DOES INEQUALITY CAUSE CONFLICT?

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Abstract: This paper suggests that economic inequality is important to explaining civil conflict, but that the links are not as direct as is often supposed. It is important to focus on the variety of ways in which inequalities are managed by societies, and the significance of varying *kinds* of inequality. It is also important to understand the transmission mechanisms that enable a relatively peaceable durable inequality to turn into a violent conflict. These considerations, together with the poor quality of the available inequality data, should make us more cautious about the conclusions reached by cross-country empirical studies into the causes of conflict which ascribe a strong predictive power to measures of inequality. Copyright © 2003 John Wiley & Sons, Ltd.

1 INTRODUCTION

The role of economic inequality in economic growth and in the political economy of violent conflict has remained elusive. This paper discusses why. One problem is how weak the empirical foundations remain for any argument or finding based, especially, on inter-country comparisons. Another is that there are common problems in the way in which we define and analyse inequality as well as shortcomings in our ability to measure it. Some treatments of the inequality/conflict relationship, for example, suggest the presence of transhistorical consequences of given degrees of income or asset inequality. In contrast to these explanations—with their slash and burn approach to historical specificity and their rape and pillage of the 'social'—an alternative perspective on inequality may offer greater insights for the understanding of violence. This alternative is determinedly relational and historical: it starts not from some superficial outward signs of inequality, for example the Gini coefficient, but from the historically conditioned social relations that, given their infinitely open set of specificities, nonetheless sometimes produce similar outward signs. This perspective also stresses that it is not so much the *extent* of inequality as the *kind* of inequality that is likely to matter. The paper ends with a short introduction to how this perspective can be applied usefully to analysing violent conflict in Angola and Rwanda.

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2   HOW ORTHODOX ECONOMICS ANALYSES INEQUALITY

After enthusiasm for the suggestiveness of the Kuznets Curve waned under closer scrutiny, interest among economists in inequality faded. However, in the past few years inequality has 'come in from the cold' (Atkinson, 1997). This has partly been because of a change of direction to the inquiry. Rather than continuing to explore for some systematic distributional outcome from economic growth, economists have increasingly focused on the effect of distribution on growth. Through either a 'human capital' mechanism or a political mechanism it has been argued that inequality constrains investment and—given the rehabilitation of investment in so-called new growth theory—therefore slows growth. There has been a growing literature in this vein, claiming that there is a general pattern according to which inequality is bad for growth. It is not clear if this claimed association, like the Kuznets Curve at its peak of popularity, has yet 'acquired the force of economic law' (Robinson, 1976). However, its generally appealing conclusion has helped earn it widespread credibility.

Accumulation of 'human capital' and its contribution to growth is limited if persistent inequality perpetuates across generations a misallocation of investment in education. Hence, in this case, the wealthy commit resources to high quality education of their offspring irrespective of their innate intelligence while bright children of poor families cannot afford proper schooling. Poor labour market opportunities and a lack of incentives to entrepreneurial innovation among the disadvantaged reinforce this private aversion to investing in education.

Alternatively, inequality can constrain growth through political effects. In democratic but significantly unequal countries—so the argument goes—the median voter presses for a government that taxes capital in order to fund redistribution. However, this taxation works as a disincentive to invest and consequently restricts economic growth. Taken further, however, inequality might prompt some people to resort to cruder means of making their claims, i.e. by violence. Hence, Alesina and Perotti (1996) argue that inequality is correlated with a greater incidence of political instability (measured by indicators such as the number of political murders annually) and that, in turn, political instability is a statistically proven disincentive to investment.[1] Therefore, again, inequality indirectly reduces the possible rate of growth. At the extreme, of course, this same chain of causality might bring about not isolated political assassinations but full-blown organized violence, for example in the form of a civil war.

Here the recent endogenous growth literature, with its foundations in neo-classical economic theory, converges with an older argument in political economy and political science that one of the most important sources of civil war is a high degree of inequality of wealth and income (e.g. Gurr, 1970; c.f. Boyce, 1996, on El Salvador; and also on Latin America Booth, 1991 and Wickham-Crowley, 1992).[2] These arguments may have been influenced—as Binswanger *et al*. (1995) argue—by the works of Moore (1966), Wolf (1999) and Scott (1976), among others. However, while the focus of these seminal works was certainly on tensions in rural political economies, they in no way lent themselves to a straightforward understanding of the role of economic inequality while all of them identified the causal significance of profound changes in social relations rather than in quantified degrees of inequality *per se*. Nonetheless, Binswanger *et al*. (1995) elide a range

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[1]That instability might not necessarily be a disincentive to investment is shown, for the case of foreign investment in Nigeria, by Frynas (1998).

[2]For a critique, highlighting the lack of correspondence between conflict (and the end of conflict) and obvious changes in inequality as opposed to shifts in regional ideology, see Grenier (1996).

of very different examples of principally rural violent conflict into a set of outcomes produced by unequal land distribution. Cases as divergent as El Salvador and post-independence Mozambique come to support the claim that as well as incurring static and dynamic efficiency costs, large farms and the land inequality that goes with them entail social costs including unrest and civil war (Binswanger *et al.*, 1995, p. 2060).

