

Correspondence

Reassessing Net Assessment

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To the Editors:

During the past decade, discussion of the conventional balance in Europe has divided into two distinct debates—the real debate among serious analysts, conducted largely in scholarly publications, and a propaganda debate, conducted mainly in the media. At one end of the real-debate spectrum are *qualified optimists* like myself who believe that current NATO forces can probably thwart a Soviet blitzkrieg, but are not strong enough to warrant removing nuclear weapons from NATO's deterrent posture.¹ At the other end are *qualified pessimists*,

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1. For full citations of my relevant works and those of Joshua Epstein and Barry Posen, two other qualified optimists, see Eliot A. Cohen, "Toward Better Net Assessment: Rethinking the European Conventional Balance," *International Security*, Vol. 13, No. 1 (Summer 1988), p. 51 n. 2. (This article is hereafter cited in the text as "Cohen" with page numbers.) For other examples of qualified optimism, see inter alia Anthony H. Cordesman, "The NATO Central Region and the Balance of Uncertainty," *Armed Forces Journal International*, July 1983, pp. 18–58; Brigadier General Christian Krause, "The Balance between Conventional Forces in Europe," working paper (Bonn: Friedrich Ebert Stiftung, December 1982); William P. Mako, *U.S. Ground Forces and the Defense of Central Europe* (Washington, D.C.: Brookings, 1983); John Stokes, *Threat Assessment*, Report submitted to Western European Union (WEU) Assembly by WEU Committee on Defense Questions and Armaments (Paris, November 2, 1987); and *Soviet Readiness for War: Assessing One of the Major Sources of East-West Instability*, Report of the Defense Policy Panel of the Committee on Armed Services, U.S. House of Representatives, 100th Cong., 2d sess.,

who argue that NATO will probably lose, but grant NATO a significant chance of defending successfully. For example, Andrew Hamilton concludes that "current NATO conventional forces might be able to thwart a Pact attack, but their margin of safety is woefully thin and the possibility of a NATO defeat is quite real."² No participants in this debate argue that NATO has a robust conventional deterrent; all concede the real possibility of NATO defeat. Nor do any participants seriously argue that NATO is hopelessly outnumbered. Even Kim Holmes of the hawkish Heritage Foundation wrote in his recent contribution to *International Security* that "if NATO receives adequate warning and promptly mobilizes its forces, it has a good chance of stopping a Warsaw Pact invasion without resorting to nuclear weapons."³ Thus, analysts in the real debate agree that NATO has a force that might be able to defend successfully; they disagree on matters of degree.

The media debate bears little resemblance to the real debate. It is dominated by *defeatists*, who assert that the Warsaw Pact enjoys crushing conventional superiority over NATO and who therefore expect a quick and decisive Pact victory in the event of war. Defeatists have not produced a single serious analysis to support their claims. Thus the defeatist view is a school without scholarship, an opinion asserted without proof. Nevertheless, this view is often parroted by politicians and less-informed members of the press.⁴ The

December 5, 1988. Also see Malcolm Chalmers and Lutz Unterseher, "Is There a Tank Gap? Comparing NATO and Warsaw Pact Tank Fleets," *International Security*, Vol. 13, No. 1 (Summer 1988), pp. 5–49, an important analysis of the tank balance on the Central Front.

2. Andrew Hamilton, "Redressing the Conventional Balance: NATO's Reserve Military Manpower," *International Security*, Vol. 10, No. 1 (Summer 1985), p. 111. Other examples of qualified pessimism include Pat Hillier and Nora Slatkin, *U.S. Ground Forces: Design and Cost Alternatives for NATO and Non-NATO Contingencies* (Washington, D.C.: Congressional Budget Office, December 1980); William W. Kaufmann, "Nonnuclear Deterrence," in John D. Steinbruner and Leon V. Sigal, eds., *Alliance Security: NATO and the No-First-Use Question* (Washington, D.C.: Brookings, 1983), pp. 43–90; Carl Levin, *Beyond the Bean Count: Realistically Assessing the Conventional Military Balance in Europe*, 2nd ed. (Washington, D.C.: Senate Armed Services Subcommittee on Conventional Forces and Alliance Defense, July 1988) and *ibid.*, 1st ed. (January 1988); Frances M. Lussier, *U.S. Ground Forces and the Conventional Balance in Europe* (Washington, D.C.: Congressional Budget Office, June 1988); and NATO Center Region *Military Balance Study, 1978-1984*, Report prepared by Department of Defense Steering Group for Center Region Analyses, July 13, 1979.

3. Kim R. Holmes, "Measuring the Conventional Balance in Europe," *International Security*, Vol. 12, No. 4 (Spring 1988), p. 172.

4. For a discussion of why the press repeats defeatist arguments, see Lee Feinstein, "What's in a Number?" and George Perkovich, "Conventional Balance Coverage: Reporters on the Record," both in *Deadline*, Vol. 2, No. 1 (May/June 1987), pp. 1–2, 8–9.

media have occasionally reported the views of those who argue that NATO is not hopelessly outnumbered, but such voices have not been quoted often, and their claims have not had much impact on public perceptions. This is why the conventional wisdom about the European balance has long been that NATO stands little chance of standing up to a Soviet attack.

Analysts like me have challenged defeatists to provide analysis to back up their pessimistic claims. Instead, Eliot Cohen has issued a general attack on the qualified optimists in his "Toward Better Net Assessment: Rethinking the European Conventional Balance" in *International Security*, Vol. 13, No. 1 (Summer 1988), pp. 50–89. Cohen indicates his general agreement with the defeatist view, but does not offer his own assessment of the balance; instead he provides a wide range of criticisms of others' work. He focuses his fire on a group that he calls *optimists*, a category in which he includes Barry Posen and myself.

This letter responds to Cohen's criticisms of my work, none of which have merit. These criticisms are of four varieties. First, by arguing that individuals working with unclassified data simply cannot provide useful balance assessments, he challenges the feasibility of the enterprise I have attempted. Second, he constructs and attacks straw-man caricatures of my views, thereby confusing the issues in dispute. Third, he advances a wide variety of implausible war scenarios in an effort to show that NATO might be beaten in ways unforeseen by me. Finally, he tries to discredit the data that underpin my work. I dispose of each category of criticism below, although due to space limitations my remarks are synoptic.

CHALLENGING THE FEASIBILITY OF UNCLASSIFIED ASSESSMENT

Cohen makes two claims to support his argument that individual analysts will be unable to produce useful unclassified net assessments. First, he suggests that a conventional war would be decided by a great many factors, and that almost all of them must be examined in detail when doing net assessment (Cohen, pp. 54, 81, 84–85, and examples on pp. 65–66, 71–72). Therefore, a vast number of scenarios for war in Europe must be considered before reaching conclusions. He concludes that a team of analysts, not an individual, is best suited for such work, which by its very nature must fill several volumes. Second, Cohen argues that unclassified information is too sparse or unreliable to support open-source net assessment (Cohen, pp. 58–60, 85). Trustworthy classified information is available, however, so first-rate net assessment must be classified.

If Cohen's approach were adopted, net assessment would indeed be extremely difficult. In fact, his advice is a recipe for analytic paralysis. However, analysts need not consider everything before they can say anything useful. Many factors that Cohen would have us consider would have little effect on the outcome of a NATO-Pact conflict, and many of Cohen's war scenarios lack plausibility, as discussed below. In fact, only a handful of scenarios for war in Europe merit serious attention. Furthermore, it is sound analytic technique to make simplifying assumptions about important factors or scenarios, as long as the analyst makes reasonable assumptions, explains that they are not statements of fact, and clarifies their effects on the analysis. Simplifying assumptions often help to illuminate key issues, and are standard tools of scientific inquiry.

Cohen's second claim is equally unpersuasive. It assumes important discrepancies between the classified and unclassified data bases on the European balance, but this assumption is dubious. First, experts with and without access to classified material often talk to each other, and knowledgeable "insiders" rarely make the argument that the data bases conflict. Second, many so-called optimists have had security clearances, and would know of such a problem—if it existed. Third, so much classified material is leaked that it seems unlikely that the classified and unclassified data bases differ in important respects. Finally, if Cohen's claim were true, there is reason to believe that much of the unknown data would buttress the optimists' case. Because of budget battle politics, data emphasizing Soviet strength leak more freely than data indicating Soviet weakness, so secret data would tend to favor the optimists' view more than publicly available data.

