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“There are infinite reasons a man may have for hating or loving another man; [he] reduced the history of the word to one sordid economic conflict” (Jorge Luis Borges, “The Shape of the Sword”, in *Collected Fictions*).

Introduction

As in the study of economic growth so in the recent efforts to understand the political economy of violent conflict the role of inequality has, despite a great deal of attention, remained elusive. However, a closer focus directly on the political economy of inequality itself might not only help us understand both economic growth and violent conflict better, but also it might throw interesting light on the analytical limitations of much economics and political economy in general. This paper briefly reviews the ‘state of play’ in the debate in the economic literature on inequality and development and its possible role in causing civil conflict.¹ Whatever the apparent consensus in this field at any particular moment, however, it can never be stressed enough how weak the empirical foundations remain for any construction based, especially, on inter-country comparisons. This empirical uncertainty, together with the influence on the debate of shifting fashions in economic analysis, are highlighted in the example of the development of Collier’s models (Collier and Hoeffler, 1996, 1998) of civil war that claim explanatory and predictive power. For between an initial and a later, more formally published version of Collier and Hoeffler’s first model of civil war there was a dramatic shift in the causal power attributed to the inequality variable.

If a critical analysis of the general economic literature on inequality and conflict leaves us confused about whether, or to what degree, inequality causes conflict then this might be because of the way in which we define and analyse inequality as well as because of shortcomings in our ability to measure it. For we cannot hope to understand conflicts in terms of essentialist models of understanding – whether they highlight primordial ethnic loyalties or the transhistorical consequences of income or asset inequality or, indeed, the robotic reflexes of the rational choice individual confronted with the stimuli of distorted markets and lootable commodities.

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¹ From the start, our analysis is hobbled by the ungainly category of ‘civil war’. Arguably, there is no such thing as a purely civil or intra-state armed conflict, since all such conflicts (to varying degrees) involve regional and international political economy relationships. For the purposes of this paper, however, for want of anything much better I will refer to civil conflict or civil war to distinguish this range of conflicts from obvious inter-state conflicts, as well as from less overtly polarised, open military conflict including high rates of homicide, violence against women, routine and violent repression of children, violence that is associated with organised crime or gangs but cannot ever fully be made out as non-political, etc. Indeed, it is important to this paper that the possible existence of a continuum of forms of violence and conflict makes it far more complex to understand the consequences of inequality than is often assumed. Hence, the paper is a first step towards greater subtlety simply by concentrating on one main aspect of these consequences.

In place of these functionalist explanations – with their slash and burn approach to historical specificity and their rape and pillage of the ‘social’ – some alternative perspectives on inequality may offer greater insights for the understanding of violence.² Influenced by Hirschman (1981), Tilly (1999) and others this alternative is determinedly relational: 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.

The Role of Inequality in Economic Growth and in Violent Conflicts – a Consensus?

After enthusiasm for the suggestiveness of the Kuznets Curve waned under closer scrutiny, interest 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 and instant 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 intelligence while bright children of poor families cannot afford proper schooling. Poor labour market opportunities and a lack of incentives to x-efficiency among the disadvantaged reinforce this private aversion to investing in education.³

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 more crude 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.⁴ Therefore, again, inequality indirectly reduces the possible rate of

² An example of how the apparently social is brought back into formal economics is the enthusiasm for models ‘explaining’ growth or war with the independent variable of an ethno-linguistic fragmentation index (Easterly and Levine, 1996; Collier, 1996; Arcand et al, 2000). This index has been used in extraordinarily ahistorical, functionalist ways in econometric models recently: for a critique see Cramer (2000a) and for a broader critique of how neo-classical economics has sought to explain uncomfortable ‘residuals’ through bringing back in the social, see Fine (2001).

³ A good presentation of the individual utility maximising agent assumptions of this kind of argument is Bardhan and Udry (1999); however, variations in assumptions, e.g. concerning inter-temporal substitution preferences, produce alternative models (Chiu, 1998).

