Inequality and Conflict

A Review of an Age-Old Concern

Christopher Cramer

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Acronyms

CHE	complex humanitarian emergencies
EI-PC	economic inequality–political conflict
FRELIMO	Frente de Libertação de Moçambique *(Mozambican Liberation Front)*
GDP	gross domestic product
OECD	Organisation for Economic Co-operation and Development
RENAMO	Resistência Nacional Moçambicana *(Mozambican National Resistance)*
SRP	scientific research programme

Summary / Résumé / Resumen

Summary

The links between inequality and violent conflict are among the oldest concerns in political economy. It is almost a universal assumption that an inequitable distribution of resources and wealth will provoke violent rebellion. And yet it is just as obvious and historically established that sharply skewed income and wealth distribution does not always or even usually lead to rebellion. Usually, this is taken to mean that the inequality is legitimized in one way or another; that the inequality comes with a degree of power and repression that are simply too great to overcome; or that there are various obstacles preventing collective action.

This paper by Christopher Cramer develops an overview of the main currents of thinking about the inequality-conflict debate, with a focus on the link from inequality to conflict. The author says that, in spite of the fact that inequality and violence are a constant in human society, organized violent political conflict only takes place from time to time and is interspersed with periods of peace. He says that this could be due to three possible reasons: (i) inequality might not be a cause of conflict, or it is perhaps neither necessary nor sufficient for violent conflict; (ii) rather than the mere fact of inequality, particular characteristics of inequality might be more relevant; and (iii) perhaps something in the intensity of inequality, measured in various ways, may be relevant to the outbreak of violent conflict (implying a threshold that itself may vary with social, political and cultural conditions as well as with the average level of income).

The study of inequality usually involves the study of symptoms and outcomes. This is especially true of large sample quantitative studies of the links between inequality and political conflict. However, Cramer says that to understand the links from inequality to conflict—rather than just trying to identify statistical patterns of event regularity—it is important to study the factors that produce and underpin inequality and how this might relate to conflict. This is all the more necessary if large sample quantitative studies do not generate unequivocal results.

The paper argues for a relational analysis of inequality and conflict, discussing alternative conceptions of such an analysis. Section one examines whether different claims about inequality fit neatly into distinct theories of conflict; and section two assesses the various social science claims about the links between (chiefly income) inequality and violent political conflict.

Cramer says that the long history of interest in the links between inequality and violent conflict has not been matched by an evolving progression in theory or empirical certainty. There remains huge indeterminacy in the discussion of linkages between economic inequality and violent political conflict. The paper highlights the empirical weaknesses of the vast majority of claims made in this field.

Cramer maintains that in terms of research that generates a growing body of knowledge, much of the literature, when viewed in these terms of conflicting claims based on large samples of countries, has been fruitless. Two main reasons for this are lack of clarity in categorization systems and definitions, and poverty of data (including on inequality, political violence and civil war). The latter is due to the shortcomings and lack of comparability in much of the data from developing countries and the fact that the consequences of violent political conflict make it difficult to collect reliable data.

While universal claims about the inequality-conflict link are not wholly convincing, there has nonetheless been some fruitful theoretical thinking on inequality that might generate new empirical research into its role in the origins and spread of violent political conflict. Cramer feels that future research should be encouraged to develop comparative case studies that have historical depth and look at specific problems in varying contexts, using smaller samples of comparison.

Christopher Cramer is Senior Lecturer in Development Studies and Programme Convenor for the Master's degree course in Violence, Conflict and Development in the Department of Development Studies, School of Oriental and African Studies, United Kingdom.

Résumé

Les liens qui peuvent exister entre inégalité et conflit violent sont à l'origine d'une des plus anciennes inquiétudes qui soient en économie politique. Selon une hypothèse quasi universelle, une répartition inique des ressources et des richesses provoque une révolte violente. Et pourtant, l'histoire montre et même prouve qu'une répartition très faussée des revenus et des richesses ne débouche pas toujours ni même généralement sur la révolte. On en déduit le plus souvent que l'inégalité est légitimée d'une manière ou d'une autre; qu'elle s'accompagne d'un pouvoir trop grand et d'une répression trop sévère pour être vaincus ou que divers facteurs font obstacle à une action collective.

Ce document de Christopher Cramer donne une vue d'ensemble des grands courants de pensée qui se sont penchés sur la question des rapports entre l'inégalité et le conflit, en s'intéressant surtout au lien de cause à effet entre inégalité et conflit. L'auteur explique que, bien que l'inégalité et la violence soient des constantes de la société humaine, le conflit politique violent et organisé n'éclate que de temps à autre et alterne avec des périodes de paix. Il invoque trois raisons possibles à cela: (i) l'inégalité pourrait ne pas être une cause de conflit, ou n'est peut-être ni nécessaire ni suffisante pour provoquer un conflit violent; (ii) plutôt que l'inégalité en soi, des caractéristiques particulières de l'inégalité pourraient être génératrices de conflit; et (iii) un certain degré d'inégalité, mesuré de diverses manières, pourrait avoir un rapport avec l'éclatement d'un conflit violent (ce qui supposerait qu'il existe un seuil qui lui-même peut varier selon les conditions sociales, politiques et culturelles et avec le niveau de revenu moyen).

Lorsqu'on étudie l'inégalité, on est amené le plus souvent à en étudier les symptômes et les résultats. Cela vaut en particulier pour les études quantitatives des liens entre inégalité et conflit politique, qui portent sur de larges échantillons. Cependant, selon Christopher Cramer, pour comprendre les liens entre inégalité et conflit—plutôt que d'essayer de dégager un schéma statistique de la régularité des événements—il est important d'étudier les facteurs qui produisent et sous-tendent l'inégalité, ainsi que les rapports qu'ils peuvent avoir avec le conflit, surtout si les études quantitatives effectuées sur de larges échantillons donnent des résultats ambigus.

L'auteur plaide pour une analyse relationnelle de l'inégalité et du conflit, en traitant des diverses manières de concevoir une telle analyse. Dans la première section, il examine des assertions différentes sur l'inégalité pour voir si elles cadrent bien avec telle ou telle théorie du conflit et, dans la deuxième section, analyse les différentes propositions des sciences sociales sur les liens entre l'inégalité (principalement des revenus) et le conflit politique violent.

Selon Christopher Cramer, si les liens entre inégalité et conflit violent suscitent depuis longtemps l'intérêt, la théorie ou la certitude empirique n'en a pas pour autant évolué ou progressé. Il reste beaucoup d'indétermination dans le traitement des liens entre l'inégalité économique et le conflit politique violent. L'auteur met en évidence les faiblesses empiriques de la grande majorité des affirmations avancées dans ce domaine.

Selon lui, si la recherche a produit un corpus de connaissances qui vont en s'accumulant, une grande partie de la littérature, si l'on y voit des affirmations contradictoires fondées sur de larges échantillons de pays, a été stérile. Le manque de clarté des systèmes de catégorisation et des définitions, et l'indigence des données (sur l'inégalité, la violence politique, la guerre civile, etc.) en sont deux des principales raisons. Cette dernière est due au caractère lacunaire d'une grande partie des données provenant de pays en développement, à leur absence de comparabilité et au fait que les conséquences du conflit politique violent rendent difficile la collecte de données fiables.

Si les affirmations universelles sur le rapport entre inégalité et conflit ne sont pas totalement convaincantes, il y a eu néanmoins une réflexion théorique fructueuse sur l'inégalité, qui pourrait donner lieu à de nouvelles recherches empiriques sur le rôle de l'inégalité dans la genèse et la propagation du conflit politique violent. Christopher Cramer estime qu'il faudrait à l'avenir encourager la recherche à faire des études de cas comparatives qui aient une profondeur historique et examinent des problèmes spécifiques dans des contextes divers, sur la base d'échantillons de comparaison plus modestes.

Christopher Cramer est maître de conférences en études du développement et coordonnateur du programme pour le cours de maîtrise sur la violence, le conflit et le développement au Département des études du développement de la School of Oriental and African Studies au Royaume-Uni.

Resumen

El tema de la relación entre la desigualdad y los conflictos violentos es uno de los más antiguos temas de interés de la economía política. Constituye casi un supuesto universal el decir que una distribución desigual de los recursos y la riqueza generará una rebelión violenta. Y sin embargo, resulta igualmente obvio y está históricamente establecido que una distribución marcadamente asimétrica de los ingresos y la riqueza no siempre, ni siquiera con frecuencia, se traduce en rebelión. Por lo general, este hecho se interpreta como una legitimación de la desigualdad, como que la desigualdad trae consigo cierto grado de poder y represión que es simplemente demasiado grande para superarla, o que existen diversos obstáculos que evitan la acción colectiva.

Christopher Cramer presenta en su documento una exposición general de las principales corrientes de pensamiento sobre el debate desigualdad-conflicto, y se centra en el vínculo que lleva de la desigualdad al conflicto. El autor señala que, a pesar de que la desigualdad y la violencia son una constante en la sociedad humana, los conflictos políticos violentos organizados se presentan únicamente cada cierto tiempo, y entre ellos se intercalan períodos de paz. Sostiene el autor que ello podría deberse a tres razones: (i) la desigualdad podría no ser una causa de conflicto, o quizás no sea una causa necesaria ni suficiente para generar un conflicto violento; (ii) en lugar del simple hecho de la desigualdad, quizás resulten pertinentes ciertas características específicas de la desigualdad; y (iii) quizás haya algo en la intensidad de la desigualdad, medida de distintas maneras, que resulta pertinente para el inicio del conflicto violento (lo cual implica un umbral que podrá variar con las condiciones sociales, políticas y culturales, así como con el nivel promedio de ingresos).