There are divergent views on how significant, as a determinant of political violence and civil war or revolution, is land distribution as distinct from nation-wide income distribution. Muller and Seligson (1987) argue that national income distribution is far more significant than land distribution. They claim that national income inequality is a strong predictor of political violence even where land distribution is relatively equal, but that maldistribution of land is a weak predictor of such violence where national income inequality is not extreme. The explanation rests on the assumption (among others) that rural populations are difficult to mobilize effectively for political action.

Some of the recent literature on the economics of conflict and/or complex humanitarian emergencies agrees that inequality is a strong predictor of conflict. The main example is Nafziger and Auvinen's (1997) analysis of humanitarian emergencies. Their regressions 'indicate that high income inequality (measured by a Gini coefficient) is associated with political conflict and complex humanitarian emergencies' (Nafziger and Auvinen, 1997, p. 41; c.f. Nafziger and Auvinen, 2002, p. 155). This convergence of opinion might suggest a consensus. However, although there is widespread support for these kinds of argument it is not inevitable that either neo-classical economics or non-orthodoxy development economics produces the conclusion that a higher Gini coefficient leads to a greater probability of armed conflict or civil war.

An intriguing example of the way in which mainstream economics treats the relationship between inequality and conflict is found in two models constructed by Collier and Hoeffler (1996, 1998). These models make strong claims, based on foundations in rational choice methodological individualism. The authors set out to 'explain why [civil wars] occurred in terms of underlying economic variables' (Collier and Hoeffler, 1996, p. 1).[3] They conclude that four variables (income per capita, natural resource endowment, inequality and ethno-linguistic fragmentation) 'make a huge difference to the chances of civil war' (Collier and Hoeffler, 1996, p. 7).

In the first version of this model inequality does not cause conflict: in fact, 'greater inequality significantly reduces the risk and duration of war' (Collier and Hoeffler, 1996, p. 7). The data and empirical testing that show how inequality is *inversely* related to conflict are explained by a political argument. A high degree of inequality reflects the existence of a dominant elite whose attachment to the status quo means that they will allow the government temporarily to tax them in order to raise the funds for any necessary war effort to protect this status quo. Observing a high degree of inequality, prospective rebels will calculate that the government will have a greater capacity than perhaps otherwise to finance a military campaign against them, thus reducing the chances of success in rebellion. Inequality, then, is bad for conflict. In a revised version of that model, this clear and rationally justified role of inequality as disincentive to conflict vanished. Collier and Hoeffler (1998) claim that inequality is insignificant and makes *no* contribution to the probability of civil war. Collier (2000) states: 'Inequality does not seem to affect the risk of conflict. Rebellion does not seem to be the rage of the poor ... Conflict is not caused by

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[3]Much of the analytical groundwork for neo-classical economic thinking on conflict was laid out especially elegantly by Hirshleifer (1994).

divisions, rather it actively needs to create them . . . ' (pp. 10–11). Meanwhile, again using the same inequality dataset as Nafziger and Auvinen, Fearon and Laitin (2003, p. 20) find that whether in a bivariate model or part of a multivariate equation the Gini coefficient estimates 'do not come close to either statistical or substantive significance'.

Econometric analysis appears capable of supporting completely contrasting causal mechanisms for the same variable through modest changes in the specification of models.[4] Further, the rationalization of a given causal relationship involving inequality and conflict seems to be driven by an apparently arbitrary selection of assumptions (if high inequality has a given effect on the likelihood of conflict in one model through the fiscal preferences of the elite why does it not work this way in another model?). This does not give much confidence in the treatment in mainstream economics of the role of inequality in the origins of conflicts. The example highlights the fragile empirical grounds of this kind of analysis, it appears to illustrate the power of shifting intellectual fashions within the mainstream over the production of predictive models, and it further suggests that inequality is completely misconceived within this kind of economic model.[5]

3   THE WEAKNESSES OF THE EMPIRICAL FOUNDATION FOR CLAIMS ABOUT INEQUALITY AND DEVELOPMENT OUTCOMES

Both those arguments directly concerned with inequality and economic growth and those more interested in the distribution/conflict relationship are unstable empirically. Indeed, efforts to construct reliable event regularities across time and across countries involving inequality have never proven robust. There are significant weaknesses in the methodology employed in large data sets tested with econometric techniques applied to various models of causation among the variables. However, a more basic weakness lies in the quality of the data on inequality itself. As Fearon and Laitin (2003, p. 20) put it: 'The poor quality of the inequality data . . . does not allow us to go beyond the claim that there appears to be no powerful cross-national relationship between inequality and civil war onset'.