⁴ That instability might not necessarily be a disincentive to investment is shown, for the case of foreign investment in Nigeria, by Frynas (1998).

growth.⁵ At the extreme, of course, this same chain of causality might bring about not isolated political assassinations but full-blown organised 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). Much of the support for this argument comes from Latin America, where individual case studies and more general surveys have stressed the significance of inequality in producing violent outcomes. Most of the authors in Boyce et al (1996), for example, assume or explain violent political conflict in El Salvador principally by the role of inequality, particularly unequal distribution of land. This inequality – which on most measures has been fairly extreme – was embedded in social relations with the expansion of the coffee economy in the late nineteenth century, when powerful landowners forcibly expropriated the land of smallholders, and was reinforced by the concentrated power of this landed oligarchy throughout most of the twentieth century, a power sustained by the enduring domination of the coffee sector in the overall economy but reinforced by oppressive governments. Political protest developed towards more violent forms from the 1930s onwards, in response to the brutal repression of labour protests, and was ratcheted up till it turned into full-blown war during the 1970s and 1980s. Based on the premise of the causal role of inequality, particularly in land ownership, land reform was made a central provision of the Chapultepec settlement whose aim was to secure a lasting peace. That land reform has been slow and partial while the peace has basically held does not actually dismantle the political and conflictual significance of distributional problems. For although there has been no return to civil war, as in so many other parts of the world the end of a prolonged war by a negotiated peace settlement has been followed by the transformation of violence into still pervasive but different forms, including localised rural conflicts and widespread urban violence. Similar arguments to Boyce's are made in the work of Booth (1991) and Wickham-Crowley (1992).⁶

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 leant 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, and with a slightly cavalier attitude to the precision of argument, Binswanger et al (1995) elide a range 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, 2060).⁷

⁵ A variant on this story is Amsden's (2001) argument that much of the difference in rates of industrialisation among "the rest", i.e. principally Asian and Latin American countries, derives from the politics of economic inequality. For Amsden, a low degree of inequality in East Asian countries like South Korea and Taiwan freed developmental states from pressures to disperse thinly the "intermediate assets" that were theirs to give (subsidies, protective measures, etc.) and enabled them to concentrate these assets in a handful of big businesses that led export growth and became "national leaders". In Latin America, by contrast, the political consequences of inequality were that states had to spread these assets more thinly: consequently, Amsden argues, they failed to stimulate the concentration of industrial learning-by-doing and economies of scale that would have driven more rapid, more effective industrialisation.

⁶ 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).

⁷ Large-scale farms, according to Binswanger et al, produce static efficiency losses; dynamic efficiency losses thanks to reduced profitability of free peasant cultivation and poor incentives; resource costs

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. Thus, contrary to the assertions of Binswanger et al (1995), and indeed of Boyce et al (1996) and, before them, Huntington (1968), Muller and Seligson (1987) argue that national income distribution is far more significant than land distribution. Muller and Seligson 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 mobilise effectively for political action.

Some of the recent literature on the economics of conflict and/or complex humanitarian emergencies concurs with these views that inequality is a strong predictor of conflict. The main example is Nafziger and Auvinen's (1997) econometric analysis of humanitarian emergencies. Their regression analyses "indicate that high income inequality (measured by a Gini coefficient) is associated with political conflict and complex humanitarian emergencies" (ibid. 41). Their brief case studies are designed to show how this might be the case both for unequal but poor and stagnating economies and for unequal economies where real per capital GDP is rising (Nigeria) or where a disadvantaged group's economic position is improving, "as for nonwhite South Africans from the 1970s through the early 1980s" (ibid.).

The Weaknesses of the Empirical Foundation for Claims about Inequality and Development Outcomes

There is, then, a strong argument – rooted in notions of relative deprivation and, perhaps less acknowledged, in an idea of envy – that inequality is bad for growth and that it is a powerful source of armed conflict. However, both those arguments directly concerned with 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.

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. However, despite the availability of improved data sets, the empirical basis of inequality research remains often fragile.⁸ To take two examples, both Rwanda and Indonesia have often been regarded as countries with low Gini coefficients. However, to draw from this published data the conclusion that either is a low-inequality country would be misleading, even absurd. Indonesia has probably experienced rapid increases in income and wealth inequality in recent years, a fact that is directly observable to the eye in and around Jakarta, for example, with its extravagant shopping emporia coexisting

thanks to rent-seeking efforts to create and maintain the distortions supporting large farms, which contribute to poverty and inequality plus lower employment; and social costs including peasant uprisings and civil war (2690). On a critical note, regarding Mozambique it is clear that Frelimo's rural policies contributed to widespread distrust of the post-independence government in rural areas and that this came even to fuel support, tacitly or overtly, for Renamo rebels (Clarence-Smith, 1989); however, firstly, it is far-fetched to attribute to this the principal cause of the war and, secondly, it was poor policy management rather than large farms per se that mattered (just as it was not the size of farms but the policies implemented on those farms and in rural areas generally during colonial years that generated support for Frelimo's anti-colonial struggle – see, e.g., Isaacman, 1996).

