early 1990s there has been a growing concern with the costs of the conflict. The Colombian costs-of-violence literature has been spearheaded by the National Department of Planning, with many other state agencies, think tanks and NGOs chipping in during recent years (see, for example, DNP 1998).

There are two deadweight papers that deserve attention, especially because they converge on their main conclusion: Colombia suffered an economic slump from the early 1980s, caused by the conflict. The first one (CEDE, 2003) illustrates this very well, using a graph that maps, department by department<sup>15</sup>, the rates of growth in the 1960s and 1970s versus those of the 1980s and 1990s. The 45° line is where the growth rates were equal, so that points below the line are the departments where growth was higher in the early decades. The majority of departments fall below the line, which means that after the onset of war the economy slowed down. Cárdenas (2007) wants to make the same point using a more sophisticated tool, by trying to capture the effect of an exogenous shock that, he hypothesises, was the initiation of the conflict. The common problem of both exercises is that they completely neglect what was taking place beyond the country in that period, with the whole of Latin America cooling down economically in the 1980s. In fact Colombia fared better than the Latin American average. Growth rates were better in the country in the 1960s and 1970s, but this is no exception<sup>16</sup>. Latin American countries can be categorised between those in which agriculture predominates in the GNP, and those that are more industrialised. According to this categorisation, Colombia would pertain to the first set, which fared poorly from 1980 on. Colombia was an agricultural country that did relatively well, above the average. Would this allow us to infer that thanks to the conflict the country was protected from an exogenous shock that affected others much more severely? On the contrary, it would rather suggest that in Latin America agricultural structures may be the true underlying deadweight, an intuition that sits well with both quantitative (Collier 2003) and historical-qualitative (Moore 1993) studies.

All the Colombian bullionist exercises proceed according to the following algorithm:

1. Create the most complete list of social wrongs generated by the conflict (or by a particular practice, like kidnapping), both with respect to growth and distribution;

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<sup>13</sup> Of course, wars that buttress technological change and innovation are particularly atrocious; here I am restricting myself to the purely economic effects.

<sup>14</sup> Practical politicians, contrary to academics, are quite aware of these kinds of issues. Weaver (1974) reports that, when Nixon was requested to put a ceiling on the sale of weapons to Latin America, he retorted that the only result of this would be a sacrifice of US jobs.

<sup>15</sup> In Colombia the department is an administrative unit above the municipality but below the nation.

<sup>16</sup> The only true exception is Chile, where things occurred the other way round.

2. Evaluate the rents captured by non-state armed actors;
3. Estimate the monetary value of the list of social wrongs, and deduct it from the GNP;
4. Arrive at an estimation of the aggregated losses.

This means that they share the following problems:

1. Not one separates analytically the three dimensions of the costs of violence. As seen above, to make a list of costs and then deduct them from the GNP as a deadweight is simply wrong. Besides, the results of such exercises are extremely unstable. For example, they range from an estimated 2 percent to a full 15 percent in loss of GNP growth due to the existence of the conflict (Rubio 1995)<sup>17</sup>.
2. They fail to take into consideration the complexity of the externalities of the conflict. For example, there are the claims that the conflict has increased the chances for illegal actors, like drug cartels, to act (Sanchez 2003; Collier 2003). This entails a cost for the state, in the form of illegality, increased investment in security and justice, foregone tax revenue, etcetera. But the other side of the coin is the contribution of coca production to the economy, which according to the most conservative and reasonable estimates is around 3 or 4 percent of GNP. Tillian effects (in which sense is money invested on justice lost?) are also disregarded.
3. There is a tendency towards making unsubstantiated claims. For example, Martinez (2001) suggests that one of the indirect costs of the conflict is the deterioration of the legitimacy and credibility of the state. This may or may not be true generally, but in Colombia there is no evidence whatsoever that one of the long-term results of its war is decreased state legitimacy. According to several opinion polls, the contrary may in fact be the case: in the regions where the conflict has historically been more intense, people ask for more state presence, although this is probably selective (they want some agencies and not others, following a historical trend).
4. Reckless use of figures can also be seen. Good and bad estimates can be different by a factor of three or four, as the cases of Peru and Colombia reveal below. For example: to assess the economic impact of kidnapping, the DNP utilised the figures of the federation of cattle ranchers, which – as recognised by the federation itself – are very unreliable; to assess the impact of displacement on the state, the entire special budget dedicated to displaced people was taken as a cost in Pinto(2004), omitting the fact that in any case the majority of displaced peasants were poor, and thus would have been targets of diverse social policies; and there is no agreement about the true number of internally displaced persons.
5. Neglect of the anti-intuitive externalities of the conflict: while the list of its negative consequences is long, the other side of the coin has been carefully avoided.
6. Confusion of normative and economic perspectives and enthusiastic but hazy political correctness. For example, some authors tag narcotrafficking as ‘unproductive’ without specifying in what sense it is (Pinto 2003). Others include in the accounting of costs negative externalities such as the destruction of the environment (Perez 2002).

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<sup>17</sup> A vast amount of gullibility is needed to believe that a peaceful Colombia would have grown at 20% annual rates.

While this may be a relevant consideration, nevertheless environmental costs are not included in Colombian national accounts, so they will not affect the GNP figures<sup>18</sup>.

Bluntly put, with the techniques used up to now to estimate the costs of the Colombian violence, any conclusion is possible.