El estudio de la desigualdad por lo general implica el estudio de síntomas y resultados. Esto es cierto sobre todo en el caso de los estudios cuantitativos de grandes muestras sobre las relaciones entre la desigualdad y los conflictos políticos. Sin embargo, Cramer argumenta que para poder entender el vínculo que lleva de la desigualdad al conflicto—en lugar de simplemente intentar definir patrones estadísticos de la regularidad de los eventos—es importante estudiar los factores que producen y apuntalan la desigualdad y la forma en que esto puede relacionarse con el conflicto. Esto es aún más necesario si los estudios cuantitativos de grandes muestras no arrojan resultados inequívocos.

En el presente documento, el autor postula un análisis relacional de la desigualdad y el conflicto y examina distintas concepciones de dicho análisis. En la sección uno se discute si distintas afirmaciones sobre la desigualdad encajan claramente en teorías diferentes de conflicto. En la sección dos se evalúan los diversos argumentos de las ciencias sociales sobre los vínculos entre la desigualdad (principalmente de ingresos) y los conflictos políticos violentos.

Cramer sostiene que la larga historia de intereses en las relaciones entre la desigualdad y los conflictos violentos no ha conocido una progresión equivalente de la teoría o la certidumbre empírica. Queda una enorme indeterminación en los debates sobre los vínculos entre la

desigualdad económica y los conflictos políticos violentos. En el documento se destacan las deficiencias empíricas de la gran mayoría de las afirmaciones hechas en esta materia.

En cuanto a las investigaciones que generan un volumen creciente de conocimiento, Cramer sostiene que buena parte de la bibliografía especializada, al enfocársele desde esta perspectiva de afirmaciones encontradas basadas en grandes muestras de países, resulta poco productiva. Dos de las principales razones que explican esta situación son la falta de claridad en los sistemas de categorización y las definiciones y la insuficiencia de datos (sobre desigualdad, violencia política, guerra civil, etc.). Esto último se debe a las carencias y falta de comparabilidad entre gran parte de los datos de los países en desarrollo, y al hecho de que las consecuencias de los conflictos políticos violentos dificultan la recolección de datos fiables.

Si bien las afirmaciones universales sobre el vínculo desigualdad-conflicto no son totalmente convincentes, ha habido cierta reflexión teórica fructífera sobre la desigualdad que podría generar nuevas investigaciones empíricas sobre el papel de ésta en los orígenes y la propagación de conflictos políticos violentos. Cramer opina que debería fomentarse la conducción de nuevas investigaciones para desarrollar estudios de caso comparativos que tengan profundidad histórica y se ocupen de problemas específicos en contextos diversos, a partir de muestras más pequeñas.

Christopher Cramer es Catedrático Principal de Estudios de Desarrollo y Coordinador de Programa del curso de maestría sobre Violencia, conflicto y desarrollo del Departamento de Estudios de Desarrollo de la Escuela de Estudios Orientales y Africanos, Reino Unido.

Introduction

The links between inequality and violent conflict is one of the oldest concerns of political economy. "We maintain that if a state is to avoid the greatest plague of all—I mean civil war, though civil disintegration would be a better term—extreme poverty and wealth must not be allowed to arise in any section of the citizen-body, because both lead to both these disasters" (Plato, cited in Cowell 1985:21).[1] It is almost a universal assumption that an inequitable distribution of resources and wealth will provoke violent rebellion. For example, in 1562 Montaigne met a small group of Indians from Brazil (the subject of his essay, On Cannibals) and asked them what they found most remarkable about their visit to France. One of the answers was that they

> had noticed among us some men gorged to the full with things of every sort while their other halves were beggars at their doors, emaciated with hunger and poverty. They found it strange that these poverty-stricken halves [sic] should suffer such injustice, and that they did not take the others by the throat or set fire to their houses (Montaigne 1981:119).

And yet it is just as obvious and historically established that sharply skewed income and wealth distribution does not always or even usually lead to rebellion. This is generally taken to mean that the inequality is legitimized in one way or another; that the inequality comes with a degree of power and repression that are simply too great to overcome; or that there are various obstacles preventing collective action.

Inequality in some form or other is a constant of all hierarchical social order. Violence too is prevalent in human society. However, while organized violent political conflict ebbs and flows: it is interspersed with periods of peace.[2] Thus

- inequality may not be a cause of conflict and may not perhaps be necessary or sufficient for violent conflict;
- some characteristics of this inequality, rather than inequality itself, may be more relevant; and
- perhaps something in the intensity of inequality, measured in various ways (including but not limited to the Gini coefficient[3]) may be relevant to the outbreak of violent conflict (implying a threshold that itself may vary with social, political and cultural conditions as well as with the average level of income).

Studying inequality usually involves the study of symptoms and outcomes. This is especially true of large sample quantitative studies of the links between inequality and political conflict. However, to understand the links from inequality to conflict (if they exist), rather than just trying to identify statistical patterns of event regularity, we need to study what produces and underpins inequality and how this might relate to conflict. This is all the more necessary if large sample quantitative studies do not generate unequivocal results.

Yet the long history of interest in the links between inequality and violent conflict has not been matched by an evolving progression in theory or empirical certainty. Lichbach (1989) frets over the lack of theoretical progress. And Cramer (2003) argues that empirically there remains huge indeterminacy in this discussion of linkages between economic inequality and violent political conflict. One of the more recent trends in thinking about violent conflict has been the ascendancy of neoclassical economic analysis and modelling. Much of this eschews reference to

1 "At least since Aristotle, theorists have believed that political discontent and its consequences—protest, instability, violence, revolution—depend not only on the absolute level of economic well-being, but also on the distribution of wealth" (Nagel 1974:453).

2 Conflict is an ever-present feature of all societies, but organized political violence is less constant within particular societies. This is so even for societies such as medieval Europe where interstate warfare was more or less institutionalized, war was "what states did" and where ruling classes were essentially war-oriented. The very idea of a lasting social and political peace is, according to Howard (2000), a modern invention.

3 A measure of inequality within a population.

earlier studies, as though work in other disciplines were a territory without history.[4] However, there is really little, if anything, new in this neoclassical economic explanation of violent conflict. Most of the explanations have been put forward in earlier rational choice, methodologically individualist work.

This paper develops an overview of the main currents of thinking about the inequality-conflict debate. Its focus is on the link from inequality to conflict, neglecting the extremely interesting reverse relationship, whereby—as is common—violent political conflict has powerful distributional effects. It also highlights the empirical weaknesses of the vast majority of claims made in this field, particularly those based on large sample statistical tests of inequality-conflict hypotheses. The paper also argues for a relational analysis of inequality and conflict, discussing alternative conceptions of such an analysis. The argument is that even in relational analyses there has been remarkably little attention to the characteristics of "late late development", especially given the prevalence of armed conflicts in low- and middle-income countries. Section one examines whether different claims about inequality fit neatly into distinct theories of conflict. Section two assesses the various social science claims about the links between (chiefly income) inequality and violent political conflict.

1. Theoretical Frameworks for Research on Conflict and Inequality

There are various ways of distinguishing among theories of violent conflict at a general theoretical level: that is, distinguishing what kind of theoretical tradition they spring from. This section briefly characterizes a few of these schema and general forms of theoretical framework for explaining violent political conflict. The set of approaches highlighted might not be exhaustive but does capture most of the range as well as illustrating a dynamic aspect of the question, such as how various theoretical approaches have fared during the past 20 or 30 years. The discussion makes one broad illustrative point. A comparison of these frameworks shows that claims about the link from inequality to violent political conflict may reflect more than one analytical approach. Overlapping claims about an inequality-conflict relationship may contain contrasting theories of the mechanisms by which inequality "translates" into violent conflict. This review partly hopes to disentangle these claims. It also hopes to highlight the enduring imprecision in most discussions of what Lichbach called the "economic inequality–political conflict (EI-PC) nexus" (Lichbach 1989).

Since the literature on inequality and conflict is generally located within the political economy of development, the discussion is restricted to conflict theories that have an obvious bearing on or source in this development field, including development economics, new growth theory, historical political economy and political science. Among various possible ways of organizing and classifying theories of conflict (particularly "internal war", "intra-state conflict" or "civil war"), three are particularly useful. One of these divides all such theories at a "branch point" where a fundamental choice of perspective is made: between contingency and inherence. A second distinguishes between "deprived-actor" theories and "rational-actor" theories. A third cuts the pack three ways: into theories, or stories, emphasizing behaviour, ideas or relations. However, this section also briefly highlights neoclassical economic theories of violent conflict. These theories have become increasingly popular and, in some circles, influential since the mid-1990s. Although representing one strand of rational-actor theory, orthodox economic theories or models themselves are somewhat inconsistent and allow for varying distributional arguments.

4 The image of a territorial encounter, between disciplines, is appropriate for a field in which some claim (either with pleasure or revulsion) that there has been a process of "economics imperialism". On the implications of neoclassical economic theories of civil war and other violent conflicts, see Cramer (2002).