CONSTRUCTING AND ATTACKING STRAW MEN

Cohen opens his article by portraying the debate about the conventional balance as an argument between defeatists and what he calls optimists (Cohen, pp. 50–51). The defeatists are described as "senior statesmen and military commanders," while the optimists are identified as "academic analysts." Cohen gives the impression that the optimists, as the label implies, have little doubt about NATO's prospects against a Pact offensive: they are Panglossians who believe that "NATO faces an acceptable conventional balance in Europe." We are told that the optimists' arguments have made little headway in government circles, but have "achieved a great deal of credence in the academic world." Thus Cohen implies that only dreamy academics

believe that NATO stands a good chance of defending successfully, while their optimistic views are rejected by responsible policymakers.

The real debate bears little resemblance to Cohen's caricature. There are important disagreements among experts, but the spectrum of views is much narrower than Cohen suggests. As noted above, all systematic studies of the balance fall somewhere within the range from qualified optimism to qualified pessimism. The Panglossian optimists that Cohen depicts simply do not exist. Nor do I know of any scholarly studies that support the defeatist view.

Cohen cites with approval General Bernard Rogers' often-stated claim that NATO would axiomatically be overrun by Soviet armies (Cohen, pp. 50, 82, 88). However, neither Rogers nor anyone else has provided empirical support for that assertion, and assertions based solely on authority carry little weight in serious discourse. Rogers' views lie outside the boundaries of the real debate about the balance; without analytical backing, they should be treated with much skepticism.

Furthermore, Cohen's description of a sharp and systematic difference in view between academics and government officials is incorrect. One could point to many examples of official expressions suggesting that NATO may well be able to defend successfully. In the mid-1970s, for example, Secretary of Defense James Schlesinger—relying on careful analysis—concluded that NATO had the wherewithal to thwart a Soviet offensive in Europe.⁵ The *New York Times* recently reported that the Joint Chiefs of Staff did a classified net assessment of the conventional balance in the summer of 1987 and concluded that "NATO has sufficient conventional strength to make a Soviet attack highly unlikely."⁶ NATO generals, including Frederick Kroesen and Glenn Otis, both of whom commanded American forces in Europe during the 1980s, have also argued that NATO is likely to do well in a conventional war with the Pact.⁷ Senator Carl Levin, a congressional student of conventional force

5. Joerg F. Baldauf, "Implementing Flexible Response: The US, Germany, and NATO's Conventional Forces" (Ph.D. dissertation, MIT, February 1987), chap. 2. This dissertation describes in detail the analysis on which Schlesinger relied.

6. Bernard E. Trainor, "U.S. Rates Non-Atom NATO Arms," *New York Times*, November 30, 1987, p. 4.

7. Kroesen, for example, remarked in 1982 that, "the quality of our Army is excellent. We have good, well prepared war plans, and our leaders are quite confident that we can defend West Germany—and they should be." See "GI's in Europe: 'Good Now, Going to Get Better'," *U.S. News and World Report*, August 9, 1982, p. 23. Also see Charles D. Odorizzi and Benjamin F. Schemmer, "An Exclusive AFJ Interview with: General Glenn K. Otis, Commander-in-Chief of the U.S. Army in Europe and Commander of NATO's Central Army Group," *Armed Forces Journal International*, January 1987, pp. 44–47.

matters, recently released a report on the balance that concludes that, “the potential for Warsaw Pact failure is real, and the risks associated with such failure are great indeed.”⁸

In short, the views expressed by John Mearsheimer, Barry Posen, and other qualified optimists are not confined to the world of ivory towers and are not sharply at odds with the conclusions reached by other students of the conventional balance. On the contrary, it is the defeatists’ assertions that lie outside the boundaries of serious analytical discourse.

Cohen’s second straw man is embodied in his claim that he is a true Clausewitzian, while the qualified optimists are mere systems analysts (Cohen, pp. 53, 88).⁹ This argument has two components. First, he maintains that unlike him, I “reject the Clausewitzian notion that ‘war is not waged against an abstract enemy, but against a real one who must always be kept in mind’” (Cohen, pp. 82, 86). Specifically, he charges that I do not pay serious attention to Soviet military capabilities and doctrine, and thus provide a disparaging and occasionally inaccurate assessment of the Soviet threat.

This charge mischaracterizes my work. In fact I assume the Soviets to be a formidable adversary, and in my writings I pay close heed to Soviet military doctrine. But of course, I do not accept that the Soviets can do everything that they claim they can do, or everything their doctrine requires. Having familiarized themselves with Soviet doctrine, analysts should form an independent view of Soviet capabilities, and should not let Soviet boasts define Western estimates of Soviet strength, as many defeatists do.

Second, Cohen claims that my work largely ignores politics, contrary to Clausewitz’s dictum that war is an extension of politics by other means (Cohen, pp. 54–58). Specifically, he complains that I pay mere lip service to the largely political question of whether NATO will mobilize promptly in a crisis. My principal sin is to assume that NATO will mobilize in timely fashion. Moreover, he asserts that I incorrectly assume that NATO would operate with the same unity and purpose in wartime as would the Pact. In fact, Cohen argues, NATO is likely to split apart because it is a coalition of independent states, not a command alliance like the Pact. The Soviets could probably cause defections by bribing, bullying, or cajoling NATO members.

8. Levin, *Beyond the Bean Count* (2nd ed.), p. iii.

9. Also see Eliot A. Cohen, “Guessing Game: A Reappraisal of Systems Analysis,” in Samuel P. Huntington, ed., *The Strategic Imperative* (Cambridge, Mass.: Ballinger, 1982), pp. 111–136, which provides an elaboration of Cohen’s views on systems analysis.

Cohen's charge that I ignore such political considerations is simply wrong. I have often emphasized the vital importance of prompt NATO mobilization in a crisis. Specifically, I have written that if NATO mobilizes promptly, it is likely to thwart a Pact offensive; if not, military defeat is almost certain; therefore prompt mobilization is essential.¹⁰ My 1982 analysis does assume a prompt NATO mobilization (lagging the Pact's by six days or less), but it is legitimate to adopt this assumption and then focus on whether NATO can defend successfully under such circumstances.¹¹ Such a research design does not denigrate the importance of the political issue of mobilization, nor is it tantamount to saying NATO will mobilize in a crisis. It simply allows us to assess NATO's prospects if NATO leaders give prompt mobilization orders, while also recognizing the importance of their doing so.

It seems reasonable to assume that NATO unity in crisis and wartime would equal that of the Pact. Each alliance has structural problems that could reduce its cohesiveness, but each side's problems offset one another; they may, in fact, weigh in NATO's favor. The Pact operates by Soviet command, but suffers greater internal conflict than NATO. NATO does not operate by command, but its members have greater common interest. Thus NATO has no leader that can enforce unity, but its members are more inclined to act in concert without being coerced. NATO also derives political advantage from being the defender. This advantage causes balancing dynamics—the tendency of threatened states to coalesce against the aggressor—to strengthen NATO cohesion, while the same dynamic would not operate within the Pact. Balancing dynamics should also frustrate Soviet attempts to bully NATO members into defecting: external intimidation would tend to increase, not diminish, NATO cohesiveness.¹²

It is fashionable these days to wrap one's arguments in the mantle of Clausewitz, much the way politicians invoke the mantle of patriotism as evidence of their qualification for public office. Clausewitz's work is very

10. John J. Mearsheimer, "Why the Soviets Can't Win Quickly in Central Europe," *International Security*, Vol. 7, No. 1 (Summer 1982), pp. 38–39.

11. *Ibid.*, pp. 8–9.

12. The theoretical literature and the historical record show that when states are threatened by an aggressor, they usually do not bandwagon with their foe, but balance against it. See Stephen M. Walt, *The Origins of Alliances* (Ithaca: Cornell University Press, 1987). Also on bandwagoning and balancing, see Kenneth Waltz, *Theory of International Politics* (Reading, Mass.: Addison-Wesley, 1979), pp. 123–128. For a discussion of how Soviet attempts to coerce NATO during the Cold War have failed, see Jonathan Dean, *Watershed in Europe: Dismantling the East-West Military Confrontation* (Lexington, Mass.: Lexington Books, 1987), pp. 82–85.

wise, but claims of exclusive intellectual lineage from Clausewitz should be viewed with great skepticism.

ADVANCING UNLIKELY SCENARIOS

Cohen devotes considerable space to describing seven different scenarios for a NATO defeat that he claims are likely but which I purportedly ignore. Of these, two focus on political considerations, four involve varieties of surprise, and one posits a view that differs from mine of the forces and events that will decide the campaign. I have just addressed the two that involve political factors (NATO fails to mobilize in the crisis; NATO fragments in a war): the possibility that NATO might not mobilize promptly in a crisis is a potentially serious problem, and is not one I ignore, while NATO fragmentation seems at least as unlikely as Pact fragmentation.

