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Must War Find a Way? A Review Essay

Stephen Van Evera, Causes of War: Power and the Roots of Conflict
Ithaca, N.Y.: Cornell University Press, 1999

War is like love, it always finds a way.
—Bertolt Brecht, Mother Courage

Stephen Van Evera’s book revises half of a fifteen-year-old dissertation that must be among the most cited in history. This volume is a major entry in academic security studies, and for some time it will stand beside only a few other modern works on causes of war that aspiring international relations theorists are expected to digest. Given that political science syllabi seldom assign works more than a generation old, it is even possible that for a while this book may edge ahead of the more general modern classics on the subject such as E.H. Carr’s masterful polemic, The Twenty Years’ Crisis, and Kenneth Waltz’s Man, the State, and War.¹

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For comments on a previous draft the author thanks Stephen Biddle, Robert Jervis, and Jack Snyder.


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There is one particular book to which Van Evera’s especially begs comparison. Apparently unconcerned about having order clerks in college bookstores confuse his own book with a contemporary classic, he chose the same title as historian Geoffrey Blainey. In this generation, Blainey’s is the book most similar in scope, although different in approach and style, and it is one that will ultimately last at least as well. Blainey examined and debunked more than a dozen popular notions about why wars happen and, by process of elimination, settled on one main conclusion: “Wars usually begin when two nations disagree on their relative strength.” At least one of the nations in conflict must miscalculate who would succeed in a test of arms, or the weaker would yield without a fight. Thus a clear pecking order in international relations may not necessarily produce justice, but it promotes peace. A roughly even balance of power, in contrast, makes miscalculation easier. The key to peace is clarity about the distribution of power. Blainey arrived at these spare and powerful conclusions inductively, and his book is a tapestry of unconventional questions, analytical excursions and examples, and ironic observations that social scientists would consider literate and lively but unsystematic.

Van Evera travels a different route, one that proceeds more methodically and deductively and aims to honor the canons of social science. He arrives at a place apparently different from Blainey’s, but actually similar. Van Evera argues for five hypotheses but concentrates on one: “War is more likely when conquest is easy.” This seems to contradict Blainey’s view that imbalance favors peace because weakness encourages compliance with the stronger power. In the course of the book, however, Van Evera makes clear his conviction that the main problem is quite different from the one stated in the initial hypothesis: it is not that conquest actually is easy, but that most often it is not, yet is mistakenly perceived to be easy. This amendment is quite compatible with Blainey’s bottom line.

Van Evera also has an ambitious aim. Where Blainey remains strictly empirical, Van Evera seeks to provide prescriptive analysis that can show policymakers how to manipulate causes of war and deploy countermeasures against them. As a good realist, he does not claim that he can make war obsolete, but he does imply that if statesmen understood the analysis in the book and acted on it, searches for security could be relaxed and more wars could be foregone.

Because it is mainly mistaken beliefs about threats to security that account for war, education—reading this book—should contribute to a solution.

This is a good book. It covers a lot of territory in a clear and organized manner. It focuses on a coherent set of issues and schools of thought and evaluates them systematically. It distills much of the conceptual and theoretical apparatus that evolved in strategic studies during the Cold War and applies the ideas broadly. It makes strong arguments. The book takes off from the essential premise of structural realism—that international anarchy is the permissive cause of war—and investigates more specific ways in which beliefs about the options provided by certain kinds of power may yield sufficient causes. Van Evera does not demonstrate that any single cause of war is sufficient, nor does he claim that any, except perhaps one—the ease of conquest—is likely to be. His analysis does provide reasonable grounds to believe that combinations of some of the causes he examines may well be sufficient in many cases. The book’s reach turns out to exceed its grasp, but it is better to aim high and fall short than to fulfill a trivial mission.

Remember all this as you read on. A review essay cannot justify article length if it simply lauds its subject. It must do one of two things. It can use the assignment as a pretext for an excursion of the reviewer’s own, and ignore the work that is supposed to be examined. This common ploy is irresponsible. The alternative is to dwell on the limitations of the work under review. That is what this essay does. This emphasis would be unfair to Van Evera if readers forget the generally favorable regard within which my criticisms are wrapped.

Social science has more inherent limitations than natural science, and all important works have important limitations. Because it is easier for academics to score points by finding such limitations than by developing unassailable theories of their own, every major book becomes a target. This one goes out of its way to draw fire, however, by claiming to offer a “master theory.” The claim overreaches, for four main reasons.

First, the volume aims for great prescriptive utility, but its conceptual apparatus is deeply rooted in two events of one century (our own): World War I, and the maturation of nuclear deterrence between the United States and the Soviet Union. This may not limit the lessons for the twenty-first century if we are concerned only about interstate wars among great powers, but it is less certain that this frame of reference is the best guide to understanding the majority of contemporary wars: intrastate struggles among groups vying to determine the scope of communities and the character of regimes.

Second, the book does too much. The master theory (offense-defense theory) turns out to roll so many things into the definition of one master variable (the
offense-defense balance) that the distinction between the author’s theory and those he seeks to surpass erodes. The improvement over theories that focus on power in general, such as Blainey’s, becomes hard to discern. Van Evera’s book does not make the concept of offense-defense balance identical to relative power—his definition does leave out some significant aspects of the latter—but it comes too close for comfort.

Third, the book does too little. Like most realist theories, it avoids considering the substantive stakes of political conflict. Such issues may be secondary to the main causes that lie in the distribution of power, but they are still too important to be excluded from a master theory, especially one that deals with cost-benefit calculations of attackers and defenders. Van Evera piles his most important arguments onto the question of when and why states believe that conquest is “easy.” This is a vague adjective on which to rest a master theory. It would be more specific and relevant to say instead: when states believe that conquest of a desired objective is achievable at acceptable cost. That in turn depends not only on the odds of military success but on the value that the government or group places on the objective. As with structural realists of other stripes, there is an apolitical flavor to some of the assumptions that animate the analysis.

The fourth significant limitation is the book’s normative bias in favor of the status quo and the prevention of war. Perhaps all civilized people in the enlightened and satisfied West can agree on these aims. They contaminate analysis, however, when they spill over into an empirical assumption that states normally seek security and cause problems for others because of misguided notions that security requires expansion. This premise is really a hypothesis that remains to be demonstrated, and contradicts the traditional realist view that states’ ambitions grow as their power expands. It is all the more significant because Van Evera’s fundamental diagnosis is like Pogo’s (“We have met the enemy and he is us”). Given that real insecurity is rare, misperception to the contrary becomes “a self-fulfilling prophecy, fostering bellicose policies.”

4. See Fareed Zakaria, “Realism and Domestic Politics,” *International Security*, Vol. 17, No. 1 (Summer 1992), pp. 191–192, 196; and Zakaria, *From Wealth to Power: The Unusual Origins of America’s World Role* (Princeton, N.J.: Princeton University Press, 1998). Van Evera cites the United States as a good test of his theory’s prediction that states with high security will be less aggressive. He dismisses Zakaria’s alternative explanation (weakness of the state apparatus) by saying that it does not explain the “decline of U.S. activism after 1815 or after 1991?” (pp. 182–184, 184 n. 224). U.S. activism, however, did not decline in these periods. In the decades after 1815, the United States doubled in size, taking half of Mexico. Washington has been remarkably active since disposing of the Soviet empire, fighting two medium-sized wars in less than a decade since the Cold War ended, engaging in several smaller interventions on three different continents, and sporting the mantle of world orderer with alacrity.
As a result, “the prime threat to the security of modern great powers is . . . themselves” (pp. 191–192; ellipsis in original).

“Security” is a polymorphic value, as hopelessly slippery as “national interest.” Van Evera does not define “security” in detail, but he implicitly identifies it with the political status quo, especially in terms of what states control what territory. Dispossessed groups with grievances, however, have no reason to identify security that way, and no more reason to see resort to force for revising an unjust status quo as illegitimate than Van Evera has to see use of force for defending the status quo as legitimate. Or greedy groups may want to take what others hold irrespective of whether what they hold themselves is secure.