Distributional data are notoriously poor for the construction of time-series for individual countries and they are even worse for purposes of inter-country comparison. This point has been well established for some time. Despite the availability of improved data sets, the empirical basis of inequality research remains often fragile. Recent research in Latin America confirms the dangers of building causal arguments on inequality data. Székely and Hilgert (1999), analysing household survey data from eighteen Latin American countries, find that rankings among these countries in terms of conventional inequality indicators are illusory. Scores are driven by variations in the characteristics of the data and their treatment. Hence our 'ideas about the effect of inequality on economic growth are also driven by quality and coverage differences in household surveys and by the way in which the data is [sic] treated' (Székely and Hilgert, 1999, p. 1). This confirms the criticisms that have long been made of distributional data and the inferences that can be drawn from them (Fields, 1989; Moll, 1992; Bowman, 1997).

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[4]The sample size varies between Collier and Hoeffler's models, but not that dramatically. Collier and Hoeffler (1996) draws data from the Small and Singer dataset, including 61 countries of which 15 had civil wars in the 1960–1992 observation period. Collier and Hoeffler (1998) includes 98 countries of which 27 had civil wars over the same period. Collier and Hoeffler (1999) has a sample of 152 countries, of which 53 had civil wars between 1965 and 1995; however, there they state that for 'complete' war data they are restricted to 24 war episodes or, with some manipulation, 40.
[5]Cramer (2002) presents a critique of neo-classical economic theories of conflict more generally.
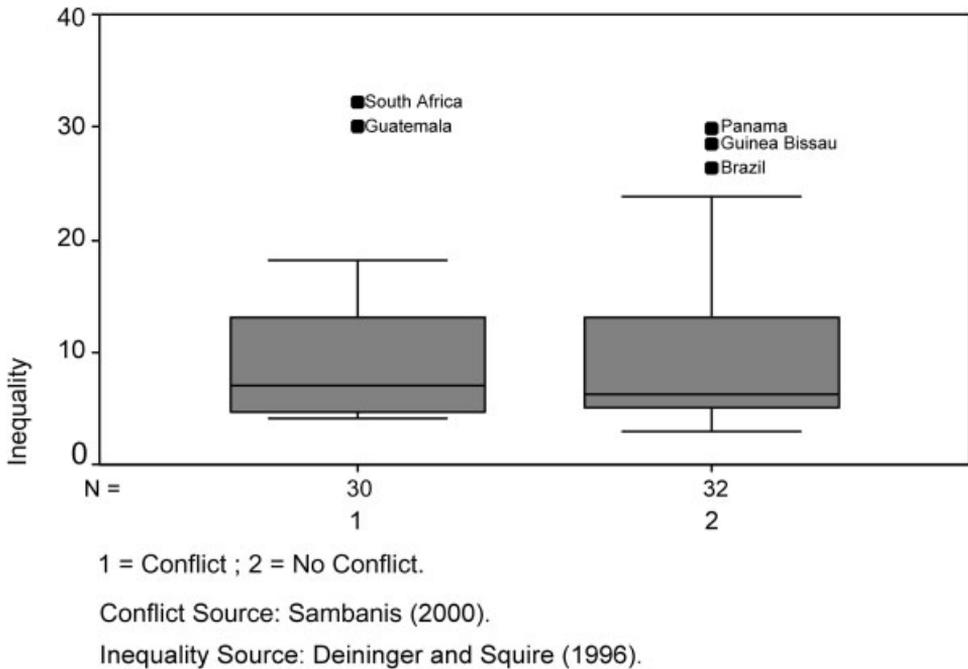
Figure 1. Civil Conflict 1944–2000

A cursory exploration of descriptive data is sufficient to demonstrate the lack of credibility in an overall pattern causally relating inequality to civil conflict. Figures 1 and 2 below show the distribution of conflict and non-conflict countries in terms of inequality. Figure 1 reports data for inequality and for civil wars between 1944 and 2000, drawing on the 'high quality' sub-sample of the Deininger and Squire (1996) dataset on inequality and on Sambanis (2000) for civil wars. Figure 2 relates the same inequality data to the Wallensteen and Sollenberg (1998, 2000) data on post-1989 armed conflicts.[6] The strong

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[6]Inequality data are taken from Deininger and Squire (1996) and include only those countries accepted in the smaller, 'high quality' sample within that dataset. Observations between 1988 and 1992 are included, on the basis that (a) generally, inequality measurements only rarely change significantly over time and (b) older observations are less likely to be reliable. However, the odd exception has been made to try to gain the maximum from the interaction of good quality distribution data with information on conflict: thus, e.g., Rwanda has been included because of its conflict and its inclusion in the 'high quality' dataset, even though its most recent 'acceptable' inequality observation was in 1983.

Conflict data are taken from two datasets: the Sambanis (2000) that covers 'civil wars' from 1945–2000 and Wallenstein and Sollenberg (1998, 2000) covering armed conflicts from 1989 onwards. Within this latter post-1989 dataset only those conflicts classified 'intermediate' (more than 1000 battle-related deaths recorded during the conflict but fewer than 1000 in any given year) and 'war' (more than 1000 battle-related deaths during any given year), whose sum is termed 'major armed conflicts' are included, and within this subset only those conflicts that can reasonably be termed 'civil wars'. Sambanis includes civil wars from 1944 up to those that had ended for two years or more by early 2000 but also includes eight ongoing conflicts. His definition of a civil war is like most others but it should be noted that the definition includes those conflicts in which there have been more than 1000 deaths, contrary to some that require more than 1000 annually.