In my past work I devoted little space to the remaining five scenarios because I believe they are highly implausible. The first scenario posits that an unsuspecting NATO falls victim to a surprise attack (Cohen, pp. 58–61). This scenario has two variants, both implicit in Cohen's article. In one, the Pact suddenly launches a bolt-from-the-blue attack against NATO without first mobilizing its forces. With the second variant, the Pact mobilizes its forces for a massive offensive, but NATO fails to detect these developments. However, the bolt from the blue is highly unlikely because the Pact must know that NATO has the wherewithal to deny the Pact a decisive victory in this scenario. Specifically, NATO would outnumber the Pact by 28:24 in divisions available for a bolt from the blue, and by a larger margin in armored division equivalents (ADEs). (More on this ratio at p. 142 below.) A scenario involving an undetected Pact mobilization also seems very unlikely, because NATO has reliable means to detect such a mobilization promptly. Abundant professional opinion supports this judgment.¹³

In the second scenario, NATO is surprised and beaten by Soviet employment of unconventional weapons—chemicals and blinding lasers, for example—or unconventional forces like *Spetsnaz* (Cohen, p. 66). This is a common argument of defeatists: new wonder weapons or special operations forces

13. Richard K. Betts, *Surprise Attack: Lessons for Defense Planning* (Washington, D.C.: Brookings, 1982), the most comprehensive study of the surprise attack problem and NATO, is revealing on this point. Betts pays little attention to either variant of this first scenario and concentrates on the question of whether NATO will mobilize promptly when it has some indication that the Pact is mobilizing. Also see *Soviet Readiness for War*, an important study that throws cold water on the surprise attack scenario.

will provide the key to a Pact victory. However, they do not advance serious evidence to support the claim that these weapons or forces could have a significant impact on the course of a campaign, and the *prima facie* case that they could do so is exceedingly weak. Analysts cannot be asked to refute the plausibility of scenarios that their advocates have barely described. Therefore, the burden of proof on this issue should fall on Cohen, not me, until more evidence for his scenario is provided.

Cohen's third scenario has NATO surprised by a Soviet attack through Austria and Denmark, leading to a giant encirclement of NATO's forces on the Central Front (Cohen, p. 65). Analysts of the conventional balance rarely discuss this scenario because it is so unattractive to the Soviets. Neutral Austria has an army and might very well use it to contest the invading Soviet forces. Moreover, the terrain in Austria is covered with some formidable obstacles that would facilitate the defender's task. However, even if the Soviet forces met no resistance from the Austrians, they would still have to cross the Austrian-German border, parts of which are covered by very rugged terrain, and the remainder of which would be defended by the formidable German II Corps. Furthermore, even a successful Pact advance into southeastern Germany would not constitute a grave threat to NATO, since the advancing Soviet forces would still be hundreds of kilometers from NATO's main body of forces. Denmark is an even less attractive route into Germany. Since Denmark has no land border with any Warsaw Pact country, the Soviets would have to land amphibious forces on the Danish coast, move them across Denmark, and then strike down through Schleswig-Holstein into the heart of West Germany. NATO has capable naval assets in the Baltic Sea; hence a large-scale Soviet amphibious operation against Denmark is likely to end in disaster. If the Soviets were able to land in Denmark, they would surely meet some resistance from the Danes, not to mention the Germans, who would be lying in wait for the invasion of Schleswig-Holstein. NATO commanders should hope that in the event of war the Soviets pursue such an impractical scheme.

Cohen's fourth scenario challenges my assumption that Germany's terrain would confine the Soviets to a handful of confined axes of attack—the North German Plain, the Göttingen Corridor, the Fulda Gap, and possibly a few less attractive avenues around the obstacles that litter the intra-German and Czech-German borders (Cohen, p. 64). Cohen argues that history often shows tanks going where they are not supposed to go, and suggests that

the Pact could locate additional axes of advance even in areas with obstacle-littered terrain.

My assumption that possible axes are limited in number is widely shared by other analysts. It is clear to me and to these other analysts that large portions of this terrain are covered by formidable obstacles—mountains, forests, marshes, rivers, and urban sprawl—that would greatly impede the movement of large armored forces. Cohen therefore owes his audience specific evidence about the terrain in West Germany that supports the opposite view. Yet he offers no such evidence, instead relying on dubious historical analogies that tell us little about the case at hand. His argument cannot be given much credence until he provides more support.

In Cohen's final scenario, the campaign is decided by Pact operations that take place outside the breakthrough battle area, or by Pact forces located outside the breakthrough battle area that are brought to bear on the breakthrough battle (Cohen, pp. 66–72). In other words, events or forces exogenous to the breakthrough area are decisive. Specifically, Cohen complains that I miss three important exogenous factors: the impact of air power on the breakthrough battle; the Pact's ability to mass artillery in sectors adjoining a breakthrough sector and fire into the breakthrough sector from its flanks; and the deep operations the Soviets would conduct in NATO's rear to paralyze NATO. On the latter point, Cohen argues that I fail to understand that a war in Europe is likely to begin with a confused and turbulent battle occurring throughout the depths of the Western position in Europe.

Cohen is correct when he says that I downplay such exogenous factors. I believe that the opening stages of a conventional war would be decided mainly by the performance of the big battalions that would meet head-on in the crucial breakthrough sectors. Regarding Cohen's specific charges, the fact that I do not factor in air power does not bias my assessment in favor of NATO, because it is widely recognized that the air war would work to the Pact's disadvantage. The Soviets can certainly direct artillery fire from neighboring sectors into the breakthrough sector, but so can NATO, and there is no reason why this tactic should favor the Soviets. In fact, NATO's marked advantage in command and control at the tactical level, coupled with the fact that orchestrating an offensive is usually more demanding than coordinating a defensive, should make it easier for NATO to benefit from this tactic.

Assuming no breakthroughs, there can only be battles of strategic significance in NATO's rear if the Soviets can insert and sustain major fighting

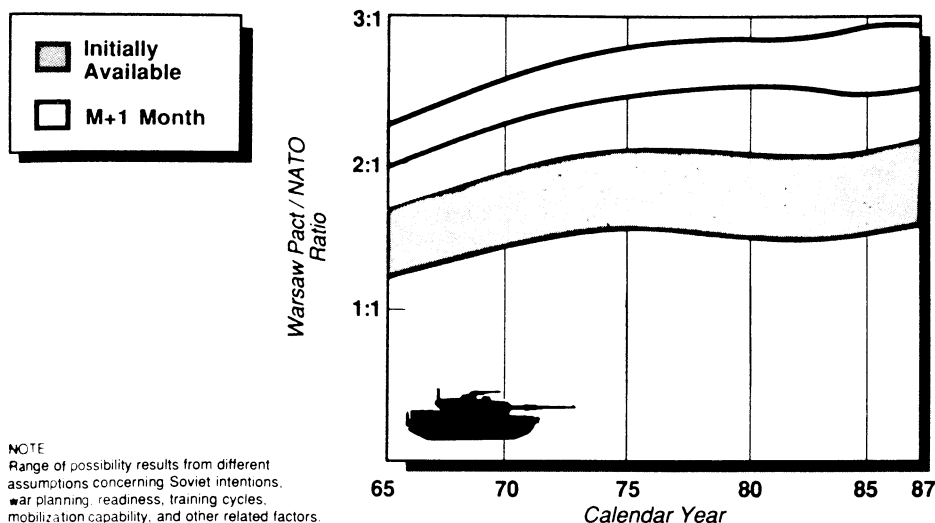
forces in NATO's rear. A handful of *Spetsnaz* or heliborne units operating in rear areas simply cannot have much effect on the outcome of a European war. NATO's rear area infrastructure is not so fragile that small attacks could bring it down, and a few Soviet intruders would not last long behind NATO's lines. No doubt the Soviets would like to insert operational maneuver groups (OMGs) and large numbers of air assault brigades behind NATO's forward deployed forces, but no one has explained how the Soviets could do this in the face of NATO's air and ground defenses. Moreover, if such an operation were plausible, NATO would surely consider mounting a similar operation—for example, dropping the 82nd Airborne and 101st Airmobile Divisions into the Soviets' rear. However, no serious analyst advocates such a scheme, a tell-tale indicator of its impracticality. Yet Cohen asserts as an article of faith that the Soviets can place large units in NATO's rear areas. Again, we can hardly credit his argument without seeing more evidence to support it.