### *Peru*

The Peruvian conflict started in 1980 and drew to a close in 1991, after the leader of the main guerrilla organisation, Shining Path, was jailed for life. It was more virulent than the Colombian, but much shorter and more territorially circumscribed. Far fewer denominations participated in it: only two guerrilla organisations, and no well armed paramilitary groups, at least not in the form that they are known in Colombia.<sup>19</sup> The Peruvian conflict seems to be a canonical example of what the costs-of-violence scholars are looking for. The 1980s was a decade of brutal warring and socio-economic trauma, and was followed by stabilisation, and growth the prerequisite for which was, according to the policy makers, the elimination of Shining Path.

Contrary to the Colombian literature, the Peruvian costs-of-violence literature is small, basically due to the fact that the conflict ended before the theme became highlighted. Data production and quantitative analysis were much less developed in the country when the conflict was taking place than they are today. The few papers that can be found focus on a preliminary assessment of the economic implications of the loss of human lives (see, for example, CVR 2003 and Comisión Especial del Senado 1989)<sup>20</sup>. They are similar to the Colombian bullionist exercises, separating direct and opportunity costs, without inclusion of a detailed balance sheet. Frequently basic information (for example, the costs calculated in constant or current units) is not provided.

However, the Peruvian case has one extremely relevant lesson for the costs-of-violence literature: we should be very concerned about the quality of the data, and understand clearly that in some situations it is impossible to draw statistical inferences from certain data sets. I think a consensus on this problem can start from the following premises: first, marginal or relatively small amounts of noise (wrong, incomplete or vague information) may not be so terribly damaging provided the conclusions are robust; second, transforming imperfect data (by logarithmic or categorical transformations, for example) is frequently a good solution, and missing data issues are also treatable; and third, quality problems cannot be waived away, because sometimes they do become quite serious. What the Peruvian case shows is that a naïve approach to the issue of quality can produce conclusions that are significantly inaccurate. While in Colombia one suspects that in many situations the data is critically poor, in Peru it was possible to actually demonstrate that this was so, and to what degree. When studying violence, researchers seldom have at hand censuses, or random samples. Instead, they have ‘convenience samples’, which are deeply biased. Because they are not random, convenience samples do not fulfil even the minimal requirements to be submitted to statistical treatment, and inferences based on them are spurious. The best tool to unbiased the analysis is

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<sup>18</sup> Some authors do understand that these types of mistakes should be avoided. See for example 1998.

<sup>19</sup> Some special army units performed terrorist operations against the opposition and population. The state and the army buttressed the creation of peasant Defence Committees, but these acted as a civilian support base, not as an armed actor.

<sup>20</sup> Reasonably enough, Peruvians are much more focused on evaluating the human and social losses that the conflict entailed.

multi-systems estimation, a refinement of capture-recapture techniques that needs at least three samples and a set of technical preconditions (allowing the possibility of matching cases) to be used (see, for example, Ball 1996 and 2001). The Peruvian Truth Commission had adequate data sets, and commissioned a work to establish a reasonable approximation to the real number of casualties caused by the conflict. The figure jumped from more than 20,000 as initially found in the data sets to nearly 70,000 (Truth Commission (2003)). This is a case of brutal under-estimation, but of course because of the deeply political nature of issues related to violence, gross over-estimations can also happen. The message is simple: when evaluating political homicides, kidnappings and other types of violent events, the quality of information and inferences should be treated carefully.

**Reconsidering the cases**

*The deadweight perspective*

What is the size of the Colombian and Peruvian deadweight? Colombia showed a relatively good performance in the 1980s, which was a ‘lost decade’ for the rest of Latin America. From then on, it has achieved above-average growth. Three statistical exercises were performed to see if this was idiosyncratic. First, following CEDE (2003), a comparison was made of Latin American growth rates over two periods (1960-1970 and 1980-1990). If Colombia had grown at the average Latin American, or Latin American rural, rhythm, it would have fared worse than it actually did. If Peru had grown at the average Latin American, or Latin American rural, rhythm, it would have had a basically identical performance. Several statistical tests using different counterfactuals and techniques were performed, and not one suggests the existence of a deadweight. Second, a MANOVA (multivariate analysis of variance), basically repeating the Ammons (1996) design, was employed. A set of ‘control’ countries with a similar level of development and type of economy, and which had not experienced war, was established (Bolivia, Ecuador, Honduras, Costa Rica and Uruguay), and compared to a second set up of countries that did experience war (Colombia, Peru, Nicaragua, El Salvador and Guatemala). The results showed that the differences between them were not statistically significant (Tables 2 and 3). Actually, there are some anti-intuitive results related, for example, to foreign investment, although these might be idiosyncratic. A panel exercise for the period 1980-2006 was made, separating countries that had, and had not, internal conflict. Table 4 shows that there are no statistically significant differences with respect to growth.

**Table 2:**

Effect		Value	F	Hypothesis GI	Error GI	Significance
Violence	Traza de Pillai	1,728	4,759	8,000	6,000	,037
	Wilks’ Lambda	,006	6,219(a)	8,000	4,000	,048
	Traza de Hotelling	47,168	5,896	8,000	2,000	,153
	Roy’s Major Root	44,170	33,127(b)	4,000	3,000	,008

a. Exact statistic

b. The statistical is F’s superior limit that offers an inferior limit to the significance level

c. Design: Violence

**Table 3:**

<b>GDP growth</b>	<b>Coef.</b>	<b>Std. Err.</b>	<b>Z</b>	<b>P&gt; z </b>	<b>[95% Conf. Interval]</b>	
<b>Violence</b>	0.352	0.796	0.44	0.658	-1.208	1.913
<b>FDI net</b>	0.000	0.000	0.17	0.865	0.000	0.000
<b>Gross savings</b>	0.021	0.039	0.53	0.594	-0.055	0.096
<b>Services</b>	-0.009	0.048	-0.2	0.844	-0.103	0.085
<b>Trade</b>	0.007	0.011	0.61	0.542	-0.015	0.028
<b>Agriculture</b>	0.032	0.056	0.57	0.566	-0.078	0.142
<b>Manufacturing</b>	0.007	0.065	0.11	0.909	-0.119	0.134
<b>Mortality</b>	-0.032	0.014	-2.3	0.021	-0.060	-0.005
<b>Constant</b>	3.349	3.798	0.88	0.378	-4.095	10.793