The Cold War hard-wired the value of stability in the minds of Western strategists, but stability is as normatively charged as any value. Sanctifying stability, as E.H. Carr argued, represents the international morality of the “haves” against the “have-nots.” If war is always a greater evil than injustice, and if prevention of war is the only value at issue, the solution is simple: pacifism. In an important sense, as Clausewitz notes, wars are begun not by invaders but by defenders who resist them. (As George Quester once pointed out to me, this is why we say World War II began in September 1939, when Poland resisted the German invasion, rather than in March 1939, when Czechoslovakia did not.) If the aim of avoiding war is subordinate to some other normative value, and if a book on causes of war is to be one for all seasons, it is a leap of faith to accept that the uniform answer is the one Van Evera prefers: securing stability through recognition that defense is easier than attack.

The book proceeds by examining five hypotheses. (A handy appendix, pp. 259–262, lists the numerous corollaries that go with the main hypotheses.) Each hypothesis gets a chapter, which reviews the arguments, relates Van Evera’s impressions of what his years of research have suggested to him about the bulk of evidence that bears on the hypothesis, and presents a few case studies to test the hypothesis. Four of the five hypotheses are dealt with in the first half of the book. The core of the book is the second half—the exploration of the fifth hypothesis and the body of offense-defense theory associated with it. This essay comments on the arguments in the book in the same order and proportion.

5. Carr, Twenty Years’ Crisis, pp. 53–55, 79–84.
The First Four Hypotheses

The early chapters present a good survey of how several aspects of power and perceptions—especially what Van Evera calls the “fine-grained” as opposed to gross structure of power—create incentives for war. The author strives to encase his arguments in social scientific rigor, but he does not shrink from recognizing some of the limitations of evidence that make historians skeptical of sleek generalizations. Indeed, Van Evera’s forthrightness in laying out the logic, and the illustrations that he believes test it, eases the critic’s task.

The first four hypotheses (p. 4) are that war is more likely when:

- “states fall prey to false optimism about its outcome.”
- “the advantage lies with the first side to mobilize or attack.” (This hypothesis encompasses “stability theory” and concerns about how incentives for preemptive attack can be inherent in certain configurations of capability.)
- “the relative power of states fluctuates sharply—that is, when windows of opportunity and vulnerability are large.” (This encompasses concerns about hegemonic transitions and impulses to preventive war.)
- “resources are cumulative—that is, when the control of resources enables a state to protect or acquire other resources.”

The analysis of the fourth hypothesis, on cumulativity of resources, is most convincing. It engages the question of economic costs and benefits of war and the logic of imperialism, and rests on the notion that if the cost of conquering territory to extract resources exceeds the utility of the resources, “cumulativity is negative” (p. 106), and incentives for war are reduced. In agricultural eras cumulativity was high, but in recent times cumulativity has declined because knowledge-based economies are harder to loot than are smokestack economies.

7. There are questions about Van Evera’s method that this essay does not have room to explore in detail. He sets out criteria for case selection and theory testing that are somewhat confusing as stated in the book (e.g., saying that the case studies “proceed by comparing the case to normal conditions,” without indicating what a normal condition is supposed to be), notes that orthodox social science methodology would count his tests as weak because they do not adhere to certain rules, and dismisses the criticism by saying that he has “never found these rules useful,” but without saying why (p. 12). He also reports that he surveyed thirty wars (mostly modern ones) in background research (p. 13 n. 24), but does not indicate why he chose those particular thirty. (Why, for example, the Falklands/Malvinas War but not the much larger Chaco War? or the Indochina War but not the more consequential Russian, Spanish, or Chinese Civil Wars?) For clarification of Van Evera’s views on methodology, see his Guide to Methods for Students of Political Science (Ithaca, N.Y.: Cornell University Press, 1997).
(pp. 114–115). Van Evera presents a balanced and reasonable argument that conquest still pays sometimes, but less often than in the past.

The first three hypotheses are more problematic. They are compromised by underwhelming evidence, or prove less convincing as explanations of war than of strategy and timing—explanations of “how” and “when” more than of “whether” to fight.

UNSURPRISING OPTIMISM

Van Evera’s first hypothesis says that governments are more likely to fight if they believe they will win handily, and that they are often mistaken in this belief. This is convincing but not surprising, especially for one who has read Blainey. Yes, false optimism “preceded every major war since 1740” (p. 16). It must also have preceded most wars before 1740, but so did valid optimism. When a war has a winner and a loser, Van Evera’s proposition and its opposite are both true by definition: the victor’s optimism proves correct and the loser’s mistaken. If neither side is optimistic about its chances, on the other hand, war is less probable, because neither side sees much to gain from starting it.

The hypothesis is more relevant for understanding pyrrhic victories or stalemates that expose false optimism on both sides, or cases in which attackers start wars that they expect to win but wind up losing. The hypothesis is important if such cases are more typical of war than cases in which one side wins at acceptable cost, that is, if most cases are ones in which optimism by either of the contenders is invalid. Determining that would require a substantial empirical project. World War I is a case of that sort, and it is omnipresent in Van Evera’s theoretical apparatus and empirical analysis, but being an important case does not make it typical.

How much does it help to cite a case like Frederick the Great’s invasion of Austria (p. 17), where the defender’s optimism proves wrong and the attacker’s right? That combination confirms common sense. Or what about cases where indications of false optimism are awash in other evidence of pessimism, such as U.S. decisions on Vietnam (pp. 17, 23)? A problem that afflicts analyses of government decisions in many cases is that one is likely to find ample

8. Van Evera says that “U.S. officials recurrently underestimated their opponents in Vietnam,” but the record in general does not support this. See Leslie H. Gelb with Richard K. Betts, The Irony of Vietnam (Washington, D.C.: Brookings, 1979); and H.R. McMaster, Dereliction of Duty (New York: HarperCollins, 1997). Van Evera cites a Joint Chiefs of Staff estimate that 205,000 U.S. troops would be needed to achieve goals in 1961, but does not mention that U.S. troops were not committed in force until four years later, when communist forces were stronger and army and marine corps estimates of needed U.S. manpower ranged between 500,000 and 700,000.
evidence of both optimism and pessimism, as policymakers confronting a clash of incentives and constraints are racked by ambivalence. When historians know that whatever sample of available evidence is incomplete, how firm a lesson can we draw from the many cases in which, say, five statements reflecting optimism can be stacked against three reflecting pessimism?

STABILITY THEORY

Exploring the second hypothesis, Van Evera amends and salvages “stability theory” and its emphasis on “first-move advantages.” He argues that incentives to mobilize or strike first do not cause war in the manner of Thomas Schelling’s image of “reciprocal fear of surprise attack,” but that first-move advantages cripple peace-seeking diplomacy by leading countries to conceal their grievances, capabilities, and plans (p. 39).

Van Evera asserts that real first-move advantages are rare, while the illusion of such advantage is common (p. 71). This could be true, but the point is not demonstrated by the analysis. In one paragraph, he gives a list of cases he believes support his assertion (pp. 71–72), but does not present the bases for that conclusion. Instead he presents three case studies in detail that together do as much to question as to confirm his arguments that first-move advantages are rare, and that belief in them prompts war: World War I, China’s entry into the Korean War, and the 1967 Arab-Israeli War. In these three cases, there were real first-move advantages. Van Evera reports that on the eve of World War I French General Joseph Joffre warned “that France would lose fifteen to twenty kilometers of French territory for each day mobilization was delayed” (p. 71 n. 31), as if Joffre was wrong. If a mobilization lag would make no difference in where the lines wound up, Van Evera does not say why. As it was, by September 1914 the Germans penetrated close to 100 kilometers into France—about 20 kilometers from Paris—and they managed to fight the rest of the war on French territory. That seems to be a significant advantage from moving first and fast. Similarly, in the other cases used to explore the hypothesis, the Chinese army rocked the 8th Army back into the longest retreat in U.S. military history in 1950, and Israel clobbered Egypt and Syria in 1967.

Perhaps Van Evera’s point should be a different one—that first-move advantages rarely are great enough to win a war. If so, it would be clearer to focus on decisive first-move advantages. The Germans did lose World War I four

9. “If surprise carries an advantage it is worthwhile to avert it by striking first. Fear that the other may be about to strike in the mistaken belief that we are about to strike gives us a motive for striking, and so justifies the other’s motive.” Thomas C. Schelling, The Strategy of Conflict (New York: Oxford University Press, 1963), p. 207.
years after they overran northern France. But the Israelis won decisively in 1967, and the Chinese succeeded in settling the war in 1953 on terms that secured their gains on the battlefield and ratified the expulsion of United Nations (UN) forces from North Korea (which they had almost entirely liberated before the Chinese threw them back). To his credit, Van Evera recognizes that first-move advantages were real in at least the 1950 and 1967 cases, but his argument still claims more than he demonstrates. Additional data might support his views, but the evidence selected for presentation in the chapter does not demonstrate that even decisive first-move advantages are rare.