DISCREDITING DATA

Cohen's indictment of my data contains six key charges. First, he correctly notes that my 1982 and 1988 articles¹⁴ state that the balance of standing forces in Central Europe favored the Pact by 1.2:1 in ADEs and that the original source of that ratio was a 1976 Department of Defense (DoD) study (Cohen, pp. 72–73). He maintains, however, that the conventional balance worsened for NATO after 1976, that the 1.2:1 ratio was badly outdated by 1982, and that it remained so through 1988. His key piece of evidence is a chart found in the *FY 1989 Annual Report of the Secretary of Defense*, which, Cohen writes, shows the balance of standing forces in 1987 ranging somewhere between 1.5:1 and 2:1 in the Pact's favor, a far cry from my 1.2:1 ratio (see Figure 1). If true, this charge would seriously undermine my analyses.

This charge is unfounded, however, for two reasons. First, the *Annual Report* chart cannot be used to challenge the 1.2:1 ADE ratio for 1982, 1988, or any other year, because that chart does not express the Pact-NATO ratio in ADEs, but rather in some other units of measure. The DoD does not identify what these units are, but we know they are not ADEs because the chart shows a Pact-NATO ratio of between 1.4:1 and 2:1 in 1976, which differs sharply from the 1.2:1 figure that the DoD has used for the 1976 ADE

14. Mearsheimer, "Why the Soviets Can't Win Quickly"; and Mearsheimer, "Numbers, Strategy, and the European Balance," *International Security*, Vol. 12, No. 4 (Spring 1988), pp. 174–185.

Figure 1. Chart from DoD 1989 *Annual Report*.

SOURCE: FY 1989 *Annual Report* of the Secretary of Defense, Chart I.B.2, "Ratio of Warsaw Pact to NATO Ground Forces in the WTVD, 1965-1987: Pre-Hostility Combat Potential," p. 31.

NOTE: The relevant information is depicted by the lower of the two bands in the chart, marked "Initially Available," which represents the standing forces in Europe. Cohen uses this chart to challenge my assertion that the force ratio in Europe favored the Pact by 1.2:1 in ADEs in 1982 and 1988. He makes two arguments: (1) the chart shows a Pact advantage of greater than 1.2:1 during the 1980s, and (2) there has been an adverse shift in the force ratio since 1976, the year in which DoD estimated the ratio at 1.2:1. However, the chart is not denominated in ADEs; this is revealed by the force ratio that the chart depicts for 1976, which differs from the 1.2:1 ADE ratio calculated by the DoD for 1976. Moreover, the chart's authors have not explained in what units the forces are being measured, nor have they defined what forces are included and excluded from their count. Hence the chart is not a useful guide to absolute force ratios. In any case, the chart shows no significant adverse shift in the force ratio since 1976.

ratio.¹⁵ Second, the *Annual Report* chart contradicts Cohen's claim that the balance of standing forces has shifted against NATO since 1976. In fact, it shows that for NATO the balance actually improved slightly between 1976 and 1982, and thereafter deteriorated slightly, so that by 1987, the last year

15. Conversations with DoD officials have also verified that this chart is not denominated in ADEs. It is also important to note that the *Annual Report* chart might not count exactly the same units that were counted in the 1976 figure.

covered in the chart, the balance stood roughly where it did in 1976. Thus, Cohen's own chosen evidence supports my case, rather than his own.

Cohen's second charge concerns the Soviets' ability to concentrate forces on a breakthrough sector (Cohen, pp. 65, 67, 69–71). He maintains that I badly underestimate the Soviets' ability to mass forces along a narrow frontage. Where I suggest that the Soviets could concentrate only one division per 15 km of frontage, he argues that they could locate one division on a 4–5 km breakthrough sector. He goes so far as to suggest that the Soviets could place an entire Front (a force that would normally comprise about 2–3 armies, each with 2–3 divisions in forward positions) on a single 20 km frontage.

Cohen's thinking about force concentrations is based on a misreading of the literature that both he and I cite on this matter. He has overlooked the distinction—made in virtually all the sources on this subject—between a *sector* frontage, which is the entire length of frontage assigned to a particular unit, and an *attack* frontage, which is that part of the sector where the unit will concentrate some portion of its force for the main attack.¹⁶ As a result, he then misread the Soviets to claim that they will concentrate all of a given unit's forces on a narrow attack frontage, while the Soviets are actually saying they will place all of that unit's forces along the much wider sector frontage.

An example illuminates Cohen's error. An attacking Soviet division is usually assigned a 15 km sector of frontage, but would launch its main attack on about 4–5 km of frontage. The division would not concentrate all of its forces on the 4–5 km attack frontage, because it must cover the remaining 10–11 km of its sector, from which it would launch secondary attacks. But Cohen misreads the literature on attack frontages to say that the Soviets would assign an attacking division a *sector* frontage of 4–5 km, not 15 km. This error allows him to pack many more Soviet divisions along the forward edge of a Soviet main axis of attack than others do, including the Soviets themselves. For example, whereas the U.S. Army, Posen, and I assign about three Soviet divisions to spearhead an attack along a 45–50 km-wide main axis, Cohen's calculations allow him to place as many as twelve Soviet divisions along such a frontage.¹⁷

16. David Isby, for example, writes, "each Soviet unit is assigned a sector to attack or defend. In addition, each unit of division size or larger will have a main and secondary axis of advance *within* this sector." Isby, *Weapons and Tactics of the Soviet Army*, rev. ed. (London: Jane's, 1988), p. 24 (emphasis added). *Sector* frontage and *attack* frontage are my terms; other analysts use different terms to distinguish these concepts.

17. Concerning the U.S. Army's view on this matter, see U.S. Congress, Senate Armed Services

The same mistake allowed Cohen to reach the startling conclusion that the Soviets could place an entire Front on 20 km of frontage. In the passage from the source Cohen used to draw this conclusion, the author writes: "The Front's entire zone of responsibility may be from 300–400 km wide—whereas the strike groupings penetration sector would be no narrower than 20 to 25 km—and might be as wide as 50 km."¹⁸ Cohen misread this to mean that the entire Front would be capable of striking along a 20 km frontage.¹⁹

Cohen's third data-related charge concerns my argument that NATO divisions would outnumber Warsaw Pact divisions by 28:24 in a bolt-from-the-blue surprise attack scenario, and that NATO therefore has the wherewithal to thwart such an attack (Cohen, p. 74). Cohen claims that I exaggerate available NATO forces and underestimate Pact forces. He maintains that in addition to the 24 Soviet divisions stationed in East Germany and Czechoslovakia, I should have included East Germany's five divisions, whose peacetime readiness, he says, equals that of Soviet divisions, and Soviet divisions located in Hungary. He also maintains that I should have excluded French divisions from my NATO total, and should not have counted those NATO forces (Danish and Dutch divisions) that would require substantial reserve call-ups to reach nominal strength. Thus, he concludes my 28:24 ratio is unrealistically favorable to NATO.

In the surprise attack scenario under discussion, the Pact could not use forces that when alerted would produce a security breach, or forces that would need to be moved long distances to join the battle. Otherwise the Pact would lose its element of surprise because NATO would detect Pact alert measures or force movements. These factors do not constrain NATO to an equal extent, because hastily prepared divisions are more effective in defense

Committee, *Hearings on Department of Defense Authorization for Appropriations for Fiscal Year 1981* (Part 5), 96th Cong., 2nd sess. (Washington, D.C.: U.S. Government Printing Office [U.S. GPO], 1980), pp. 3053–3078; and Headquarters, Department of the Army, *FM 100-2-1: The Soviet Army: Operations and Tactics* (Washington, D.C.: U.S. Department of the Army, July 16, 1984), pp. 4-4-9, 5-18-5-22.

18. John G. Hines, "Soviet Front Operations in Europe—Planning for Encirclement," in *Spotlight on the Soviet Union*, Report No. 1/86 of the Alumni Association of the Norwegian Defence College (Oslo, 1986), p. 86. For further elaboration, see *FM 100-2-1*, pp. 4-4-4-5.

19. Cohen implies that my sources on this subject are outdated and that more recent literature allows for greater Soviet concentration on narrower frontages. Cohen, p. 70, notes 61–62. In fact, it is apparent from the more recent literature that the Soviets now require more frontage per unit than they did in the late 1970s. See Barry R. Posen, "Correspondence: Reassessing Net Assessment," *International Security*, Vol. 13, No. 4 (Spring 1989), pp. 140–160, at 150–152. This is not surprising since Soviet fighting units have grown in size over the past decade.

than in attack (attack requires more preparation), and because the battle comes to the defender, so units far behind the line need not travel as far to join the fight. Hence NATO, as the defender, is not equally confined in the units available to it. This is an important reason why the Pact cannot gain a decisive advantage in surprise attack scenarios, and why it does better in scenarios that include a period of mutual mobilization.

