



Homo Economicus Goes to War: Methodological Individualism, Rational Choice and the Political Economy of War

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Summary. — Neoclassical economic theories of violent conflict have proliferated in recent years and, with their application to contemporary wars, have influenced donors and policy makers. This paper reviews the intellectual foundations and empirical substance of such theories and offers a critique drawing on a political economy perspective. There are strong grounds for arguing that orthodox economic theories of war are reductionist, speculative, and misleading. Theories that are driven by methodological individualism are compelled somehow to model “the social” as it affects contemporary war for example, by appeal to indices of ethno-linguistic fragmentation but do so in ways that fail to capture reality and its variations.

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1. INTRODUCTION

Neoclassical economic theories of conflict have proliferated in recent years. They have an increasing influence in donor, academic and nongovernmental organization circles, though they are often “consumed” only through easily digestible, nontechnical presentations with an obvious, if arguably only partial, relation to the realities of contemporary conflicts around the world. Such theories also commonly arouse disquiet, or what their proponents might feel is a knee-jerk reaction; but widespread discomfort with neoclassical economic theories of armed conflict in developing countries is also typically weakened by an incomplete appreciation of the theories and models in question. This paper contributes to the literature on conflict by offering a short survey of the common themes and intellectual foundations of these theories and by laying out a critique from a political economy perspective. This is especially relevant since many noneconomists welcome economists’ models of conflict because they offer the economic, or material, content that is often missing in other analyses. Yet, this paper argues, rational choice theories of conflict based on neoclassical economics are unconvincing theoretically and where they have empirical content this is largely arbitrary. Further, alter-

native approaches in political economy can provide this economic content more effectively. At its close, the paper suggests some of the basic elements of a richer analysis of contemporary conflicts, predominantly in less developed and/or transitional economies.

Development economics has arrived rather late to the field of conflict. Economic theory in general has not been much devoted to explaining war. Exceptions include Keynes’s two extraordinary works conceived in response to the problems of war—*Economic Consequences of the Peace* (1920) and *How to Pay for the War* (1978)—rather than focusing on war itself; Lange’s work (1970), which provided some grounds for the parallel analysis of war economies and centrally planned socialist economies as ‘mobilized economies’ (Sapir, 1990); and, in the field of development, Prest’s (1948) work on the economics of war in primary commodity producing countries. In the somewhat broader field of historical political economy, there are

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enduring classics such as Wolf (1969), Moore (1967), and Scott (1976).

But both development economics and development studies in general were largely silent on the subject during the later 1970s and the 1980s, for whatever reason, though the overwhelming ideological hold of the Cold War doubtless had something to do with this. During the 1990s all this changed, and rapidly. Reflecting a general encroachment into the social sciences by orthodox economics, economic models of conflict based on methodological individualism and rational choice began to proliferate, led by Hirshleifer (1987, 1994) and Grossman (1991).¹ Of course, an old critique of neoclassical economics was precisely that it evaded matters of power and conflict. Now, however, neoclassical economics has been wheeled out to confront these issues head on. The result has been what some call an “expansion of the domain of economics” (Alchian, quoted in Bowles, Franzini, & Pagano, 1999, p. 7) and what others call a process of “economics imperialism” (Fine, 1999, 2001). Hirshleifer’s tongue is only partly in cheek when he writes, of the intellectual continent of violence and conflict on which economists have alighted relatively recently, that:

As we come to explore this continent, economists will encounter a number of native tribes—historians, sociologists, psychologists, philosophers, etc.—who, in their various intellectually primitive ways, have preceded us in reconnoitering the dark side of human activity. Once we economists get involved, quite properly we will of course be brushing aside these a-theoretical aborigines. (The footnote to this quote explains: “When these researchers do good work, they are doing economics!” (1994, p. 3)).²

In claiming “the leading role among the *dramatis personae* of social science” (Bowles *et al.*, 1999), *Homo economicus* has clearly revealed a more passionate aspect by engaging in conflicts, seeking power or “consuming positional goods” (Pagano, 1999). How well this creature plays the part in these roles remains questionable. Rational choice models of conflict follow the precepts and developments of neoclassical economic theory well. I argue, however, that they also follow neoclassical economics by repeating its well-known shortcomings. War is often an especially sharp reflection of tendencies and characteristics common in societies not at war. Likewise, perhaps economic theories of conflict and civil war give especially sharp insights into the

characteristics of orthodox economic theory and its pretensions to colonising the social sciences in general. Thus, I argue that rational choice theories of conflict typically lay waste to specificity and contingency, that they sack the social and that even in their individualism they violate the complexity of individual motivation, razing the individual (and key groups) down to monolithic maximizing agents. Arguably, the extension of orthodox economics to explaining armed conflict does not even have a logical justification in terms of the fundamental principles of neo-classical economics (Zafirovski, 2000).

The following section sets out the main common features of mainstream economic theories of conflict, clarifying the ways in which a variety of models build upon shared assumptions of choice theoretic logic. Thus, models of conflict provide for a combination, as it were, of a felicific and a bellicose calculus, or—as Hirshleifer (1994) puts it—for a combination of the way of Coase with the way of Macchiavelli.³ These economic models of conflict have a strong appeal, even to many who are made uneasy by their attempt to capture, or reduce, conflictual relations in the language of algebraic identities and differentiation. The next section of this paper briefly explores the sources of this appeal and also identifies some immediate causes for analytical concern. Following sections discuss in more detail two principal weaknesses. The first is revealed in empirical applications of the neoclassical logic, which have thus far dealt in highly unsatisfactory (empirically and conceptually) proxies for the proposed behavioral drives of *Homo economicus*. The second major flaw in these theories is the analytical awkwardness with which they manage the inevitable need to model “the social.” Finally, in the conclusion, in response to the colonizing campaign of neoclassical economics the paper offers a manifesto for an analytical liberation struggle.

2. OPPORTUNITY KNOCKS: ANALYTICAL FOUNDATIONS OF NEOCLASSICAL MODELS OF CONFLICT

Rational choice, methodologically individualist models of conflict typically build on the idea of two groups or “players.” In some the game pits a ruler or incumbent against a rival. Others (Grossman, 1991; Azam, 2001) have the state in conflict with subject-rebels. In each case

there is little or no disaggregation or stratification of the players.⁴ In some models outcomes (in terms of social waste and political instability) may depend on whether the incumbent or state has an edge (Mehlum & Moene, 2000); in others, e.g., Collier (various), the presumption is that wars start with the decision of rebels and the analysis abstracts from much of state policy and from state violence. There is no discussion of where the “players” come from other than as the product of relevant economic calculations, to be discussed below. While in some models the players are simply rivals for the rent associated with power, in most they are granted some social or collective identity, typically in the form of ethnic or religious labels. This is discussed further, below; but it might be noted that in Hirshleifer (1994), for example, collective identity is regarded as a residue of Darwinian survival processes generating durable animosities, rather than having modern, political and ideological roots and influences. Both players or organizations are assumed to be maximizing agents and the paucity of discussion of either group leads to a common elision between these agents and “individuals.” Typically, they are driven by the urge (this seems to be the only occasion where compulsion is relevant, all else being decided in the realm of choice) to maximize power or windfall gains from victory (or just from conflict itself).