Whether imagined or decisive, first-move advantages are hard to cite as causes of two of the three wars Van Evera examines under this hypothesis. Rather they were causes of *strategy*. The Chinese would not have refrained from intervening in Korea if General Douglas MacArthur had not made it easier for them by splitting his forces in the advance to the Yalu. Van Evera offers a convoluted rationale for why Beijing could have decided to stay out if U.S. signals had been different, but notes nevertheless that Mao Zedong decided to intervene as soon as U.S. forces entered North Korea, before they approached the Yalu (pp. 56–61). Similarly he says, “Egypt and Israel had many reasons for war in June 1967 and probably would have come to blows absent a first-strike advantage” (p. 68).

**WINDOWS**

The third hypothesis is that fluctuations in power cause war by creating windows of opportunity and vulnerability and thus incentives for preventive or preemptive action. Declining states are tempted to launch preventive wars and take bigger risks; diplomatic alternatives to war are weakened because declining states cannot make credible offers and rising states conceal their grievances to avoid triggering preventive war; and if the balance of power shifts peacefully, the new disequilibrium may produce war if the fallen state refuses to yield privileges. This chapter produces a generally sensible discussion.

Again, however, the three cases Van Evera chose to test his hypothesis provide very mixed evidence; two of the three do not provide good support. The best of the three, and a quite powerful one, is Japan’s 1941 attack on the United States (pp. 89–94). Without the windows created by Washington’s combination of escalating demands and lagging mobilization, Japan might never have struck first. In regard to Germany in 1939, however, “window theory” proves convincing as an explanation of the timing rather than of the decision for war. The book provides no reason to disbelieve the widely held view that
Hitler wanted war from the beginning. If Hitler was determined to have a war, his strategic decisions were about when, not whether. Van Evera argues (pp. 94–99) that this was true only of German designs in the East, that Hitler’s aims did not require war against France and Britain. The Allies’ guarantee to Poland, however, meant that Hitler could not take Poland without a war in the West, and could not move against the Soviet Union without first defeating France. Alliances account for his decisions to attack in the West; windows may account for when he did so. A cause of strategy is not the same as a cause of war.

The third case is the U.S. military buildup in the early 1950s, which Van Evera discusses as a cause of increased American belligerence. This confuses cause and effect. The United States did act more cautiously when it was weak in 1950–51, and more toughly when a tripled defense budget gave it more power a few years later. It did not, however, become more belligerent because it became more powerful, but the reverse. The Korean War prompted the militarization of containment. The desire for better strategic options to support deterrence produced the buildup, which enabled prior policy to be pursued with more confidence. Actions could catch up with aims. This example is not a case where a window caused war, or even one where it caused belligerence, but where the buildup was caused by the desire to avoid war—and succeeded! This example belongs in a different volume, Causes of Peace.

**Master Theory? Overmilking Nuclear Deterrence and 1914**

The heart of Van Evera’s book is the promulgation of “offense-defense theory” (henceforth, ODT) as a “master key to the cause of international conflict,” a “master theory” that “helps explain other important causes” and provides “the most powerful and useful Realist theory on the causes of war” (pp. 190, 117; emphasis added). So much for Thucydides, Machiavelli, and Hobbes, or Carr, Morgenthau, and Waltz! This is no mean claim. The claim is all the more bold because it feeds on a theory conceptually rooted in very recent times. ODT has been overwhelmingly influenced by the frame of reference that developed around nuclear weapons, and the application of deterrence theorems to the conventional case of World War I.

10. Van Evera suggests a more dangerous result when he cites evidence that U.S. leaders thought about preventive war in this period (p. 100). On this, see especially Marc Trachtenberg, *History and Strategy* (Princeton, N.J.: Princeton University Press, 1991), pp. 103–107. Indications that officials ruminated about preventive war does not mean that it reached the point of being considered a serious option for deliberation and decision at the highest levels.
NUCLEAR THINKING

Military historians, and budding arms controllers in the interwar period, have written on the import of shifting advantages of attack and defense. The recent evolution of theory on the subject, however, reflects a systematic and sustained focus on the idea of strategic stability, which crystallized only in the nuclear deterrence theory of the Cold War. American deterrence theorists became preoccupied with how specific configurations of weaponry and targeting options might autonomously induce war or peace. The key was how combinations of technology could provoke or constrain decisionmakers by the prospect of how a nuclear engagement would turn out, depending on who fired first.

The nuclear framework provided a simple model of mutual deterrence based on a few variables—types of targets, types of forces, and their relative vulnerability—and clear distinctions between offensive and defensive operations. None of the weapons that figured in standard scenarios and calculations were dual-purpose. For example, intercontinental ballistic missiles (ICBMs) could be fired only to attack the adversary’s territory, not to intercept an adversary’s attack on one’s own territory. Attacking and defending forces could support each other (counterforce first strikes could boost the effectiveness of antiballistic missile (ABM) defenses by drawing down the number of the victim’s retaliating forces that had to be intercepted), but the scenarios of targeting and force interaction that dominated nuclear theorizing were few and uncomplicated compared to the historical variety of conventional warfare. The literature on nuclear strategy drew analysts toward a fixation on autonomous military causes of war and peace, and away from attention to the substantive political issues and motives in international disputes that had traditionally preoccupied diplomatic historians and political scientists. If the seedbed of deterrence theory does not account for the production of recent ODT, it certainly accounts for the intellectual receptivity of its consumers in the peak period of attention in the 1980s.

Common to academic analysis of the nuclear era were convictions that strategic stalemate was inherent in the nature of military technologies and force structures of the times; resort to war (at least nuclear war) was irrational because it was tantamount to suicide; recognition of this reality should prevent war; the prime danger to peace was misperception and miscalculation; and stability might be enhanced through measures of arms control that reinforced confidence in mutual deterrence. Bits and pieces of such ideas can be unearthed from earlier writing about military matters, but coherent application of them to conventional strategy became prominent only in the surge of debate
about the balance between the North Atlantic Treaty Organization (NATO) and the Warsaw Pact during the Cold War.

Van Evera and other contemporary exemplars of ODT—George Quester, Robert Jervis, Jack Snyder—were all steeped in nuclear deterrence theory before venturing into analysis of conventional military strategy.\textsuperscript{11} The companion image of the “security dilemma” also figured heavily in ODT (it is even evident in post–Cold War applications to unconventional civil wars).\textsuperscript{12} The Cold War agenda made ODT especially appealing. Most obviously in the form presented by Jervis, the theory offered a way out of the security dilemma.\textsuperscript{13} If capabilities could be configured to the advantage of defense, and Washington and Moscow could recognize and codify the situation, they would be less susceptible to inadvertent war or expensive arms competitions arising from a spiral of misperceptions. If only the conventional balance could be made like the nuclear balance, competition could be stabilized.

Given the strength of mutual nuclear deterrence, the main debates about stability in the Cold War devolved onto the conventional military balance between NATO and the Warsaw Pact. Doves wanted to find an excuse for less reliance on the threat of deliberate nuclear first use to deter Soviet conventional attack, and warmed to analyses suggesting that NATO's conventional forces were less hopelessly incapable of holding the line than typically assumed; hawks wanted stronger military capabilities across the board and denied the defensive adequacy of Western forces on the central front. ODT reinforced one


\textsuperscript{13} Jervis, “Cooperation under the Security Dilemma,” pp. 186–214. In principle, ODT is independent of normative preferences for stability and defense dominance. Its association with these preferences may have been stronger during the Cold War, given the prospective costs of war between the superpowers, than it will be in the future.
side of the debate by offering ideas for affordable ways to make a Soviet armored offensive as unpromising as a nuclear strike.

Events of 1914–17 seemed to illustrate a configuration of capabilities that should have done this had the defense-dominant nature of that configuration been recognized. Failure of the Schlieffen Plan, years of inconclusive carnage on the western front, and costs far greater than the belligerents would have chosen to bear had they foreseen them showed that understanding of the offense-defense balance should have led to restraint in the crisis of 1914. If the chances of successful conventional offensives on the central front in Europe in the Cold War could be reduced to what they had been in 1914, and this could be demonstrated to political leaders, East and West could compete in peace. Implicit in the promise of ODT was the hope for a technical fix to the Cold War military competition.