Four specific points bear mention regarding Cohen's charges concerning my 28:24 ratio. First, he ignores part of my rationale for excluding non-Soviet forces. I specified that not only are they "three-quarters or less manned," but that "alerting them of a forthcoming offensive might lead to a security breach."²⁰ Second, the Soviet divisions in Hungary would have to traverse about 250 km of Austria to reach an obstacle-littered region of Germany where it would be difficult to influence the war's course significantly. Hence these forces are too far away to be very useful in the early stages of an offensive with fully mobilized forces, much less a bolt-from-the-blue attack. Third, it is perfectly reasonable to count French forces stationed in West Germany as part of NATO's total, and many analysts do so. They are located close enough to join the battle, and their government is committed to fight with NATO. There is no reason to exclude them. Fourth, Cohen is correct that the small number of Dutch and Danish divisions are not maintained at as high a level of peacetime readiness as American and West German divisions. However, they are sufficiently ready to deliver substantial combat power. Moreover, if we are to draw fine distinctions in unit readiness, it bears mention that Soviet divisions are less ready than American and German divisions. In evaluating a standing start scenario, however, I have not drawn such distinctions, nor translated divisions into ADEs, because a fine-grained analysis is not necessary to demonstrate that the Soviets would not enjoy a significant force advantage in that scenario.

With his fourth data-related charge, Cohen argues that I misapply the 3:1 rule of thumb by using it to analyze breakthrough attempts against NATO corps, when the rule, according to Cohen, applies only to operations involving battalions and brigades (Cohen, p. 77). This point, he says, is made clearly in one of my key sources: the 1976 version of *FM 100-5*, the U.S.

20. Mearsheimer, "Why the Soviets Can't Win Quickly," p. 5. This is a common assumption. Betts, for example, writes: "Chances are small that the Soviet Union could achieve absolute surprise, unless it chose to strike alone and without reinforcement." Betts, *Surprise Attack*, p. 207; see also p. 181.

Army's principal field manual. In fact, however, that field manual, which "is intended for use by commanders and trainers at *all* echelons," says nothing about restricting the rule's application to battalion and brigade level.²¹ The 3:1 rule simply applies to forces directly engaged in a breakthrough battle.²² There is no fixed limit on the size of those forces. Thus, the rule can apply to corps versus corps engagements as well as to corps versus battalion or battalion versus battalion engagements.

Cohen's fifth charge is that the widely used ADE measure is out of date and is a poor indicator of relative combat power (Cohen, p. 79). In fact, the ADE remains the best publicly available measure of combat power. This judgment has been shared by the Department of Defense, and is reflected in the DoD's longstanding use of the ADE measure. Although the basic methodology was developed in the 1970s, analysts have kept the measure current by incorporating new information into their later ADE estimates. Moreover, there is no evidence that use of ADEs biases the analysis in NATO's favor.

Finally, Cohen maintains that I mistakenly argued in my 1982 article that if NATO and Pact non-divisional assets were added to their divisional assets on the Central Front, the balance would shift in NATO's favor (Cohen, pp. 74–75). It is widely recognized that the Pact places a greater percentage of its total manpower directly into its divisions than does NATO, while NATO invests relatively more in non-divisional support forces. For example, an important Pentagon study estimates that after 30 days of mobilization, the Pact would have an advantage in divisional manpower of 943,000 to 821,000, while NATO would have an advantage in non-divisional manpower of 963,000 to 632,000.²³ This basic picture also obtains both before mobilization and after lengthier periods of mobilization. Thus NATO, not the Pact, appears stronger when non-divisional assets are included in the analysis. Cohen does not cite or offer his own systematic analysis that challenges this point. Instead, he lists some Soviet units for which he claims no NATO analogue exists and then simply asserts that the Warsaw Pact has more non-divisional

21. *FM 100-5: Operations* (Washington, D.C.: U.S. Department of the Army, July 1, 1976), p. i (emphasis added). Discussion of the 3:1 rule is found on pp. 3–4, 3–5, 5–3.

22. See John J. Mearsheimer, "Assessing the Conventional Balance: The 3:1 Rule and Its Critics," *International Security*, Vol. 13, No. 4 (Spring 1989), pp. 54–89.

23. *NATO Center Region Military Balance Study, 1978–1984*, p. I–24. This study was written in 1979, and these were projected figures for 1984. This study is illuminating on the subject of non-divisional assets. See especially Appendixes D and E. Also see Mearsheimer, "Why the Soviets Can't Win Quickly," p. 7.

assets than NATO. It is impossible to give this argument much credence without support.

CONCLUSION

A well-fought scholarly debate between the qualified optimists/pessimists and the defeatists would profit the security affairs field. However, Cohen has not gotten that debate off to a good start. It should have begun with a full explication of the defeatists' view of the balance. Instead Cohen has filled the air with ungrounded charges and left the case for his own view of the balance unmade.

Such a debate would have important policy ramifications. Probably the most important danger that NATO faces at the conventional level is that it might not mobilize promptly in a crisis. Such a decision would be influenced by many factors, but one crucial consideration would be how the relevant policymakers view the balance of military forces. I see some danger that defeatists like Cohen will convince policymakers that there is no hope for NATO on the battlefield and therefore not much point in worrying about mobilization. In fact, NATO leaders should be deeply concerned about mobilization, because NATO does have a formidable military posture if its forces are mobilized promptly in a crisis; the decision to mobilize could decide the war—or, still better, could deter the war if Soviet leaders recognize the defensive power of mobilized NATO forces. In short, the pessimism of defeatists like Eliot Cohen about NATO's capabilities may work to undermine the political resolve that he fears NATO would lack in a crisis.

—John J. Mearsheimer
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To the Editors:

In "Rethinking the European Conventional Balance,"¹ Eliot Cohen levels a number of general criticisms against a group of defense analysts whom he terms the "optimists,"

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1. Eliot A. Cohen, "Toward Better Net Assessment: Rethinking the European Conventional Balance," *International Security*, Volume 13, No. 1 (Summer 1988), pp. 50–89. Subsequent references to this article are in parentheses in the text.

and directs certain specific criticisms at my own work. In doing so, he commits three errors: he misunderstands and mischaracterizes the nature of scholarship in the security studies field, the military analytic activity, and the associated activity of military modeling; he misrepresents certain aspects of my own work; and he invites the readers of *International Security* to join him in four analytic shortcuts that obfuscate the debate on conventional forces in the West.

Cohen has leveled many criticisms, but I will deal here only with those aspects of Cohen's work that have the greatest potential to confuse or mislead the *International Security* readership. In future work on this subject I will undoubtedly address many others.

ANALYSIS, MODELING, AND THE SCHOLARLY DEBATE

Several of Cohen's criticisms rest on the general proposition that the only proper way to assess the relative competitiveness of NATO and the Warsaw Pact in a conventional war is to do a detailed analysis of all the factors that might contribute to the outbreak and conduct of the war: political, economic, military, and probably even social (pp. 54–58, 82–84, 85, 87). At best the magnitude of such a task leads to paralysis; at worst it leads to endless opportunities for the introduction of the $n+1$ assumption required to produce whatever conclusion the assessor hopes to reach.

Of course, nobody would dispute the proposition that there are many factors that are not strictly military that would influence both the outbreak and conduct of a superpower war. But this does not mean that nothing sensible or useful can be said by breaking the problem down into its component parts. Scholarship on national security matters is, like all scholarship, a collective enterprise. This is the reason that we have professional journals such as *International Security*. For any number of reasons, individuals choose to focus their own work on particular questions. They publish their findings; this invites criticism from their colleagues, provides foundations and inputs for the work of others, and offers raw material for university courses. By this method the community as a whole advances our understanding of specific methodological and substantive issues and of the field as a whole.

Ten years ago there was almost nothing to assign to students in security studies courses on the conventional balance in Europe or on how to go about assessing it. Today we have a number of studies on both questions, and a lively debate. The field moves forward in spite of the fact that no single study has offered or purported to offer an entire political-military-economic-

social analysis of a possible East-West conventional war in Central Europe. Some of these issues are certainly worthy of scholarly attention. The profession will no doubt deal with them over time. But there is no reason to rush since, regardless of our official capacities, when we operate in the open source academic and professional literature, we do not face the arbitrary deadlines imposed by bureaucratic rhythms or contractor sponsors. Indeed, it would be wrong for us to behave as if we did. And it is simply unnecessary to deal with all these issues simultaneously in order to make meaningful contributions to the debate on the NATO-Pact military competition.