This is particularly stark in Collier and Hoeffler’s models, stressing as they do greater predictive power in the causation of civil war for “loot” than for “justice,” for “greed” than “grievance,” but actually the same core reasoning holds in all such models, including those from which their work is derived. For the common key to these models is the idea of a tradeoff. For Hirshleifer people face a choice between producing and appropriating. If the opportunity cost of appropriative activity, i.e. violence and conflict, is not prohibitive then violence will ensue; put differently, actors choose conflict where this is more profitable at the margin than exchange. Grossman (1991), Azam (2001), and others similarly focus on a risk versus payoff tradeoff: conflict is dangerous but if the payoff outweighs calculated risk, war is chosen or more time is allocated to insurgency. Conflict, civil war, or insurrection is then an investment or resource allocation designed to raise the probability of toppling the government or of drawing monopoly profits from the loot or instant taxation of primary

commodities. Rebellion is thus clearly akin to rent seeking (Neary, 1997): it is a distortionary devotion of resources to the pursuit of the fruits of imperfect competition.⁵ The occurrence and implications of conflict may be influenced by the symmetrical or asymmetrical features of the strategic “game,” e.g., by whether the incumbent powerholder has an “edge” (Mehlum & Moene, 2000).

If this tradeoff is the basic premise of models of conflict, then it is necessary to determine the factors that might trip the switch activating the choice of war. In other words, under what circumstances do groups develop “mutually inconsistent opportunity sets” (Hirshleifer, 1994) that make fighting more profitable on the margin than peaceable exchange? One of the most basic circumstances is poverty or slow growth. Poverty may “cause” conflict in various ways, but the clearest expression of its treatment in neoclassical models of conflict is Hirshleifer’s argument that the poor have a comparative advantage in violence. While it seems obscene in the light of the widespread and persistent evidence of coercive recruitment of children and young adults into military groups in, e.g., Sierra Leone or Angola, the argument is nonetheless made that the opportunity cost of insurgency for, especially, young males in poor countries where there are few alternative opportunities for gainful employment is clearly low. Essentially, this is simply a formal version of the widespread common sense assumption that in poor countries “life is cheap.”

There are other factors, however, affecting the probability of the relative profitability of violent conflict. For example, Azam (2001) claims that the likelihood of war turns on whether there are credible signals of inter- and intraethnic redistributive resource allocations, given an initial endowment of “ethnic capital.” Redistributive mechanisms may take various forms. For example, high public sector wages for officials drawn from across ethnic groups, enabling an intra-ethnic trickle down, might be more effective in keeping the peace than relatively anonymous provision of public goods (Azam, 2001). Collier (Collier & Hoeffler, 1996, 1998) begins by stressing the gains from victory and the government’s potential expenditure on defence as the factors determining the utility of rebellion. The calculation by prospective rebels is mediated by how easy it will be to mobilize support, i.e. by the characteristics of the collective action challenge in a given society. As

shown in greater detail below, this is understood principally in terms of “ethno-linguistic fragmentation.” Later versions of Collier’s models (Collier, 2000; Collier & Hoeffler, 1999, 2001), placing less emphasis on the probability of victory, highlight the availability of lootable primary commodities and the availability or otherwise of peaceable economic activities to young males. So-called greed variables such as these are found by Collier and Collier & Hoeffler to be far more robust predictors of civil war than “grievance” factors such as repression or exclusion, somewhat contrarily to Azam’s (2001) otherwise analytically related model.

The orthodox economist’s toolkit has other compartments too, yielding nuanced variations around this basic model. Hirshleifer (1987), for example, wields diminishing returns, comparative advantage, and Cobb–Douglas production functions as relevant to the incidence and outcomes of conflict. Thus, production functions may relate fighting inputs on two sides to outputs in the form of victories and defeats. Azam (2001, p. 432) introduces the assumption that there are “locally increasing returns to scale in rebellion, such that there is a critical mass of resources to be invested in rebellion activity before there is any chance of overthrowing the government.” Few would argue with this probably: though this is insufficient to legitimate the overall model. Meanwhile, another compartment of the toolkit reveals a kind of adhesive holding the model together, i.e. “uncertainty.” Imprecision and inconsistency in the application of this variable in the models suggests that it is something of a residual used to patch up the holes in a model and stop it from collapsing. Hirshleifer (1994, p. 5) suggests that “uncertainty on the conflict side swamps that on the side of co-operation;” but acknowledges that it is not clear how exactly uncertainty will affect the likelihood of conflict as an outcome of the interaction of opportunities and preferences (on preferences, see below).

Finally, again in the more applied models there is more attention to collective action problems and varieties of market failure. For example, Collier’s (2000) explanation for his empirical findings that greed variables predict civil war better than do grievance variables turns on these issues. Rebellion against injustice has something of the qualities of a public good and, therefore, will also display the weaknesses of a public good, primarily susceptibility to free riders: hence, injustice might exist but will not

produce sufficient conflict. Similarly, there is a time-consistency problem in that actors of violence are presumed not to trust the promises of leaders, reasonably expecting them to renege on their mobilizing pledges to put right a range of social wrongs or sources of grievance. On the other hand, appealing to people’s demand for instant gratification through loot nicely overcomes these difficulties.

3. THE LURE OF ORTHODOXY

Such are the axioms, assumptions and logic that have informed much recent thinking on the economics of war, particularly of so-called civil wars in developing countries. They have also filtered into policy debates, especially through their influence on the research initiative of the World Bank on the economics of civil war, crime and violence.⁶ (The debate has in some arenas been reduced to the overly simplistic “greed versus grievance” dichotomy.) This kind of explanation of conflict has a wider appeal too, perhaps for three main reasons. Firstly, many of those concerned by a high prevalence of civil conflict—a crude term used here simply for convenience—tend to recoil in righteous horror from the implications of the widely cited new barbarism thesis associated with Robert Kaplan.⁷ Kaplan (1994) suggested that West African and Yugoslav wars manifested a problem of “loose molecules” stirred into a frenzy of violence by, essentially, Malthusian pressures and the nightmare of urbanization. The rational choice theory of conflict offers an analytical godsend to those made viscerally anxious by the “senseless anarchy” story.⁸

Second, neoclassical economic explanations of conflict offer a corrective to the assumption that inequality produces resistance and conflict. Since Plato at least it has often seemed obvious that injustice and gross inequality would produce conflict. Much work on Central American conflicts prioritises the causal role of inequality (Boyce, 1996; Booth, 1991), while others find a more general significance (albeit probabilistic and along with other factors) of income inequality in the origins of “complex humanitarian emergencies” (Nafziger, 1996). But, as Trotsky and others have suggested, if the objective conditions of revolution were sufficient to cause revolutions, then much of the world would be in a more or less permanent condition of revolution. Moreover, it has become clearer that injustice and inequality do not inevitably

or in any direct, functional sense produce conflict.⁹ Grenier (1996), for example, shows well how structural injustice explanations tally weakly with the incidence, timing and outcomes of Central American conflicts.¹⁰ In another example, from the Jubba Valley in southern Somalia, the question is

how to understand Goshu peoples choices to participate within and actively seek incorporation into a society which subjugated them, and how to understand their creative ability to manage the juxtaposition of domination and accommodation (Besteman, 1999, p. 9).