⁸ The main data sets currently used are Deininger and Squire (1996) and UNU/WIDER and UNDP (2000).

with extreme poverty and, further afield, dire indigence in rural areas (Asra, 2000, p.97 & p.102).⁹ Rwanda also is not quite the Cuba or Kerala of its Gini image. As discussed below, a number of researchers have shown at the micro level how intense the distributional conflict is and how sharply rural society has undergone inequalising pressures in recent years (e.g. André and Platteau, 1998).

Recent research in Latin America confirms the dangers of building causal arguments on inequality data. Szekely 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” (Szekely and Hilgert, 1999, 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; Kanbur, 1998; Bowman, 1997; Cramer, 2000b). These criticisms amount to more than modest qualifications: rather, they suggest that the data simply do not allow for meaningful inferences, about patterns of event regularity involving inequality in wealth and income, from large sample inter-country comparisons. This paper, therefore, eschews technique-driven and inappropriate econometric modelling and, while drawing with caution on available evidence for more modestly illustrative purposes, suggests the need to shift the focus of research in the political economy of distribution and development.

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 Wallenstein and Sollenberg (1998, 2000) data on post-1989 armed conflicts.¹⁰ The strong visual overlap in each figure between

⁹ For a visual image of this inequality, see the photographs of Jakarta in Salgado (2000, p. 355, p.360).

¹⁰ Inequality data are taken from the Deininger and Squire (1996) set 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, (b) older observations are less likely to be reliable and (c) confining the observations to a five-year span is more likely to include relatively comparable measurements, to the extent that this is thought to be at all possible. 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’ Deininger and Squire 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 1,000 battle-related deaths recorded during the conflict but fewer than 1,000 in any given year) and ‘war’ (more than 1,000 battle-related deaths during any given year), whose sum is termed ‘major armed conflicts’ is 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 1,000 deaths, contrary to some that require more than 1,000 annually.

It may be useful to draw attention to some of the anomalies and problems in the conflict data. The Sambanis data includes some conflicts that would be classified under Wallenstein and Sollenberg as minor conflicts. Kenya (1991-93), Mexico (1992-94) and Romania (1989) are included in Sambanis but not Wallenstein and Sollenberg. Moldova is included in the former but in the latter only noted as a minor conflict. The Wallenstein and Sollenberg data might be taken to show the post-Cold War world, in which perhaps the inequality/conflict relationship might appear different: however, it should be

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.*¹¹

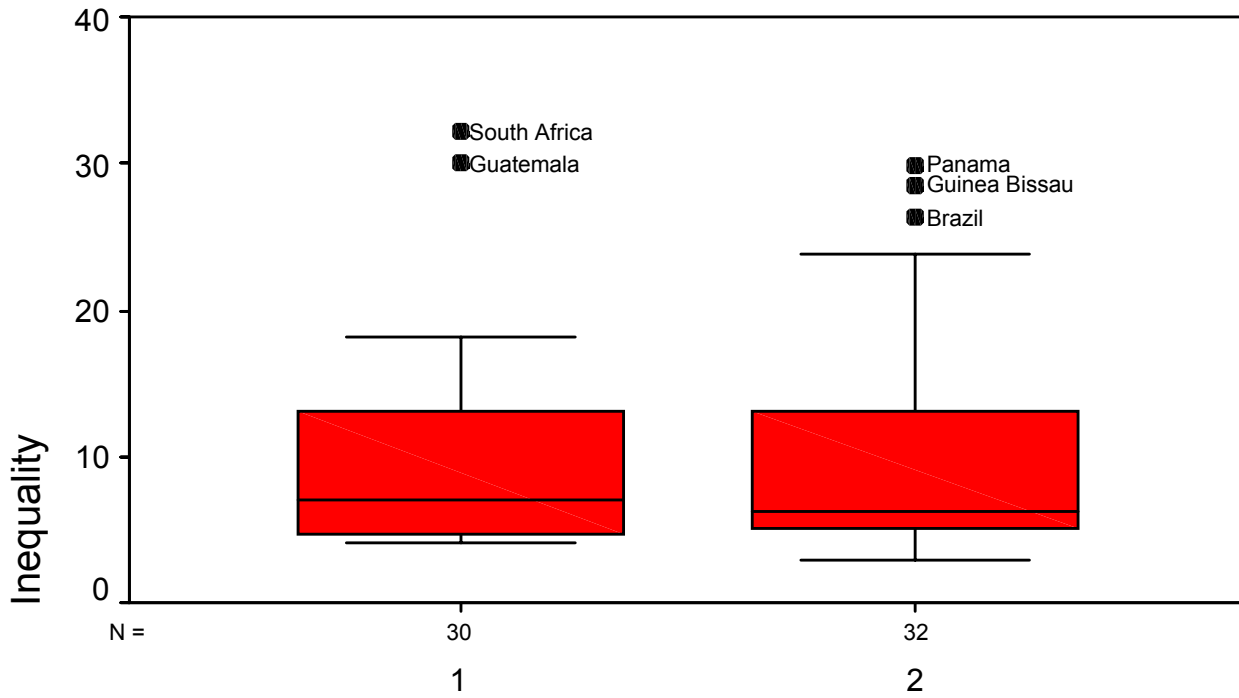
noted that some of the conflicts included in their dataset are clearly wars that had their origins in the Cold War and petered out at the end of the 1980s or in the early 1990s.