Table 1: The role of inequality in different schema for analysing violence conflict

Approach	Analytical characteristics	Affinity with inequality-conflict linkages
Inherency/contingency		
Inherency	Violence is an ever-present option in social life, and it takes rather little to be "chosen" over non-violent action.	Violence does not especially "need" structural inequality, but some forms of collective inequality might be relevant as sources of coordinated interests leading to violence. Coercive balance is of primary importance.
Contingency	Violent collective action is a rare event, produced by an uncommon combination of factors heavily influenced by contingency or "accident".	Inequality is an important source of perceived "relative deprivation", but requires a delicate combination of other factors to lead to violence. "Virulent effect" is more important, coercive balance less so.
Ideas, behaviour and relations		
Ideas	Beliefs, values, ideologies are the key to violent conflict as to other social actions.	Ideas and ideologies influence the politicization of, or—by contrast—the legitimization of (or diversion from), inequality. More critical for some than others as a driving force of conflict.
Behaviour	People are driven by "inherited" behavioural reflexes.	Violence may stem from some "innate aggression" but competition rather than inequality is a more likely key.
Relations	Systematic, institutionalized social relationships are the source of violence/violent conflict.	Inequality may trigger violence as either the basis of comparison (envy) or the product of direct relations of exploitation.
Deprived actor/rational actor		
Deprived actor	Hearts and minds, grievances, preferences and sympathies are what matter. It focuses on expectations that are formed relative to others.	Grievances and anger/frustration are generated by discrepancies in conditions and expectation shortfalls, leading (sometimes) to violent political reaction.
Rational actor	Opportunities, costs and benefits, and resources are key. Objectives are not formed relative to others.	Increasing inequality may lead to rising absolute deprivation, and this may possibly lead to rebellion if collective action constraints are overcome. On balance, high inequality will not lead to rebellion (not providing sufficient "opportunity").
Neoclassical economics		
Endogenous growth theory	A large range of variables (investment rates, including in "human capital", constant or increasing returns, policy variables, and so on) may influence outcomes and make growth rates "endogenous" rather than exogenous.	Inequality produces market and policy distortions that emit investment disincentive signals to private investors. One of these effects might be political violence.
Economic theory of conflict	Rational choice of individuals determines the "choice" of violence, depending on conflict/cooperation trade-off and the opportunity cost of violence.	Given that inequality is not a central concern of neoclassical economics, it plays an unsurprisingly little role in recent neoclassical economic theories of conflict. Allocative/distributive conflict is more about rent-appropriation and elite rivalry, and sometimes poverty, than social inequality. This is captured in the "greed versus grievance" distinction.

Eckstein (1980) argued that all theories of conflict prioritized at a fundamental level either the significance of contingency in unleashing collective violence or that of inherent propensities to violence. Collective violence driven by contingency is, according to this framework, largely affective, though this does not rule out some purposive, rational influence. By contrast, theories stressing an inherent disposition toward collective violence emphasise rational calculation over "irrational", affective outbursts.[5]

From the perspective of contingency, war and other forms of collective violence are rarities or abnormal; and they are produced by extraordinary circumstances that work, typically, through what has been called the "frustration-aggression" nexus.[6] Processes of development are responsible for eruptions of collective violence since they cut people loose from their "traditional" moorings (and mores), producing extreme frustrations, while the formation of new political institutions, which might manage the consequences of frustration, often lag behind. A range of mediating factors, secondary but still significant, and the locus of contingency, are necessary prompters of violence: cultures of violence, perhaps; ideological provocations; and/or the balance of coercive powers. The most renowned, and still influential, example of this kind of theory about collective violence was Gurr's (1968, 1970) relative deprivation theory. Relative deprivation captured people's "perceptions of discrepancy between their value expectations (the goods and conditions of the life to which they believe they are justifiably entitled) and their value capabilities (the amounts of those goods and conditions that they think they are able to get and keep)". Note that this is a tension akin to Gerschenkron's (1962) in late industrializing nations between the promise of the benefits of industrialization and the obstacles blocking the way to reaping those benefits. Thus, for Gurr, relative deprivation was likely to increase during the early stages of development, producing frustration and a socially unusual proclivity to aggression that might materialize into actual collective violence, given the specific constellation of (contingent) mediating factors. Gurr and Duvall (1973) formulated the link to political violence thus:

$$\text{Magnitude of political violence} = RD + (RD \times JUST \times BALANCE) + e$$

where RD is relative deprivation, JUST is justification (beliefs, traditions), BALANCE captures the distribution of coercive and institution resources of dissidents versus the state, and e is an error term. This formulation neatly captured the mediating role of balance and justification. "Justification" has been sustained in the literature, for example, the recent emphasis on "perception" of grievance, inequality and so on, rather than on their objective presence only as an important determinant of conflict (Keen 1997; Stewart 2000).

So-called inherency theories do not presume that collective violence is produced by aberrant conditions or rare combinations of extraordinary forces. Rather, they rest on an assumption that violence is an ever-present, entirely normal disposition in political life: violence is just one stretch of a continuum of collective action encompassing a range of forms of political competition. And violence, or non-violence, is picked out from this continuum of options on the basis of rational calculation. Processes of development may or may not present cost-benefit judgements that favour collective violence. Eckstein (1980) pictured this kind of analysis in terms of collective action theory. Thus, political life is dominated by interest groups vying for power. Non-members of some core ruling alliance of interests would make claims, demanding entry or recompense for claims made upon them. Depending on the reaction of the core group, this conflict of interests might lead to violence, for example, around "multiple sovereignty" claims. Especially important in this model is the balance of coercive power and resources

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[5] Something is contingent if "its occurrence depends on the presence of unusual (we might say aberrant) conditions that occur accidentally—conditions that involve a large component of chance" (Eckstein 1980:138). Something is inherent, by contrast, if either it always will happen or the potentiality for it always exists. Just when the inevitable happens or hindrances are removed is decided by contingencies. With inherency, according to Eckstein's distinction, we want to know why the inherent did not occur sooner, what blocked it; but in contingency approaches the puzzle is why, rather than why not.

[6] A comparable approach is Nairn's (1998) model of the delicate and rare combination of conditions whose assembly makes for the "genocidal conjuncture" by analogy with thermonuclear fusion.

between groups. For it is precisely on the basis of this balance that groups will calculate the utility of war. Given that tactical choice based on objective indicators matters more than "virulent affect" (Eckstein 1980) in inherency theories, cultural traditions and social memory matter far less than in contingency theories. Nonetheless, it is still unclear why collective violence is not more common, from this perspective; and the answer will to some extent be driven by contingencies, accidents of history affecting facilitating factors and the coercive balance.

Another review of the inequality-conflict linkage (Lichbach 1989) argues for a fundamental division between analytical frameworks or "scientific research programmes" (SRPs) in terms of the deprived-actor/rational-actor distinction. Deprived-actor explanations stress the role of preferences, beliefs, values, hearts and minds. People form expectations, but in deprived-actor stories these are formed relative to other people's attributes, experience or performance. Rational-actor stories, on the other hand, assume that expectations and objectives are identified not relative to others but in self-referential processes. And decisions are taken largely in calculative terms: choices are made, based on cost-benefit judgements, and the keys lie in resources and opportunities more than beliefs.

Eckstein associates inherency theory especially with the work of Tilly (1970) on state formation and war, particularly in early modern Europe; and Lichbach identifies Tilly as a particular critic of the deprived-actor theory. In Tilly's work, with its focus on the politics of resource mobilization, challengers make claims that are incompatible with the survival of the state in its existing form. These claims gather legitimacy when the state alienates significant groups of people by its failure to meet obligations or make reciprocal transfers to balance its own claims (taxes, levies) upon the population. If the central power cannot or will not effectively block the scope for extreme action, there is then a moment of multiple sovereignty in which mutually exclusive claims generate violence. Collective violence, in this kind of scenario, is a function of a lack of power, that is, where constraints on an inherent disposition toward violence are not effectively blocked.

Tilly himself (2000) has produced an alternative schema for distinguishing between theories of violent conflict. Thus, there are ideas theories, behaviour theories and relations theories of violence. For the purposes of this paper, it is important only to note the following. First, Tilly observes that many explanations of social and political violence involve combinations of at least two of these underlying theoretical perspectives. Second, this analytical schema suggests a further distinction. For while Tilly is viewed as a member of the rational-actor, inherency-theory camp, this camp itself is not harmonious. Tilly's work is clearly "relational" compared, for example, to neoclassical economic theories of conflict that are also very much rational-actor stories. Third, and related to this, there are different versions of what constitutes rationality. Neoclassical economic theory is based on methodological individualism and rational choice, in which rationality is basically non-relational and is tied to specific maximizing utilitarian goals. However, there are other theories of rationality that prioritize relational rationality and a wider range of goals influencing rational calculation.[7] Further, an analysis may be influenced strongly by rational-actor assumptions but not be methodologically individualist.

There is no need in this paper to explore in depth the relative shortcomings and values of these alternative schemes for classifying theories of violent conflict. It is, however, worth highlighting the following points: first, there have been significant recent developments since Eckstein's and Lichbach's reviews; second, all of these various theoretical perspectives influence thinking about the linkages between inequality and conflict; and third, all of them leave important puzzles unresolved.

In recent years, neoclassical economics has escaped its own confines and addressed a far wider range of social phenomena than before, including collective violence and war. According to

7 Pizzorno 1986; Sen 1986; Zafirovski 2000.

some, there has been a period of "economics imperialism", as both its detractors and champions call it, and the analytical tools and fundamental axioms of neoclassical economics have come to dominate the social sciences in general.[8] Certainly, neoclassical economics has had increasing influence on the study of violent conflict in developing countries in recent years. This has been driven especially by the theoretical work of Hirshleifer (1994), among others, and by the empirical models and policy work of Collier (2000) and his colleagues. Cramer (2002) provides a critique of this literature.