Third, the appeal of the new economics of conflict lies in the fact that it is a materialist explanation of conflict. Whatever else contemporary armed conflicts are about, powerful material interests clearly are significant in shaping the conflicts and in their causation. This dimension was often neglected in overdependence on global ideological explanations of Cold War conflicts in Africa, Asia and the Middle East.¹¹ It has also been sidelined in the more naïve explanations of more recent conflicts in terms of primordial ethnic antipathies, in which this rigid hatred was seen as more important than any other factor. Yet there is no denying the significance of material interests in the origins of conflicts, even if at times (e.g., in Colombia) it is difficult to separate their role in the origins of conflict from their influence on the characteristics and durability of conflict. Timber and diamonds in Liberia and Sierra Leone, opium in Afghanistan, coca in Colombia, land and *qat* in Somalia, oil and diamonds in Angola, tropical hardwoods in Cambodia, and so on all feature large in any understanding of conflict. Arguably, pressure of population on land combined with collapse of international coffee prices was critical to the background to the Rwandan genocide (André & Platteau, 1998; Austin, 1996). Vicious processes of asset transfer are common to the conflicts in Sudan, Somalia and many other places (Besteman, 1999; Duffield, 1994; Ndikumana, 1998, p. 32). Meanwhile, conflict in the Kivus (north and south) in the Democratic Republic of Congo (DRC) has been driven to a significant extent by a contest for control over the profitable local section of the “marketing chain” for tantalite, the heat-resistant metal used in space craft, mobile phones and computer game consoles (Jackson, 2001; Moore, Nabudere, & Kibasambi, 2001). Time preference and credibility problems do seem to be overcome where, as is

extremely common, there is evidence of instant gratification by soldiers in the form of looting. The capacity of orthodox economic theory to make of these material issues something more than the stuff just of rich description or of consumer campaign morality has given them considerable advantage.

Despite the apparent appeal and formal elegance of orthodox economic explanations of violent conflict, there are strong grounds for arguing that these explanations are extremely reductionist, highly speculative, and profoundly misleading. For example, the presence of primary commodities in an economy or their ease of lootability is an absurdly simplistic, overly direct rationalization of the role of material resources in conflict. This approach may be contrasted, e.g., with analyses of the origins of three distinct and competitive liberation movements in Angola fighting for independence from Portugal in the 1960s and 1970s and laying the basis of the MPLA/UNITA war since independence.¹² For these analyses stress different material interests underlying political mobilization and ideology in each of the parties contending to dominate the anti-colonial war and post-independence politics: the FNLA, UNITA and the MPLA (Clarence-Smith, 1980; Birmingham, 1992). Relevant interests included the frustration of dispossessed northern Angolan coffee farmers (in the area providing most support to the FNLA) whose farms were expropriated by Portuguese settlers and whose prospects of wage employment were cut short by settler use of migrant labor from the Ovimbundu highlands. They included these migrant laborers and the farmers of the central highlands plateau (the heartland of UNITA support). And they included the interests of those employed in manufacturing and clerical positions in and around Luanda (the MPLA’s domain).

To reduce the role of the material to the lootability of primary resources (and therefore the profitability at the margin of conflict and violence over co-operation and exchange) conjures up Gramsci’s definition of economism, in which

by an economic fact it means the self-interest of an individual or small group, in an immediate... sense... [it] is content to assume motives of mean and usurious self-interest, especially when it takes forms which the law defines as criminal (Gramsci, 1971, p. 163).

Gramsci defined economism as presenting causes as immediately operative that in fact

only operate indirectly, overestimating mechanical causation and indulging in “doctrinaire pedantry” (p. 178). The economism and reductionism of these models is certainly worse than anything to be found in Marxism, other than in its most vulgar forms.

Even if axiomatic abstractions are merged with variables imported by selection from stylised facts, neoclassical economic theories of conflict remain, as theories, abstract. They are completely speculative until some empirical content is added. Given that the social, the cultural, the historical, etc. are left out of the initial framework, when they are brought back in later (which is inevitable) their incorporation is arbitrarily selective. What is the basis for assuming that people everywhere experience a choice of conflict or co-operation defined solely in terms of profitability, where historical evidence suggests that conflict often is institutionalized (i.e. the rules of the game *are* conflictual) and that the conditions of co-operation and exchange are typically forged, often slowly, out of conflict (Tilly, 1992; Howard, 2001)? What is the basis for deciding that people cannot be mobilized by ideology or promises of change (including change in material conditions of employment or production) because of time-preference or leadership credibility problems, when history and contemporary democratic politics are virtually defined by political enthusiasm for all manner of pledges despite their frequently being unfulfilled?¹³ What is the basis for deciding that collective action is necessarily framed in terms of historically fixed “ethnic capital” or scores of ethno-linguistic fragmentation, where evidence suggests that ethnicity is commonly historically dynamic and far from even in its organizing hold on people, and where there is no evidence that ethnic affiliation is always prioritized over other sources of collective identity, including class? Moreover, why must it be accepted that social phenomena are best apprehended through individual rational choice (and a very narrow notion of rationality) rather than relational rationality and social and historical constraints on choice?¹⁴ On what grounds should we ignore the gray areas between choice and compulsion in human activities including conflict? For fear and obeisance to *de facto* authority—as well as acquiescence with strong mobilizing ideologies not through acceptance but through desperate efforts to resist local structures of oppression—are motivations that might be more significant than either greed or

grievance, individual choice or unambiguous coercion, in some circumstances.¹⁵

Building on these doubts, the rest of this paper introduces three main sources of weakness in orthodox economic theories of conflict and their relevance to contemporary “civil wars.” The first weakness lies in the choice of proxies used in attempts to add empirical content to the theoretical postulates: these proxies are empirically and conceptually fragile. Second, theories of conflict rooted in micro-foundations of methodological individualism and rational choice cannot avoid somehow bringing in “the social”: yet this endeavor is, again, empirically and analytically a failure. Third, even if these orthodox economic theories and econometric applications might capture a portion of the reality of conflicts, albeit crudely, they completely fail to capture one of the most significant features of all conflicts, i.e. structural and relational *change*.

4. PROXY WARS

In *The Periodic Table*, Primo Levi (1986) finds in one of his tales the following moral:

that one must distrust the almost-the-same... the practically identical, the approximate, the or-even, all surrogates, and all patchwork. The differences can be small, but they can lead to radically different consequences, like a railroad's switch points; the chemist's trade consists in good part in being aware of these differences, knowing them close up, and foreseeing their effects. And not only the chemist's trade.

If there is anything substantial in the rational choice economist's models of violent conflict, it will emerge through empirical testing. But, where this testing has been done it has been, inevitably, through the use of proxies. What matters, then, is whether the difference between a proxy and the reality to which it is meant to be tagged makes a difference: whether their signals might divert the train of analytical and policy-making attention onto a siding. Do the proxies actually measure what they purport to measure? There are two dimensions to answering this question: are the data adequate and are the proxies conceptually or substantially equivalent to the thing they are meant to represent?¹⁶

The most widely known use of proxy measurements for rational choice models of violent conflict or civil wars currently is in the work of Collier and Hoeffler. An early version (Collier

& Hoeffler, 1996) posited that the utility of choosing rebellion (U_w) was a function of the probability of victory (p) and the gains to rebels upon victory (T), the potential for government defence spending (D), the expected duration of the conflict (M) and the co-ordination costs of mobilizing for rebellion (C), such that:¹⁷

$$U_w = \{p(D).T; M; C\}$$

The proxies chosen for the incentive for rebellion aimed to capture potential government military expenditure and revenue-raising capacity through three variables: income per capita, the natural resource base, and the degree of inequality in a society. Hence, poorer countries are more prone to conflict because, with income per capita at a low level, there is insufficient tax-raising capacity to deter rebellion. The use of inequality, measured in Gini coefficients, as a proxy for potential government defence spending is especially interesting. The reasoning adopted in Collier and Hoeffler (1996) is that high inequality reflects the presence of an elite liable to encourage the government to raise taxation of elite wealth if this is temporarily necessary to repel rebellion and thereby preserve this unequal status quo.

The gains to rebellion conditional upon victory are assessed via the proxy of the natural resource endowment, which has uncertain consequences given that resource endowment also affects the government's potential defence spending. The proxy for the costs of rebel co-ordination is the "ethno-linguistic fragmentation index" (with complete homogeneity scoring zero and maximum fragmentation scoring 100). The proposal here was that

the cost of co-ordination is... a quadratic function of the degree of cultural fractionalization, initially decreasing in it as potential rebels are distinguished from governments supporters, and then increasing in it as potential rebels are distinguished from each other (p. 5).