The fascination of many realists with ODT was not shared by all, especially Reaganite idealists who saw the Cold War problem as Soviet aggressive motives rather than unfounded mutual fear. To them, seeing the security dilemma as the essence of the problem implied moral equivalence. The strategic task was not to reassure the Russians but to press them. These skeptics rejected the canonical wisdom about the benign nature of mutual assured destruction (MAD) and promoted nuclear counterforce capabilities and across-the-board increases in conventional forces. This view carried the day in the policy arena, but was a minority position in academia, where satisfaction with stable nuclear deterrence prevailed.

As social science theories go, nuclear deterrence was exceptionally elegant, so the ODT about conventional military competition that fed on it evolved into a hot genre in strategic studies. As such, it naturally spawned criticism. One skeptical article by Jack Levy appeared at the same time as Van Evera’s initial analysis of World War I.14 Levy faulted the genre for the variety of terms in which the offense-defense balance had been defined: “the defeat of enemy armed forces, territorial conquest, protection of population, tactical mobility, the characteristics of armaments, attack/defense ratios, the relative resources expended on the offense and defense, and the incentive to strike first.” He also faulted ODT for tautological hypotheses, circular definitions, confused concepts, and other flaws, and concluded that “the concept of the offensive/defensive balance is too vague and encompassing to be useful in theoretical

analysis.” Critics chipped away at the technological formulations of the offense-defense balance, especially challenging the notion that particular weapons supported either attack or defense as consistently as they did in nuclear theory. Others counterattacked on behalf of ODT. Van Evera’s book engages only some of the critiques, and usually in brief asides.

One problem in this body of theory is confusion of dimensions of analysis in the definition of the offense-defense balance. Before the nuclear era, most conceptions of the balance were in terms of military outcomes—which side would have the operational advantage when attackers crashed into defenders in a combat engagement. Offense was identified as attack by ground, naval, or air forces against an adversary’s territory (either to seize ground or to destroy assets behind the lines), and defense as blocking the attacking forces (holding ground or intercepting bombers or missiles before they can destroy their targets). A balance was considered “offense dominant” if, other things being equal, attacking forces were more likely to succeed in advancing or destroying their target than defending forces were to keep them from doing so.

The newer variants of ODT confuse the question by sometimes shifting the definition of offense-defense balance to the level of political outcomes—the effects of operational advantages on national decisions about whether to initiate war. Confusion is most obvious in the inversion of terms when nuclear weapons are considered. Oxymoronically, most contemporary exponents of ODT cite the nuclear offense-defense balance as defense dominant—even though no effective defenses against nuclear attack yet exist—because mutual vulnerability to the operational dominance of offensive forces prevents political leaders from deciding to start an engagement. (The traditional approach calls the nuclear balance offense dominant because when fired in an engagement, attacking missiles would accomplish their mission and helpless defenders would be destroyed.) Even if the oxymoronic definition were reasonable for the nuclear case considered alone, it muddies the conceptual waters. It is inconsistent to characterize both the 1914 conventional balance and the Cold War nuclear balance as defense dominant—which most offense-defense theorists do—because that is true in terms of combat interactions for the first but

not the second, or in terms of political effect for the second but not the first.\footnote{The balance in 1914 was defense dominant only in operational terms, because decisionmakers were not constrained by accurate anticipation of what would happen when the forces collided. The reality became evident only with the test of war. One might then say that the offense-defense balance should be defined in terms of perception—what decisionmakers believe would happen—which is indeed the basis for characterizing the nuclear balance, given that there has been no test of war for this case. If belief rather than demonstrated reality is the criterion, however, the 1914 balance must be cited as offense dominant.}

It would be more accurate to call the nuclear situation “peace dominant” if the criterion is decisions in peacetime rather than results in wartime. Moreover, the inverted conception has other limitations.

None of those who surveyed the inverted definition in the 1980s approved of its purest application to conventional warfare: Samuel Huntington’s proposal to shift the logic of NATO’s conventional deterrent from denial to punishment, from a defensive blocking strategy to a retaliatory counteroffensive. Huntington transposed the scorpions-in-a-bottle logic of nuclear deterrence directly to conventional strategy, arguing that a threat to retaliate against Soviet assets in Eastern Europe was a more potent deterrent to Soviet adventurism than an uncertain capability to hold the inner-German border against Soviet tanks.\footnote{Samuel P. Huntington, “Conventional Deterrence and Conventional Retaliation in Europe,” \textit{International Security}, Vol. 8, No. 3 (Winter 1983/84), pp. 32–56.} Proponents of ODT were uniformly aghast at Huntington’s proposal and insisted on a NATO conventional strategy that was literally rather than oxymoronically defensive.

ODT’s benign view of the effects of nuclear weapons on the danger of war rests on an old belief that MAD guarantees military restraint, a belief that was never universally shared among analysts or policymakers. Those who lacked confidence in MAD’s pacifying effect focused on the logical tension between confidence in mutual deterrence between the superpowers at the nuclear level and confidence in mutual deterrence at the conventional level. This debate about the credibility of threats to escalate bedeviled NATO strategy for more than three decades and was never resolved. If mutual nuclear deterrence was so solid that neither side would ever strike first, then the world was safe for conventional war: the side with conventional superiority could win while both sides’ nuclear forces held each other in check. To threaten deliberate escalation, even unto intercontinental strikes, in order to deter conventional attack—NATO’s official doctrine for decades—mutual nuclear deterrence must have an exception. Either mutual fear of nuclear annihilation is so strong that neither side would ever strike first, or it is not. Theorists and policy analysts spilled gallons of ink in arcane and contorted attempts to square this circle, and never
succeeded without resorting to more oxymoronic solutions like “the rationality of irrationality,” or simple assertions that the “balance of resolve” would favor the side defending the status quo.19

ODT glosses over the fact that the view of MAD as pacifying or defense dominant prevailed only among academics and diplomats, and lost where it counted—within policy circles that determined procurement, force posture, and war plans.20 (This will be a problem for students reading Van Evera’s book. A decade past the Cold War, few of them now know any more about nuclear strategy than they read in the works of theorists, and believe that MAD captures all they need to understand about the subject.) The glossing may reflect Van Evera’s barely concealed contempt for the foolishness of those who have sought alternatives to reliance on MAD, a contempt that occasionally leads him to obiter dicta or errors—for example, his charge that Air Force Chief of Staff Curtis LeMay “urged general nuclear war during the missile crisis” (p. 141 n. 90).21

The biggest disappointment of this volume is the brevity and pugnacity of Van Evera’s chapter on nuclear weapons, which is far less scrupulous in argumentation than the rest of the book. It reads like a polemical bit of arm waving written for some other purpose and tacked on to the book. This chapter ignores all the old unresolved debates about nuclear strategy and conventional stability, says next to nothing about real dilemmas, and simply asserts the ineluctability of MAD. It adds nothing to the literature on nuclear strategy or to the arguments in the rest of the book. For example, Van Evera proclaims without any consideration of counterarguments that “conquest among great powers is almost impossible in a MAD world. . . . States also can better defend third parties against aggressors under MAD. . . . the United States deployed troops to Germany as a tripwire during the Cold War” (p. 246). It is as if history stopped in 1957, before the long and intense debates over extended deterrence,

flexible response, the credibility of NATO doctrine, and prolonged conventional defense—which the author does not even mention let alone analyze.

OFFENSE-DEFENSE THEORY OPPOSES DEFENSE
Ironically, Van Evera is most adamant against the idea of real nuclear defense dominance—that is, a world in which defense dominates at the operational level of combat effect. He coins the cute acronym BAD (for “Both Are Defended”) to contrast this sort of world to MAD, and opines that “if BAD is ever achieved, it will not last long. One side will soon crack the other’s defenses.” Perhaps, but the same could as easily be said of any defense dominance constructed at the level of conventional combat. Van Evera then rails that “the lives of states in BAD will be brutal and short. They will conquer and be conquered at a fast pace” (p. 251). As to why, he gives not a clue. The cavalier quality that diminishes his offensive against nuclear defenses is particularly unfortunate, because academic conventional wisdom opposing ballistic missile defense is now on the losing end of policy debate; it dominated policy in the 1970s, and fought a successful delaying action against Ronald Reagan’s Strategic Defense Initiative in the 1980s, but has lost its influence in the 1990s. Even Bill Clinton—who populated his foreign policy team with liberals and arms controllers who would fall on their swords to save the ABM treaty—has accepted that the United States should deploy antimissile defenses.