Kenya (1991–1993), Mexico (1992–1994) and Romania (1989) are included in Sambanis but not Wallensteen and Sollenberg. Moldova is included in the former but in the latter only noted as a minor conflict. Shortcomings in the data for both conflict and inequality mean that we are left with a dataset only including those countries clearly experiencing civil wars and also offering 'high quality' data on economic inequality. Conflict-affected countries such as Zaire, Yemen, Sierra Leone, and Turkey are, therefore, excluded.
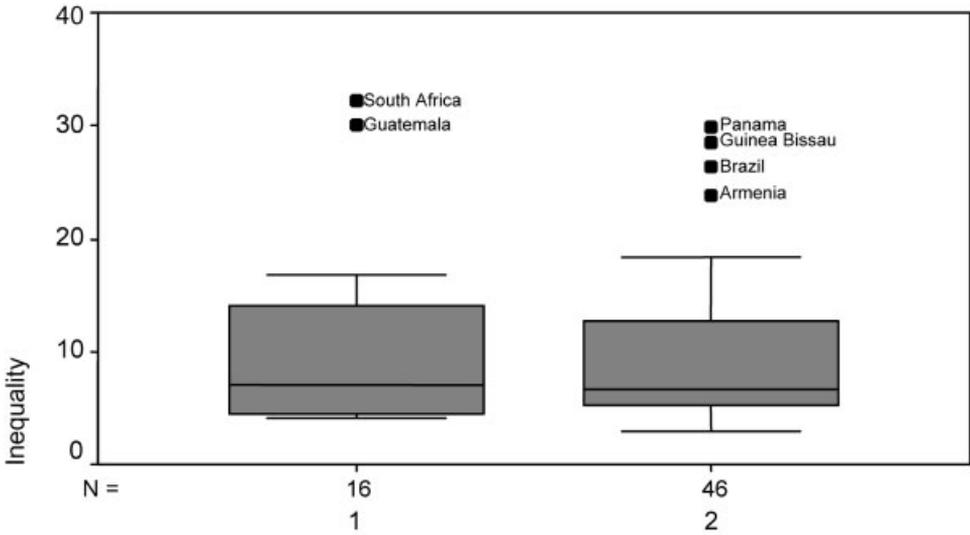
1 = Conflict; 2= No Conflict.

Conflict Source: Wallensteen and Sollenberg (1998, 2000).

Inequality Source: Deininger and Squire (1996).

Figure 2.   Post-1989 Civil Conflict

visual overlap in each figure between the (civil) conflict and non-conflict boxes suggests immediately that there is no clear direct relationship between civil conflict and inequality. The closeness of the medians between the two groups in each figure confirms this, where the median is shown by the horizontal line within each box. Statistical tests suggest overwhelmingly that we cannot reject the null hypothesis that there is no difference between the conflict and non-conflict groups in terms of their distribution by income inequality as captured by the best available comparable data.[7]

Looking at the outliers in each figure again shows how complex the problem is. For the five countries identified as outliers in Figure 1 are clearly characterized by extreme inequality by most measures (and probably are even more so than is captured in these data observations). Yet two of them have had (very different) civil conflicts while three have not. The comparison also shows the fragility of the conceptualization of violence and conflict in civil war studies. Brazil is clearly a very violent society, while Guinea-Bissau (not captured by the Wallensteen and Sollenberg post-1989 data on major armed conflicts)

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[7]Applying both tests of the means of these populations and non-parametric tests of the distributions confirms this picture. Thus, testing for the null hypothesis that there is no significant difference between the medians, we find for the 1944–2000 conflict/non-conflict population a *t*-test score of 0.117, with a 2-tailed significance score of 0.907, suggesting very powerfully that we cannot reject the null hypothesis. The same test for the post-1989 period reveals a *t*-score of 0.408 and a 2-tailed significance score of 0.685. Applying non-parametric, Mann–Whitney tests to the groups in the two periods confirms these grounds for extreme scepticism about any empirical regularity (let alone a convincing causal story at the general, cross-country level) between income inequality and civil conflict. In this case, 2-tailed significance tests produce scores of 0.554 for 1944–2000 and 0.872 for the post-1989 period.

experienced violent conflict in the late 1990s, overlapping to some extent with unrest in the Casamance region of southern Senegal. In a country like Brazil inequality looked at in various ways is extreme. There has been no civil war in Brazil, yet, on the one hand, there is a strong propensity to (more or less) non-violent conflict reflected, for example, in the land occupations of the Movimento Sem Terras (MST) and arguably in the prolonged distributional conflict managed through inflationary macroeconomic policy. On the other hand, there has been a high incidence of violence, reflected in a reported homicide rate of some 20 per 100 000 people and in evidence of structurally persistent and pervasive 'everyday violence' (Scheper-Hughes, 1992).[8] Hence, it may well be that inequality does produce conflict, and often violence, but that this need not take the form of civil war. Yet it is just as clear that homicide rates do not vary in a linear form with degrees of economic inequality. Reported homicide rates of Finland, South Korea and Costa Rica are not markedly lower than those of ostensibly far more unequal economies like Venezuela.