Further, there are unclear cases. For example, Wallenstein and Sollenberg (1998) cite a number of ambiguous cases in 1997, including China, Iran, Myanmar, and Nepal.

Finally, 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.

¹¹ 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.

Civil Conflict 1944-2000

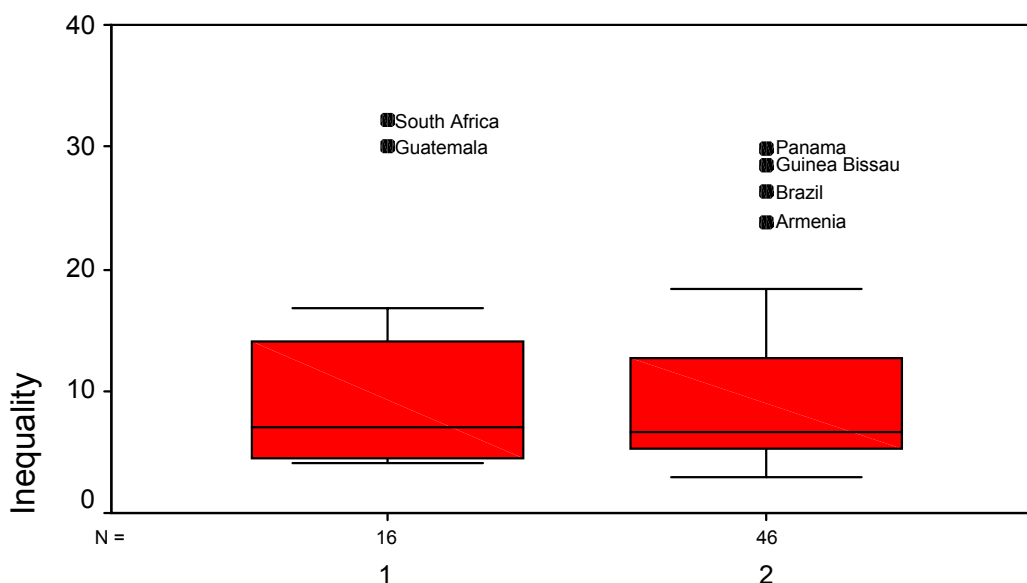


1 = Conflict ; 2 = No Conflict

Conflict Source: Sambanis (2000)

Inequality Source: Deininger & Squire (1996)

Post-1989 Civil Conflict



1 = Conflict; 2= No Conflict

Conflict Source: Wallensteen & Sollenberg (1998,2000)

Inequality Source: Deininger & Squire (1996)

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 characterised 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. At any rate, three have not in the normal definition of a civil war: for this comparison also shows the fragility of the conceptualisation 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) 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' (Sheper-Hughes, 1992).¹² 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. New York's notoriously high homicide rate may be related to powerful economic inequalities drawn chiefly along racial lines; however, this notoriety is something of a myth and the homicide rate has dramatically declined in recent years. Places with far more even distribution of wealth and income such as Sweden have higher homicide rates (Sutcliffe, 1998, 252). Reported homicide rates of Finland, South Korea