One apparent inconsistency in Collier—or, alternatively, one source of overlap between contingency and inherency theories—is that he claims to look for the rare but his assumptions, which, given his models, really derive theoretically from Hirshleifer, are that violence is a constant possibility, an option available to choice-making individuals.[9] Arguably, the root of this ambiguity lies in an assumption that normality contains market perfection, since this is, after all, the benchmark against which he judges opportunities for violence to spring from market "distortion", suggesting that capitalism itself is naturally pacific. Keynes too assumed that economic activity and capitalism would overcome a fundamental disposition to violence, turning interests against passions (cited in Hirschman 1977).

Recent work by neoclassical economists has also created an analytical distinction between *greed* and *grievance* as alternative sources of civil war. This distinction also affects judgements of the role of inequality in the origins of wars. The greed and grievance distinction has achieved a fairly widespread influence. Although proponents of this distinction do not couch their analysis explicitly in these terms, grievance echoes "affective" outbursts of violence generated from the frustration-aggression nexus, while greed reflects cool calculative rationality associated with the inherency theory.

Inequality may have a role in more than one of these analytical perspectives. At first sight, though, inequality fits most neatly into a tradition of Eckstein's contingency theories, or Collier's (rejected) "grievance"-based war. Inequality as a source of conflict is not precisely equivalent to relative deprivation. For relative deprivation focuses on the frustration engendered by current deprivation relative not necessarily to others within a polity but to what people perceive as justifiable. A society could have a fairly even distribution of goods, assets and even opportunities but still fail to live up to people's perceptions, which no doubt would cause widespread frustration. Nonetheless, various indicators of inequality within a society may be a fair proxy for relative deprivation, since it would seem to justify grounds for frustration relative to those doing well in that society. Inequality, in other words, is likely to reveal or provoke a sense of relative deprivation. Nafziger and Auvinen make the link explicit, arguing that relative deprivation "often results from vertical (class) or horizontal (regional or communal) inequality" (Nafziger and Auvinen 2002:154).

The important implication of relative deprivation theory, however, is that inequality itself, and even perceived relative deprivation, will not cause violence without other mediating factors, notably "justification". That justification may come from "culture". However, another possibility, more awkward to contain within Eckstein's schema, is that justification may come from political and economic process and social relations. At one extreme, it is argued by Grenier (1996) that inequality, relative deprivation and grievous social conditions are a constant in most societies and that, therefore, the significance of "ideological moments" is more than just a secondary factor. Grenier argues that the incidence of collective violence in Central America ebbs and flows with the influence of particular ideologies of various times such as, for example, at one stage the communist revolutionary ideas affecting Central American intellectuals in the aftermath of the Cuban Revolution and, at another stage, the influence of liberal democratic ideology. In this kind of story, the contingency of external ideological influence is elevated to a

[8] The metaphor of imperialism may in some ways be misleading since, for example, it implies a more involuntary submission to "foreign rule" than has been characteristic of the absorption by other social sciences of neoclassical economic assumptions and logic.

[9] This assumption is confirmed by the fact that there is no theory of how violence comes into the equation (literally).

central role, fitting nicely in Tilly's (2000) "ideas" category of theories of violent conflict. For Stewart (2000), relative deprivation, to cause violent conflict, must have an objective and subjective base in horizontal inequality, that is, unequal distribution along regional, religious or ethnic lines. The point here is that violence will emerge from collective action among competitive groups, mobilizing people around collective identity. Again, in this kind of story (see below for further detail) the brick wall dividing collective action from relative deprivation theories, or inherency from contingency theories, disintegrates.

Indeed, a priori there is nothing in most assumptions that inequality causes conflict that forces a fundamental choice between whether human society has an inherent, ever-present disposition toward collective violence and whether society is fundamentally peaceable. Nor is there a predetermined choice of whether—in violence potentially caused by inequality—violence is rational or affective. Inequality, therefore, may play a role both in theories that stress one or the other, or a combination of the two. Table 1, summarizes these different analytical perspectives and their basic implications for analyses of the EI-PC conflict linkage.

2. What Claims Have Been Made About the Relationship Between Inequality and Conflict?

> *It is perhaps not surprising that the politically most unstable nations are often characterised by market inequalities (Midgley 1984:65).*

One approach to the problem has been to look for observable event regularities on the basis of multicountry samples, usually testing cross-sectional distributional data against the incidence of various forms of collective violence, including civil wars, protests, political murders and unconstitutional regime changes, and the one more recently in vogue, complex humanitarian emergencies (CHEs). Distribution in these studies tends to be captured through either the income or the land Gini coefficients. A number of such studies did claim earlier to find some correlation but differed according to whether the data and their testing supported either a linear or a curvilinear relationship between distribution and collective violence. Thus Russett (1964) found evidence of a linear relationship in a study relating the Gini coefficient of land distribution to a cumulative score of violent political deaths per million between 1950 and 1962. On the other hand, Nagel (1974) argued for a curvilinear relationship, in this case starting from data from different provinces in Vietnam but also looking at a wider sample of cases. The curvilinear relationship takes the form of an inverted U shape: discontent begins at zero in an egalitarian society and increases up to some threshold level, beyond which discontent then tails off again as extreme inequality is approached. The argument would be that comparison is more odious where the difference between one person and a chosen comparator is fairly small, but that as this difference increases the tendency to compare oneself enviously, and hence to be locked into the frustration-aggression nexus, diminishes. Zimmerman (1980:202), summarizing his own review of the literature on inequality, among other macro-statistical variables, argues: "Altogether, the limited empirical evidence available suggests a linear positive relationship between socio-economic inequality and political violence". He cautions, nonetheless, that the data and selection methods in sampling undermine confidence in most such studies and highlights the weaknesses in relying on cross-sectional rather than longitudinal datasets.

Recent proponents of the argument that economic inequality brings about violent political conflict and instability include Muller (1997) and Nafziger and Auvinen (2002). Muller puts the argument in the context of debates over the relationship between development and democratization. While a rising level of income per capita might well be a nurturing source of democratization, largely because of the changes in the class composition and balance of society, Muller argues, there is typically a countervailing tendency insofar as rising levels of income usually imply increasing inequality in the distribution of income (accepting the Kuznets inverted-U hypothesis). This is because "a high level of income inequality radicalizes the working class, enhances class polarization, and reduces the tolerance of the bourgeoisie for political participation by the lower classes" (Muller 1997:137). Muller (1997) cites his own (1988) cross-national study of 33 countries that showed a correlation between income inequality and the binary variable of stability versus instability of democracy between 1960 and 1980. Nafziger and Auvinen (2002) list high-income inequality along with stagnation and decline in real gross domestic product (GDP), a high ratio of military expenditures to national income, and a tradition of violent conflict as the main sources of humanitarian emergencies. "Large income inequality exacerbates the vulnerability of populations to humanitarian emergencies" (Nafziger and Auvinen 2002:155). Nafziger and Auvinen use a Gini coefficient measure of inequality, applied to data from Deininger and Squire (1996), widely seen as the most reliable available data on inequality. This analysis explicitly claims that the evidence supports "objective grievances" contributing to war and humanitarian emergencies. The analysis also moves from a Gini coefficient measure of inequality to case study evidence of regional and ethnic, or "horizontal" inequalities arising from government discrimination (including Nigeria, South Africa, and Chiapas in Mexico).

There is also an argument from an orthodox economic perspective, stating that one of the reasons why inequality is bad for growth is that it provokes political instability and conflict. Nafziger and Auvinen (2002) cite in support the work of Alesina and Perotti (1996), whose cross-section study of 71 developing countries found that (for 1960–1985) income inequality was associated with social discontent and sociopolitical instability (measured, for example, by the incidence of political assassinations), which in turn are correlated with lower investment. Thus, inequality has of late been folded into the endogenous growth literature. In this literature, there are three main mechanisms by which inequality harms growth. First, inequality perpetuates a distorted human capital market, misallocating resources over time so that the wealthy overinvest in the education of their offspring and the poor underinvest. Suboptimal human capital investment then feeds into lower growth. Second, income inequality works on growth rates through a democratic political mechanism. Thus, where inequality is high, the "median voter" may be expected to vote for redistributive taxes which, in turn, will reduce the return to investment and therefore act as a disincentive to capital. Third, inequality may reduce growth rates by deterring investment through political instability or war. Such claims are backed up by the literature on income inequality, human and "social" capital, and violent crime. Fajnzylber et al. (1998), Hsieh and Pugh (1983) and Kennedy et al. (1998) all find in cross-sectional studies that increasing income inequality raises violent crime rates, while Wilson and Daly (1997) argue that life expectancy itself may be a psychosocial determinant of risk-taking behaviour, but that including in the model a measure of economic inequality adds significant extra predictive power.