**Table 4 : Panel Para Latinoamerica 1980 - 2006**

	<b>Dependent Variable</b>		
	<b>Growth GDP</b>	<b>Growth GDP</b>	<b>Growth GDP</b>
<b>Conflict</b>	-1.3382 (significant to 5%)	-0.1537	0.3520 (0.7961)
<b>Agriculture</b>		-0.0095	0.0322 (0.0561)
<b>Gross Savings</b>			0.0205 (0.0385)
<b>FDI</b>			1.42e-11 (8.33e-11)
<b>Services</b>			-0.0094 (0.0479)
<b>Trade</b>			0.0067 (0.0110)
<b>Manufacture</b>			0.0073 (0.0645)
<b>Mortality</b>			-0.0324 (0.0140)*
<b>Constant</b>	2.838235	2.9553 (significant)	3.3494 (3.7980)
<b>Rho</b>	0	0	0.04056981
<b>Wald</b>	0.0340	0.9276	0.3601

The lack of a deadweight is a puzzle in itself. Perhaps the opportunities that the conflict offers to illegal agents explains part of it (see below). Please note that a rough comparison between Peru's behaviour in the 1980s and the 1990s would suggest a sharp difference in favour of the latter. But this shows nothing, as the gist of the problem is the actual performance with respect to what would have happened had there not been conflict.<sup>21</sup>

<sup>21</sup> In other words, we should not compare actual Peru in 1980 with the actual Peru in 1990, because there are potentially infinite confounding factors. In the Peruvian case it is particularly obvious that the behaviour of growth was much more associated with economic policies than with the zigzags of the conflict – plugging in a dummy for policy and a small time lag has much more explanatory power with respect to growth than anything else. We have to compare the actual Peru with a counterfactual Peru in the 1980s, actual and counterfactual in the 1990s, and so on.

***The bullionist perspective***

How should the bullionist perspective be built? I would advocate four complementary ways of dealing with the problem:

- a) The operations involved should be guided by an explicit criterion of relevance. If the main interest is to contribute to the analysis of the impact of the conflict on growth<sup>22</sup> then only items that appear in the national accounts should be considered.
- b) Side effects ought to be taken into account. Let us discuss the attacks against the national infrastructure in Colombia. It may be the case that sometimes they produce a reaction – in the form of investment and expansion – that more than offsets the initial destruction. For example, if a road is destroyed it may be the case that the state rebuilds it and creates a supporting network to permit the rapid access of troops in case of future attacks. The cost of attacks against the infrastructure in Colombia between 1985 and 2004 was 335,197.28 million pesos (nearly 168 billion dollars). Making a bivariate correlation between attacks against the infrastructure (the violence variable) and public investment in it, we can obtain an estimator of this ‘anti-intuitive externality’. Once it is calculated we can indulge in the standard reasoning – given the coefficient B, we propose that one percent increase in attacks increases investment in B. Then we multiply this by the delta of yearly growth of costs, and subtract it from the delta of yearly growth of investment. The simple formula is presented in Table 5, whilst the results are shown in Table 6 and it can be seen that while a fair amount of destruction appears to have produced no anti-intuitive externality in transport infrastructure, the opposite seems to be the case for electrical infrastructure.<sup>23</sup>

**Table 5:**

<b>Component</b>	<b>Cost</b>	<b>Net effect</b>
Infrastructure	335197.28 million pesos	$(\Delta \text{ Cost}) - (B * \Delta \text{ infrastructure investment})$

<sup>22</sup> As said above, this is always only a secondary tool, as the deadweight counterfactual is indispensable, even if the bullionist exercise is done well.

<sup>23</sup> As said above, the Peruvian case counts on worse data (it was shorter, it took place before costs-of-violence literature became important, etcetera). Curiously enough, though, using a proxy for investment (installed electrical capacity), in Peru the response to terrorist attacks in the electrical sector would also seem to have more than offset the initial losses.