Collier and Hoeffler (1998) amend this model, retaining its core hypothesis and some of the proxies but combining ethno-linguistic with religious fractionalization and dropping inequality completely from the model. The purpose of econometric analysis using proxies to quantify indirectly the proposed variables in the models is, of course, to achieve predictive (probabilistic) power and Collier and Hoeffler (1998) generates implications such as: "For example, a highly fractionalized society such as Uganda would be about 40% safer than a ho-

mogeneous society, controlling for other characteristics."

Collier and Hoeffler (1999) and Collier (2000) engage more directly in the greed versus grievance question, separating the two motivations and selecting proxies for each. Greed or loot seeking is proxied by the share of primary commodities in total exports—because this represents the ease of instant taxation by looting—and by the proportion of young males in the total population combined with average years of schooling (the latter supposed to capture the availability of economic opportunities other than violence and looting in an economy). Grievance or justice-seeking as a cause of civil war is captured by the "almost-the-same" or "practically identical" proxies of economic growth in the five years prior to a conflict, scores of repression and restricted political rights, inequality (reappearing not as a proxy for potential government revenue but for grievance), and ethno-linguistic fragmentation. A little confusingly, some of these proxies, e.g., dependence on primary commodities, are switched in Collier and Hoeffler (2001) to stand not for *motive* (i.e. greed) but for *opportunity*.¹⁸

The use of these proxies does seem to be patchwork. The argument that high degrees of inequality generate a greater capacity for taxation of capital is derived from Alesina and Rodrik (1994). Their argument, however, had nothing to do with the self-preservation instincts of the wealthy but rather proposed that where there is high inequality there will be democratic pressure, exercised by the median voter, for higher capital taxation, which in turn would harm growth prospects as this taxation would reduce incentives to invest. The inequality/taxation proxy for potential government military expenditure and hence for the incentive/disincentive for rebellion is also flawed in two other respects. First, it makes something of a naïve assumption about the willingness of elites to be taxed: it is not clear that even in democratic Latin American states, for example, high inequality has made a substantial dent, through effective tax increases, in the Gini coefficient (Székely & Hilgert, 1999). Tax evasion remains extremely widespread in many such countries. In countries threatened by conflict capital flight also has to be expected.¹⁹ Second, the financing of war is rather more complex than is conveyed by this notion of readily flexible tax rates. Most wars have been financed to significant degrees, historically, by borrowing and by inflationary money printing

(Galbraith, 1975). Certainly tax increases have been critical over the long-term but where this has been so, for example, during the consolidation of most European states, this has tended to involve hard-won innovations in fiscal institutions rather than quick fix appeals to the coffers of the rich (Tilly, 1992; Ferguson, 2001). The commitment by the wealthy, or by capitalists, to supporting the funding of war has been more complex, varied and politically contested than can be captured by a proxy like this.

Confidence in these proxies must be undermined, also, by the way in which one proxy, inequality, is used across models: first it is taken as almost-the-same as the self-preservation instinct of elites and hence of the fiscal prospects for war finance; then it is dropped as not the same as anything; then it is reintroduced as almost-the-same as the source of injustice and hence potential grievance-fuelled conflict.

Ethno-linguistic fragmentation is used fairly constantly as a proxy for co-ordination costs and so for the likelihood of collective action. For this to be effective as a proxy, to be “practically identical” to the actual possibilities of and constraints on mobilization for collective action, two conditions must be met. The first is that ethnicity is the only significant category of collective identity and affiliation, and that it operates in the same way across all societies such that more, or less, of it (fragmentation) will have much the same consequences irrespective of geographical and historical context. The second condition is that there are no other effective sources of mobilization. Evidence suggests that neither condition holds. Ethnicity has played a very secondary role at most in many conflicts and the intensity of ethnic animosity does not seem to vary with differences in the index of fragmentation. Its significance has been highly particular and very different across, for example, Somalia, Rwanda, Sierra Leone, Liberia and Angola. Likewise, ethnicity may have played a role in Central American conflicts but it has hardly been a dominant cause in many, and clearly its role has varied between El Salvador, Mexico and Nicaragua. In Yugoslavia ethnicity only came to matter politically in a distinctly “modern” way, that is, as an outcome of economic, administrative, and political policies and experiences from the 1950s onward (Schierup, 1992, 1993); and then in a cumulative, modular or mimetic way drawing on real and imagined pasts in a way very similar to the spread of nationalism as an “imagined community”

(Anderson, 1991). What matters more, then, is surely *how* ethnicity or race or whatever form collective identity takes does or does not come to play a significant role in the origin and feasibility of conflict (Turton, 1997).²⁰ Meanwhile, regarding the second condition, it is increasingly clear that in apparently obviously ideological conflicts such as the Zimbabwean liberation war and in apparently clearly “ethnic” conflicts such as Rwanda, there is a range of sources of mobilization, and motivation, enabling violent collective action. Primary among these is force itself. Coercion, whether subtle or brutal, is a common constant of most contemporary conflicts. This in itself suggests that, however neat the “fit” with some quadratic function of the role of ethnic fractionalization or indeed with the availability of instantly “taxable” primary commodities, such incentives and organizing principles are typically insufficient to make war work, either for rebels or incumbent governments. Meanwhile, there is evidence of a range of agendas of social conflict that affect people’s readiness to fight or logistically to support civil conflict, aside from straightforward profitability at the margin. These include gender and generational conflict and highly personal or interpersonal relations of rivalry and envy.

A further difficulty with the proxies used in these models is that it is far from obvious that the proxy is being assigned to the appropriate object. Preponderance in exports of primary commodities might indicate the availability of loatable goods and so make violence more profitable at the margin than the dreary grind of underemployment and poverty. But in doing this it might just as well be a proxy for failed policy, missing economic dynamism, a probable shortage of consumer goods and imports and widespread grievance or dissatisfaction with this predicament. There is the further problem that the category “primary commodities” is absurdly broad. Open cast or alluvial diamonds might be easy to loot (for soldiers) but not deep-mined diamonds, or aluminum, oil, etc. Other primary commodities like cotton or cocoa do not generate profit levels associated with conflict commodities. Others still, such as coffee, have often been associated with conflict but not in a direct way through lootability. One approach that has added a much needed detail to the analysis of commodities in conflict—and that finds still that commodities are subsidiary to political dynamics—is the political geography of Le Billon (2001).

