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on modern American special operations forces, this is the work that should be on your bookshelf.

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Neverending Wars examines an important issue in international relations: why, since the end of World War II, have civil wars been so long? It attempts to answer this empirical puzzle by offering a fresh theoretical framework, a “sociological neo-institutional” approach (what in political science is normally labeled *constructivism*) (p. 10), anchored by both statistical analysis and short interspaced case studies. At the heart of Hironaka’s book is the argument that more attention needs to be paid to structural (international) explanations for the durability of civil war. (p. 149) This is a welcome challenge to a literature that, as she rightly notes, tends to focus on local causes of civil war.

Unfortunately, the argument as presented in the book is weakened by a seriously flawed analytical framework and a series of logical errors. These blemishes are exacerbated by selective and shallow case studies that could often just as easily refute her thesis.

The author begins by stating that she will “. . . explain the increased duration of civil wars, not the original causes.” (p. 3) But her analysis repeatedly slips into a discussion of the origins or causes of those wars. For instance, the central structural (international) explanation, borrowed appropriately from Robert Jackson and Carl Rosberg, is that the post-war international system fostered the creation of weak (“quasi-states”), a condition that fosters civil war. (p. 13) It is also a condition that enables protracted civil war. (Ibid) At other times she conflates duration and cause. Such as “[C]onsequently, the analysis incorporates and reflects the intuitive notion that long wars represent more “civil war activity” than short ones.” (p. 43) Therefore, she argues that her framework can determine which characteristics of states are associated with varying rates of civil war activity. (Ibid)

She is likely right, but her theoretical framework and selected case studies only weaken the argument. She essentially compares the weak states of the South to the strong states of the North (not their peers; see page 46). There are two problems with this framing. First, if structural factors are the key independ-

ent variable we would expect that they would have a uniform effect on the agents of her study (as she notes on page 52 and page 106). They do not. Second, she should therefore create a rubric that allows for comparing and contrasting the relative strong/weak states in the South.

Take Africa (I am an Africanist), on which she leans heavily in her analysis. Far fewer states have had civil wars than have not, even as the continent is replete with weak states. You, of course, cannot use civil war (or its durability) as a marker of state weakness, if it is the cause of that weakness (endogeneity). Her use of Angola illustrates this point. While Angola did have a protracted civil war, 1974-2002, in the African context it was and is a relatively strong state. Not only was it relatively healthy, but the UNITA insurgency did not find safe haven in the periphery of the state (as she correctly points out is typical in a weak state) but rather in the central highlands of the Ovibindu homeland. She also repeatedly misconceptualizes the conflict. She, for instance, questions why South Africa did not annex Angola, "which arguably lay within its military capability." First, South Africa did not need to annex Angola as Namibia lies between the two, and furthermore, the South African Defense Force created special buffer zones along the Namibian-Angolan border, which they cleared of populations, and their resource bases effectively stabilizing the border. Second, after South Africa's defeat at the battle of Cuito Cuanavale in 1987, it was certainly not within its power. Finally, the last 13 years of that war occur outside her Cold War framework.

She makes (pp. 44-45) the blanket assumption that the period in which a state becomes independent defines them. This structural effect meant that older states faced greater international pressures to develop military capabilities than did post-World War II states. But her early analysis of Angola would seem to belie that point. Outside intervention certainly exacerbated the civil war in Angola. But the war went on long after the superpowers disengaged (and in fact negotiated a tentative peace accord). The war continued between oil (the MPLA government) and diamonds (UNITA) for 13 years.

One of the longstanding debates she weighs in on is how much "ethnicity" can explain conflict, in this case civil war. Given her structural bent, she discounts the "ethnicity" argument. But again, while there is much to support her position, she frames the argument incorrectly. She compares ethnicity and conflict across all states. Given her framework, she should compare it across a sample of weak states.

While she picks and chooses her case studies to make her point (selection bias), within those cases she ignores competing explanations. For instance, she uses the intervention of Libya to explain the civil war in Chad. Outside influences certainly mattered. But it has been argued elsewhere that Chad's institutional and structural challenges contribute to its conflict: it contains between 72 and 110 different language groups and rule is traditionally highly segmented,

which engenders shifting alliances among warring groups. Even as she concludes by emphasizing the “structural” factors explaining the recurrence of civil wars, she returns to domestic explanations of why they are susceptible to systemic (international) pressures.

There are many small mistakes in the book. On page 13 referring to Figure 2.1, she notes the rise of civil war activity in the early and mid-nineteenth century, but the tables indicates the twentieth century. On page 41, she curiously associates anti-secessionist discourse with civil war but states on page 99 that secessionism is strong in Africa and Asia. What is the systemic effect here? She argues that outside influences extended the Congolese civil war (p. 94), while they may have ended it. On page 98, she calls Sri Lanka a “moderately strong state,” while later she notes “. . . a weak state such as Sri Lanka.” She uses Zimbabwe to illustrate the division of the international community into two camps (East versus West), but ZANU and ZAPU fighting within Zimbabwe (Rhodesia) were supported by China and the USSR respectively. On page 137 she avers that if Libya had directly intervened in Chad “significant international rebuke would have followed.” The rebuke came anyway, as she described in previous pages.

From a regionalist perspective, books pitched at this level of theory making or theory confirmation often seem flawed. In fact, the book reflects a very superficial understanding of the countries covered. But this kind of flaw can be compensated for at least from a theory building perspective, in part, with rigorous modeling. In this case, the modeling exacerbates the problem. The case studies are a perverse form of selection bias that actually weakens the thesis.

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Caplan, Richard, *International Governance of War-Torn Territories: Rule and Reconstruction*. New York: Oxford University Press, 2005

Richard Caplan’s new book examines the “New Interventionism” (p. 5), beyond peacekeeping and different from colonial rule, military occupation, mandates, and trusteeships. Although these provide precursors on which Caplan occasionally draws, he argues persuasively that UN Transitional Administrations (TAs) are qualitatively distinct. TAs are internationally recognized and accepted. They enjoy multilateral support. They operate with many other international organizations (IOs) and NGOs on the ground. They explicitly strive to implant liberal, democratic, and welfarist capitalist values and structures. Nor are they