**Table 6:***In million pesos at 2004 rate*

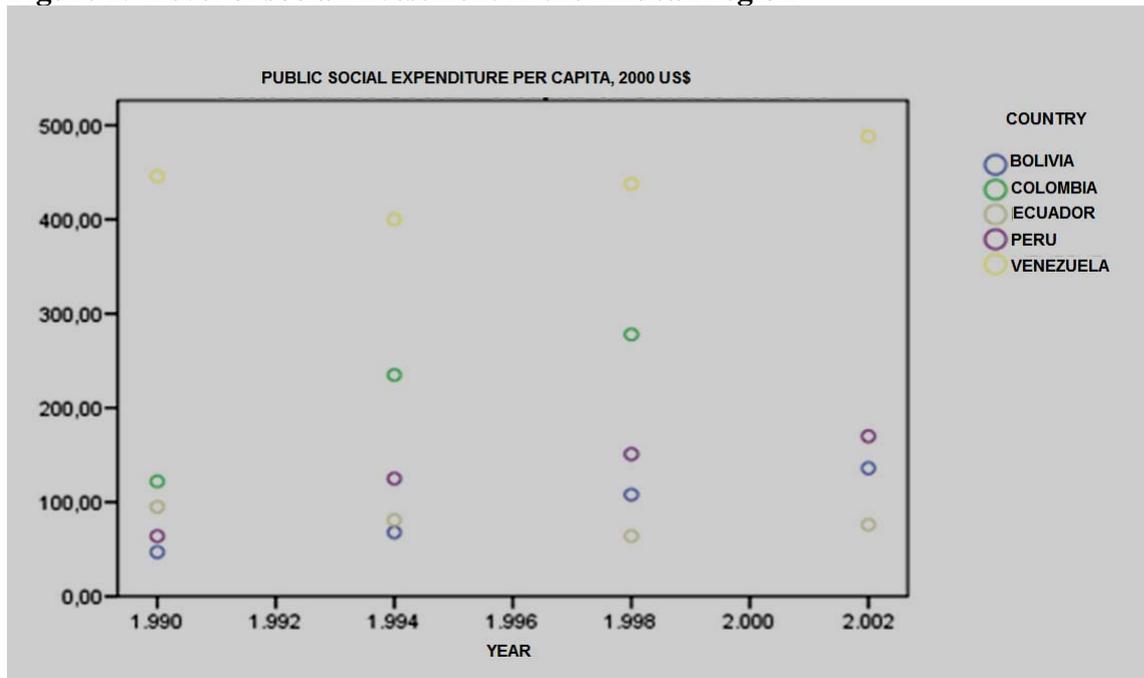
	<b>Net cost road Infrastructure</b>	<b>Net cost air infrastructure and railway</b>	<b>Net cost electrical sector</b>	<b>Net infrastructure effect</b>
<b>1987</b>	0.00	0.00	181.10	181.10
<b>1988</b>	0.00	0.00	265.59	265.59
<b>1989</b>	0.00	0.00	59.73	59.73
<b>1990</b>	0.00	0.00	-601950.18	-601950.18
<b>1991</b>	0.00	0.00	-192118.17	-192118.17
<b>1992</b>	0.00	0.00	-55820.30	-55820.30
<b>1993</b>	0.00	0.00	-377316.12	-377316.12
<b>1994</b>	8773.75	42919.43	-492913.70	-441220.52
<b>1995</b>	0.00	0.00	-692123.73	-692123.73
<b>1996</b>	0.00	0.00	43137.49	43137.49
<b>1997</b>	25834.67	93005.80	1113790.59	1232631.06
<b>1998</b>	24407.38	18711.56	-851731.41	-808612.47
<b>1999</b>	22830.22	4098.46	87004.81	113933.49
<b>2000</b>	38074.03	19281.65	-394730.98	-337375.29
<b>2001</b>	8486.60	0.00	1412303.27	1420789.87
<b>2002</b>	26220.17	0.00	-19437.22	6782.95
<b>Total</b>	154626.8157	178016.9088	-1021399.23	-688755.5009

- c) Indirect impacts act in different directions. As noted above, the indirect impacts of the conflict have been counted in a very one-sided way. For example, the argument according to which the presence of a conflict opens windows of opportunity to a huge amount of opportunistic illegal activity is irrefutable. These undertakings are a cost to the state. But at the same time they contribute to the economy in two ways: as an activity in its own right, and through spillovers that feed the legal sectors. Both dimensions should be captured<sup>24</sup>. If this is done, the results are different from what Colombian and Peruvian scholars normally conclude. The contribution of conflict to GNP growth – through facilitating narco-trafficking as in Peru, or directly promoting it as in Colombia – may have been positive in both countries, though this conclusion of course will depend on the quality of the guesstimates of the weight of narco-trafficking in the economy.
- d) The expansion of the security sector has to be evaluated. It impacts on the development of infrastructure and employment (as is also the case with armed challengers), but it also has a territorial dimension that is not captured well by standard economic studies (due to the armed challenge the state is forced to establish its presence in places where it had been regularly absent). Once more, taking this into account bullionist calculations do not sit well with conventional wisdom.
- e) Other state policies should not be assumed away. The standard claim is that the conflict forces the state to divert resources from social policies to military investment.

<sup>24</sup> This does NOT contradict point (a). Narco-trafficking is not counted in the national accounts, but it is relevant because it contributes to growth in the way the Colombian state presently conceives it. This does not apply to environmental externalities.

In Colombia, this does not hold: not only has the country's social investment been permanently above the regional average during the conflict (see Figure 1), but the military investment took a very long time to grow; when it did, the health and education budgets were not deflated. This may be quite idiosyncratic (US military aid may have allowed social spending to continue), but the general point is not: the state reacts in different ways to armed challenges, and one of them is combining repression and redistribution (which is also true in Peru and El Salvador, among many other cases).

**Figure 1. Level of social investment in the Andean region**



*Source: Calculations based on CEPAL, PANORAMA SOCIAL DE AMÉRICA LATINA, 2005<sup>25</sup>  
Spss graphic*

### Distributional problems

For many reasons it can be comfortably conjectured that wars affect the poorest and the most vulnerable. First, they are the most likely candidates to be cannon fodder. Second, they have no capacity to pay private security, so their assets and income can be looted by warlords. Third, and following the same logic, they seldom, if ever, are able to pay for private access to electricity, education, health, etcetera, when state infrastructure is destroyed. If they ever leave the battlefield, they end up in refugee camps. This is a very important part of the story, but there are many other ones. Armed conflict – whether initiated by the state or the rebels – can have distributional objectives, as in South Africa, the US civil war and the communist-agrarian revolutions of Asia. Sometimes, large quantities of wealth are passed from the rich or the middle class to the poor (especially the rural poor). Risk prone, or unlucky, poor people that join an army may have previously been unemployed. If the assumption that this

<sup>25</sup> At <http://www.eclac.org/cgi-bin/getProd.asp?xml=/publicaciones/xml/4/23024/P23024.xml&xsl=/dds/tpl/p9f.xsl&base=/tpl/top-bottom.xsl>

always, or generally, holds (Collier and Hoeffler 2004) is clearly unwarranted (Gutiérrez 2008), it may sometimes happen. More substantially, getting enlisted is in many contexts a very central, sometimes the only, channel for upward social mobility. During the Latin American independence wars, but even today – for example, US recruitment for the Iraq conflict – thousands of people joined the army to become enfranchised and acquire full citizen status. In Britain, women obtained the right to vote in return for their participation in the war effort<sup>26</sup>.