Similarly, a low average for years of schooling might represent the lack of decent opportunities for (nonconflictual) gainful employment and therefore reveal a low opportunity cost of conflict: however, it might just as well reflect or be directly a source of social anger.²¹ Evidence for this is especially sharp for Sierra Leone, where poor educational attainment has, reportedly, fuelled grievances among young age cohorts who have appealed for improved resource allocations to schools, in a country with a particularly strong educational heritage in sub-Saharan Africa (Richards, 1996). Since loot has been a central feature of the Sierra Leone conflict, within the framework of the orthodox economic models of conflict this presents a paradox of apparently coexisting grievance and greed.²²

One way to resolve the paradox is to appeal to the “false consciousness” argument, as Collier (2000) does by arguing that the “narrative of grievance” has “no informational content.”²³ Another is to reject the categorical distinction between greed and grievance as false. In this case it becomes clear that greed and grievance do not just coexist in the origin of conflict, but that they might be internally related to one another.²⁴ Put differently, where does “greed,” or loot-seeking, or aggressive goods and profit acquisition behavior come from? Is it not feasible that greed emerges from grievance?²⁵ If Kosovo Albanians want better material conditions (and even if some of them emerge as wartime entrepreneurs, profiteers, etc.) is this a manifestation of the aggregated greed of individual Kosovo Albanians or of a historical and relational grievance given particular historical and political opportunities?²⁶ How big is the difference and might the two combine in the mobilization and decision making of individuals? Another example of the ambiguous mingling of greed and grievance as a motivation for violence is given in the widespread political violence during the 1990s in Congo-Brazzaville, chiefly by a range of militias with different political affiliations. Looting in Brazzaville in 1997 was known as “slaughtering the pig” or, referring to the Elf Aquitaine-controlled oil field of N’kossa, “N’kossa, everyone gets his share,” both capturing a criticism of the failure by the political elite to redistribute the wealth of the country. As one militiaman put it, when accused of theft:

You call that stealing? When they incite us to kill, they call it ‘human folly,’ then afterwards they drink cham-

pagne together. That is called ‘national unity.’ While this is going on, we get nothing. Have you ever seen any of the national leaders losing one of their own children in this war? Why is it always us? This has gone beyond a joke. We are going to loot all their houses and tomorrow, when we rebuild the country, perhaps we will finally have a job to go to (quoted in Bazenguissa-Ganga, 1999, p. 48).²⁷

The claims of political leaders or the statements of individual rebels or soldiers are bound to be partial and subjective, but surely it would be absurd to regard them as having no informational content? As Keen (2001) argues, a more useful focus for research than the separation of greed and grievance is the way in which they interact, most notably how the greedy manipulate the grievances of others.

Further, to complicate matters, it is far from obvious that a given conflict, e.g., a “civil war,” is a single phenomenon whose quantifiable aggregates clearly “reveal” individual preferences. For example, participants in a conflict may take part through varying degrees of coercion or voluntary choice and may represent a range of agendas of differing passions and interests that overlap in the overall conflict. Kriger (1992) shows how many different forms of conflict were fought under the mantle of the liberation war in Zimbabwe. Among these conflicts, many women were more or less coercively mobilized behind the Zanu-PF war effort but still used the war to advance their own gender-related conflicts. Similarly, part of the motivation for young males joining the war was not necessarily grand hopes of relief from racial discrimination, or immediate material gains, but escape from the oppressive hierarchy of rural life dominated by male elders. Each of these levels or types of conflict subsumed in the greater war contains its own specific history of social relations, and its own complex mixture of direct interest in material gain with a push for changes in the rules or customs of social relationships. This degree of complexity has not, to date, been captured by neoclassical theory, nor by the proxies used in applications of the theory.

The false consciousness argument is based on a rigid conceptual distinction between greed and grievance, it is sustained by the conceit that the proxies used in applications of the models are genuinely “almost-the-same” as their objects, and lastly it depends on the reliability of the data explored to test the model. But, there are hugely significant empirical weaknesses with most, probably all of the proxies employed. Inequality data are notoriously frail, especially

for use in crosscountry comparisons (and *a fortiori* for sub-Saharan Africa, where many recent conflicts have taken place).²⁸ Production data emphasizing the preponderance of primary commodities are prone to errors, thanks to unrecorded manufacturing activity and the elision in some data sets (particularly concerning mining) between manufacturing and primary commodity activities (Riddell, 1990; Yeats, 1990). Data on educational attainment have their weaknesses, but beyond the crude figures there are problems in assuming a direct, universal and predictable relationship between educational attainment and viable “economic opportunities,” e.g., in employment (Bennell, 1996; Pritchett, 1997). The relationship depends, among other things, on the availability of agricultural wage labor (rarely captured effectively in rural survey data) and on the particular set of policies adopted by governments. Finally, the ethno-linguistic fragmentation index is far from foolproof (McIlwham, 1998). Needless to say, data observations for countries already affected by conflict are even more unreliable, when many goods are traded illegally and statistical services do not have the same reach as in peacetime (Cramer & Weeks, 2000).

The use of conceptually and empirically vague proxies is an instance of the more general problem of the empirical weakness, arbitrary prioritization of particular variables, and manipulation of technique over and above the search for “truth” in econometrics. Mainstream economists and econometric practitioners such as Mayer (1993) and Leamer (1983) have often cautioned against the frequent, almost institutionalized abuse of technique. More radical critics, such as Lawson (1997), have argued for the more fundamental failure as an analytical device of econometrics.

5. FIGHT FOR YOUR PREFERENCES! THE CONFLICT BETWEEN METHODOLOGICAL INDIVIDUALISM AND “THE SOCIAL”

When orthodox economics deals in ethnic fragmentation or in collective action questions, it is engaging with “social issues.” Again, this is not unusual: mainstream economics has become more open in confronting the social in its sweeping colonization of the social sciences. But, the encounter between methodological individualism and rational choice is fraught.

Arrow (1994), for example, argues for the reliance of *all* neoclassical models on “irreducibly social” categories and at least suggests that the implications are not always compatible with “neo-classical paradigms, particularly rational choice” (p. 8). The discussion above of proxy measures suggests, for example, how ungainly economic models can become when they deal in variables such as ethno-linguistic fragmentation. Hirshleifer (1994) introduces the social element through the role of preferences, explained in terms of, for example, strong group identification or its converse, xenophobia (both accounted for in Darwinian evolutionary terms). Ethnicity is the main preference or attitudinal variable, as we have seen, in Collier’s various models. But when the social is stripped from the starting principles and axioms of a model and then reintroduced later, it is not surprising that the fit is awkward.

This shows particularly strongly in empirically applied models, where the social, accounting for preferences, has to take a quantifiable and comparable form such as the cumbersome ethno-linguistic fragmentation index. The result is a virtually arbitrary appeal to the social and a distinctly functional understanding of social variables. Azam (2001) acknowledges that ethnicity “falls short of providing even the beginning of an explanation” of conflicts in Africa. Nonetheless, in setting up a model in which state formation is a transitional process “starting from an institutional endowment of ethnic division,” and in which the objective of state formation seems to be to “provide a credible substitute for ethnic capital” (p. 430), Azam repeats the assumption that ethnic affiliation is the primary form taken by collective identity and social organization in Africa and that its operations are equivalent throughout Africa. Indeed, the

ethnic group is the natural component of a rebellion against the state, as the many links that exist among its members provide an efficient way of overcoming the free-rider problems involved in mobilizing a rebellion or insurgency.

If this were so self-evident, some rebels in Africa—e.g. Renamo in Mozambique—have made remarkably poor use of “ethnic capital.” Ethnicity seems to have been relatively weak compared with organization around nationalist ideals and a shared local history of repeated oppression in the relatively straightforward relationship between rebels and local population

in the “dark forests” of Matabeleland during the liberation war in Zimbabwe (Alexander, McGregor, & Ranger, 2000). The “dissidents” leaving the NZA after independence did so not so much from an Ndebele will to collective rebellion against the state but from a desperate compulsion to self-preservation (Alexander *et al.*, 2000). In the war in Liberia it is fairly clear that ethnicity played at most a minor and secondary role (Ellis, 1999). As Azam himself points out, the RUF in Sierra Leone had “no ethnic support and no national program” (2000, p. 439); and the urban rebellion in Mali that led to the overthrow of General Moussa Traoré was not an ethnic rebellion but a general response to Traoré’s “policy of gradual reduction of state involvement in the accumulation of human capital.” Lemarchand (1994, p. 4) agrees: that the tribalist argument “is singularly unhelpful for a comprehension of African conflict situations has been demonstrated repeatedly.” Indeed, the “central paradox of the Burundi situation (is how) centuries of relatively peaceful commingling between Hutu and Tutsi, cemented by their shared loyalty to a common set of institutions, (could) suddenly dissolve into fratricide” (Lemarchand, 1994, p. 3).