If Alesina and Perotti (1996), Muller (1997), Nafziger and Auvinen (2002), and others subscribe implicitly to the linear relationship thesis, other work, mainly in the form of case studies, takes forward the idea of a curvilinear relationship. However, curvilinear relationships come in two basic varieties, each mirroring the other exactly—one taking a U shape and the other an inverted U shape. A U-shaped relationship, in which (possibly for each level of income) there is some optimal distribution that minimizes conflict, holds that either an increase or a decrease from this middle range of inequality will disturb the social peace. It might be in this middle range of inequality that Hirschman's (1981) "tolerance for inequality" dominates (see below, section four), according to which some people, observing the incomes of others pulling away from their own, interpret this as a signal of social mobility and patiently await a turn for the better in their own fortunes. However, at higher degrees of inequality this effect would break down, as many people severed their own expectations from those of the beneficiaries of a higher Gini coefficient. And lower levels of inequality might reflect stagnant hopes and a free-for-all. By contrast, an inverted U curve would trace a pattern in which rising inequality (across or within societies) is associated first with increasing violent conflict and then, when inequality has reached extreme levels, with a decline in the incidence of violent conflict. Higher degrees of inequality would, perhaps, be associated with extremes in the repressive capabilities of the state and elites. Nagel's (1974) version of the curvilinear relationship mapped differences in the intensity of violent conflict across societies marked along a range of degrees of inequality. However, it might be necessary to break down the relationship within particular societies. Thus, within a given society there might be both a sharp total range of inequality and also a series of smaller, more localized gaps in wealth, income, opportunity and so on. Potentially, these smaller but more localized gaps might be more relevant in the origin of violent conflicts than the full social spread captured, for example, in a single Gini coefficient. Formally, this might represent a curvilinear relationship between inequality and violence where inequality is increasing in localized areas, while national distributional extremes are less relevant to conflict. In other words, a curvilinear relationship might hold at local levels irrespective of whether total social inequality is high or low by comparison with other societies.

Alternatively, local distributional conflicts over a relatively short range of inequality may combine with the dynamics of conflict related to grander national inequality (or, indeed, to other causes). André and Platteau (1998), for example, argue that inequality nationally was not the prime source of the paroxysm of violence in Rwanda in 1994, but that, nonetheless, the increasing local level intensity of distributional tension contributed to the speed with which political violence spread through Rwandan society. Their own evidence shows an increasing intensity of local social conflict and disputes as a consequence of demographic pressure plus extensive rather than intensive agricultural development policies. The productivity constraint arising from this policy led to a shrinkage of average farm sizes and the emergence of a (formally illegal) land market, thanks to distress sales. Thus, some people were able to accumulate larger landholdings, generating inequality and tension. Other evidence suggests that while the overall Gini coefficient for Rwanda may not be extremely high by international standards, it is both an underestimate and on the increase (Storey 2000).

A related example is Kriger's (1992) work on peasant involvement in the liberation war in Zimbabwe. According to this argument, the liberation war did not succeed on the basis of the mobilization of the rural masses around a national redistribution project but because of the combination of nationalist ideologies with a patchwork of localized agendas of change, most involving one or another form of control over allocation decisions—for example, young men's interest in overturning gerontocratic village order, or women's struggles with men.

Within studies of the linkages of inequality to conflict, there is a distinction between arguments favouring the central role of *land* inequality and those stressing the significance of national and principally urban income inequality. André and Platteau's (1998) argument takes an intermediate and subtle position in this debate: claiming that rising land and economic inequality in rural Rwanda (driven by population increase and agricultural policies that failed to encourage intensification) was a secondary causal factor in the genocide, helping to determine the intensity of violence but not its basic origin. Others are more direct in their claims. For example, Binswanger et al. (1995) reduce 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 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).[10] Russett (1964) presented evidence of a linear relationship between the land tenure Gini coefficient and a cumulated score of violent political deaths (per million population) from 1950–1962, for 47 countries. And Huntington (1968:375) argued that "Where the conditions of land-

[10] Large-scale farms, according to Binswanger et al. (1995), produce static efficiency losses; dynamic efficiency losses due to reduced profitability of free peasant cultivation and poor incentives; resource costs because of 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. On a critical note, regarding Mozambique, it is clear that the Frente de Libertação de Moçambique's (Frelimo) rural policies contributed to widespread distrust of the post-independence government in rural areas and that this came to fuel support, tacitly or overtly, for the Resistência Nacional Moçambicana (Renamo) rebels (Clarence-Smith 1989). However, it verges on the far-fetched to attribute to this the principal cause of the war; and, second, poor policy management was the key factor rather than large farms per se (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).

ownership…are inequitable and where the peasant lives in poverty and suffering, revolution is likely, if not inevitable, unless the government takes prompt measures to remedy these conditions".

Midlarsky and Roberts (1985) argue that El Salvador and Nicaragua bear out the "land maldistribution" hypothesis, since compared to other middle-income developing countries, they experienced above average population growth in the 1950s and 1960s, their land Gini scores (around 0.8) were well above the global mean of 0.6, and landless households represented a high proportion of the total labour force. Muller and Seligson (1987), however, argue that this case is weakened by a closer comparison within the Central American region. Both Costa Rica and Panama, during the same period, were characterized by remarkably similar preconditions (presumed often to be the "objective conditions of grievance and conflict") and yet remained peaceful. Costa Rica's population growth rate from 1960 to 1970 was, at 3.4 per cent annually, higher than Nicaragua's or El Salvador's and among the highest in the world. Land concentration as captured in the land Gini coefficient was about the same in Costa Rica and Panama as it was in El Salvador and Nicaragua; and the first two countries also had substantial shares of landless households in the total labour force.[11]

Against these arguments—proposing linear or curvilinear relationships between inequality and violent political conflict or contrasting land with national income inequality, and from a variety of analytical perspectives—there are other claims that inequality simply is not a significant causal variable in the origins of conflict. 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 divisions, rather it actively needs to create them." (pp. 10–11). Using the same Deininger and Squire (1996) dataset mined by Nafziger and Auvinen (2002), with some of their own interpolations, Fearon and Laitin (2003) find contrary results: notably, 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. The poor quality of the inequality data, available for only 108 countries, 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" (p. 20). Mitchell (1968) and Parvin (1973) also offered evidence that, if anything, equality was more disturbing of social peace than higher inequality, which—through a combination of encouraging expectations of mobility and an increase in the repressive powers of elites—would contain conflict. An early version of Collier's models of civil war (Collier and Hoeffler 1996) also set out the hypothesis that higher degrees of inequality would be a disincentive to rebellion, since under these conditions it would be expected that elites would allow the state to raise taxation on their wealth in order to fund a military response sufficient to protect the status quo. Weede (1987), among others, gives evidence in support of this argument that basically there is no meaningful relationship, and thus that violent political conflict is typically caused by factors other than inequality. A final point here is to note that, historically, inequality varies very little and very slowly, and yet violent conflict and political violence appear to fluctuate more widely. Hence, at the very least, inequality must be insufficient to explain conflict, whether or not it is necessary.

Such an array of hypotheses and claims is bewildering. One explanation might be, as Lichbach (1989) argues, that the authors of different studies do not read one another; and that there is a babble of monologues rather than any constructive dialogue. "For example, students of black protest in the United States and of conflict cross-nationally have both been concerned with the EI-PC nexus, yet both have neglected each other's work" (p. 436). There have been some efforts to overcome this; for example, the World Bank's Web guide to inequality and its linkages to economic performance and social phenomena includes literature on violent crime as well as on civil war. Nonetheless, to some extent there is still a problem of insufficient engagement across

[11] Yashar's (1997) contrastive analysis of the history of Costa Rica and Guatemala focuses more on patterns of class formation and balance among interest groups (for example, the military, the state, large coffee plantation owners and coffee-related financial interests), and on the history of coalition formation as key to the sharply different political histories in the two countries.

studies and, indeed, methodologies. However, this can only help explain part of the problem. As we have seen, even studies using the same dataset (for example, Deininger and Squire's inequality dataset) come to different conclusions about the relationship between income inequality and violent political conflict, civil war or complex humanitarian emergencies.[12] This suggests that estimations of the relationship in statistical studies are sensitive to model specification and/or sampling variations. However, there are two larger issues that probably help to account for the cacophony of claims about inequality and its sociopolitical consequences. One is the poverty of relevant data, which is discussed in the next section. The other is how best to conceptualise inequality and to what extent systems of classification may impose artificial discontinuities on social realities, which is also discussed below, in section four.

3. Empirical Problems

Some of those who have recently argued that inequality is not a cause of political violence acknowledge that inequality data are highly imperfect and that this clouds our assessment of the relevance of inequality in conflict models. Thus Collier and Hoeffler (1998) note that "there is insufficient data to introduce distributional considerations into the empirical analysis". Meanwhile, as mentioned earlier, Fearon and Laitin (2003) state that because of the poor quality of the inequality data, it is difficult to go beyond the claim that there is no cross-national relationship between inequality and the onset of civil war. Therefore, rather than using this problem as a basis for rejecting the political significance of inequality in its entirety and substituting some other claim, the role of inequality might simply evade this kind of quantitative analysis. Section four below returns to this issue, while this section focuses on the empirical problem.