How can all this be put together? A distributional analysis of the conflict should identify at least the following dimensions:

- a) Programmatic effects – sometimes the combatants intend to take over the assets, income or political rights of their adversaries or third parties;
- b) Incentive systems – to boost combat morale, to mobilise different sectors of the population, the state and its challengers distribute prizes and punishments. It may be the case that the state, the challengers, or both, are warring about rents;
- c) Patterns of action – even without being conscious of changing the state of the world in a certain direction, armed actions can generate radical changes that in turn produce massive redistribution;
- d) Organisational structures – the type of organisation that participates in a conflict is a crucial variable, and can have long-term implications over the patterns of distribution that prevail in a given country;
- e) Base lines – conflicts do not take place in a distributional vacuum. For state challengers, the ability to lure recruits into their ranks and to promote high-risk collective action depends on the existence of a disaffected, and/or risk-prone critical mass.

The first two are distributionally motivated, the next two are repertoire driven and the last one constitutes the socio-economic and institutional dimension. Keeping in mind the different types of distributional dimensions, let us turn our attention to our case studies.

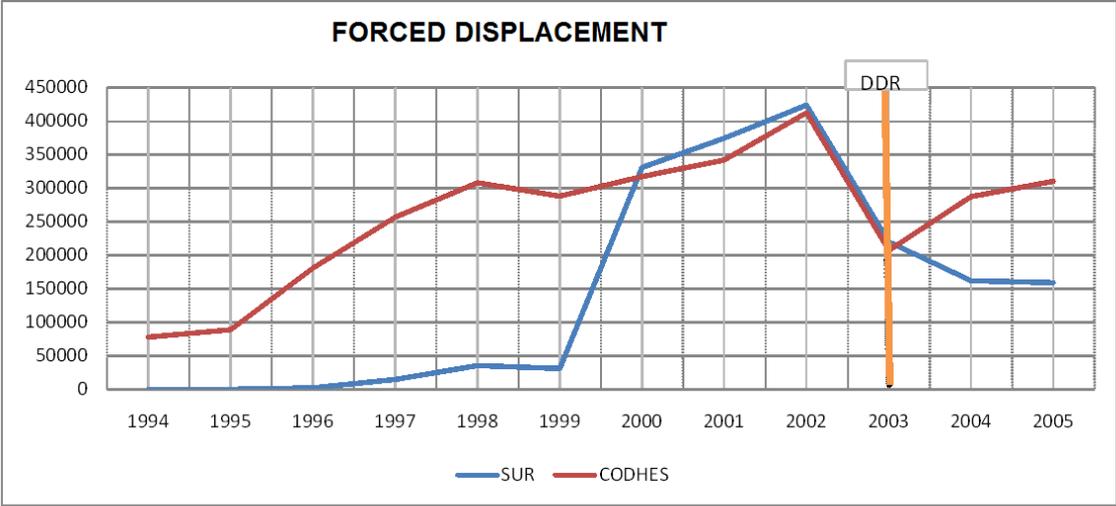
### ***Colombia***

Due to its conflict, Colombia has suffered an inverse agrarian reform in the last twenty years. Hundreds of thousands of people have been displaced. Their lands were taken over by guerrillas and landlords, but in particular by the paramilitary. Despite the fact that this is quite visible and constitutes a serious and unresolved policy problem, no consensus has been arrived at with respect to how many people have been displaced in Colombia. The estimate of the true number of victims has been fraught by technical and political difficulties. The difference between the figures offered by the state and by the main NGOs is sharp (see Figure 2). The human capital lost – which has been estimated in different manners by the Planning Department and other agencies – also mainly hurts the poor.

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<sup>26</sup> Note the difference with South Africa. Here the reason for the conflict was enfranchisement and equality for Blacks. The war waged by Britain against the Nazis was not about the political rights of women, but it offered the motive and the context to institutionalise them.