Beyond the weaknesses of distribution data, there are weaknesses in data on violence and war. These stem both from straightforward data shortcomings and from complex problems in defining and categorizing violent conflict. Practical challenges and complications of interest undermine the collection of reliable data on the incidence of war-related violence and of various forms of inter-personal 'social' violence. Thus, for example, civil registration and health information systems often collapse in wartime (Murray *et al.*, 2002). And whatever decisions are made concerning categorical distinctions, say between civil war and other forms of collective violence, there are then typically inconsistencies and unresolved challenges in defining clearly the coding rules for inclusion or exclusion of particular events. Sambanis (2002) dissects the 'civil war' category in quantitative analyses, showing how even those purporting to use the same coding rulebook vary in their observations, with significant implications for the sign and degree of significance of correlation between civil war and a range of common explanatory variables.

A brief and, arguably, appropriately crude empirical analysis, then, confirms that there is no obvious regularity in the interaction between income inequality and civil conflict. The data are too poor and what roughly credible data do exist suggest no straightforward causal relationships. Rather, their variations suggest the value of exploring 'contrastive explanations' as a research method. Why is it that countries with roughly similar income distributions appear to experience no clear pattern of civil conflict? There may be two broad ways of answering this kind of question. On the one hand, it may be that inequality does have an underlying tendency to promote violent conflict of one form or another, but that this needs to be studied in conjunction with a large, potentially infinite range of other variables. Given a potentially infinite range of other variables, it might not be easy to assign a clear degree of causal priority to economic inequality. On the other hand, the answer might lie more in the social relations within which economic inequality is embedded, the relations that produce outwardly visible signs likely to be captured in household survey data, Gini coefficients and the like. Here the precise score is less relevant than the precise and historically evolving characteristics of those social relations.

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[8]Brazil's reported homicide rate is one of the highest for a 'non-war' country: other particularly violent non-war countries, by this measure, include Bolivia, Jamaica and the Bahamas, Estonia and Russia, and Zimbabwe and Zambia (Sutcliffe, 1998, p. 252).

4   FROM FUNCTIONALISM TO RELATIONS—THE MANY GUISES (AND EFFECTS) OF INEQUALITY

One possible avenue of research and explanation into the consequences of inequality is to ask what kind of inequality prevails, what form it takes, and within what mould of relations inequality is cast. An example from the literature on distribution and economic growth concerns gender inequality in wage employment in manufacturing. Seguino (1997, 2000) argues that rapid growth in East Asian economies depended on institutionally maintained wage differentials between men and women. A significant factor driving this growth was the expansion of a number of specific manufactured exports, including textiles, whose competitiveness owed much to low wages and whose production was dominated by female employment. Differences in years of schooling, and other variables, are unable to account for the differentials in wage earnings between men and women. This argument runs directly counter to the argument that inequality is bad for growth: it suggests, uncomfortably, that there are some kinds of inequality that might be good for growth. Furthermore, it suggests that some kinds of inequality might work this way precisely because they do not generate either democratic pressure for capital taxation or political instability or civil conflict.

This example opens the way for a more realistic political economy of inequality, one that is not derived from the mechanistic axioms of abstract neo-classical economics. Above all, it suggests that historically established social relations that lie behind observable manifestations of inequality are more important, for understanding the consequences of inequality, than those manifestations themselves. Wage differentials between men and women represent one among a large number of possible pairings that make for what Tilly (1999) calls 'categorical inequality'. Other examples include black versus white in South Africa, Brazil or the USA, sharp ethnic polarity such as that in Rwanda, 'nationals' versus immigrants, wage workers versus capitalists, agricultural labourers versus landowners, etc. If most inequality takes the form of such categorical pairings, it is the nature of the bonds between people rather than aggregated interpersonal differences in essences such as innate intelligence or aggregated interpersonal differences in attributes or outcomes such as income per capita that determine the social and political significance of the inequality. How does it come to be that women can be paid less than men with equivalent education, etc.? What form of exploitation lies at the root of this set of relations? What forms of 'opportunity hoarding' within this categorical inequality help sustain it? What kind of adaptation is made by those disadvantaged by this relationship?[9] Another distinction is that between divisible conflicts (over more-or-less) and non-divisible conflicts (either-or) (Hirschman, 1995). Boundaries of categorical inequality might be defined in terms of such a distinction; and each kind of conflict (and the range of ambivalent cases combining elements of both) could have different implications for the kind of conflict.