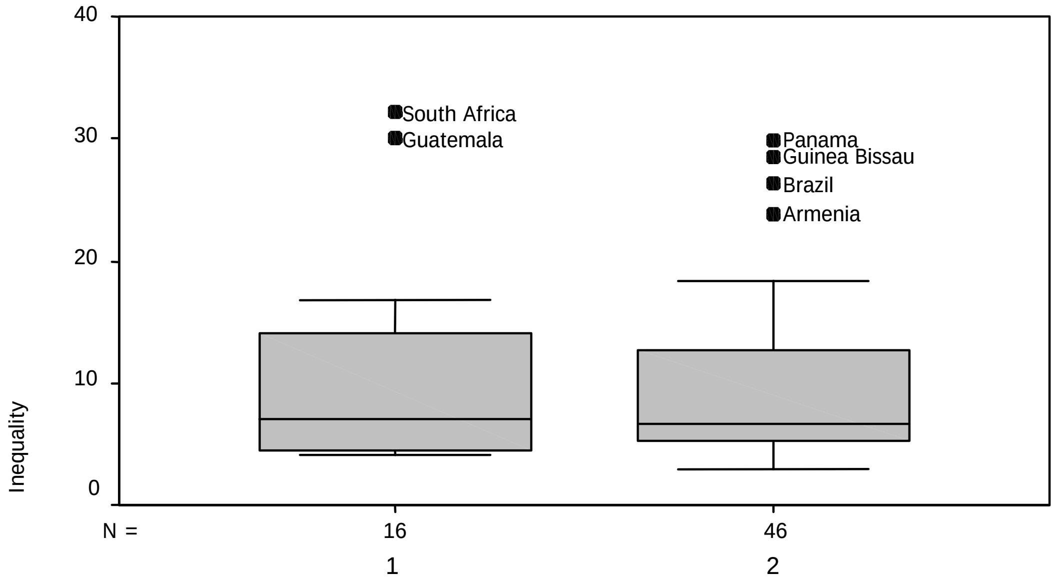
This empirical problem is in fact extreme. It extends beyond the quality of Gini coefficient data, though it certainly encompasses this issue. There are great difficulties in the measurements of both dependent and independent variables in trying to assess the role of inequality in the origin of violent political conflict. Data on income inequality are often insufficient, especially as a basis of intercountry comparison. Measuring land inequality is far from straightforward. And there are problems with available data (as well as categorical bases of datasets) for violence and conflict.

Figures 1 and 2 show that available evidence on Gini coefficients does not enable any claim to be made about a clear, systematic link between income inequality and "civil war". This is patently clear from the figures. The first illustrates only the post–Cold War period (sometimes associated with a sharp increase in the incidence of conflict), while the second summarizes the situation from 1944–2000. If the "conflict" box, that is, those countries that have had civil conflicts, was clearly linked to degrees of inequality, then that box would be distinctly higher in the chart than the "no conflict" box. However, statistical tests of this evidence confirm that there are no strong grounds for claiming a significant correlation between income inequality and civil war.[13] However, the data presented in these figures raise a number of questions. For example, are the data and the rankings (in terms of inequality) that they produce remotely reliable? Is the relationship between income inequality and violent political conflict effectively encompassed by defining the dependent variable as "civil conflict"? Are there clear distinctions between those countries that have experienced civil war and those that have not?

[12] Nafziger and Auvinen (2002) relate Gini coefficient data to CHEs, Fearon and Laitin (2003) relate Gini data to civil wars, and Cramer (2003) relates Gini data to both civil wars from 1944–2000 and post-Cold War civil wars.

[13] 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 (the amount of variation away from the average) 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. Thus, although there is marginally greater difference between the means in this more recent period, it is still overwhelmingly the case 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. 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.

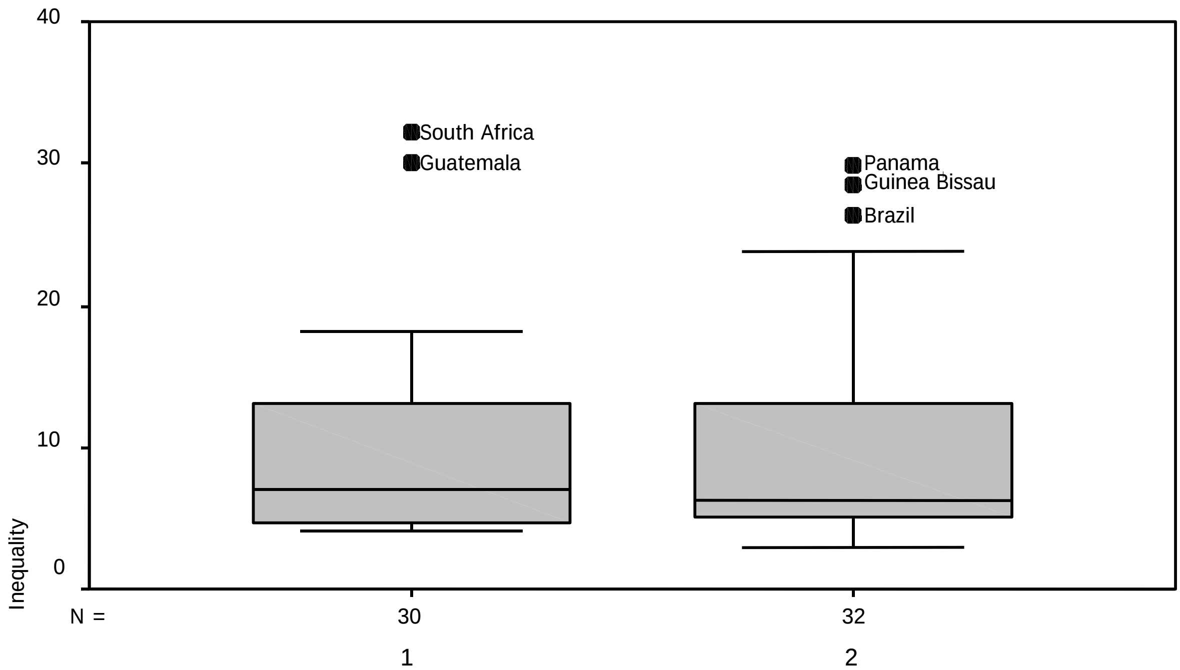
Figure 1: Post-1989 civil conflict



Notes: 1 = civil war; 2 = no civil war.

Sources: Wallensteen and Sollenberg 1998, 2000 (for figures on conflict); Deininger and Squire 1996 (for figures on inequality).

Figure 2: Civil conflict 1944–2000



Notes: 1 = civil war; 2 = no civil war.

Sources: Sambanis 2000 (for figures on conflict); Deininger and Squire 1996 (for figures on inequality).

However, we can neither accept nor reject the hypothesis that economic inequality is relevant to the outbreak of civil war if the data on inequality for purposes of comparison among a sample of countries is insufficient. Despite recent claims to improvements in Gini coefficient datasets, the data remain highly imperfect, to the point perhaps of being unusable in multicountry statistical tests. To take two anecdotal 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 of them 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 in and around Jakarta, for example, with its extravagant shopping emporia coexisting with extreme poverty and, further afield, dire indigence in rural areas (Asra 2000).[14] Rwanda is also more unequal, and increasingly so, than its Gini observations have suggested.[15]

Recent research into economic inequality in Latin America confirms the dangers of building causal arguments on inequality data. Székely and Hilgert (1999), analysing household survey data from 18 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 1999:1). This research only confirms the criticisms that have long been made of distributional data and the inferences that can be drawn from them.[16] These criticisms arguably amount to more than modest qualifications: rather, they suggest that—even if it were methodologically supportable—it is not possible to draw credible inferences from the available data on intercountry comparison about patterns of event regularity involving inequality in wealth and income. The problem with inequality data is not exclusive to poor countries. Atkinson and Brandolini (2001), for example, show the dramatic impact of adopting different datasets on inequality on rankings among countries of the Organisation for Economic Co-operation and Development (OECD).

If the focus is on "land inequality" rather than national income inequality (or the land Gini coefficient versus the national income Gini coefficient), there are still difficulties in measuring effectively for comparative purposes. For Central America up to the late 1970s, for example, the land Gini scores are virtually identical. Yet, historically, the structure of rural society in Central America has varied considerably. One may, for example, get more sense of variation from measures of "*minifundización*", that is, of the extent of impoverished smallholders in the presence of large farms. Thus Midlarsky's (1988) "patterned inequality" captures the concentration of landholdings between smallholders and large farmholders across different countries; while Muller et al. (1989) propose an index of "bifurcated inequality" derived from the proportion of small farms and from the average sizes of small farms relative to large farms. Or one may focus on the share of total agricultural land dominated by the largest farms, to capture their control of land supply (Brockett 1998:172). Further, rural inequality involves tenancy, and tenancy arrangements vary widely in security and the income-generating potential of the land they farm. Beyond this it is necessary to examine also the characteristics of rural labour markets and how these mediate conditions of life and political organization.

Meanwhile, there are considerable complications with the empirical analysis of "violent political conflict".[17] These complications take two main forms: the reliability of evidence for instances and

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14 For a good basic visual image of this inequality, see the photographs of Jakarta in Salgado (2000:355, 360).