**Figure 2. Displaced people data sets in Colombia: contradictory assessments**

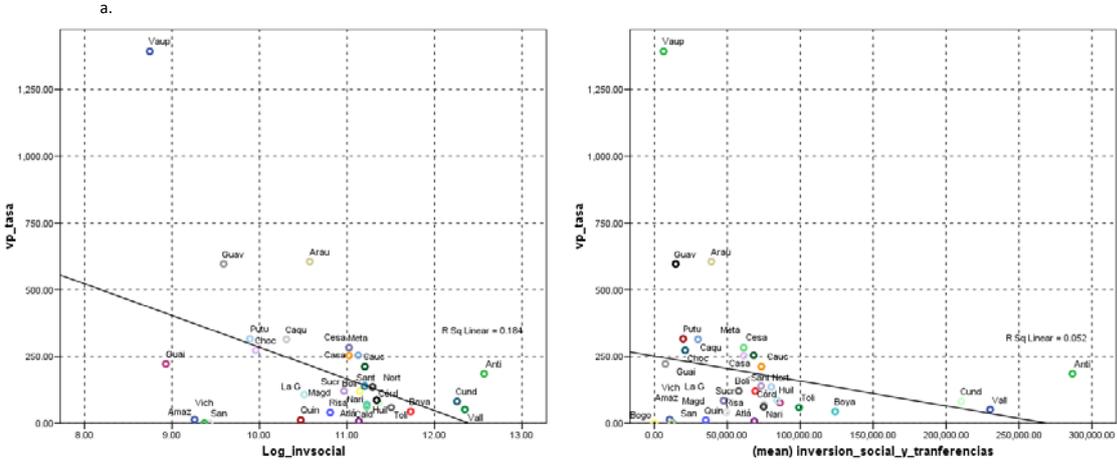


\*Annual displacement data - Total population  
Source: OCHA

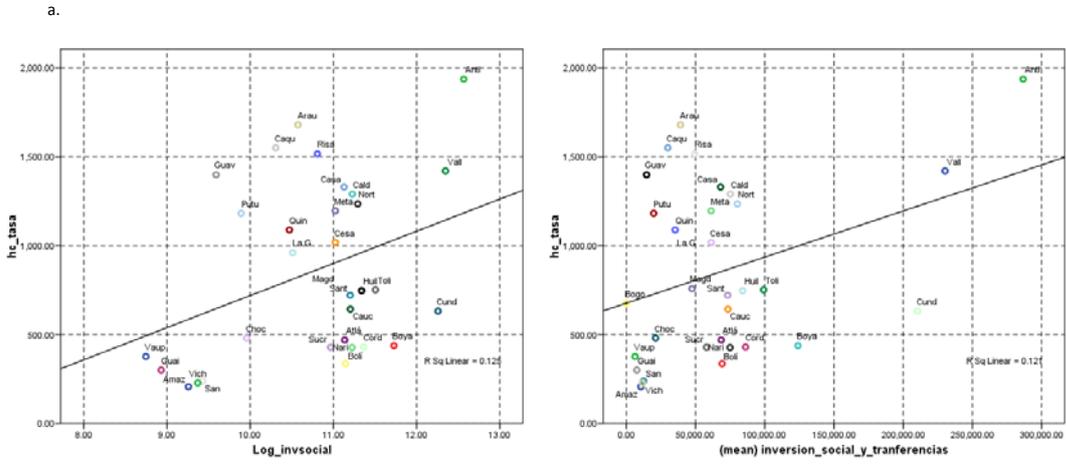
Source: Gonzalez (2007) Master’s degree dissertation

This is by far the main programmatic result in Colombia, though there are others. The country has been at war for more than three decades, and over the same period there have been ongoing peace negotiations. Those which have come to fruition have almost always had distributional clauses, mainly with regional emphases. Much more serious are the consequences of targeted policies, designed specifically to tame violence through the provision of services to the population. It can be shown that these ‘prophylactic’ designs were already an obsession of the country’s policy makers long before the onset of the present wave of conflict. Perhaps the best example of this is the Plan Nacional de Rehabilitación (National Plan for Rehabilitation) that intended to combat violence by fighting poverty. The Plan was issued in 1982, and tried to target the poorest municipalities to prevent violence. After it ended, its programme was assumed by other agencies; and it appears that there is an association between investment and transferences from the central state to subnational units between 1990 and 2005, and violent events (Figures 3 to 6) as captured by four databases (Table 9 to 11).

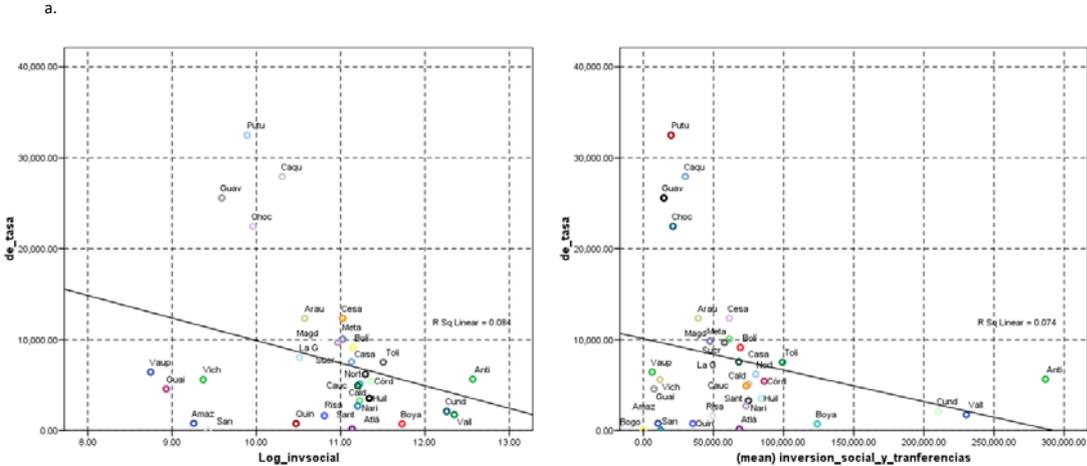
**Figure 3: Annual weight: Political Violence rate for 100 thousand habitants vs Net Social Inversion at constant prices**