If there are severe problems with the way in which “the social” is introduced into rational choice, methodologically individualist models of conflict, there are also problems deriving from the *failure* to incorporate the social, or to embed the economic and individual in the social, relational and historical. Greed, for example, is not conceived in these models as a relational concept. Greed relates individuals to objects directly: other people become simply obstacles that may be overcome by violence if the opportunity cost of this action is sufficiently low. In other words, the objects (the lootable primary commodities) are of primary causal significance, other people of secondary significance at best aside from some fixed and prior “ethnic capital.” Hence, greed in these models contrasts with concepts such as envy or mimetic rivalry, which are inherently relational and provide grounds for rooting an analysis of the role of resources and commodities within specific relational structures and histories.²⁹ Where relations are concerned, it is necessary to explore their precise characteristics in different contexts, an analytical process that, even where comparative, does not lend itself to large cross-sectional samples and econometric testing for regular patterns of events freed from context.

In short, rational choice economic models of conflict are extreme in their reductionism and fail in explanatory terms. Arguably, this failure is predictable from the very founding principles of neoclassical economics. For, as Zafirovski argues, neoclassical economics does not offer a basis for generalization of rational choice to all social phenomena. Thus, many neoclassical economists, and their founding fathers, acknowledge the distinctiveness of noneconomic phenomena, accept that *Homo economicus* is an abstraction and an abstraction, at that, of one dimension of human behavior, and even in the market economy eventually realize that life is “ontologically irrational” (Zafirovski, 2000, p. 453).³⁰ The point may be made by showing how one particularly open exercise in building on neoclassical foundations an explanation of power relations basically accepts its own limitations. Pagano (1999) distinguishes between private goods, public goods and positional goods. Positional goods have properties that are the opposite of those of public goods: for the consumption of positional goods, rather than implying like a public good the impossibility of excluding consumption by others, implies the ineluctable inclusion of *negative* consumption by others. One person’s enjoyment of a positional good deprives others of that good (Hirsch, 1997); e.g., if the good confers power on its positive consumer, the negative consumer has to experience subordination. Hence, the consumption of positional goods is necessarily conflictual. Positional competition may affect the “*ex ante* desirability of having market transactions for positional goods” (p. 71). Here we have not the happenstance of Hirshleifer’s “mutually inconsistent opportunity sets” but a directly relational analysis: a problem not of the “natural scarcity” economics is accustomed to dealing in, but of “social scarcity.” The difficulty with this analysis is, however, that despite its pretensions it is not consistent with methodological individualism and rational choice. The focus on the positive/negative consumption of positional goods depends for its coherence on a set of positions in social structures, positions into which individuals slot more or less interchangeably and that are the nuts and bolts of a structure of social relations. Yet this demotes the individual within the analytical framework and constrains individual choice within social structure and what must often be (in the positive/negative consumption image) basically power relations. Furthermore, while natural

scarcity may stress a problem common across societies, "social scarcity stresses a problem which differs widely from one society to the other and is strictly related to changes in social relations." Pagano also recognizes that one way to expand positive consumption of positional goods is by manipulating the preferences of others, i.e. raising their willingness to consume the corresponding negative amount; and that achieving a less conflictual society may involve diluting people's taste for positional good consumption. He notes that economics commonly takes preferences as given (as the models discussed above do), and hints strongly that neoclassical economics is not up to the challenge of studying these problems of changing preferences.³¹ Others might more explicitly recast this discussion in terms of the role of ideology, culture, hegemony, and political struggle.³² We seem here to be in the opposite predicament to that claimed by Hirshleifer, i.e. it might be argued that when economists do good work they are doing political economy, not neoclassical economics.

Arguably, then, orthodox economic models of conflict begin with a set of arbitrary assumptions; efforts to test them empirically have so far foundered on misleading use of proxies; and these models have not succeeded in incorporating the irreducibly social on which they depend. The emphasis on profitability at the margin or loot-seeking as a cause of war does not necessarily lead to, but certainly in instances has led to, a fetishizing of commodities. Yet how a war is paid for is not equivalent to what caused a war. Two examples make this clearer. First, oil and diamonds fuel war in Angola. These resources clearly affect the durability of this conflict and they determine many of its characteristics. But they are not the original cause of conflict. Indeed, an earlier phase of this war was sustained less by these natural resources and more by Cold War military aid to both sides, yet while this aid did have more to do with the origins of the conflict still that financing (and attendant political meddling) was only a part of the origin of the conflict. Second, tantalite deposits are crucial to the characteristics of warfare in the Kivu regions of eastern DRC; but again, they did not by themselves cause the conflict. Conflict in Kivu had its origins both in the Rwandan war and genocide and its aftermath and in the collapse of Mobutu's domination of Zaire (and in a history of Rwandan involvement in the region of the Kivus).

If the question of how to pay for a war is not the same as that of what caused a war, nonetheless the modalities of war finance, or the material reproduction of conflict, is not a simple technical question. For, first, the means of war finance may sustain capital accumulation (and hence an interest in continued conflict) and, in the process, accelerate social change including disruptive class formations and a long-term political settlement. Second, those engaged in economic activities during war that both sustain conflict and enable private capital accumulation are often very much people whose material and political interests were at stake in the origin of the conflict. This is not the same as saying that their quest for loot of Sierra Leonean diamonds or Cambodian tropical hardwoods or Afghan opium is precisely what caused the war.

I am suggesting that the roots of conflict do lie in political economy, but that this involves investigating the changes in social relations and material conditions within which individuals act constrained by available "social knowledge" (Arrow, 1994), compelled by instincts of survival as much as by the drive for political power, not so much making rational choices as resolving internal struggles, and so on. In developing a political economy of conflict, it is also necessary to include the significance of the means of conflict themselves, i.e. the conditions of availability of guns and related technology; and to acknowledge that violent conflict, like other forms of social organization and interaction, is modular and commonly reproduced by a form of copying or spillover.³³ Otherwise it is not possible to answer questions like the one posed by Lemarchand (above) for Burundi: why conflict at a particular moment when many of the "variables" have been in place for long periods of time without provoking violent conflict. A political economy approach, rooted in the analysis of social relations and the powerful influence on those relations of material conditions, clearly has to give a prominent role to the analysis of capital and of not just class formation but class disintegration too. From this perspective two more factors emerge more clearly as influencing conflicts than they are credited in many of the models outlined above. First, policy and domestic political struggle within countries dominate real outcomes. Specific political decisions, strategies and contests in the former Yugoslavia before the 1990s, in Nicaragua leading up to and following the Sandinista revolution, in Uganda

and Rwanda leading up to the 1990s, and so on had immense impact on the generation of armed conflict. Second, capital and capitalism is an international phenomenon and always has been. None of the so-called civil wars of recent times can be explained without the dimension of the interests and activities of international capital linkages. Trends since the end of the Cold War in production of arms (privatization and diversification of production sites internationally, partly prompted by evasion of tighter trade regulations in some countries than others—see Lumpe, 2000); the interests of mining companies, mineral traders and industries associated by a variety of linkages to these activities; privatized security firms; and indeed the businesses of aid (ICRC, 2000): all these and more are features that help to reproduce violent conflict internationally (Duffield, 2001).