15 See Braeckman (1996); Maton (1994) and Storey (2000).

16 Fields 1989; Moll 1992; Kanbur 1998; Bowman 1997; Cramer 2000.

17 There are also, of course, immense difficulties with establishing reliable and meaningful datasets for other variables typically used in quantitative studies of conflict. One example is the treatment in such studies of ethnicity. There are two weaknesses in the use of a quantitative ethnicity variable. First, to quantify ethnic diversity involves constructing an index that scores for diversity, but this imposes a uniform social significance of ethnic identity or affiliation across a wide range of societies, such that a given score of ethnicity "means" something similar throughout the sample. This cannot be done without drawing (usually implicitly) on a "primordialist" view of ethnicity that precludes any subtlety of historical construction, manipulation or flux in collective identity and its social significance. Second, the most common measure of ethnic diversity is the (Soviet origin) index measuring the probability that

degrees of intensity of conflict; and the choice of categorization systems. The first problems arise with the reporting of violence. In wartime, health information systems and, particularly, civil registration systems that would record deaths and their causes, often break down (Murray et al. 2002:346). Moreover, there are complications of interest as well as practical constraints that undermine reliable data collection involving political violence, armed conflict or even some forms of interpersonal or "social" violence. Research from the Medical Research Council (Jewkes and Abrahams 2000) in South Africa, for example, uses the analytical device of an "iceberg of sexual violence and coercion": rapes reported to and recorded by the police represent only the tip protruding above the social surface. In northeast Africa, there have been claims of a trend of rising violent conflict between herders and farmers; however, it is difficult to confirm or reject such claims when there are only occasional and variable exercises in recording such conflict and, thus, there is no reliable time-series data (Hussein et al. 1999). As Murray et al. (2002) point out, most published analyses of deaths from conflict have relied on press reports of eyewitness accounts and of official pronouncements from conflict contestants. Both sources are clearly subject to error and political manipulation. Brockett (1992) argues that it is difficult to assess the relative merits of competing arguments concerning the political consequences of land inequality when they rely on "the grossly inaccurate data set provided by the *World Handbook of Political and Social Indicators* (Taylor and Jodice 1983)" (Brockett 1992:169). It is not just that this dataset underestimates the incidence of political violence in various countries, but, more significantly, that "a more accurate reporting would alter both the rankings between countries and the magnitude of the intervals between country scores, thereby substantially altering quantitative analyses utilizing this data set" (Brockett 1998:170). Thus, "the *Handbook* reports deaths in Honduras during the mid-1970s as twice as numerous as in either El Salvador or Guatemala! In reality, Honduras would rank fourth with Guatemala far ahead for all five" (Brockett 1998:170). Remote rural deaths are less likely to attract news coverage than urban assassinations. Brockett also gives an example of variation in news coverage tied to particular interests. In 1975 a "bananagate" scandal involving a US multinational's allegedly corrupt ties to the president of Honduras led to unprecedented coverage of Honduran affairs in the *New York Times*, including a mention of six people killed at a peasant training centre in June of that year. Similar incidents prior to the scandal had gone unremarked.

A further problem is the counting of indirect and non-battlefield deaths. Kaldor (1999), among others, claims that the ratio of civilian to military deaths in war had swung from 1:8 in the early twentieth century to roughly 8:1 by the end of the century. The ratio of indirect to direct conflict deaths has been quoted in recent years as 9:1 (Levy and Sidel 1997). However, these claims have slightly shaky foundations that are not always acknowledged.[18] Murray et al. (2002:347) cite one cross-sectional study indicating that "the total disability adjusted life years lost in 1999 due to the indirect effects of military conflicts occurring between 1991 and 1997 was about the same as the number lost due to the direct effects of all wars in 1999".

A different kind of empirical problem arises in establishing classification systems as the basis of datasets for statistical studies. At one level, this is a matter of selecting the coding rules to adopt: which rules determine whether a particular conflict, for example, is included in a dataset on "civil wars". At another level it is a matter of whether the civil war category itself, or any other, is a valid one. Sambanis (2002) discusses in detail the problems of different rules applied to the inclusion or exclusion of events from civil war datasets—a variation that holds in some cases even for researchers claiming to use the same coding system. The main obstacles include the setting of a threshold for inclusion of a conflict as a "proper" civil war and deciding whether or not to include non-battlefield, civilian deaths. Variations in coding civil wars lead to substantial differences in the results of regressions of the most typical variables used in civil war models on the incidence of such conflicts. However, one can question why a specific dataset of civil wars makes sense. From the perspective of research on inequality and its

the next person one meets is from a different ethnic group in a given country—the "ethno-linguistic fragmentation index". However, measuring diversity is not entirely straightforward. One clear example is Schetter's (2002) observation that for Afghanistan one estimate puts the number of ethnic groups in the country at 200, while another study estimates 50 different groups.

18 A similar claim that has recently circulated with little critical analysis is that 90 per cent of all conflict-related deaths are caused by small arms and light weapons.

consequences, it might make more sense to look at a wider range of phenomena of violence, perhaps including civil wars but not exclusively so.[19]

Returning to figures 1 and 2, above, it is not clear that we fully appreciate the EI-PC relationship, or even the kind of research question to be asked, by restricting the analysis to civil wars. Looking at the outliers, for example, both Brazil and Guinea-Bissau, in very different ways, have been characterized by political violence in recent years. Guinea-Bissau in the 1960s and 1970s was the site of an independence war (these wars are often excluded from datasets on civil war or "internal war" and categorized as "extra-systemic conflicts"), and has been disrupted by violent conflict in the past few years, partly affected by spillover from the conflict in the Casamance region of southern Senegal. Brazil is characterized by an extraordinary array of forms of social and political violence—from the "everyday violence" (Scheper-Hughes 1992) suffered by the poor (both actual physical violence and "structural violence"), through the violent conflicts surrounding the institutionalization of private property rights on the northern Amazonian frontier (Alston et al. 1997) and the conflict between large landowners (in some cases with police connivance) and the landless movement, the Movimento Sem Terras (MST), to the urban violence of "funk balls",[20] drug-related violence, violence against street children, and the institutionalized incentives encouraging police violence (Human Rights Watch 1997). The distribution of income and wealth in Brazil is widely accepted to be one of the most unequal in the world. For example, Deininger and Squire (1996) give a Gini observation (with an "OK" quality assurance) of 63.42 for 1989. Sutcliffe (2001) shows, in one illustration of Brazilian inequality, that average wages of Brazilian white males are some three times greater than those of their black male compatriots.

Studies of the relationship between economic inequality and violent political conflict have looked at a range of categories of violence and conflict, from homicide rates to civil wars. Sometimes claims are made that shorten the results of different studies without fully acknowledging these differences. For example, Nafziger and Auvinen (2002) draw on the claims of Alesina and Perotti (1996) for their argument that inequality does matter in the origins of CHEs. Yet Alesina and Perotti claimed to find a pattern linking inequality with political instability measured through the incidence of political assassinations, which is a very different category from the CHEs. There is possibly a need for more direct research on the whole range of manifestations of social violence and violent political (collective) conflict. Alternatively, there is a need to focus on a greater range of more specifically defined categories. For the analytical challenge is to establish constructive categorization systems that help to reveal more than they obscure, given that virtually all classification systems impose artificial discontinuities on reality. The problems are especially intense where variation within a given category is as great as that between categories. This is almost certainly the case, for example, with the distinction between "civil wars" and other political conflicts.

The upshot is that there are no grounds for accepting any "event regularity" holding across countries when it comes to inequality and its consequences for violent conflict. There is a baffling array of empirical claims. The data on which statistical studies of the problem are based are massively unreliable. And the literature has paid too little attention to the pitfalls of commonly used classification systems and their implications for the enquiry into the consequences of inequality. Faced with this predicament, some take the view that one can quietly discard the problem and push an alternative claim about the causes of violent political conflict. However, the real implication of such uncertainty is the nagging possibility that inequality might still be significant, at least sometimes and to the origins or "epidemiology" of some conflicts. The next section looks at another way of exploring the problem.

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[19] Another problem is the very definition of "civil war", when (i) there are several "regional conflict complexes" internationally, involving a variety of cross-border spillovers (Wallensteen and Sollenberg 1998) and (ii) every so-called civil war is significantly internationalized in various ways (including through market ties and/or political linkages).

[20] Funk balls usually take place in the favelas (slums). They are similar to dance clubs, except that the music is used to incite fights, usually between rival gangs.

4. Processes, Mechanisms and Relations

All the statistical studies reviewed above are focused on outcomes. They relate the observable, specifically quantifiable (arguably, at any rate), facts and signs of inequality. They are propelled by a widespread belief in the possibility of finding event regularities of a probabilistic kind, captured by econometrics, polimetrics and in historical studies by cliometric analysis. At least for the study of inequality-conflict linkages, they have not been very successful. This should encourage an exploration into what lies behind this quantifiable surface, and into the causes of the inequality that various techniques try to measure. Since it is not likely to be just the fact of inequality or even a given degree of measurable intensity of inequality that provokes a certain class of political or social outcomes such as civil war, it is worth looking deeper into the processes, mechanisms and relations that generate and sustain inequality. Hirschman's (1981) notion of the varying "tolerance for inequality" is one route to this enquiry. More recently, Stewart's (2000) analysis of "horizontal inequality" is another. And Tilly's (1999) concept of "categorical inequality" sustained by particular mechanisms is, arguably, the most fruitful recent contribution. These approaches are briefly reviewed here, and their implications for future research highlighted.