There are two further implications of the discussion above. Firstly, in moments of dramatic (but potentially long drawn out) social and economic change, e.g. those

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[9]At the extreme, Levi (1989, p. 28) discusses how similar impulses of survival and relating to power help to sustain a profoundly exploitative and violent system: 'in contrast with a certain hagiographic and rhetorical stylization, the harsher the oppression, the more widespread among the oppressed is the willingness to collaborate with the power. This adaptability is also variegated by infinite nuances and motivations: terror, ideological seduction, servile imitation of the victor, myopic desire for any power whatsoever, even though ridiculously circumscribed in space and time, cowardice, and finally lucid calculation aimed at eluding the imposed orders and order'.

associated with the spread of capitalist relations of production, violence and conflict can be as much the mechanisms that bring about relational and measurable economic inequality as the outcome of prior inequality. In the process, inequalizing violence can contribute to class formation, though the precise character of emerging classes varies and is far from predictable. Examples include Indonesian military accumulation through ownership of industries and plantations, and suggestions that this accumulation strategy on the part of the military is what is fomenting much of the violence, post-Suharto, in various parts of Indonesia including Aceh and the Moluccas;[10] and Sudan, where destitution, famine and forced migration were direct policy outcomes from a strategy to acquire landed property in the south (Keen, 1994). Given the common wishful thinking about the possibility of smooth, linear, peaceful transitions to capitalism, it is easy to forget how disruptive and vicious, how typically tragic, is the process by which capitalism takes root (Vogel, 1996).

Secondly, the stress put in previous paragraphs on the relational highlights the reality that in the majority of civil conflicts the intensity of violence is conflict at close quarters, it is about visible and felt inequalities at the local level rather than the extremes of the Gini coefficient and the ratio between earnings of the richest and poorest quintiles of the population. Conflict has more to do with point blank relations and distinctions of inequality than those in the field of focus of macroeconomic multivariate analysis. In Liberia, much of the violence in the early 1990s discharged resentment by young rural militia boys and men against *known* ex-village residents who had 'made good' in the city and had failed to fulfil expectations of some largesse by distributing pickings of this new wealth back to the village (Ellis, 1999). In the Zimbabwean war of independence, there is substantial evidence that many peasants not only were coerced into fighting for or supporting logistically the ZANU-PF forces, but also, once they were involved, they used the war to pursue principally local agendas of struggle (women fighting oppressive patriarchy, youth fighting against stifling male gerontocracy) (Kriger, 1992).

5 ANGOLA AND RWANDA—HOW DID INEQUALITY MATTER?

Angola and Rwanda share dramatic inequalities: in income, in access to resources and to services such as health and education, and in suffering. Both have experienced a history in the late colonial and post-colonial periods of extremely violent conflict. However, their histories are substantially different. In each case, to the extent that economic inequality has been significant, it is only understandable in terms of the historical patterns of social relations and variations in policy that determined this (poorly measured) inequality. Inequality came to matter in different ways in each country.

In Angola from the late fifteenth century onwards, social, economic and political relations were disturbed by the commercial encounter with Portuguese merchants trading mainly alcohol, cloth and guns for ivory, copper, beeswax and, increasingly, people.[11] The ability to allocate European imports enabled African kings to secure increasing centralized power but this depended on generating rising numbers of slave exports. Consequently, a frontier of violence swept slowly inland from the ports, especially Luanda. Over time, the terms of trade and the balance of forces within African kingdoms shifted in favour of

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[10] See, for example, article in *Detik*, 21 January 2001: 'Military/police arrested in Ambon for inciting conflict'.
[11] For the historical literature on Angola see Miller (1988) and Birmingham (1992).

provincial leaders, and this in turn led to a series of power struggles and civil wars. At the same time, the slave trade and the paroxysms of violence with which it was linked to local political economies helped to bring about sharper divisions between the principal kingdoms and more peripheral regions of the hinterland, for example the south-central highlands where large numbers of refugees from violence moved, contributing gradually to the formation of a new 'Ovimbundu ethnicity'.

There were divisions too between major African kingdoms and the political ambitions of the Portuguese and, for a period, also the Dutch. Eventually the Portuguese established more stable political control, turning their trading interests into a rather loosely held together colony in the twentieth century. Categorical inequalities of various degrees of formal definition, and asymmetries of interest, power, wealth and wellbeing, together with relations of exploitation, opportunity hoarding and adaptation, took a variety of forms in colonial Angola. Pairings of categorical inequality included, for example: coastal Creoles and white Portuguese in Luanda; poor immigrant Portuguese farmers and indigenous farmers in the rich northern coffee-growing areas, many of the latter forcibly dispossessed by the former; these same farming classes and migrant labourers from the Ovimbundu highlands working on coffee plantations; and the white population of Luanda and the African population. With political developments in Portugal, involving the end of the monarchy and the advent of the *novo estado* and Salazar's dictatorial regime, in Angola the position of the Creole population was eroded and racially defined inequalities in access to public sector employment, to education, and so on were more sharply defined.