Analysis is about dividing problems into their component parts to permit focused, specialized attention on the parts. It is analogous to the division of labor in modern society. In the world of military affairs, "optimists" are not the only people who hold political variables constant to permit focused attention on military questions. As Cohen intimates, his attack on those who do this is not merely directed at academics such as John Mearsheimer and myself, but effectively at the entire analytic community, most of which is employed by the Department of Defense or the Armed Services (Cohen, p. 53 n. 10). The recently declassified "Center Region Balance Study" of the Pentagon Office of Program Analysis and Evaluation and the RAND Corporation's "Conventional Arms Control Revisited" both hold political variables constant to permit military analysis.²

Two examples demonstrate the flaws in Cohen's critique of my decision not to consider all factors at once in my analysis, as well as some of the pitfalls of his own preferred approach.

First, Cohen judges that no assessment that excludes detailed consideration of the political circumstances of a war can tell us much useful about NATO's military situation (pp. 54–58). But the question that should be asked of any analysis of the European conventional balance that purports to be of general significance is: Has it set a *demanding* political case? Both NATO and the Warsaw Pact face plausible alliance cohesion problems. Since Cohen concedes this, he ought to agree that my assumption (and that of many others who have done this kind of work), that all members of both alliances participate willingly in the war, is not naively favorable to our side. Indeed when one

2. Office of the Assistant Secretary of Defense for Program Analysis and Evaluation, "NATO Center Region Military Balance Study: 1978–1984," July 13, 1979, declassified July 13, 1985; James A. Thomson and Nanette C. Gantz, "Conventional Arms Control Revisited: Objectives in the New Phase," RAND N-2697-AF (Santa Monica, Calif.: RAND, 1987).

recalls the historical record of political unrest and violent repression that has prevailed in the Soviet empire, a record Cohen ignores, this assumption is conservative. To deal with the often-expressed concern that NATO will vacillate in the face of ambiguous warning, I make the assumption that NATO's mobilization decision lags that of the Pact by seven days. Given that NATO planners now appear to assume just a four-day lag for planning purposes, my assumption again is conservative.

Second, Cohen asserts that my military analysis is flawed because it omits many details about Soviet organization, tactics, and doctrine (p. 71). Instead he should ask if my analysis captures an important, central aspect of the plausible Soviet war effort. The modeling of military engagements is essentially a theoretical activity. It is *about* generalization and simplification. It tries to abstract from the "fog of war" some of the variables that seem to be most important to the issue at hand, and to capture their interrelationships. This is particularly true of simple models of the kind that I employ. (Models are to be distinguished from high-resolution simulations, which try to capture much detail, and war games, which try to capture chance, maneuver, skill, and even individual leadership qualities.) Models, like theories, should not be reified. They are not reality, but tools for its apprehension.

The Attrition-FEBA Expansion Model that I use portrays the basic armored warfare problem of breakthrough. Assuming that there are no places along a defender's front that are undefended or weakly held, an armored attacker must concentrate his forces in a few promising places to overwhelm the defender and break through the crust of the fighting forces. The attacker then tries to push his own forces through the hole before the defender can counter-attack. Once through, the attacker may attempt either an envelopment or to go deep to disrupt the enemy rear and destroy the cohesion of his force. The effort is mightily aided by multiple efforts and diversionary attacks elsewhere in the theater to fix as many of the defender's forces as possible, so that they cannot concentrate against a single breakthrough effort. This is the basic historical record for armored warfare wherever it has been fought, and this is how a war is likely to unfold in the Center Region. Breakthrough is the core of the Soviet technique; the model tests NATO's ability to thwart multiple Soviet breakthrough efforts.³

3. On the breakthrough battle see Barry R. Posen, "Measuring the European Conventional Balance: Coping With Complexity in Threat Assessment," *International Security*, Vol. 9, No. 3 (Winter 1984/85), p. 49. On the model see *ibid.*, pp. 54-59, and notes 11 and 12. See also Posen,

Of necessity, not everything about the adversary's effort can be captured by a simple model. Cohen complains that I ignore the initial Soviet air offensive against NATO air bases—"The Frontal Air Operation"—and its potential suppressive effects on NATO's Close Air Support (CAS) effort (pp. 65–67, 71). True. I also omit NATO's efforts to attack Pact airfields, interdict reinforcements, obstruct the commitment of additional echelons at the point of attack, and suppress Pact air defenses. Are these omissions favorable to NATO? No, for three reasons. First, enemy attacks on Main Operating Bases are unlikely to degrade significantly the operations of the NATO Close Air Support assets that I do count, since those assets are, for the most part, attack helicopters, which do not require airfields, and austere fighters such as the A-10 capable of (indeed, designed for) operation from fairly undeveloped airstrips. Second, no analyst who has discussed the Frontal Air Operation has shown that it has a high probability of success. Third, the Pentagon's own recent effort to measure the NATO and Pact air orders of battle in the Center Region does not show a raw capabilities ratio markedly in favor of the Pact; indeed in three out of four cases the ratio is close to 1:1, and in the worst case is not above 1.5:1 in favor of the Pact.⁴ This ignores the quality of pilots, training, weapons, and maintenance crews, all of which almost certainly favor the West. The essential question is: Have I held things out of the analysis that bias the results in favor of NATO? In my judgment, here and in other areas I have not; indeed, this particular assumption favors the East.

COHEN'S SPECIFIC CRITICISMS

In four particular instances Cohen criticizes my work directly. In a fifth he attacks it by implication. In all five, either he has misread his own sources, or mischaracterized my work.

First, Cohen challenges my estimates of the attrition that NATO and Pact aircraft might suffer, and the number of adversary armored fighting vehicles they might destroy, on an average close air support sortie (p. 67). He believes that 5 percent attrition per sortie is too low for NATO aircraft, half an armored vehicle kill per sortie too high, and two sorties per day too many. In response,

"Is NATO Decisively Outnumbered?" *International Security*, Vol. 12, No. 4 (Spring 1988), note 28.

4. U.S. Department of Defense (DoD), *Annual Report, FY 1989*, p. 32.

I would simply refer the readers to my development of those values. They are conservative. Five percent sustained attrition per sortie, accompanied by the low average kill rate of .5 vehicle per sortie, is far from wildly optimistic for NATO aircraft.⁵ Four to five percent attrition per sortie for attacking aircraft on a *sustained* basis is quite high by historical standards, given that I also assume low effectiveness consistent with a high degree of evasive maneuver and suppressive fire.⁶ Tactically, Western CAS aircraft operate below the minimum effective altitude of most Soviet air defense radars; the attack helicopter often operates in defilade, hovering low behind hills, trees, or buildings. Exposure to Pact air defense weaponry is thus reduced. Readers should understand that a U.S. A-10 fighter with a full load of ammunition carries 1,200 30mm rounds, or thirty “trigger squeezes,” plus chaff cartridges and flares to decoy SAMs, as well as bombs and missiles. A U.S. Cobra attack helicopter carries eight TOW missiles, plus a 20mm automatic cannon. These payloads provide ample weaponry for suppressive fire to degrade Pact air defenses and permit the kill rate per sortie I assume.⁷ Finally, two sorties

5. Lt. Col. D.J. Alberts, USAF, *Deterrence in the 1980s: Part II, The Role of Conventional Air Power*, Adelphi Paper No. 193 (London: International Institute for Strategic Studies [IISS], 1984), tests attrition rates of 2 percent, 5 percent, and 10 percent for NATO fighter aircraft in Close Air Support (CAS) missions, but assumes *one* tank destroyed per surviving aircraft per sortie (p. 57). He notes elsewhere that there is no guarantee that each sortie would destroy a tank, but notes that, “the technology is available, and is now being fielded, that would appear to guarantee on average at least such a kill rate per sortie overall in clear daylight or on clear nights” (p. 29). Given that tanks are the toughest armored vehicles to kill, my assumption of one-half an armored fighting vehicle killed per sortie, which includes tanks as well as softer armored personnel carriers, infantry fighting vehicles, and self-propelled artillery, is certainly conservative by Alberts’s standard.

6. For aircraft attrition figures see Posen, “Measuring,” p. 72 n. 36. U.S. strategic bombing attrition in World War II averaged out at 2 percent per sortie, as did U.S. Air Force attrition during the brief Christmas bombing campaign of Hanoi. The Argentine Air Force suffered nearly ten percent attrition per sortie in the Falklands War, but this proved unsustainable. Alberts, *Deterrence in the 1980s*, p. 50 n. 27. According to USAF General William Momyer, tactical fighters on attack sorties over Hanoi between 1965–68 rarely suffered more than 4 percent attrition. General William F. Momyer, *Air Power in Three Wars* (Washington D.C.: U.S. Government Printing Office [U.S. GPO], 1978), p. 119. One source reports that during the second and worst day of the 1973 war, Israeli attack sorties suffered 4.7 percent attrition, but that the average for the whole war was about 1.5 percent. Historical Evaluation and Research Service, *The Development of Soviet Air Defense Doctrine and Practice* (Dunn Loring, Va: T.N. Dupuy Associates, 1981), chap. 6, pp. 31–32. Other sources put Israeli Air Force (IAF) attrition for the whole war (some 20 days) at .8 percent; see Posen, “Measuring,” p. 72 n. 36. Cohen’s charge (n. 55) that my 5 percent attrition figure is taken from the experience of long campaigns is simply wrong. Indeed it is consistent with the “heavy losses” that he notes were taken by the IAF at the outset of the 1973 war, which he cites as an example that I should consider.