There are still convincing reasons for continuing to oppose investment in active defenses (although there is no excuse for opposing passive civil defense).22 For example, no one has yet demonstrated how any system designed to date will overcome efforts of a major power to saturate it with attacking forces, or of a minor power to circumvent it by using alternate means of delivery. The case is not nearly as clear, however, as it was during the Cold War. For example, the United States and Russia might hypothetically agree to field limited and comparatively cheap active defenses that would not be sufficiently effective to negate their deterrents against each other (given that each could retain 1,000 or more deliverable weapons), but that could blunt attacks by third countries with small numbers of missiles. North Korea, Iran, or Libya will not have the resources to field more than a few ICBMs. Even China, in 1999, has fewer than twenty ICBMs; to deploy enough to saturate a U.S. or Russian defense might require a tenfold increase, a daunting expense

for a country with other priorities. This sort of speculation may not justify missile defense programs, but Van Evera does not consider any of the arguments for them in the chapter that condemns them out of hand.

If old-fashioned mainstream academic deterrence theory is all we need, maybe we can rest easily with the oxymoronic notion that lack of defenses constitutes defense dominance. Such faith smacks of hubris. The strength of nuclear deterrence theory came from its parsimony. That in turn reflected the theory’s blindness to big problems revealed by a focus on different variables. Bruce Blair, John Steinbruner, Desmond Ball, and Paul Bracken showed how fundamental assumptions about stability in mainstream deterrence theory were thrown into serious doubt if analytical focus was shifted from the vulnerability of forces to the vulnerability of command systems.23 These conceptual revisionists took a fundamental assumption of standard deterrence theory (the physical capacity to effect retaliation if the victim’s forces survived attack), subjected it to empirical analysis, and showed it to be shaky.

Finally, the combination of political and military criteria exemplified in definitions of the nuclear offense-defense balance raises questions about consistency in operationalization of the concept. Van Evera writes, “I use ‘offense-defense balance’ to denote the relative ease of aggression and defense against aggression” (p. 118 n. 2). “Aggression” is a value-laden term implying unprovoked attack, which does not cover all cases of first-strike or invasion. Which ones it does cover depends on evaluation of motives and attributions of illegitimacy to particular attacks. Unless Van Evera really meant to say just “initial uses of force” rather than aggression, this definition of the offense-defense balance mixes normative political and empirical military criteria in ambiguous ways. Was an offense-defense balance that enabled a defensively motivated Israel to save itself from destruction in 1967 by initiating a devastating conventional offensive one that was offense dominant or defense dominant? All in all, it is better for analytical accuracy, clarity, and simplicity to return to strictly military operational criteria for categorizing the offense-defense balance, and to keep separate the purposes to which the operational advantages may be put or the decisions they may prompt.

HOW MUCH DOES WORLD WAR I PROVE?
The utility of offense-defense theory for stabilizing the Cold War was under-written by another wave of analysis of the dynamics of the 1914 crisis. Had decisionmakers in European capitals then understood that the military equation of the time favored the defense, rather than assuming the reverse, peace might have endured. The diplomatic events of 1914 and the military events of 1914–17 inspired theoretical excursions that were applied more generally to conventional military strategy. From extensive reading and thinking, Van Evera has concluded that “perceived offense dominance is pervasive and it plays a major role in causing most wars. Knowing exactly which wars would still have erupted in its absence requires a close analysis of each case—impossible here. But the evidence does indicate that it had a vast role” (p. 185, emphasis added).

It is frustrating to hear the author say that he cannot show the evidence, but then hear him report what the evidence shows. Apart from one case, the book presents scant evidence of instances in which policymakers decided to initiate war because of perceived offense dominance—that is, where it is clear that the perception was a principal cause rather than one among a dozen on a laundry list of reasons. The one exception is World War I, which gets a whole chapter of its own as well as space in other chapters. Although he notes five other analysts of World War I who doubt that a “cult of the offensive” caused it, Van Evera reports that the “test” case of World War I confirms twenty-four of twenty-seven predictions from ODT, and concludes, “We have a verdict: war is markedly more likely when conquest is easy, or is believed easy” (p. 233; see also p. 238 n. 180).

Eyebrows might be raised when we see the case that inspired much of recent ODT being presented as the test of the theory, especially when two pages later Van Evera mentions offhandedly that some other cases show offense dominance as conducive to peace. Similarly, the book has a short section on “Qualifications: When Offensive Doctrines and Capabilities Cause Peace” (pp. 152–160), but gives little indication of how heavily these qualifications should weigh. Implicitly, they are minor exceptions that do not vitiate ODT’s presumption in favor of defense. World War II, however, is a rather big exception. As Van Evera recognizes, “The Western allies of the 1930s needed offensive capabilities and might have deterred the war had they adopted more offensive strategies”; belief in defense dominance contributed to war (pp. 154, 235). Why should we be sure that we have more to learn from 1914 than from 1939?
Fine Grain or Gross? A Bloated Concept of Offense-Defense Balance

The most powerful concepts are ones that can be defined simply and operationalized clearly. Most attempts to promulgate simple and powerful concepts get battered if not discredited by the complexity of real life. This did not happen with nuclear deterrence theory because reality never provided any test cases (nuclear wars in which the conditions of deterrence failure and the results of combat engagements could be studied). Because of the volume of evidence on conventional warfare, in contrast, the simplest versions of ODT ran aground. Recent debate highlights the trade-off between accuracy and elegance in the theory.

CONCEPTUAL ENLARGEMENT

Refining conventional offense-defense theory proved to be trickier than the thought experiments and exchange calculations of nuclear strategy, where only a few scenarios were at issue and weapons could be used operationally only for attack or defense rather than for both. Critics raised questions about how far the simplicity of nuclear terms of reference could be transferred to conventional war, where attack and defense are advantages that shift in the course of a war; the same weapons can be used for both purposes; multiple weapons can be reorganized and combined with doctrines in different integrated systems with different implications; the tactical offensive and strategic defensive (or the reverse combination) can be combined in campaigns; or even tactical offensive and defensive actions can reinforce each other within the same engagement. For example, in May 1940 the Germans expanded bridgeheads by using “interior lines and the ability to concentrate against one face of their salients while standing on the defensive on the other two”; in 1973 the Egyptians used air defense missiles offensively to screen the advance of ground

24. Archer Jones, The Art of War in the Western World (Urbana: University of Illinois Press, 1987), p. 531. ODT promoters also sometimes cite the authority of Clausewitz when he proclaims that “the defensive form of warfare is intrinsically stronger than the offensive” (emphasis in original), but neglect to mention that Clausewitz says this to endorse tactical exploitation of defense in the service of a strategic offensive. Given that defense has a negative object, it “must be abandoned as soon as we are strong enough to pursue a positive object. When one has used defensive measures successfully, a more favorable balance of strength is usually created; thus the natural course of war is to begin defensively and end by attacking. It would therefore contradict the very idea of war to regard defense as its final purpose” (emphasis added). Clausewitz, On War, p. 358; see also pp. 370, 600.
forces, and the Israelis used attack aircraft and tanks defensively to hold their line; and so forth.