It was not just where such pairings were sharply and formally defined that collective action might take place, including violence. For this was also possible where formal boundaries between and around the pairs were not drawn boldly and institutionalized, but where they were experienced clearly but at the same time where there were conditions of extreme flux and threat to various interests. Hence, these fluid, violent and exploitative relations—involving poor white immigrant farmers, Bakongo farmers either pitted against white plantations or forced off their land, and the migrant agricultural labourers from the central Ovimbundu highlands—became one of the main sparks for the anti-colonial rebellion. In 1961 a protest by migrant workers in demand of payment of wage arrears exploded into a cycle of extreme violence involving these three groups.

Within this context, the three main protagonists of the early fight for independence emerged in different geographical and social regions of the country: the FNLA, mainly in the north; the MPLA in Luanda and its hinterland; and what would be the basis for UNITA in the Ovimbundu highlands and plateaux. A competitive bid for monopoly over Angolan nationalism combined with the very different material foundations of political ideology in each group, and increasingly during the 1960s and 1970s with divergent international interests providing financial and organizational backing, to create the grounds for an extraordinarily durable and intense armed conflict. During this period, increasing oil and diamond production has raised the stakes of conflict and together with war itself has fuelled rising inequality (Hodges, 2001).

In short, inequality is a hugely important factor in the prolonged history of violent conflict in Angola, but only if conceived from the outset in these political economy terms, in which the 'economic' is internally related to the social and political: economic inequality exists by virtue of the social and political forces that give rise to it, just as material forces shape the social and political. Similarly in Rwanda there is nothing to be gained by artificially abstracting economic inequality, in the form of a poorly measured Gini coefficient, from the country's history, from the combination of population pressure

on land and a history of poor policy choices, from the vagaries of international commodity markets, from the agency of individuals and groups, and from international interests and the timing of international demands for 'democratization'.

There is substantial evidence that income inequality in Rwanda has increased in recent years (Storey, 2000).[12] This might at least suggest that sharply *increasing* inequality is directly related to conflict.[13] However, to the extent that Rwanda was formerly not characterized by Latin American or South African extremes of income inequality, it was nonetheless a country whose bitter political divisions and history of violent conflict were obvious without post-1990s hindsight. Indeed, this history of conflict is probably just as significant to the vulnerability of civil war as a variable like inequality, given how often war seems to breed war.[14]

Inequality took three particular forms: one reflected in the accumulation antics of a clique whose central actor was the wife of President Juvenal Habyarimana; one defined in terms of the Hutu/Tutsi categorical pairing *par excellence*, though the terms of this relationship and its distinctions shifted over time, influenced far more by policy than by essential attributes; and one evolving on the edge of survival in rural population, land and policy dynamics.

There are widespread accounts of the crude accumulation strategy of a group of Rwandans—known as the *akazu*—closely related to the wife (and then widow) of Juvenal Habyarimana (Gourevitch, 1999; Storey, 2000). Their methods seem to have involved fairly standard manipulation of access to assets that could be distributed by the state as well as a highly effective harvesting of international aid flows (Storey, 2000). This group was not only an increasingly wealthy one; it also clearly depended for its continued material reproduction on controlling national power. Given the failure of the government's economic policies combined with adverse international market conditions, this group therefore had a strong interest, when it came under pressure to democratize during the early 1990s vogue for African democratization, in deflecting social and political resentments onto a scapegoat. Rwandan history offered a classic scapegoat in the form of the Tutsi population.

In classic insider/outsider mode, Hutu politicians adopted the so-called 'Hamitic myth' of the sharply foreign origins of the Tutsi, a myth that—like most national or ethnic myths—locked Tutsi identity into a primordially assigned, essential difference. During the late colonial period the Belgian colonial regime contributed greatly to forcing the Hutu/Tutsi distinction or categorical pairing into a viciously unstable institutional arrangement. The colonial regime did this both by hardening the boundaries around and between the two groups, e.g. by insisting on ethnic labelling on identity cards, and by discriminatory policies. At first, colonial authorities systematically favoured Tutsis—in education and employment policies—viewing them as more 'refined' and 'European'; however, in the last gasp of colonial rule the authorities switched discriminatory allegiance to the Hutus. The first major paroxysm of violence based on this categorical inequality came during the Hutu Revolution of 1959, when massacres of Tutsis forced thousands of survivors into exile in neighbouring countries.

One of the bitter ironies of the Rwandan predicament is that in the late 1980s a repetition of this kind of categorical inequality occurred in Uganda, this time more in

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[12]The 'high quality' data observation included in Deininger and Squire (1996) is for 1983. Maton (1994) estimates that the Gini coefficient rose from 0.357 in 1982 to 0.505 in 1989, an unusually sharp increase if this is accurate.

[13]Income distribution was most likely becoming yet more skewed in the late 1990s and early 2000s, as Rwandan interests secured increasing control over the flow of high value resources mined in the Kivus of eastern Congo (Jackson, 2001).