Hirschman's reflections of the proposed workings of a "tunnel effect" were prompted specifically by two episodes of violent political conflict in the late 1960s and early 1970s: the Biafra war in Nigeria and the civil war in Pakistan leading to the independence of Bangladesh.[21] Observing that both these "development disasters" followed periods of developmental surges rather than the protracted stagnation so commonly associated nowadays with civil wars, Hirschman argued for the existence of a social mechanism that could suppress relative deprivation, the frustration-aggression nexus, or, more plainly, envy. As development proceeded, some people's fortunes improved while others were left behind, and thus inequality typically increased. But the expectations of those left behind might be raised rather than overwhelmed by bitterness. Greater inequality effectively gave information about social and economic change that could be interpreted as a signal of hope even for those not immediately benefitting from development. However, this tunnel effect must be qualified. First, it would not last indefinitely and would at some stage stop functioning. Second, the suspension of envy would be unlikely to operate where the opportunities provided by economic development were, or were seen to be, monopolized by certain groups, while excluding others.[22] Third, beneficiaries of change are not always politically quiet or supportive of inequality: a plausible overlapping of the end of the tunnel effect with the continued politicization of educated middle-class urbanites around redistributive objectives would provide the classic conditions of revolutionary coalition building.[23]

There are elements in Hirschman's analysis of the changing tolerance for inequality of an interest in what has come to be known recently as "horizontal inequality". Thus, Stewart (2000) argues that most studies of the relationship between economic inequality and political conflict have understood it only in terms of "vertical inequality", that is, the distribution of income across the whole population of individuals from richest to poorest and as captured by the Gini coefficient. However, she argues, horizontal inequality is far more significant, reflecting as it does differential standards of living and access to public sector employment, political rights, education opportunities, and so on among collective groups within a society. These groups might fall into various classification kinds, for example, religious, regional, class or ethnic. On this basis, Stewart constructs a matrix of possibilities, while acknowledging the difficulty in

[21] The image of the tunnel is taken from Hirschman's initial explanatory analogy with traffic in a two-lane tunnel traffic jam. The traffic jam is legally confined to one lane but initially stirred into hope by movement in the second lane; eventually some drivers will illegally cross into that lane, if it seems that the logjam appears to be clearing there.

[22] "If in segmented societies, economic advance becomes identified with one particular ethnic or language group or with the members of one particular religion or region, then those who are left out and behind are unlikely to experience the tunnel effect: they will be convinced almost from the start of the process that the advancing group is achieving an unfair exploitative advantage over them" (Hirschman 1981:49).

[23] This formulation matches the emphasis of Eric Wolf (1969) on the need—for peasant revolutions—for a combination of a peasantry politically mobilized and the mobilizing power of small outside groups.

pinning down precisely (by quantitative scores, for example) the comparative role of horizontal inequality across a sample of countries. On one hand, this scheme of horizontal inequality is simply another form of outcome description, a 90-degree rotation of vertical inequality, as it were. On the other hand, however, the conception in terms of exclusion from certain rights and opportunities, as well as a focus on average income levels, shifts attention to discriminatory social relationships. Stewart's matrix of horizontal inequality raises many questions and qualifications, some of which she herself addresses. For example, why is it that a situation of high intragroup inequality in Kenya (among the Kikuyu) seemed to work against collection action to redress exclusion from the chief political opportunities, while high intragroup inequality in Rwanda (among the Hutus) did not stop mass murder through a deflective, scapegoating mechanism?

The politics of inequality and the significance of varying degrees of institutionalization of group inequality are issues tackled in Tilly's (1999) framework of "categorical inequality". Like Hirschman, Tilly effectively discusses the tolerance for inequality, addressing the question of the extraordinary durability of social inequality and highlighting the mechanisms that account for this durability.[24] However, perhaps in a similar vein to the horizontal inequality analysis and dovetailing to some extent with Hirschman's note on heterogeneous societies, Tilly's framework highlights inequality between social pairings—citizens versus non-citizens, men and women, black and white, Anglican and Catholic in pre-emancipation England, the differential property rights of large landowners and peasants, and so on. Categorical inequality is distinguished sharply from the continuous distributions of income captured by, say, the Gini coefficient and in some cases tying inequality in income to returns to the purported distributions of individual attributes.[25]

In this organizational (rather than principally individual or ideological) analysis, inequality between categorical pairings is sustained by social mechanisms: exploitation is at its core; where there is exploitation there is, typically, opportunity hoarding by one group (of access to jobs, control of certain markets, political position and so on); emulation spreads categorical inequality through various branches of a society and internationally; and adaptation ensures that even the disadvantaged develop footholds of survival and acquiescence in the system.[26] In this approach, exclusion is not simply a negative, an absence, but a directly relational factor that is at the same time a form of inclusion in a given social system.[27] Where the state takes a direct interest in creating and supporting categorical inequality, as is common, it adds the coercive element that further fixes the paired inequality. According to this perspective, redressing this kind of inequality is only possible through sustained political action and in circumstances where the benefits accruing to the beneficiaries from exploitation and/or opportunity hoarding fade and the costs of maintaining the system rise. Examples would include the rising cost of discrimination against English Roman Catholics during international wars, when legal restrictions prevented the army from recruiting Irish Catholic soldiers; and the rising cost to South African capitalists of that most extreme form of institutionalized categorical inequality, apartheid. For the most part, the "histories of landlord-tenant relations, religious inequalities, and social movements indicate…that organizers generally have a difficult time stimulating shared awareness of oppression and determination to resist, that even with intense organizing efforts they fail except in special structural circumstances" (Tilly 1999:225).

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[24] "Because exploitation and opportunity hoarding often involve an effective means of control over members of excluded and subordinated categories, because emulation naturalises distinctions by making them ubiquitous, and because adaptation ties even exploited groups to the structure of exploitation, most categorical inequality stays in place without sustained, overt struggle" (Tilly 1999:225).

[25] Tilly's book includes a critique of Herrnstein and Murray's (1994) argument, in *The Bell Curve*, that group differences are to a large degree hereditary and therefore intractable—that success and failure in the US economy are determined by genetic distribution, the distribution of hereditary intelligence.

[26] Wright (1999) characterizes Tilly's theory of durable inequality as, first, essentially a functionalist theory (organized around the primacy of a problem-in-need-of-a-solution mechanism) and, second, far closer to Marxist theory than Tilly's own claim of providing a bridge between Marx and Weber.

[27] This echoes Joan Robinson's (1962) comment that it is better to be exploited than not to be exploited at all.

It is true that there is little to distinguish, in Tilly's analysis, between various types of inequality. Some might argue, as does Wood (1995:258), that the difference that constitutes class identity "is, by definition, a relationship of inequality and power, in a way that sexual or cultural 'difference' need not be". This might lead one to look for varied types of political conflict over inequality—where, for example, the intensity of conflict might be greater where the fundamental form of exploitation and the character of direct relationships of power and inequality defining society are at stake. In such conditions—essentially those of that protracted, disruptive, uneven moment of transition commonly known as development—it is also worth noting the relations among relations, or the relations between identity sources. Thus, the fundamental institutional arrangements of power and exploitation may be in flux and at stake, and may be seen in class terms (such as landlords, peasants, wage labourers and capitalists), but political mobilization in reaction to the social crisis often takes other identity terms. Hobsbawm (1992), for example, stresses how ethnic nationalism is typically a defensive reaction to modern, transitional crises. This fundamental developmental perspective is not always stressed even in the process-related or relational accounts of inequality and conflict, or indeed the outcome-based statistical explorations of the problem. And yet the fundamental upheaval of capitalist development must be central to most modern violent political conflict. Capitalist development, or industrialization, is the modern form of a more general phenomenon of transition—or societal metamorphosis. In transition, as Gellner argued (1964), "tomorrow is not just another day; it is an *other* day, altogether". From this perspective, illiberal politics and violent conflict are unsurprising features of what might be called—reinventing Gramsci's use of the phrase—a "war of position". Retreating from class analysis, even formal economic theorists have engaged with this predicament. Pagano (1999), for example, draws on Fred Hirsch's (1977) concept of "positional goods" to argue that the fact that consumption by some people of certain "goods" necessarily implies their "negative consumption" by others and that, consequently, the existence of such goods makes conflict likely.

Conclusion

Claims about the link between economic inequality and violent political conflict are linked by filaments of assumption to different underlying approaches to the understanding of the origins of conflict and violence. These filaments are not always explicit in specific arguments about the inequality-conflict relationship. Lack of clarity here can lead to overlaps among assumptions of different approaches, or at least to a problem, usually not addressed, of how elements of different fundamental approaches may be effectively combined. This analytical predicament has led to a plethora of claims about the inequality-violent conflict relationship. Many of these claims support the idea of more or less universal "event regularities" or predictable patterns. The range of patterns includes U-shaped curvilinear relationships, inverted-U relationships and linear relationships. There is no consensus. This can be seen, for example, in the conflicting claims of recent publications by economists using, for the study of conflict, the same source of data on inequality, for example, in Collier (1998) and Nafziger and Auvinen (2002).

In terms of research generating an accumulating body of knowledge, much of the literature, when viewed in these terms of conflicting claims based on large samples of countries, has been fruitless. Two of the main reasons for this are a lack of clarity in categorization systems and definitions, and a poverty of data (on inequality, on political violence, civil war, as well as on other relevant variables). Data problems are especially intense in this field of research, given the shortcomings and lack of comparability in much of the data from developing countries and given the consequences of violent political conflict for the collection of reliable data.

It may also be that the pursuit of event regularities and probabilistic predictive claims is unlikely to be successful for this kind of social inquiry, and that the methodology is inappropriate. One alternative is to look at discussions of what lies behind the inequality that is measured in Gini coefficients and other quantitative indicators. Attempts to theorize the sources of inequality and their political significance include Hirschman's "tolerance for inequality" and

Tilly's theory of "categorical inequality". This review drew out the main features of these approaches and other related analyses, and argued for the centrality of late development to the understanding of inequality and its consequences.

Thus, while universal claims about the inequality-conflict link are not wholly convincing, there has nonetheless been some fruitful theoretical thinking on inequality that might generate new empirical research into the role of inequality in the origins and spread of violent political conflict. Future research starting from this analytical grounding might well eschew econometrics and polimetrics, the statistically predictive and "postdictive". Instead, it might be best encouraged to develop case studies with historical depth and to develop clusters of contrastive exploration looking at specific problems in varying contexts (but involving small samples of comparison, for example, from two to five cases).

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