ODT supporters have generally responded by introducing qualifications that reduce the emphasis on particular military technological determinants and take the theory further from parsimony. Some buttress the theory by adding and subtracting components to cope with anomalies, and making the theory’s claims more limited. In an otherwise impressive article, Charles Glaser and Chaim Kaufmann complicate the specifications of the theory so much that a reader would need to keep a cue card just to be able to think about how to apply it. They also introduce simplifying assumptions that ease theorization but do not comport with important aspects of the empirical record of modern conventional war, and that essentially eliminate strategy from the strategic equation. They insist that “the offense-defense balance should be assessed assuming optimality—that is, countries choose the best possible strategies and force postures,” and they eliminate differences in “military skill” from consideration.25 By this restricted definition, the offense-defense balance must have favored France over Germany in 1940, Egypt over Israel in 1967, Argentina over England in 1982, and Iraq over the United States in 1991! The authors appropriately keep their claims restrained, saying that ODT “does not claim that the offense-defense balance is in general a more important determinant of military capabilities than is power or skill.”26

Van Evera reacts to critics’ challenges about how much ODT can explain in a different way, and in the bargain sidesteps debates about what elements should be included in the concept. Instead of refining the definition of the offense-defense balance, he broadens it;27 instead of resolving debates about what should be included, he includes almost everything that has ever been suggested; and instead of modifying claims, he expands them. Although he criticizes other realists for misdirected attention to “gross” power rather than the “fine-grained” structure of power, he crams so many things into the definition of the offense-defense balance that it becomes a gross megavariable. His offense-defense balance includes “military technology and doctrine, geography, national social structure, and diplomatic arrangements (specifically,

26. Ibid., pp. 48–49.
27. This point leaps out. It is also made by all three critics responding independently to a preview of Van Evera’s book. See Davis, Finel, and Goddard, “Correspondence: Taking Offense at Offense-Defense Theory,” pp. 180, 187, 190–191.
defensive alliances and balancing behavior by offshore powers),” as well as manpower policy (the *levée en masse*), strategic choices in campaigns (the maldeployment of UN forces in the advance to the Yalu), popularity of regimes, and the breakdown of collective security organizations (the League of Nations) (pp. 122, 160–166). Phew! If we could get him to throw in population and financial resources, we could almost call the offense-defense balance “relative power” and be back to basic realism. Van Evera does not eliminate the distinction between offense-defense balance and power, but he blurs it enough to make the difference in their implications unclear.\(^\text{28}\)

Van Evera would have none of this, because he sees his approach as a basic variant of structural realism differentiated from others’ focus on polarity. He distinguishes four schools (p. 10): Type I, classical realism; Type II, structural realism, which focuses on polarity; his own preferred Type III (“fine-grained structural realism”); and Type IV (“misperceptive fine-grained structural realism”). He does not believe that herding a horde of variables into the offense-defense balance washes out the fine grain. Indeed, he insists that his expansive definition is very parsimonious because it “achieves simplicity, binding together a number of war causes into a single rubric. Many causes are reduced to one cause with many effects” (p. 190). In a response to critics he argues, “A theory is not shown to lack parsimony simply by demonstrating that its concepts include a diverse range of lesser-included concepts. . . . All concepts are aggregations of lesser concepts.”\(^\text{29}\) But why is it more useful to consider all the variables lumped in the definition above as “lesser-included” elements of an offense-defense balance than it is to consider a more coherently specified balance as a lesser-included concept of “power”? It might be if we had a formula that converts all the “fine-grained” elements of Van Evera’s balance to a metric that is more illuminating than relative power as a whole. But there is no indication of how all these elements in the concept should be aggregated, and thus no clear way to measure the offense-defense balance. Van Evera is reduced to relying on “author’s estimates” when he comes to categorizing whether the balance in actual periods of history favored offense or defense.

\(^{28}\) There is also a problem of virtual circularity in the theory. A big advantage for defenders is that other status quo powers often align with them. Keeping alignment out of the equation makes the offense-defense balance a much weaker predictor. Putting it in the equation, however, makes strategic behavior both a cause and an outcome. See Davis, ibid., p. 180; and Goddard, ibid., p. 191. On whether he uses the behavior of states to explain the behavior of states, Van Evera says, “I plead guilty. . . . I don’t see a problem.” Ibid., p. 199.

\(^{29}\) Ibid., p. 196.
How then can other analysts apply this concept? Without a clear formula, it is hard to see how far we are left from Blainey’s conclusion that war hinges on disagreements (miscalculations) about relative power.30

DISTINGUISHING THE OFFENSE-DEFENSE BALANCE FROM THE BALANCE OF POWER

There is a difference between the offense-defense balance and power as a whole that is clearer in more limited formulations of ODT. Blainey views parity of power as destabilizing because he does not see decisionmakers differentiating the convertibility of resource inputs for combat outputs—that is, he does not consider that governments might take fewer risks if they understood that equal resources would produce unequal effectiveness in attack and defense. Rough parity lets governments miscalculate easily in Blainey’s world because either side can convince itself that a modest edge in the balance of power gives it an opening to gain victory. Power parity promotes peace in a recognized defense-dominant world, in contrast, because both sides know they would need a much bigger edge than they have in aggregate resources to overcome the other’s defense.

Van Evera preserves the crucial criterion that the balance should be some sort of pound-for-pound ratio, rather than a measure of aggregate probability of combat success, when he formulates the concept as “the probability that a determined aggressor could conquer and subjugate a target state with comparable resources” (p. 118 n. 2, emphasis added). But in the next breath, he subverts it by admitting alliance shifts to the calculus, because realignment can overturn the balance of resources—indeed, that is its purpose. This is why the focus on “gross” power (polarity) of the Type II structural realists from whom Van Evera wants to differentiate himself is likely to remain most important. If not, one should expect that Van Evera’s Type III focus on the fine grain should yield different or more useful explanations and predictions about who would win wars. But does it? Would his framework have told statesmen more than “gross” power analysis about what they would have wanted to know before going to war in the Persian Gulf or Kosovo? Perhaps, but it is hard to know without a formula that converts the offense-defense megabalance into a sort of “unit-price” measure of relative combat efficiency.

30. One problem is uncertainty about what aggregate “power” means, or whether there is such a thing. Realists tend to see power as a conglomeration of material capabilities, mainly economic and military, that is fungible. Others who do not see power as fungible insist that one must ask, power to do what?—which is what an offense-defense balance is about. See David A. Baldwin, “Power Analysis and World Politics: New Trends vs. Old Tendencies,” World Politics, Vol. 31, No. 2 (January 1979), pp. 161–194.
One reason Van Evera focuses on the fine-grained structure of power is that he considers it “more malleable than the gross structure; hence hypotheses that point to the fine-grained structure yield more policy prescriptions” (p. 8). Others such as Quester also see the benefit of focusing on the offense-defense balance as its comparative malleability (although he keeps the concept more manageable, by restricting it to the level of military capabilities and techniques).\(^{31}\) It is indeed sensible to concentrate on causes that can be manipulated, and a central mission of strategic studies is to direct attention to the causal significance of military force structures and strategies in international relations. There are two problems, however, in touting the malleability of the balance.

First, if we keep the concept more coherent than Van Evera does and we focus on the military elements of the offense-defense balance, we can see that they are more manipulable than some factors such as geography, but not necessarily more manipulable than others such as alliances or diplomatic strategies. For example, what was crucial in setting off World War II in Europe was not any notion of a military offense-defense balance or changes that the Germans could make in it. Rather it was the stunning German-Soviet nonaggression pact, which solved Hitler’s problem of a potential war with great powers on two fronts. “Fine-grained” aspects of power (such as innovations in doctrine for armored warfare) may explain why the Germans did so well in early phases of the war, but the gross distribution of power (poles and their alignment) tells us more about how the war got started.\(^{32}\)

Second, if we accept Van Evera’s sweepingly inclusive definition (which simply incorporates the Hitler-Stalin pact in the offense-defense balance), it becomes more important to remember that manipulability works two ways. Van Evera is interested in presenting a menu of variables that can be manipulated to reinforce peace, but there is nothing that ordains that status quo powers will shape the balance to their purposes more successfully than revisionist powers will shape it to theirs. Manipulability is necessarily conducive to peace only if we assume that the problem lies entirely in the security dilemma, that countries in conflict will welcome a structured stabilization of capabilities that appears to make conquest hard for either of them.

Packing many elements into the offense-defense balance concept does not alter the fact that the elements are very different things. Whether “lesser-

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included” or not, this variety means that there are numerous points of entry for a revisionist strategist to try to overturn a benign balance. This does not mean that it can always be done. In some conventional military equations, structural constraints may prove irremediably favorable to the defense. But the more complicated the equation, the more variables and the more potential combinations a revisionist strategist has to work with. Organizations and doctrines may be developed to change the effects of combined arms operations, and force structures may be adapted to apply existing technologies in novel combinations; or innovation and research may be channeled to develop technologies to counter tactical defensive advantages; or strategic plans may be found to capitalize on tactical advantages of defense for strategically offensive purposes; or allies may be acquired or detached from adversaries; and so forth. Indeed, this is what strategy is supposed to be all about. Jonathan Shimshoni illustrates how successful militaries do this, how they strive to make balances dynamic and evolving, rather than static and confining, to give them an edge. 33

An assumption that countries will seek stability if only its foundations can be made clear to them is an assumption about motives, not capabilities.