¹² 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, 252).

and Costa Rica are not markedly lower than those of ostensibly far more unequal economies like Venezuela. Again, however, the poverty of the data severely undermine any powerful claims about empirical relationships here: to take one obvious example, the UN Survey of Crime Trends and Operations of Criminal Justice Systems reports a homicide rate of eight per 100,000 inhabitants in India that, apart from the obvious factor of the relatively immense Indian population, surely distorts the image of actual prevalence of interpersonal and communal violence in India.¹³

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.¹⁴ Why is it that countries with roughly similar income distributions appear to experience no clear pattern of civil conflict?¹⁵ Or why does inequality appear to be linked to such a variety of manifestations of violence and conflict, such that in some cases inequality effects might be displaced onto intra-household violence, in others they might promote high prevalence of ‘non-political’ homicide, and in others still they might indeed be part of the group of factors making a country vulnerable to civil war?

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. This perspective also makes it easier to appreciate the significance of Sen’s question: equality (or inequality) of what? For there are very many different forms of inequality, even if they lend themselves to symptoms that may be captured quantitatively. Not just asset and income or stock and flow inequality matter, but also gender inequality, asymmetry in constitutional inclusion between ‘ethnicities’, regional inequalities, ineluctable class inequalities, the forms in which power relations are institutionalised, and so on. And each of these inequalities varies in its relation to specific forms of political order and hierarchy, and characteristics of ideological contest.

Rational Choice and Civil War – What Happened to Inequality?

A particularly 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 and 1998). These models make strong claims, based on their foundations in rational choice methodological individualism. Arguing that explanations of civil wars often invoke economic causes but never properly have investigated this link, the authors set out to “explain why they occurred in terms of underlying economic variables” (Collier and Hoeffler,

¹³ On the pogrom in Surat in the 1990s see Breman (1993); on violence and murder involving tensions between Dalits and landowners and police forces in Bihar see Human Rights Watch (1999).

¹⁴ For a discussion of ‘contrastive explanation’ in social science research, as opposed to econometric pursuits, see Lawson (1997).

¹⁵ For example, as Muller and Seligson (1987, 426) point out with respect to the hypothesis that unequal distribution of land causes discontent that provokes mass political violence or revolution, rapid population growth, extremely high Gini coefficients for land distribution, and the share of landless rural households in the total labour force were almost equal in El Salvador, Nicaragua, Costa Rica and Panama during the 1960s and 1970s, yet the political history in that period of Costa Rica and Panama was dramatically different from that of El Salvador and Nicaragua.

1996, 1).¹⁶ 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” (ibid. 7). Given the force of this claim, and the influence of these models, it is interesting to explore the fate of inequality in the development of Collier’s thinking.

In the first version of this model, a widely disseminated paper, inequality does not cause conflict: in fact, “greater inequality significantly reduces the risk and duration of war” (Collier and Hoeffler, 1996, 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.¹⁷ In the terms of the model, this is built into the expectations and calculations of choice by prospective rebels. Hence, observing a high degree of inequality these 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.

This model was revised before formal publication. In the process, this clear and rationally justified role of inequality as disincentive to conflict disappeared. Collier and Hoeffler (1998) claim that inequality is insignificant and makes no contribution to the probability of civil war. There are two factors that might not be coincidental in their association with this development in the model.¹⁸ Firstly, around the same time the argument reviewed earlier in this paper, i.e. that inequality is bad for growth, and that policies favouring a long-term redistribution of access to education and health services, was gaining greater currency. Secondly, Collier’s analysis was developing towards an even more distinctive theory of the likelihood of civil war, in a model (Collier, 2000) that argued that greed or opportunity was far more significant a determinant of conflict than grievances (and inequality was one tested proxy for grievance).¹⁹

Thus, econometric analysis appears capable of supporting completely contrasting causal mechanisms for the same variable through modest changes in the specification of models. Further, the rationalisation of a given causal relationship involving inequality and conflict seems to be driven by an apparently arbitrary selection of assumptions. 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. In other words, there is a problem with the data, there is a problem with the way in which economics conceives of inequality, and this example confirms some of the fundamental weaknesses of mainstream economic analysis more widely.