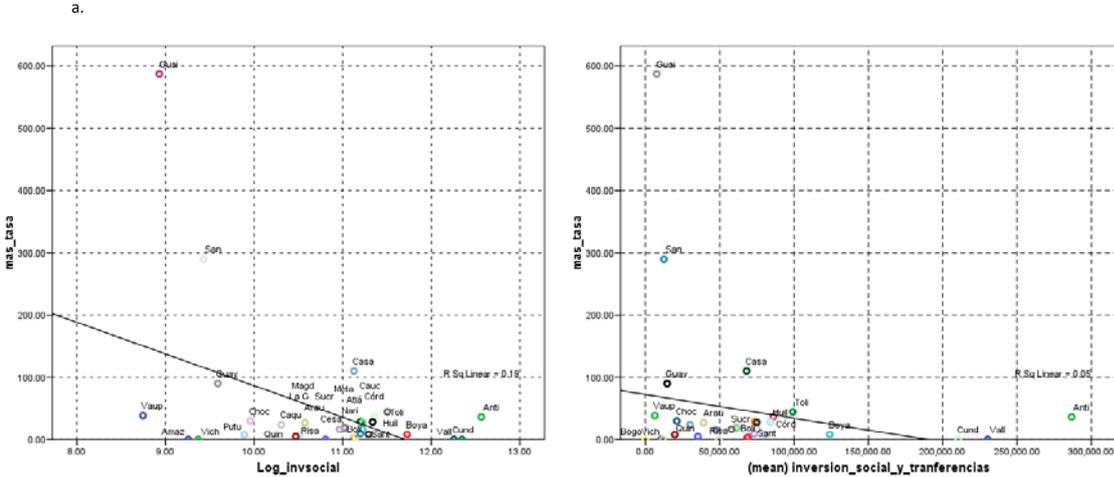
**Figure 4: Annual weights: Common homicide rate per 100 thousand habitants vs. Net Social Inversion at constant prices**



**Figure 5: Annual weights: Expelled displacement rate for 100 thousand habitant vs Net Social Investment at constant prices**



**Figure 6: Annual weights: Massacre victims rate for 100 thousand habitants vs Net Social Investment at constant prices**



**Table 9:**

Variable	Source	Temporality
Forced displacement	Acción Social	1990-2005
Political Violence	Francisco Gutiérrez Sanín	1990-2005
Massacre victims	DNP	1990-2005
Common homicides	DNP	1990-2005
Social investment transfers	DNP	1990-2005

In three cases (displacement, political homicide and massacres), the relationship is decreasing: the number of violent territories (municipalities where more homicides take

place) decreases. There is one exception: common homicide, where violent territories increase. I would draw two conclusions from this. First, the state ‘sees’ selectively those critical indicators that can and should be responded to, not only with force but with investment. This is related to both political perception (notions of order, how visible the problem is, etcetera) and feasibility (sometimes investment is not possible, or it is difficult, for lack of bureaucracy, loss of control, etcetera). Second, changing the proxies can change the sense of the association. No amount of care is enough in this realm.

The Colombian costs-of-violence literature has been keen on incentive systems, trying to understand the conflict as a ‘war system’ (Richani 1997), where the protagonists of the war are motivated by their economic gains. Indeed, the Colombian conflict is fuelled in good measure by coca and other illegal activities like kidnapping and racketeering, without which it would not be sustainable. The calculations of the National Department of Planning and other agencies and commissions show – even factoring in the weakness of some of them – that the income of the guerrillas is huge<sup>27</sup>. This, unfortunately, does not solve the Fearon puzzle. If the income is so good, why not enjoy it peacefully? These types of goods are divisible, so bargaining about their distribution should not be so difficult. Indeed, currently in Colombia several agents are able to do so – and avoid incurring the huge costs and risks of war. Also, how can the multiple peace processes be explained? Why do rent seekers suddenly decide to abandon their bad ways and come back to legality? On the other hand, empirical evidence suggests that a very substantial portion of the guerrilla soldiers, who earn no salary, were previously employed in the legal or illegal sectors, with income levels above the average rural salary (Gutierrez 2008).

Less spectacular incentive-related distributional issues have been left aside. For example, military service has also extended to ever newer layers of the population. However, in Colombia this has had no relevant egalitarian implications, mainly due to the fact that the rich and the middle classes can easily evade service<sup>28</sup>. Given the growing size of the army, there must be some impact over employment. We also know that some patterns of action have a long range economic and political effect: for example, cattle ranchers have long been the main victims of kidnapping, while peasants in their turn are the main victims of massacres.

### *Peru*

In Peru there was no peace accord; the state obtained a military victory. Peruvian scholars claim, following the Truth Commission (2003), that the main distributional programme has a racist bias against the *cholos*, who were by far and large the preferred target of all the contenders: the war was racist. This seems to be inexact; we are not speaking about motivational effects (at least not necessarily), but of patterns of action and impacts on organisational structures. The question is not purely terminological, because it bears upon the interpretation of the meaning of the war.

The Peruvian Army had, as with the Colombian policy makers, a prophylactic redistribution obsession, but for different motives and with different traditions and implications. They had managed a reformist dictatorship (1968-1978) that implemented a basically successful agrarian reform. In the 1980s, the Army switched from a scorched earth policy to one that combined repression with social policies. Critically, the Peruvian army knew with whom to

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<sup>27</sup> I am not aware of a similar estimation about the paramilitary.

<sup>28</sup> It may be the case that wars in which these backward recruitment patterns appear are particularly “non-Tillian”.

speak in the country – the layer of intermediate peasants created by the reform – and was keen to organise them. This systematic use of peasant organisation against the guerrillas ultimately proved to be the key to the success of the Peruvian state. Here the difference between Peru and Colombia appears to be related to the baseline: in the latter the state did not count on any sound and legal interlocutor, so it has acted in a vacuum; in the former the previous agrarian reform enabled it to create a social protective fabric supported by all political actors (including the legal left).