6. CONCLUSIONS: LIBERATING THE POLITICAL ECONOMY OF WAR FROM ECONOMICS IMPERIALISM

Joanna Bourke, in *An Intimate History of Killing* (1999), argues that combatants in major international 20th century wars often insisted on the myth of their own agency, asserting their own individuality and responsibility in the midst of disorder. “This is not to argue that order and consistency could actually be achieved. . . . But the acceptance of agency enabled combatants to take that one step towards the making of a bearable, and possibly entertaining war” (p. 370). The arguments set out above suggest that neoclassical economics does something very similar: faced with the complexity of conflicts, and indeed of the social in general, it insists on the myth of rational choice individualism, even in the midst of evidence of a range of structural constraints on individualism and of compulsions other than utility maximization that constrain choice and produce a diversity of war rather than a single type. Agency *is* involved in the origins of conflict, choices *are* made, and economic incentives *do* matter, as do individuals. But they are influenced by and operate very much within specific conditions and social and historical features of change. In Angola, for example, the choices open to Jonas Savimbi (and his adversaries) were shaped and constrained by the organization of political power and economic

production during colonialism and by the place Angola had in the politics of the Cold War (and its aftermath).

The question of change is important in defining the grounds for an alternative analytical framework for understanding contemporary conflicts. Neoclassical models of conflict often emphasize the socially wasteful consequences of conflict. In this they tie in with rent-seeking analysis and also with what was once called the “liberal interpretation of war” (Milward, 1970): the presumption that the consequences of conflict were exclusively negative, a presumption that was challenged by analysis of the First World War but one that has been influential in post-Cold War “costs of war” analyses (e.g., Stewart, 1993; Stewart & Fitzgerald, 2000). Like the rent-seeking literature and liberal interpretation of war exercises, neoclassical modes of conflict ignore the distinctions between conflicts that emerge from studying conflict and change, and indeed conflict and class formation and relations. Put simply, and by analogy with Khan’s (1995) discussion of rent seeking, some conflicts might have consequences that are value-enhancing in the medium to long run while others might have different consequences. This would depend on the “political settlement” forged in conflict and, in turn, on the political economy of change that contributed to the origins of conflict. (In many cases it is still too soon to be able to judge this outcome either way, however.)

Among the levels at which an analysis of change can proceed are those defined in terms of, first, an “end of empire” syndrome and, second, “primitive accumulation.” It is difficult to account for the timing and geographical incidence of conflict without including some attention to end of hierarchy moments, or regime changes and their huge capacity to open up the scope for desperate conflict over position, power, wealth, voice, survival, etc. (Howard, 2001). From this perspective it is no surprise that conflicts have broken out in rushes at the end of colonial periods or at the end of the Cold War or—as in Rwanda—where there has been external pressure for democratization.³⁴ The end of a domestic regime and the end of straightforward Cold War patterns and relations combined in Zaire/DRC to contribute to a rash of conflicts within that country since the late 1990s. Clearly, end-of-hierarchy predicaments are not sufficient to cause conflicts, but as a facilitating factor it is probably worth paying them more attention than they receive in most

analyses and conflict prevention policy-making paradigms. In the same way, the modular or mimetic dimension of conflict and in particular the tendency to regional spillover effects needs to play a greater role in explanations of conflict. Wallensteen and Sollenberg (1998) have stressed very effectively the significance in recent intrastate conflicts of "regional conflict complexes."

The other main level at which change matters far more than it is given credit for in neoclassical economic models is the level of class formation, class relations, and primitive accumulation. The dynamics of an effective transition to capitalism in Yugoslavia and the policies that affected the lives (and scope for collective action in class terms) of workers, peasants, and capitalists there threw the country into a deepening crisis in which political opportunists and primitive accumulators were able to manipulate the resources of collective imagination onto which hard-pressed Yugoslavs were thrown back (Schierup, 1992). The processes of "asset transfer" referred to by Duffield (1994) and Keen (1994) in Southern Sudan and elsewhere are also instances of primitive accumulation, in which one (potentially capitalist) class brutally prises away land, livestock and other assets from their previous owners or occupiers and, in the process, prises these people away from their means of reproduction, creating refugees, slaves, fighters and migrant laborers, all of whom might be expected to contribute over time to a process of proletarianization. Conflict in Angola since before independence took on patterns and divisions that were driven to a very significant degree by the ways in which collective identities (partly racial/ethnic/regional, partly ideological) were shaped by evolving material interests given the peculiarities and variations of the spread of capitalism within colonial Angola. The constraints imposed by the Somoza regime both on peasants and workers and on Nicaragua's capitalist class were critical to the formation of a shifting balance of class forces and political interests that led to the Sandinista revolution, that shaped that revolution, and that affected the nature of the domestic resistance to that revolution. In the Kivu districts of eastern DRC the possibilities and constraints on a moment of primitive accumulation (by Rwandan political and military officials and others, as elsewhere in the country by Zimbabwean military officers and others) is what drives violence and conflict rather than

just the fact of the presence of tantalite deposits.³⁵

This last example also highlights another dimension of the political economy of conflicts that is dealt with, if at all, very weakly in neoclassical economic models of conflict: that is, their international dimensions, or better, their role in international capitalism.³⁶ Each "civil war" that could be cited around the world during, say, the past decade has been only incompletely explicable without due attention to international political intervention and economic interests, where these interests may involve both traditionally licit and illicitly traded commodities.³⁷

Meanwhile, one further factor that plays very little role in neoclassical explanations of conflict and that itself needs to be understood in terms of changes in international political economy and particular strategies of capitalists in individual states is that of the supply of the means of violent conflict, i.e. arms. While changes (end-of-regime changes) associated with the end of the Cold War have had a major influence on the supply and cost of arms within regions including Southern Africa and Afghanistan, it is also necessary to trace shifts in production. These shifts include privatization and a diversification of production firms and national sites of production (partly as major industrialized country producers seek to relocate production in middle income countries like Brazil or Turkey in an effort more easily to evade international, e.g., EC, constraints on trade in arms).

There is not enough space in this paper to trace more fully a political economy framework for understanding conflicts: however, three brief points may be made in conclusion. First, research and analysis need to focus on relations of force rather than just choices of violence. For instance, there is a need empirically to explore the contrast between the idea of the comparative advantage of the poor (leading them to allocate more time to violence) and the possibility that violence is forced upon many people and that even if they are not directly press-ganged into militias violence may represent a horrific last resort.³⁸ Furthermore, relations of force are often institutionalized historically as, for example, they have been in Rwanda since the colonial period in particular. Second, there needs to be more research than has yet been undertaken on the role of particular policy developments. For example, some argue that market liberalization within devel-

oping countries reduces the propensity to conflict by dissipating rent; but others argue that liberalization and other associated structural adjustment processes can be associated with a heightened vulnerability to conflict (Cramer & Weeks, 2002; Storey, 1998; Herring, 2001).

Third, there needs to be more analysis of capitalism rather than commodities, of social relations rather than only individual choices, and of policy rather than merely structural determinism by objective conditions of oppression or opportunities for greed.