¹⁶ Much of the analytical groundwork for neo-classical economic thinking on conflict was laid out especially elegantly by Hirshleifer (1994).

¹⁷ It is not explained, for example, whether it makes a difference what kind of class this elite represents, if they are capitalist or not; nor why this elite should prefer this course of action rather than, say, preferring conditions of greater political stability that might be secured by some longer term redistributive policies.

¹⁸ Collier was appointed head of the research department of the World Bank in the mid-1990s. For papers by Collier and by others on related topics, see the Bank’s website on the economics of crime, violence and civil war (www.worldbank.org)

¹⁹ Among the criticisms of this greed versus grievance model one could easily question the hard and fast distinction between grievance and ‘greed’, since both may be rooted in relations of, e.g., envy or mimetic rivalry among individuals and groups and, therefore, internally related.

From Functionalism to Relations – the Many Guises (and Effects) of Inequality

Data shortcomings typically mean that there is no clarity on how much economic inequality exists in a country. The available evidence, unreliable as it is, suggests that the significance of inequality lies beyond merely its presence or its degree of intensity. 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. At one level this might still involve cross-country comparisons. An example from the literature on distribution and economic growth concerns gender inequality in wage employment in manufacturing. Seguino (1997, 2000) – with more than typical honesty about the shortcomings of the data – 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. The median voter is not as androgynous as its statistical anonymity might suggest, but appears to have a male bias. And the losers in this distributional pattern appear to be socialised in such a way that they do not cause trouble.

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. Although Seguino's example does suggest that even at this level there might be some general tendencies across countries, generally these relations are likely to take highly specific, historically influenced forms, and to interact with a great variety of outcomes with other prevailing factors, other dimensions of social relations, and significant contingencies. Before exploring two case studies that help to illustrate this point, some more general points might be made.

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 vs. white in South Africa, Brazil or the USA, sharp ethnic polarity such as that in Rwanda, 'nationals' vs. immigrants, wage workers vs. capitalists, agricultural labourers vs. 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?²⁰ For mechanisms such as these are likely to

²⁰ At the extreme, Primo Levi (1989, 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 stylisation, the harsher the oppression, the more widespread among the oppressed is the willingness to collaborate with the power. This availability 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". These motivations

explain rather better than explanations based on methodological individualism why inequalities tend to be so 'durable', as well as the circumstances in which they might be likely to crack. In other words, there are social and material reasons why sharp changes in distribution and distributional relationships are rare and why there is a considerable but variable 'tolerance for inequality'. 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.

In another example of contrastive explanation, Tilly draws on Anthony Marx's (1995) analysis of the difference between Brazil and South Africa, explaining different political trajectories in terms of the lack of systematic identification of categorical inequality, in Brazil, in racial terms despite the very real race-based exploitation and inequality.²¹ And while tolerance for inequality might be a function of Hirschman's 'tunnel effect' whereby people observing others getting sharply richer are given hope for their own prospects, this tolerance might be produced by less rosy conditions (Hirschman, 1981). For example, nowhere can have experienced so savagely inequalising a redistribution of assets and income as post-Cold War Russia (Cohen, 2000; Field, 1995). Yet, although there is considerable violence among competing elites, there has been no great revolt and there appears to be no prospect of large scale organised violence in reaction to this redistribution. As Holmes puts it (2001, 35), 'Russian billionaires are not particularly afraid of the street. They do not expect a social explosion because the public remains quiescent, poorly organised and busy eking out a meagre living. A greying population, moreover, makes it highly unlikely that enraged crowds with pitchforks will storm the palaces of the super-rich'.

The static analysis of econometric comparisons among many countries is particularly ill suited to capturing the political significance of changes in inequality and in the social relations behind inequality. *A priori* one might anticipate different possible issues. On the one hand, a kind of 'tunnel effect' might prevail, where people observe a minority rapidly accumulating wealth, but regard this as a sign of possibilities opening up for themselves too. On the other hand, social and economic changes involving sharp and obvious increases in wealth for a minority of people might create envy of the new wealth, resentment of the exploitative methods by which it is secured, bitterness at the constraints on people's own lack of economic mobility, or intense struggles driven by mimetic rivalry among individuals or groups, and so on.

