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# Haldi, Stacy Bergstrom. Why Wars Widen: A Theory of Predation and Balancing.

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Haldi, Stacy Bergstrom. *Why Wars Widen: A Theory of Predation and Balancing*. London: Frank Cass, 2003.

The sheer volume of recent scholarship stressing the effects of such factors as regime type, domestic politics, social constructs, and economic relations on the propensity of states to wage war might engender the perception that cogent realist explanations of conflict are largely things of the past. This succinct work by Stacy Bergstrom Haldi, a student of perhaps the most prominent realist "holdout" (John Mearsheimer), is evidence that such a conclusion is unwarranted. Haldi focuses exclusively on an important aspect of conflict to which relatively little scholarly attention has been paid – how and why interstate wars widen – and develops a parsimonious and persuasive thesis based on both classical and neorealist premises. Though problematic in certain respects, Haldi's model serves as a pertinent explanation of why some of the wars of the past three centuries have spread while others have remained isolated, as a well as a useful tool for predicting the likelihood of war-widening the twenty-first century.

Haldi's thesis rests on the argument that states widen wars for one of two reasons: to acquire strategic assets (predation) or to ensure a favorable distribution of relative power (balancing). The key variable influencing states' decisions to engage in war-widening is political cost, or how threatening the war is to their survival. When a potential widener's estimate of the political costs of fighting is low, Haldi posits that its incentives for predatory widening increase, as the state can take advantage of engaged combatants to enrich itself at little risk. When the estimated political costs of fighting are high, predatory incentives are diminished and predatory widening significantly deterred. Nevertheless, states may still widen wars in the face of high costs if doing so is necessary to the maintenance of a favorable power balance. According to the theory, war-widening for balancing reasons is mainly undertaken by major powers, as weaker states do not have the capacity to impact the power balance on their own. For this reason, and because there are several means by which major powers can balance other than war (e.g., deterrence through diplomacy and buck-passing to other noncombatants who value the status quo), Haldi hypothesizes that war-widening is generally most likely when predatory incentives are high and, by extension, when political cost is low. There is, however, a nuanced and important caveat to this hypothesis: when major powers do widen existing wars for balancing purposes, the likelihood that lesser powers will join the conflict increases. This is because major powers seeking even the smallest advantages in balancing against their opponents are willing to promise war spoils to minor powers in exchange for their involvement. Thus, the argument comes full circle: while balancing war-widening is relatively rare, it can be quite dangerous, as its very occurrence removes disincentives and provides further inducements for neutral minor powers to engage in predatory behavior.

Empirically, Haldi tests her arguments against the history of interstate war-widening involving the great powers from 1700 to 1973. In operationalizing the central independent variable, Haldi maintains that the political cost of warfare in this subset of wars was lowest prior to the onset of Napoleon's "total war" in 1803. Simple percentage analyses of the 40 interstate wars in this period involving one or more great powers as initial combatants or wideners support both Haldi's central hypothesis and the contention that the majority of predatory war-widening in the era of high political cost is undertaken by lesser powers. Haldi then constructs thick-descriptive case studies of four of those wars – the Seven Years' War, the French Revolutionary and Napoleonic Wars, the Crimean War, and World War I – to marshal more detailed support of her arguments. In general, her case selection is sufficiently representative of low-cost, high-cost, and transitional periods, and the theory fares well in relation to the two existing perspectives on war-widening that Haldi identifies as competitors: one stressing the role of existing alliances and the theory of "offensive dominance."

In the concluding chapter, Haldi explores the utility of her theory to twenty-first century policy makers. While the costs of war-widening in the current era remain high, Haldi argues that they can be deflated by (and that predatory incentives may therefore increase with) nuclear proliferation. The use of nuclear weapons in war may represent the ultimate in costly warfare, but the mere possession of those weapons also seems to deter other states from inflicting costs, a possibility raised by Kenneth Waltz and empirically supported by the work of, among others, Daniel Geller. This means that members of the "nuclear club" can widen wars without incurring risks to their survival. The contemporary prospects of war-widening are therefore, in the context of Haldi's theory, intimately bound to the success of non-proliferation efforts.

Haldi's theory is well-crafted, compelling, and largely borne out by the evidence she develops. Critics may have issues with the rather strict limits the theory places on the variability of political cost by tying it to different eras, but Haldi's treatment of other means by which costs can be raised or lowered within those eras (i.e., the increase of predatory incentives due to proliferation and great power balancing) fairly mitigates this concern. However, there is at least one additional problem involving the theory's specification and the degree to which it can be usefully compared to existing perspectives. Haldi's definition of war-widening is at odds with commonly employed definitions in some key regards. Haldi's theory speaks to the ways in which existing wars affect the propensity of neutrals to either join in that war or take advantage of the instability created by it to attack other neutrals, as was the case in Russia's conquest of Poland during the French Revolutionary War. It is the latter part of this conceptualization that, while acceptable, is incongruous with common widening definitions (especially the one employed by the dataset normally used to test the alliance perspective, the Correlates of War Project, which considers wars to be widened only if neutrals fight against initial combatants). As such, though

Haldi's theory and findings are significant contributions, comparisons to the limited findings produced by proponents of these perspectives may be less germane than she contends.

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Craig, Campbell. *Glimmer of a New Leviathan: Total War in the Realism of Niebuhr, Morgenthau, and Waltz*. New York: Columbia University Press, 2003.

Campbell Craig has written a well-researched and cogently argued account of the intellectual development of three twentieth-century realists, Reinhold Niebuhr, Hans Morgenthau, and Kenneth Waltz. In the first place, this book succeeds where many academic works fail: Craig does not expect his readers to know the character and history of twentieth-century realism. Instead, in the process of contributing a fascinating new account of the effect that the prospect of thermonuclear war had on the ideas of these three men, Craig also familiarizes his readers with both the prehistory of modern realism and its ideational foundations.

Craig argues that the prospect of a thermonuclear war forced these three realists, all of whom began their careers claiming warfare between states is both an inevitable and inescapable consequence either of human nature (Niebuhr and Morgenthau) or of the anarchic structure of the international system (Waltz), into an abandonment of their absolutist realist doctrine. As Craig writes in the preface, he proceeds "from the assumption that their most important ideas can be better gleaned from what they wrote publicly than from their private correspondence, from psychological analysis, or from an extensive treatment of their particular personal circumstance." (p. xiii) This book is what he calls, rather modestly, a "traditional history of ideas." (p. xiii) But, as a history of ideas, Craig does more than merely chronicle the development and the change in the ideas of these three men. Instead, he tries to show the internal dynamic that guided those changes.

For instance, he shows Morgenthau's evolution from the apparently amoral contention that the "simple and universal lust for power . . . drove international conflict" (p. 57) to his moral differentiation between the character of America's foreign policy during the Cold War and that of the Soviet Union. Thus, in his later thought power politics is no longer a contest between "undifferentiated insatiable hegemons" (p. 61), but between moral good and evil as represented by the United States and the Soviet Union, respectively – a contest the United States could only win if it engaged in the evil of power politics. Craig shows how Niebuhr's thought evolved in a similar manner.