Shining Path - a very brutal group - was much less embedded in the coca economy than its Colombian equivalents. Its link with coca was regionally circumscribed, and restricted to the final years of the conflict. They were also not heavy users of tactics like kidnapping. As in Colombia, the guerrilla organisations were quite inert distributionally in the territories where they had influence, and were much more intent on the moral education of the people than on an egalitarian agenda (in Colombia, in some regions they have actually opposed redistributive drives promoted by grassroots organisations). In Peru the guerrillas targeted quite brutally and systematically members of the legal left, cooperatives, etcetera – but also peasant markets and fairs, which were considered reactionary<sup>29</sup>.

I believe the main distributional issue of the Peruvian conflict is a regional fracture, that is, a base-line problem. This fracture permitted the conflict to develop, and was not solved by it, nor by the post-conflict policies. It seems that the protest vote is particularly strong in the region where the conflict developed: where the 1968 agrarian reform basically failed, where the liberal economic policies of the 1990s were unable to produce a take-off, and where malaise and disenfranchisement still prevail. Colombia and Peru may be highlighting the persistent importance of unresolved agrarian and territorial problems to understand the distributional logics of civil wars.

## Conclusions

The proposition that Tillian wars – that is, wars that advance state building and improve human welfare in the long-run – are confined to an historical moment long since past is hardly credible given recent history. Yoweri Museveni's National Resistance Movement in Uganda fought a guerrilla war culminating in a victory in 1986 that allowed a large part of the country to enjoy its longest period of peace and economic development since independence (Mutebi 2008a). The Rwanda Patriotic Front waged a war against an exclusionary Hutu-dominated regime and achieved victory after opponents in power carried out a massive genocide in 1994. It has since presided over a decade of peace in the country and provided the only possibility of beginning to build a more inclusive polity and a process of economic development (Mutebi 2008b). In Somaliland, war has led to the constitution of a state, still unrecognised by the international community, which appears to be providing both peace and prospects for development to inhabitants of its territory not possible since the colonial period (Ahmed and Green 1999). The theoretical possibility that the benefits of war may outweigh the costs must still inform our analysis of the costs of violence.

There is a further complication. Throughout this paper I have assumed that 'civil war' is a well-defined category. But is this true? There may be classificatory problems: providing an explicit definition of what a civil war is, can I tell with only marginal probability of error the positive events from the rest? There are in reality two types of problems here: first, separating

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<sup>29</sup> Here programmatic and repertoire driven effects blend.

wars from non-wars; and second, evaluating the consequences of different types of wars. For example, it may be the case that massive confrontation produces some types of effect, while low or medium intensity ones produce others (or none). To illustrate this, I undertook the analysis of one type of violent event: military interventions. I used the military interventions dataset because it was already adequately ‘fuzzyfied’ – i.e. the ‘degree’ of the intensity of intervention was already captured in the coding<sup>30</sup>. The conclusion is quite intuitive: ‘big’ interventions (those beyond a degree of intensity of 0.7 over a maximum of 1) are more consequential than others<sup>31</sup>. Events such as the entry of the USSR into Afghanistan, or of the USA into Iraq have different consequences to, for example, hot pursuits across a border. While the first have a significant, high negative impact on the evolution of GNP per capita, the others have a non-significant, low, though still negative one. This may sound self-evident, but it is precisely the type of thing that costs-of-violence literature and other analyses are unable to capture, because they collapse all events, big and small, which may hide highly differentiated behaviours<sup>32</sup>.

The costs-of-violence literature, in sum, has failed to address several key problems. This might be the product of the predominance of a narrow liberal understanding of development and conflict (Cramer 2006) and the collapsing of economic and normative values (if it is bad it is expensive, and vice versa) – but also it may be due to the lack of analytical checks and balances<sup>33</sup>. I have suggested here that the evaluation of the costs of violence (or of war) should distinguish three basic dimensions of analysis: bullionist, deadweight and distributive. The first one is threatened by inexact data and poor inference (in the double sense of using non-random data sets to extract probabilistic conclusions and to produce incomplete accountabilities as if they were genuine deadweights). The second one is as yet to be solidly identified, but it is the most promising, provided base lines, counterfactuals and growth models are adequately built. The third one is by far the least explored, and seems to offer several complications that have not been taken on board. All three share some serious challenges that the costs-of-violence literature has basically ignored. For example, lack of specification (what can the term ‘cost’ mean when we are speaking about impacts that span several generations); severe classificatory issues (whether there are differential effects according to the magnitude or type of armed conflict); global impacts (do deadweights vanish, or are they transferred from one nation to another); trade offs (how to evaluate costs when a generation sacrifices economic well-being so that another one obtains political rights). Employing a more careful approach to assessing the costs of war may help in understanding why actors resort to war, and why and how war may counter-intuitively lead to patterns of increased growth and welfare – at least for some people in some places at particular moments in history. Most importantly, if attempts to measure the costs of violence and war are placed on a sounder footing they might better be able to contribute to policy interventions that allow all actors, as Fearon’s rationalist puzzle puts it, to deduce post-war outcomes and thus strike bargains without incurring the human costs of war.

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<sup>30</sup> Instead of the habitual dummy. For an in depth discussion of the classificatory problem, see Gutiérrez and González, 2008

<sup>31</sup> This is still incomplete, because I am using only one differentiating criterion, magnitude.

<sup>32</sup> In the case of invasions, analysts take all of them together, without separating big and small ones, and consequently find that they have no economic effect. See for example Gutiérrez and González, 2008.

<sup>33</sup> The data is poor and as suggested above can be wrong by orders of magnitude; nobody is held responsible for inexactitudes with respect to what war lords or narcotraffickers do, etc.

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