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 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, inequalising 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 industrial plant 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 divers parts of Indonesia including Aceh

support a 'grey zone' in which even the extremely oppressed try to consolidate their minuscule privilege vis-à-vis those entirely without privilege.

²¹ Sutcliffe (1998) shows how combining different forms of categorical inequality, viewed in terms of average earnings, means that in Brazil a black woman from the north-east earns some 6 times less than a white male from the south-east and a male head of household in Brasilia earns on average 18 times the earnings of a rural female head of household in Piauí.

and the Moluccas;²² and Sudan, where destitution, famine and forced migration were direct policy outcomes from a strategy to acquire landed property in the south (Keen,). 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 focus in the range of macroeconomic multivariate analysis. This is clear from the exercise of envy invested with power in the distinctly personal motivations of individuals such as *Ortsgruppenleiter* Wesch ensuring one Düsseldorf Jewish couple were unable to board a ship for Cuba in 1941 (Zürndorfer, 1983). 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). Again, in Rwanda, André and Platteau (1998) report both on the increasing social strains in a village in the Gisenyi prefecture, in which the owned land Gini coefficient was rising but more importantly, perhaps, as distress sales and access to off-farm income became more important determinants in the late 1980s and early 1990s of land acquisition than inheritance so established relations and institutions were weakening, and disputes proliferated, most notably within households and especially frequently taking male inter-generational form. Victims of the paroxysm of murder in 1994, in this village, seemed to include particularly those perceived as ‘troublemakers’, and those whose success and accumulation of more land and/or cattle was resented, particularly where this involved people “who behaved in an individualistic way by refusing to redistribute income toward less fortunate villagers” (André and Platteau, 1998, p.48).²³

Angola and Rwanda – How did Inequality Matter?

Angola and Rwanda differ greatly. For one of them, Rwanda, there is ostensibly ‘high quality’ data available on inequality while for the other there is none and the fiction of available data on most subjects is fairly widely appreciated. Both countries share dramatic inequalities: in income (more so in Angola), 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

²² See, for example, article in Detik, January 21st, 2001: “Military/Police Arrested in Ambon for Inciting Conflict”.

²³ Similarly, Breman (1993, 1996) shows how the brutally predatory form of capitalism and its associated social formations in Surat, which generates an extremely competitive world of survival among migrant labourers, in a context of strong but complex collective identity formations, enabled the outburst of anti-Muslim violence in the pogrom of the early 1990s. These conditions of work and life, mediated through inherited and evolving categorical differences between collective identity groups, produced violence within a probably limited range of income differentials (sometimes prompted by incentives such as promised ration cards). See also Kentridge (1990) on violence in Kwa-Zulu Natal (South Africa).

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.²⁴ The ability to allocate European imports enabled African kings to secure increasing centralised 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 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 principle kingdoms and more peripheral regions of the hinterland, for example the south-central highlands where large numbers of refugees from violence moved, contributing gradually 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. In the hundreds of years of conflict over political control in Angola, the most famous actor of resistance was Queen Nzinga, a skilful manipulator of the competing international and local interests. Nonetheless, eventually the Portuguese did establish more stable political control, turning its 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 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 institutionalised, 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 accumulating wealth they could never have dreamed of in Portugal, 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, an event that formed into the largest violent uprising in all of colonial Africa, with more murders, for example, than in the Mau Mau uprising. Economic inequalities in northern Angola were not exactly of the type captured by Brazilian-style Gini coefficients. Rather, point blank inequalities mattered hugely in a context of uncertain and violent class formation and class relations.

Within this context, the three fairly distinct regions of Portuguese colonial rule came to overlap with three zones of influence of different Protestant mission churches. Each provided the centre and field of focus for the formation of new, proto-nationalist collective identities. Thus were born the FNLA in the north, the MPLA in Luanda and its hinterland, and

²⁴ For the historical literature on Angola see Miller (1988) and Birmingham (1992).

eventually the political formation that would provide 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 organisational 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.²⁵ Similarly in Rwanda there is nothing to be gained by starting analytically 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 'democratisation'.

Rwanda, according to 'high quality' data, counts as a relatively equal country. This data is somewhat untrustworthy, not least because it is dated and there is substantial evidence that income inequality has increased in recent years (Storey, 2000).²⁶ This might at least suggest that sharply increasing inequality is directly related to conflict.²⁷ However, to the extent that Rwanda was formerly not characterised 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.²⁸

Rather than draw inferences from unreliable measurements of abstract variables, then, what kind of inequality, if any, has been significant to Rwanda's economic and political developments? 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; Prunier, 1995).²⁹ This group was not only an increasingly wealthy one; it also clearly depended for its continued material

²⁵ On internal vs. external relations, see Lawson (1997).