NOTES

1. An interesting related question is whether wars themselves have changed or simply the way they are understood (Buijtenhuijs, 2000).
2. For rather different perspectives on the colonization of the social sciences by economics, see Hirschman (1970, p. 19) and Fine (1999).
3. In the way of Coase, individuals never pass up the chance of mutually beneficial exchange, while the way of Macchiavelli provides that people will not pass up the opportunity for coercion if this serves maximization goals.
4. A very good illustration of the fragmented and internally divided characteristics of one rebel movement is presented in Guevara's (2001) journal describing part of the Congolese rebellion against Tshombe's government in 1964.
5. This can lead to the implication—as it does for Azam (2001)—that donor policies must not undermine state formation and that high public sector wages might be worth the “resulting” shortfall in public goods spending if they ensure peace.
6. See www.worldbank.org/conflict.
7. The chief intellectual foundation of Kaplan's article was Homer-Dixon (1991).
8. Others have taken the idea of what might be called a new barbarism more seriously, though also viewing it in a more serious light than did Kaplan. For example, Hobsbawm (1998) claims that the 20th century represented a return to barbarism in the sense of a loss of enlightenment values. Keane (1996) focuses on what he calls uncivil war. Meanwhile, there are those like Lindqvist (1998) and Mann (1999) who, more in the vein of Walter Benjamin's comment that every document of civilization is at the same time a document of barbarism, highlight the specifically European, 19th century roots of 20th century global brutality and genocide.
9. On inequality and conflict see Cramer (2001) and Stewart (2000).
10. “There is no shortage of objective reasons for revolt in Central America, but the ideological shifts of the past four decades do not correspond directly to any obvious mutation in the socioeconomic environment. Second, there is a correlation between this ideological fluctuation and the periodization of internal wars in the region” (Grenier, 1996, p. 34).
11. Some might argue that material factors did play a lesser role during the Cold War, playing second fiddle to ideology and global politics: assuming we can distinguish materialism from these factors neatly, this argument certainly undermines recent empirical tests of neoclassical conflict models since these tests examine samples including Cold War era conflicts. On the other hand, there is a plausible argument that both left and right did neglect the intricate political economy of conflict in Angola, Mozambique, Nicaragua, and elsewhere.
12. More recent examples of a richer materialist analysis of conflicts include Keen (2001) and Duffield (2001). See also Cramer (1994) and Marchal and Messiant (2001).
13. Indeed, recent political developments in Zimbabwe, including the country's costly involvement in war in the DRC as well as populist encouragement of violence against white farmers within Zimbabwe, might suggest that conflict can be a response to fading delivery on earlier promises and hopes rather than a pre-emptive evasion based on the calculation of people's rational time preferences. Grenier (1996) argues for the powerful role of a moment of ideological mimesis (following the Cuban revolution) within Central America as a determinant of conflicts and their timing in that region.
14. Even at the level of the individual “choosing” war these models ignore the rationality of managing conflicts, rationality not necessarily confined to means-ends

calculus and not exclusively tied to maximization (Hampshire, 2001); such models also ignore individuals themselves as “sites of struggle” or internal conflict. Particularly good applications of more subtle thinking on rationality in economics and the social sciences are Sen (1986) and Pizzorno (1986).

15. On peasant support for radical rebels in El Salvador, see Grenier (1996).

16. The Collier and Hoeffler models, and other related models, differ significantly in their specification. Methodologically, the typical technique is to use probit and tobit testing to observations. With these techniques it is less clear what significance is really attributable to given variables and a great deal of the results is determined in the sampling. For example, in Collier and Hoeffler (1999) a table summarizing the model’s “predictions” lines up predicted “no” and “yes” outcomes of war against actual events. If we look at the 27 countries in the sample that did have civil wars we find that the model predicts half and fails to predict half. The dice appear a little loaded when one sees that the sample includes 20 OECD countries in the total of 98, including Australia, Canada, Finland, France, Iceland, Japan, Malta, the Netherlands, New Zealand, Norway, Spain, Sweden, the United Kingdom and the United States.

17. In the 1998 version of this model, population size and a discount rate are added to the utility function, and the proxy for rebel co-ordination costs is amended to include ethnic and/or religious fragmentation rather than just ethno-linguistic fragmentation.

18. Collier and Hoeffler (2001) appears to be a partial retreat from some of the claims of earlier versions and makes the rather more limited claim that wars occur generally where wars are possible (i.e. underlying reasons and motivations for war might be very widespread but require some mixture of opportunities if they are actually to take place, or not just to break out but to scale up and last long enough to break into the set of wars defined as substantial enough for inclusion in the sample). Thus, if dependence on primary commodity exports is a “particularly powerful risk factor” this might be because they offer scope for extortion by rebels: “Whether such extortion directly motivates rebellion, or simply makes viable the violent pursuit of other objectives, is beyond the scope of this paper” (Collier & Hoeffler, 2001, p. 2).

19. Furthermore, as one anonymous reviewer of this paper pointed out, median voter theory is rather

inappropriate in those many war contexts that cannot be characterized, even just prior to the outbreak of war, as democratic.

20. One approach to understanding the role of ethnicity in conflict is Stewart’s (2000) argument that ethnicity matters when combined with “horizontal inequality.”

21. As Stewart (2000) argues, it is not just the quantity of education that matters to socially conflictual outcomes but its distribution given categorical inequalities drawn up along lines of collective identities; thus “unequal educational access was prevalent from colonial times in Rwanda, Burundi, and until the Khymer revolution, in Cambodia. In post-colonial Burundi there were deliberate attempts to limit educational access by the Hutu, while educated Hutu were targeted for killing in the 1970s” (p. 5).

22. For a similar argument see Keen (2001).

23. It should be noted that while Collier emphasizes the apparently definitive disincentive to rebellion that is the collective action and free rider problem, Grossman (1991) in stressing expected private returns to insurgents rather than social benefits at least seems to acknowledge that individualist choice theoretic theories are *complementary* to those emphasizing social factors more.

24. See Lawson (1997) on internal relations, whereby one thing only exists in terms of its ties to another.

25. One could note that in one of the early texts of object relations theory in psychoanalysis, Klein (1997) makes precisely this argument that greed and grievance are inextricably and internally related.

26. See, for example, Independent International Commission on Kosovo (2000).

27. Similarly, the evidence of predatory taxation in Liberia does not exclude the possibility of politics in the programs of armed forces there (Atkinson, 1997).

28. Collier’s argument that inequality data are reliable because they have “successfully” been used in models explaining growth ignores the fragility of those growth models drawing on endogenous growth theory and new political economy assumptions (see Cramer, 2000).

29. On mimetic rivalry and violence see Girard (1977, 1996).

30. Ontological irrationality is conceived in terms of inconsistency, action outside means-ends frameworks, the influence of internalized habit, and so on.
31. Some, like Gintis (1998) are more confident that preferences (which for Gintis may be endogenously determined) are susceptible to policy and to economic modelling.
32. For Hirsh, who also argued that the traditional individualist analytical framework of economics was inadequate, the challenge was a moral one.
33. This is clear from the prevalence of “regional conflict complexes” (Wallenstein & Sollenberg, 1998), from the evidence of the historical persistence of war, e.g., in Africa (Justice Africa, 2000), and from the imagery of identification with global violence—most notably the Rambo lookalikes and video popularity in Somalia, in Sierra Leone, and in KwaZulu/Natal (see, e.g., Richards, 1996; and Kaarsholm, 2001).
34. On the scope for conflict to break out during moments of democratization, see Snyder (2000).
35. One of the few analyses directly to address primitive accumulation as a central feature in explaining contemporary conflicts is Moore *et al.* (2001), which focuses on the case study of the DRC.
36. On the role of war in the contemporary world economy see Cramer (2002) and Duffield (2001).
37. See Reno (1996) for outside commercial interests in Sierra Leone; Gourevitch (1998) for the combination of external interests and decision-making failures influencing the genocide in Rwanda, and Global Witness (1998, 2002) on Angola; and for one analysis of an earlier form of external intervention promoting conflict in Southern Africa, Minter (1994). The significance of borders and crossborder spillovers, especially clear recently in Liberia/Sierra Leone and Rwanda/Burundi, echoes the significance of borders to Wolf’s (1969) work on peasant rebellions.
38. This argument is analogous in social terms to the argument of Grossman (1995) that soldiers, indeed all humans, powerfully resist wherever possible the need actually to kill someone, that they go to great lengths to avoid doing so even if they may be happy to engage in the rituals of aggressive posturing, etc., and that in the absence of finely tuned conditioning the act of killing is very much a last resort for most people.

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