Beyond Power

Over time observers have cited many motives for war: God, gold, and glory, or fear, honor, and interest. 34 Like most realists, Van Evera has little to say about any of these but fear and interest. 35 Indeed, because his premise is that most wars result from security seeking, he focuses primarily on fear, and how to relieve it by adjusting the structure of power and suppressing misperceptions of it. Van Evera shares with Blainey a lack of interest in the political substance of issues at stake in conflicts among states—territorial claims, religious or ideological disputes, economic interests, and so forth. To Blainey, understanding aims and motives may provide a theory of rivalry but not of war. 36

If rivalry is a prerequisite for war, however, reducing causes of rivalry will tend to reduce the probability of war.


35. For systematic attempts to integrate the issues, stakes, or political motives of conflict in theories of war, see John A. Vasquez, The War Puzzle (New York: Cambridge University Press, 1993); and Kalevi J. Holsti, Peace and War (New York: Cambridge University Press, 1991), chap. 10.

POLITICAL STAKES AND OBJECTIVES
To Van Evera, the beauty of a benign offense-defense balance is that it can prevent war irrespective of motives, because states can see that attack cannot succeed. The common sense that appeals to Blainey is also implicit here: if a prospective war seems unwinnable, no one will start it. One reason to pay attention to political incentives as well as to military constraints is that this starkly utilitarian common sense does not always govern decisions on war and peace. For honor or other reasons that seem quaint to economists, countries may fight in the face of probable or even certain defeat.

There is also reason to consider motives within the utilitarian calculus. Sometimes the problem is, as Blainey argued, that contenders of roughly equal power do not see that they will not be able to use force effectively, especially if they believe they can exploit novel stratagems. A defense-dominant balance might solve this problem, but it would have to be lopsided; Van Evera’s concept of the balance includes so much that it would be hard to find many situations in which some elements of the conglomeration did not seem to cancel out other defense-dominant ones, leaving an opening for successful attack—for example, where geography, or social structure, or alliance choices conducive to offensive action overrode military technology conducive to defense.

Where the answer to which side would prevail in combat is a matter of probability rather than certainty, risk propensity and willingness to sacrifice enter the equation. These can depend in turn on the value of the stakes. “A place in the sun” may strike Van Evera, a humane and modern man, as no decent reason to launch a war that courts catastrophe. The value of that aim was far different to the Kaiser’s Germany, however, when it was coming late to the imperialist scramble at a time when the world was made of empires. A difference in the relative value of the stakes to two contestants may also affect their relative will to sacrifice, the amount of their power that they commit to the contest, and their chances of winning. North Vietnam had far less power than the United States, but the United States had far less stake in the outcome of the war, so committed a much smaller fraction of its power. Van Evera examines the costs of war intently, but neglects the benefits, which require examination of stakes and motives.

Realists usually ignore the stakes in dispute for theoretical purposes because they assume that conflict is more or less a constant. Something always comes up to put countries in conflict, but many conflicts persist without war. The question is what pushes conflict over the brink to combat. For a highly general theory of average outcomes over time in international politics—what interests Kenneth Waltz—this lack of attention to the substantive issues of disputes makes sense. For any particular case, however, the fact remains that there are two necessary conditions for war: a distribution of power that makes war appear an effective option, and a conflict of interest. The first without the second will not produce war. The United States has more than enough power to take Canada, but lacks sufficient political incentive to do so.

War between countries in conflict can be avoided through either of two ways: constraining their power or resolving their conflict. ODT sees more promise in the first because it seems more manipulable. But is this necessarily so? If the offense-defense balance includes geography, social structure, and other such weighty things, as in Van Evera’s formulation, why is it more manipulable than attitudes toward stakes? Washington and Moscow dueled inconclusively for two decades over nuclear arms control and the Mutual and Balanced Force Reduction talks, but the Cold War ended more because of a political decision to resolve the conflict than because of any beliefs about an offense-defense balance between East and West. Van Evera argues that Mikhail Gorbachev was willing to make concessions because he adopted ODT (p. 119). If we look at all that happened at the end of the 1980s, however, it is hard to believe that whatever thought Gorbachev gave to ODT was more than a tactical rationalization for much grander political decisions. Gorbachev did not bargain with the West to negotiate arms reductions or diplomatic deals. He simply gave up the global ideological contest that was the subject of the Cold War. Realist ODT was less evident in his thinking than liberal institutionalist nostrums about a “common home.” The Soviets did not exchange concessions with the West. They made unilateral concessions, which the West pocketed. The Cold War ended not because the Russians bought some academic idea about how much easier military defense of their interests could be, but because they gave away those interests, surrendered their empire, and left the West with nothing to fight about.

SUBCONVENTIONAL WAR

More attention to political stakes would also help if we want a theory about the causes of war in general, rather than the subset of wars that interests Van Evera and most international relations theorists: big, world-shaking, interna-
tional wars among great powers. Those sorts of wars are most important, and they could occur again. They have not been representative of the phenomenon of war since 1945, however, and with at least a temporary respite from serious great power conflict after the Cold War, hot wars may be of other types for some time to come.

Most recent wars have been intranational, internal, civil wars. Most international wars are about which countries control which territories; most intranational wars are about which group gets to constitute the state. Van Evera’s research surveyed thirty wars, 90 percent of which were international and 10 percent intranational (p. 13 n. 24). Since 1945 the proportions of war that have actually occurred are closer to the reverse. In civil wars the relative salience of concerns about political values, as opposed to material power, is usually greater than in international wars.

Some wars could count in both categories because they involve interventions by outside countries in civil wars, and some categorizations could be disputed (e.g., the Korean War could be considered a civil war between northern and southern Koreans who considered their country to be temporarily divided, or an international war between two Korean states—as American white southerners regarded what northerners called the Civil War). In terms of military character, some civil wars are similar to international ones, if the contending groups are geographically separated and they marshal conventional armies. Many contemporary internal wars differ significantly, however, from the conventional terms of reference by which recent ODT has developed. When contending groups are divided by ideology, religion, or ethnicity they are often intermingled, and combat is not carried out on one front with conventional lines of operation. Rather the front is everywhere, as groups compete to dominate villages, provinces, and loyalties throughout a country.

Irregular or guerrilla warfare does not follow the operational logic of ODT grounded in modern conventional war and high force-to-space ratios that are possible on a linear front. Irregular war is more similar to that of ancient and medieval times, where low force-to-space ratios made raids the usual form of combat. The rule of thumb purveyed by counterinsurgency theorists during the Cold War was that a government defending against guerrilla attacks needed a ten-to-one superiority in forces. This is thirty times the legendary one-to-three ratio in the rule of thumb for the tipping point between attack and defense in conventional warfare. When guerrillas can be engaged in a conventional battle where they fight on the defense, they can be annihilated. Insurgents who hide among the civilian population, in contrast, can concentrate at will to raid specific objectives, destroy pockets of defending forces, and
retreat to avoid battle with the government’s main forces. The government must disperse its forces to defend everywhere. The ability of the insurgents to hide in the population, in turn, is determined by political and social factors.

Van Evera does not agree, because he thinks about international war. In answer to a critic he writes, “Modern guerrilla war has defended many countries and conquered none. It is a fundamentally defensive form of warfare.” But the communist guerrillas in Greece, Malaya, and South Vietnam, or the anti-communist mujaheddin in Afghanistan and Contras in Nicaragua were attacking native governments. Foreign intervention on behalf of such governments internationalized the wars precisely because of the guerrillas’ offensive success.

A final reason to look beyond power, and especially beyond the offense-defense balance, is the conservative presumption in Van Evera’s theory. The book implies that war is justified only to defend what one has, not to conquer what others hold. If we were to leave open the normative question of whether conquest or coercion are ever desirable, we would leave open the question of whether it is desirable to institutionalize defense dominance. In principle, however, there is no automatic reason why it should be any more legitimate to use force on behalf of what is than on behalf of what should be. Once what should be becomes admissible as a warrant for force, there can be no consistent preference for defense over attack, because there is no automatic congruence between what should be and the status quo. Revolution may be justified in an unjust society, and international revisionism may be justified in an unjust international order. If the norm is to be that a just cause can warrant resort to force, whether peace should be preferred to war is an issue, not a premise. On this matter Blainey’s analysis is more detached and value free, because he regards peace as no more natural than war.