[14]See Justice Africa (2000).

national versus foreigner terms. For Rwandan Tutsi exiles in Uganda had contributed significantly to the military and political success of Yoweri Museveni's NRM, but after Museveni's victory they were excluded from certain rights of access to land, etc. In Austin's terms (1996) this reduced the opportunity cost of leading a military return to their homeland in Rwanda. This group, forming the Rwandan Patriotic Front (RPF) invaded Rwanda in 1990, beginning a civil war that 'ended' with their victory, after the horrendous scale of the 1994 genocide. The war added to social and economic pressures that were already well ensconced in rural Rwanda as well as threatening the power base of the ruling and accumulating class under Habyarimana.

Within rural areas there was considerable pressure of population upon cultivable land. Rising population density is not necessarily associated with a propensity to conflict. However, in the context of a history of agricultural policy that had under-invested in agriculture, particularly in trying to maximize the gains from export agriculture and/or production of crops with a high labour demand and that had failed to develop any effective diversification strategy, population pressure did contribute to rising social tensions and, moreover, to processes of social and economic differentiation and class formation that clearly disrupted established adaptations to 'tradition'. André and Platteau's (1998) localized research in one area—in the north–west—suggests that there was in the late 1980s and early 1990s a shrinkage in average landholding size but at the same time increasing inequality in size of landholdings. Though there was no legal land market, transactions did take place that reallocated land. On the one hand, pressures of rural survival and change were represented by frequent local disputes. On the other hand, they brought forth a new class of people with larger landholdings whose success was clearly resented by many others. André and Platteau are adamant that this inequality in rural areas did not in any simple way 'cause' the violence and genocide of the 1990s, but they do argue that it helped that violence take some of the bitter forms that it did.

Thus, as in Angola but in very different ways, highly specific forms of inequality were central to Rwanda's horrific recent history. Social relations were being disturbed by policy-influenced material developments: the crudest beginnings of what might become capitalist class formation were clearly evident in rural Rwanda, access to the state was a fulcrum for accumulation and differentiation, and the resentments and fragile opportunities of diverse groups of people were increasingly displaced onto the ideology of divisive collective identities. The relation between these particular kinds of inequality was what gave a murderous passion to the point blank relations of envy, grievance, greed and fear. Even in these conditions, the genocide was not mechanistically predictable but owed a lot to the role of *contingent developments*, in neighbouring Uganda, in international commodity markets, in the feebleness of the international community's management of the early 1990s peace negotiations and their manipulation by the French (Adelman and Suhrke, 1996), and in the post-Cold War vogue for democratization.

6   CONCLUSION—WHERE DO WE GO FROM HERE?

Nowhere are these issues put better, but also put into question more clearly, than in a story by the Sicilian, Leonardo Sciascia. In this story, 'Antimony', a Sicilian sulphur miner is drafted by Mussolini to fight in Franco's militias in the Spanish Civil War. The Sicilian is politicized by this experience and comes to see the difference between a war between countries and a civil war in these terms:

A civil war is not a stupid thing, like a war between nations, the Italians fighting the English, or the Germans against the Russians, where I, a Sicilian sulphur miner, kill an English miner, and the Russian peasant shoots at the German peasant; a civil war is something more logical, a man starts shooting for the people and the things that he loves, for the things he wants and against the people he hates; and noone makes a mistake about choosing which side to be on . . . (Sciascia, 1986, p. 189).

Sciascia's story seems to capture a particular, class-based variant of the idea that inequality drives conflict, although there is enough subtlety to allow for the possibility that it is not necessarily the inequality or relations of hatred that cause the conflict but that they simply make it a more meaningful one.

This paper suggests that economic inequality *is* hugely important to explaining civil conflict, but only insofar as the economic is considered inseparable from the social, political, cultural and historical. This allows for greater explanatory depth but less claim to predictive power or generalization across contexts; it also allows for the significance of varying *kinds* of inequality to become clearer. It is also clear that, as this paper has argued, the 'purely economic', addressed through a concoction of optimizing individualist axioms, restrictive indicators, and questionable econometrics, is far too crude an analytical tool to apply to the problem that needs understanding. The conclusion and argument of this paper is related to Stewart (1998, 2000), who argues that 'horizontal inequality' causes civil conflict but that 'vertical inequality', as captured in the Gini coefficient, does not. Stewart also acknowledges that whether or not an 'objectively' unequal situation translates into conflict depends on factors including the strength of the state and the particularities of ideological conditions in a society. Generally, her conclusion is that less horizontal inequality reduces the scope for violent conflict.

It is important to focus on the variety of ways in which inequalities are managed and the ways in which some progressive changes can be achieved within structures of enduring categorical difference. It is also important to understand the transmission mechanisms that enable a relatively peaceable durable inequality to turn into a violent conflict. Among the relevant factors in such a transmission mechanism (which, it must be said, is not necessarily historically undesirable) are likely to be: sharp changes in the nature of the relationship between groups; external interventions; ideological shifts whereby injustices that were previously accepted come to be regarded as grounds for conflict, violent or non-violent (Hampshire, 2000); and new possibilities of equality through the departure of dictators, the end of the Cold War, regional ideological 'moments', or waves of pressure for democratization.

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