7. It should also be understood that many of the Pact ground-based air defense assets most

per day is probably fewer than these Western aircraft could actually fly; the A-10 in particular was built to be rugged and dependable. Lower effectiveness (and sortie rate) assumptions are fully warranted for Pact CAS aircraft and helicopters due to smaller Soviet resources committed to ground maintenance, higher vulnerability due to tactical limitations on current generations of Soviet attack helicopters (which affect weapon delivery), and the weaknesses of many Soviet air-to-ground armaments. Even higher attrition assumptions for Pact aircraft would be warranted, given their tactical limitations and the apparently excellent performance that the U.S.-manufactured Stinger air defense missile demonstrated in Afghanistan, but I hold the Pact to 5 percent as well, for the sake of conservatism.⁸

Second, Cohen criticizes my estimate of how many Pact forces are likely to be concentrated in a given piece of turf⁹ (pp. 69–70). I formed my judgment from a survey of Western literature on Soviet attack planning, from conver-

likely to be encountered by NATO CAS aircraft are within range of NATO artillery, and often even of NATO anti-armor weapons. Plans exist for these weapons to participate in NATO's integrated "suppression of enemy air defenses" campaign.

8. The Mi-24 Hind, the most numerous and feared Soviet attack helicopter, seems to have design limitations that severely reduce its ability to hover at low altitudes—a fundamental requirement for helicopter survivability on the modern battlefield. This may explain why they are often seen making high altitude attack runs. See comments of General Mallory, in Scott D. Dean and Benjamin Schemmer, "Warsaw Pact Success Would Hinge on Blitzkrieg, U.S. Army Observer Says," *Armed Forces Journal International*, November 1987, pp. 29–36. U.S. Joint Staff of the Joint Chiefs of Staff, *United States Military Posture FY 1989* (mimeo, n.d.), p. 70, notes that, "the United States has long recognized the effectiveness of helicopters to support antiarmor attack. In the [new attack helicopter called] HAVOC, Soviet designers have concentrated on providing a good low-speed, out-of-ground-effect fighting capability that has not been found on current generation Soviet helicopters." The HAVOC has yet to appear in Soviet forces in significant numbers.

9. The reasons for limits on the amount of force that is likely to be concentrated in a given sector of front are varied and complex. They include limits on road capacity for divisions that contain several thousand motor vehicles; the specific nature of the terrain to be fought over, which may include ground that is simply impassable for armor; the desire to site weapons where they can exploit the terrain for cover; the desire for "maneuver room"; the desire to have multiple positions to which artillery and SAMs can be relocated in the event that they come under sustained fire; and the desire not to concentrate too many targets in such a small space that the adversary's air and artillery attacks would become highly effective, achieving multiple kills per salvo. The influence of the latter two considerations varies with the quantity and quality of artillery and air power available to the adversary. In 1944 and 1945, high Soviet and German concentrations on the Eastern front were apparently affected little by these considerations. Attempted German concentrations on the Western front were affected a great deal. See Russell F. Weigley, *Eisenhower's Lieutenants* (Bloomington: Indiana University Press, 1981), pp. 166, 198, 338, 342. A modern armored or mechanized division takes up a great deal of space. For example, even if proceeding on three parallel routes, the main body of a 1978-vintage Soviet Motor Rifle Division on the march would stretch out over 60 km of road. U.S. Department of the Army, *Soviet Army Operations*, IAG-13-U-78, April 1978, p. 3-23.

sations with people in the intelligence and analytic community whose business it is to think about these things, and from examination of several historical cases.¹⁰ An exemplary diagram of Soviet divisional and army breakthrough efforts, drawn from a U.S. Army field manual (1977), is provided in my 1984 essay.¹¹ The diagram shows a Soviet tank army arrayed for a breakthrough operation, with two divisions forward—one mounting a major effort on a 15 km sector and the other mounting a secondary attack on a 20–30 km sector—and two divisions in the second echelon. (The *highest* concentration suggested by the diagram would thus put a frontline division of an army engaged in breakthrough operations on an average of 17.5 km.) I assume, in the cases that I count as favorable to NATO, a Pact army on 50 km of front, with .75 Armored Division Equivalent (ADE) per 12.5 km, or 3 ADEs engaged in total. Today this would total 3–3.5 first-class, fully modernized and manned Soviet tank or motor rifle divisions (on the average a little more than a division every 17 km).¹² The frontline Soviet concentration I assume in cases favorable to NATO is thus consistent with a conservative reading of the particular U.S. Army diagram exhibited in my 1984 essay.¹³

My assumptions differ little from those found in one of Cohen's own sources, a recent publication that suggests a division on a minimal frontage

10. As I pointed out in my original discussion of this matter, all such estimates are rough, and there are modest variations from source to source, and over time. See Posen, "Measuring," p. 76. Additionally, most estimates are presented as ranges. My historical cases are, as Cohen says, drawn from U.S. and British experience on the Western front. The reasons for this are explained in *ibid.*, p. 83 n. 49. The concentrations assumed in my analysis fairly and conservatively represent the likely Soviet effort.

11. Posen, "Measuring," p. 77, Figure 5. The diagram was drawn from a 1977 field manual.

12. These Pact divisions are larger and more powerful than those assumed in the 1977 diagram.

13. In note 61 Cohen claims that this diagram is "badly dated," yet the new edition of David C. Isby, *Weapons and Tactics of the Soviet Army*, rev. ed. (London: Jane's, 1988), p. 50, reproduces the identical 1977 diagram, the only omission being the source. Isby used the same diagram on p. 38 of the 1981 edition of his book, a page cited and misinterpreted by Cohen (note 60) as supporting his position. On p. 20 of that edition there are additional data suggesting that a division would mount its main effort on a 10–15 km segment of a 20–40 km sector. From the same period, U.S. Department of the Army, *Soviet Army Operations*, p. 3-5, Figure 2, suggests that an Army mounting a "main attack" would operate in a sector 40–50 km wide. Although this source is nearly a decade old, it is cited at least twice in the new edition of U.S. Congress, Office of Technology Assessment (OTA), *New Technology for NATO: Implementing Follow-on Forces Attack* (Washington, D.C.: U.S. Congress, 1987), pp. 60–61. One of Cohen's preferred sources, John Erickson, Lynn Hansen, and William Schneider, *Soviet Ground Forces: An Operational Assessment* (Boulder, Colo.: Westview, 1986), includes many diagrams and charts regarding attack zones and frontages identical to those found in the 1978 Army manual, although without citation. Cohen cites pp. 162–164 of their book to support his claim of very narrow Soviet divisional attack sectors. All three graphics are identical to those in the 1978 manual. Again Cohen confuses the division's *main* effort with its *entire* effort. A careful reading of all these sources clears up the confusion.

of 15 km and an army on 50 km.¹⁴ Cohen confuses himself and others by assuming that suggested breakthrough sectors of 10 km for an army and 4 km for a division would involve concentration of the whole army or the whole division in these sectors. He implies that divisions and armies would likely be lined up shoulder to shoulder at these densities. This is simply wrong, and a careful study of the available literature shows it. These narrow sectors are the zones for the unit's major effort, not its entire effort. The 1984 U.S. Army Field Manual 100-2-1, *Soviet Army: Operations and Tactics*, is clear on this point. It expects a Soviet army tasked for a major breakthrough to be deployed in a zone 60 km wide, with each division on 20 km.¹⁵ Two often-quoted, although unofficial, current open-source discussions of Soviet ground operations echo the 20 km/division figure.¹⁶ Indeed, Western experts are beginning to assume somewhat lower concentrations of Soviet engaged forces than they did in the late 1970s. Recently General Glenn Otis, then Commander of the U.S. Army in Europe and of NATO's Central Army Group, stated that he had come to expect the density of engaged Soviet first-echelon forces opposite his command to be *roughly half of what had previously been assumed*.¹⁷

14. Michael Sadykiewicz, *Soviet–Warsaw Pact Western Theater of Military Operations: Organization and Missions*, N-2596-AF (Santa Monica, Calif.: RAND, August 1987), p. 78.