²⁶ 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 by 1992, an unusually sharp increase if this is accurate. Maton also claims that the richest 10 per cent of the rural population increased their share of total rural income from 20 to 41 per cent between 1982 and 1992.

²⁷ 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).

²⁸ See Justice Africa ().

²⁹ Note that, as Storey puts it (2000, 6), "the key link between these activities and their beneficiaries was **control of the state**" (emphasis in the original).

reproduction on controlling national power. Given the failure of the government's economic policies combined with adverse international market conditions (principally a sharp decline in the late 1980s and early 1990s in coffee prices), this group therefore had a strong interest, when it came under pressure to democratise during the early 1990s vogue for African democratisation, 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 were 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. Historians really remain confused as to the origins of the Tutsi, although to the extent that there is some historical collective history it owes a lot to waves of migration towards the Great Lakes region. However, it is often noted that Hutus and Tutsis coexisted in relative peace for hundreds of years and also that the relationship between them was defined far more in terms of class than in terms of essential attributes (height, nasal profile, degree of skin blackness, etc.). Furthermore, the boundary between them was quite fluid till the late colonial period. However, during this 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 – in education and employment policies – Tutsis, 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 national vs. 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 maximise 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'.³⁰ André and Platteau's (1998) localised research in one area – in the north-west - suggests that there was in the late 1980s and early 1990s a shrinkage in average landholding

³⁰ In the various accounts of rising rural inequality in Rwanda there is little reported research into what might be the flip side of primitive accumulation by the *akazu* and others, i.e. the beginnings of the formation of a class of agricultural wage labourers or at least those depending increasingly for their survival on access to seasonal and/or temporary wage labour opportunities. This lack of research is probably one more factor undermining confidence in published inequality measures. However, it appears that this may well be a significant trend, and a hint of it is given in Pottier's (1996) critique of the FAO's response to the humanitarian emergency of the mid-1990s.

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, the 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 could 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 Surkhe, 1996), and in the post-Cold War vogue for democratisation.

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 – as, of course, did happen – in Franco's militias in the Spanish Civil War. The Sicilian is politicised by this experience and comes to realise that he is fighting for the wrong side. He comes to a position on the difference between a war between countries and a civil war:

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 no one makes a mistake about choosing which side to be on...Despite its atrocities, a civil war is a kind of *hora de la verdad*, a moment of truth..." (Sciascia, , 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, part of, embedded in the social, political, cultural and historical. This allows for greater explanatory depth but less claim to predictive power or generalisation 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 optimising individualist axioms, restrictive indicators, and questionable econometrics, is far too crude an analytical tool to apply to the problem that needs understanding.

This paper has made the following points. First, in trying to understand the role of inequality in the production of civil conflict one of the most significant obstacles is the poor quality and frail comparability of the data. This problem is such that there are no grounds for confidence in any alleged general pattern linking inequality with obvious economic or political outcomes. Second, what data there are, treated with due caution and without technical sophistry, suggest strongly that there is no clear pattern tying income inequality directly to civil war. Third, this does not mean that economic inequality is irrelevant to the study of civil conflict. Inequality may be associated with social conflict and with violence but not necessarily with civil war. In other words, the social and political consequences of sharp economic inequality are likely to vary across countries. Why inequality appears to be linked to war in some countries (indirectly) and why it appears to be associated more elsewhere with the kind of pervasive violence in Brazil and regions of India needs further exploration. Indeed, this point suggests that much of the recent expansion of literature on civil wars and/or complex emergencies suffers from an artificial cordon that isolates its arguments from relevant fields of study into other forms of social conflict and violence. Fourth, what matters in studying the role of economic inequality in civil conflict of one kind or another is how inequality is institutionalised and shaped by history and changes in social relations. Above all, this means that there is a greater range of outcomes from a given order of economic inequality than is often realised, and that probabilistic exercises in prediction are likely to be futile. Fifth, violence, war and social conflict are themselves as much the machinery that produces inequality and new relational moulds within which economic inequality is shaped as being the outcomes of that inequality.

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