Perhaps it sounds perverse or insidious to raise this issue. But is it any accident that American or other Western analysts favor peace, when the United States and other Western great powers are status quo states satisfied with the international pecking order as it is, a bunch of prosperous, righteous countries calling the tune for others? When an unjust status quo catches the eye of Western leaders, however, they do not hesitate to see the virtue in attacking it. Witness the assault launched against Serbia to wrest an oppressed Kosovo from its control. Bill Clinton and Tony Blair were certainly glad to enjoy offense

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40. Blainey, The Causes of War, chaps. 1, 16.
dominance, as NATO air forces could range across Serbia’s skies unchallenged, methodically destroying the country with impunity.

**War Will Usually Find a Way**

Offense-defense theory is in many respects quite hardheaded and realistic about what military power can accomplish, but some aspects of it are non-Clausewitzian. ODT is most concerned with stability, how to prevent war by making it ineffective as an instrument of policy; Clausewitz thinks in terms of strategy, how to make war serve policy. Most devotees of the theory would like to paralyze strategic options for using conventional forces as thoroughly as MAD seems to paralyze nuclear strategy. The ideal offense-defense balance is implicitly some benign form of military stasis. Clausewitz, in contrast, pays little attention to the causes of war. He focuses on its nature and purpose. The strategic orientation he represents is dynamic: how to scan the environment and history to find a way around obstacles that protect the adversary, how to find a way to make war work for one’s purposes.

Some carefully exclude strategy from ODT; in contrast to Van Evera’s excessive inclusiveness, this narrows the concept too much. Shimshoni’s critique that strategic entrepreneurship can overcome a constraining offense-defense balance is dismissed by Glaser and Kaufmann as flawed because it “relaxes the assumption of optimality. All of his examples . . . hinge on states having significant advantages in military skill over their opponents.”41 This protects their conceptualization of an offense-defense balance, but does not give any reason to look to such a balance as a reliable bar to war. As Levy noted, the eighteenth century has generally been regarded as defense dominant, but the campaigns of Frederick the Great were an exception. Analysts do not call the offense-defense balance offense dominant in this period because Frederick’s success came from tactical and strategic innovation rather than technology. “Still, it cannot be denied that Frederick demonstrated what was possible given the technology of the time.”42

Strategic schemes help war find a way. ODT looks for means to bar the way, to neutralize strategy or freeze it. This proves harder in conventional warfare than in the nuclear deterrence theory in which contemporary ODT germinated.

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When Waltz quoted the line from Brecht that appears at the beginning of this essay, he followed up with the point that “for half a century, nuclear war has not found a way.” This led Waltz, John Mearsheimer, and a few others to view the spread of nuclear weapons as a welcome force for peace. Whether valid or not, this idea appeals only to a handful of hyper-realists and has no future as a policy prescription.

If ODT is to help bar the way to war, it has to do so at the conventional level of combat. In theory, this might be accomplished by finding defensive technologies as enduringly superior as offensive systems have been in nuclear forces, or by freezing defense-dominant configurations in arms control deals. So far, however, no formulation of defense dominance for conventional forces has proved immune to strategy. The stalemate of 1914–17 came closest to such robust defense dominance, but it too was eventually overcome by innovations. German infiltration tactics on the one hand, and British deployment of tanks on the other, made attack more effective and unfroze the lines. If strategic effort can find ways to overcome an inhibiting offense-defense balance, and if political stakes and motives are strong enough to provide an impetus, war will still find a way.

That is not to say that victory will necessarily find its way. There is no reason to expect that miscalculation or plain foolishness will become less common. Nor do the various problems discussed in these pages mean that the idea of an offense-defense balance is wrong or useless. The question is still open, however, as to how best to conceptualize the balance and how much to claim for it. If it is to be analytically useful, the concept’s scope should be kept limited to the dimension of military operations: relative combat effectiveness, on a pound-for-pound basis, when attackers crash into defenders in a tactical engagement or strategic campaign. There is no consensus yet on a precise definition in these terms—any one yet offered seems to run up against some question or scenario that makes it problematic—but we could do worse than to work for particular purposes with some of those that have emerged from methodological wrestling. For example, Levy’s: “the offensive/defensive balance is inversely proportional to the ratio of troops needed by an attacker to overcome an enemy defending fixed positions”; or Lynn-Jones’s: “the amount of resources that a state must invest in offense to offset an adversary’s investment

44. As James Kurth says, “There probably has not been a single foreign policy professional in the U.S. government who has found this notion to be helpful.” Kurth, “Inside the Cave: The Banality of I.R. Studies,” National Interest, No. 53 (Fall 1998), p. 33.
in defense. . . . the offense/defense investment ratio required for the offensive state to achieve victory”; or Glaser and Kaufmann’s: “the ratio of the cost of the forces that the attacker requires to take territory to the cost of the defender’s forces.”

It will not do, as Van Evera has done, to swallow up contradictions and confusions brought out in recent debates by rolling all manner of things that make conquest easy into the offense-defense balance. Including factors that affect the total resources available to contestants makes the balance almost congruent with a comprehensive net assessment of which side would win a war, and too close to just another term for how to parse “power.” To make good use of the concept of the offense-defense balance, it is necessary to return to more limited and coherent formulations of it. The last word on this subject remains to be written.

Van Evera makes an important point that is buried in a footnote early in his book: “Most of the important contributing causes of war are neither necessary nor sufficient” (p. 41 n. 19). The works of Kenneth Waltz concentrate on one of the main necessary or permissive causes of war: international anarchy. Blainey concentrates on another: disagreements about relative power. But necessary or permissive causes are not enough to understand specific wars, as any statesman will insist that a theory aspiring to prescriptive utility must do. For that, looking back toward classical realism (e.g., Carr’s emphasis on the interactions among grievance, satisfaction, and power) helps more.

More interest in efficient or sufficient causes will lead us to look more at motives and stakes than Waltz, Blainey, or Van Evera has yet done. The subjects that Van Evera announces he will deal with in a planned second volume get closer to the efficient causes. He plans to explore four explanations, that: (1) militaries cause war “as an unintended side-effect of their efforts to protect their organizational welfare”; (2) states “infuse themselves with chauvinist myths” and “underestimate their own role in provoking others’ hostility”; (3)


46. It is interesting, however, that in the vast region where militaries were most dominant in politics—South America in much of the twentieth century—there was almost no interstate war. Felix Martin shows that neither main paradigm of international relations theory—realism or liberalism—explains this, a much longer peace than the vaunted Long Peace in Europe during the Cold War. (Causes of war identified by realism existed in South America, but war did not occur, and causes of peace identified by liberalism did not exist, yet peace endured.) If anything, organizational interests and transnational professional identification—in effect, an epistemic community of military officers in the region—discouraged a resort to force across borders. Felix Martin-Gonzalez, “The Longer Peace in South America, 1935–1995,” Ph.D. dissertation, Columbia University, 1997.
states “often misperceive because they lack strong evaluative institutions”; and (4) states avoid defining their national strategies clearly, so “official thinking deteriorates,” states are less able to assess each others’ intentions, and they become “blind to the other’s concerns” (pp. 256–258).

Whether we are concerned with necessary, permissive, efficient, or sufficient causes, there is not likely to be a breakthrough on the first-order issues of debate about what causes war. The debate has cycled back and forth for centuries in many variations on a few themes in the realist and liberal traditions of international relations theory. This elemental debate is no more likely to be resolved in the next century than in the last one, if only because each tradition has a purchase on different facets of the problem, and the glass that is half full for one will always be half empty for the other.

At less cosmic levels of debate, where most research takes place, progress is more plausible even if it is likely to be more halting and inconclusive than ardent social scientists expect. There will be room for revisionist research and creative revisitations of old issues as long as historians keep political scientists honest by disciplining their oversimplifications about what past cases prove. There will be plenty of work to do, for a long time, on the question that is among the handful of fundamental issues in international relations: What causes wars? Van Evera’s book does some of that work, imperfectly like all good books, but usefully. It need not make good its claim to a master theory to merit a place at the table.