15. Headquarters, Department of the Army, *FM 100-2-1: The Soviet Army: Operations and Tactics* (Washington, D.C.: U.S. Department of the Army, July 16, 1984), Chart 4-4, shows a main axis of advance 60 km wide. "An army offensive normally has a frontage 60–100 km wide" (p. 4-6). "When attacking with three regiments in a single echelon, a division zone of attack is normally 15–25 km wide" (p. 5-18). See also the diagram on p. 5-19. The general tone of this source is somewhat tentative, and the various observations made are not all consistent. The source concedes that higher densities are possible but doubts that the Soviets want to do this.

16. Isby, *Weapons and Tactics* (1988), pp. 24–25, suggests divisions on attack sectors of 20–40 km, with main efforts within these sectors 10–15 km wide; Erickson, Hansen, and Schneider, *Soviet Ground Forces*, suggest divisional frontages of 20–30 km, Figure 4.14, p. 166; 15–30 km (p. 60); a *minimum* of 9 km (p. 154); a *minimum* of 14 km (p. 164); etc. The same source suggests 65 km for armies, p. 60. Isby suggests army attack sectors 60–120 km wide, and main efforts within these sectors 36–72 km wide, under conditions where nuclear or chemical use is *not* expected. Wider frontages and lower concentrations are suggested when the use of such weapons is expected. Isby's estimate thus contradicts Cohen's assertion (p. 70) that the Soviet density estimates that I employ represent a "nuclear scared" posture—dispersal to reduce the destructive effects of potential nuclear strikes. This is a popular argument, but it is probably wrong. Western estimates of Soviet densities have on the whole decreased, even as our fears of Soviet disdain for the credibility of NATO's nuclear threat have intensified. This pattern is inconsistent with the "nuclear scared" explanation. Far more plausible is Soviet recognition that their own divisions, especially their logistics tails, are getting larger, and that the lethality of Western conventional ordnance is growing. Both would prompt lower concentrations.

17. "An Exclusive AFJ Interview with: General Glenn K. Otis, Commander-in-Chief of the U.S. Army in Europe and Commander of NATO's Central Army Group," *Armed Forces Journal*

Third, Cohen presents a misleading discussion of my decision to assign a 1.5 multiplier to NATO's ADE scores to account for NATO's greater efforts relative to the Pact in the areas of command and logistics. Cohen suggests that this multiplier was assigned simply for differences along a single dimension—the number of major tactical headquarters in divisions (p. 75). But I assign this multiplier on the basis of the totality of NATO manpower allocated to *both* command and logistics functions, inside and outside of divisions.¹⁸ He also suggests that NATO must devote almost twice as much manpower to command and logistics functions because of Western need for creature comforts, love of paperwork, or problems of Alliance coordination (pp. 76–77). It is peculiar for him to show reverence for the macro-level professional judgments of General Rogers or General Galvin when they offer pessimistic appraisals of the course of an entire war, and yet show contempt for the organizational skills of the thousands of professional officers who have designed the NATO armies of the past thirty years.¹⁹ *Some* of this command and support that we buy, over and above what the Pact buys, should be presumed to provide the West with extra combat capability. I do not, in my analysis, grant NATO all of the credit for this that I could justify. On the basis of manpower allocated to the whole range of command and logistics functions, one could make a case for as much as a 100 percent bonus to NATO firepower. But to account for the possibility that there may be inefficiencies in this particular NATO practice, and to account for the “rough and ready” quality of available data on relative command and logistics efforts, I give NATO forces only a 50 percent bonus.²⁰

International, January 1987, pp. 44–50. Expressed in ADEs, the average breakthrough concentration of frontline Soviet combat power that I assumed in my previous analyses, for both cases favorable to NATO and cases favorable to the Pact, is actually greater than that implicit in official and unofficial sources in the last decade.

18. My reasoning is explained at length in Posen, “Measuring,” pp. 67–70, and notes 24–26, which Cohen fails to note in his discussion.

19. Cohen also ignores the fact that the U.S. Army and the U.S. Air Force have had—since the end of World War II—more experience operating fully motorized and mechanized ground forces, airborne, and airmobile ground forces, large, modern air forces, and their associated means of C³I (command, control, communications, intelligence), than any other armed forces in the world, including those of the Soviet Union. In peacetime, U.S. and other Western armed forces train and exercise more realistically and intensively than do the forces of the Pact. So on the basis of peacetime as well as wartime experience, it is not unreasonable to suggest that Western militaries know something about what it takes to command these forces in the field and to keep them supplied and in good working order.

20. My most recent published analysis indicates that a much smaller bonus, perhaps 25 percent, may be sufficient to leave NATO competitive even under rather demanding circumstances.

Fourth, Cohen implies that I have failed to acknowledge the adverse “trends” in the conventional balance, and have relied on outdated information (pp. 72–73). But he is simply wrong about the trends, and about my information. The source Cohen cites to suggest that the trends are bad shows nothing of the sort. Three of the four cases presented in the DoD FY 1989 *Annual Report*, Chart I.B.2—his source on the trends—show almost no change in the ground force ratio since 1975, either “initially” or after 30 days of mobilization.²¹ According to the chart, the initial force ratio was roughly 1.6:1 in 1975, and is 1.6:1 in 1988. To the extent that there was any change at all, it was slightly for the better from 1975 to 1982, ironically roughly coincident with the much-maligned “decade of neglect,” and slightly for the worse from 1982 to the present, coincident with the Reagan buildup. But the basic message of the chart is that there has not been much change overall.²² Moreover,

Posen, “Is NATO Decisively Outnumbered?” *International Security*, Vol. 12, No. 4 (Spring 1988), pp. 198–200.

21. U.S. Department of Defense (DoD), *Annual Report, FY 1989*, Chart I.B.2, p. 31, reproduced as Figure 1 in John J. Mearsheimer, “Correspondence: Reassessing Net Assessment,” *International Security*, Vol. 13, No. 4 (Spring 1989), p. 139. Only one of the four curves, and one that almost certainly represents a pyramid of pessimistic assumptions, indicates noticeable deterioration since 1975; with the aid of a ruler I have tried to measure the extent of deterioration in that case and it appears to be roughly 6 percent. These Pentagon estimates actually rebut Cohen’s assertions (p. 73) that there has been a wholesale deterioration in the ground force relationship since the 1970s, either in standing forces or in mobilized forces.

22. The Pentagon’s 1988 estimated best-case “Initially Available” force ratio for both 1976 and 1988 is roughly 1.6:1, which does in fact contrast with the 1.2:1 figure used by Mearsheimer and myself; 1.2:1 was first offered by the Pentagon in 1976. Of course, since the *Annual Report* does not say what methodology or what mobilization assumptions were used to develop the 1988 estimates, we cannot be sure that they are even comparable with the earlier figures, or with my updated estimates. First, the 1988 index of “combat potential” might differ in important ways from WEI/WUV I, which I use to calculate ADE scores and which was still used by the Pentagon in 1976. (Private inquiries have convinced me, however, that the two methodologies are roughly comparable.) More importantly, very different mobilization assumptions may have been employed by the DoD in its 1976 estimate and in its 1988 estimate. For example, the Pentagon suggests that the current initial force ratio would be 1.6:1; this coincides exactly with my current estimate of a 1.6:1 force ratio (using WEI/WUV I), seven days after the Pact begins to mobilize (M+7). Because I assume a one-week delay to account for the possibility of poor Western intelligence or a dilatory response, this is the commencement of NATO’s mobilization, i.e., NATO M-day. See Posen, “Is NATO Decisively Outnumbered?” Graph 1. This suggests that the Pentagon’s assumptions may have changed, not that there has been superior growth in Pact combat power. It seems plausible that the reason that the DoD’s current retrospective estimate of a 1976 force ratio of 1.6:1 contrasts with the 1.2:1 figure it offered in 1976 is that the chart in the 1988 *Annual Report* begins several days after the Pact has commenced mobilization, but just as NATO has started mobilization. The 1976 estimate probably began with a day-to-day force ratio. The DoD may also have changed the counting rules to include some Pact forces, or exclude some NATO forces, that were treated differently in the original 1976 assessment. The only conclusion that can be drawn from the Pentagon’s 1988 chart is that there has been little change in the basic ground force ratio since 1976. If Cohen wishes to claim anything else from

the data presented in my Spring 1988 essay reflect modernization in U.S., Soviet, and West German forces.²³ I have recalculated the ADE scores of these forces, on the basis of open source information, to capture their current capabilities. My own calculations indicate that the day-to-day force ratio in ADEs is still roughly 1.2:1. Finally, although I treat Category I divisions of the frontline Group of Soviet Forces Germany (GSFG) as fully manned, it is rapidly becoming an open secret that, contrary to what had been supposed, they are not.²⁴ More specifically, the expansion in the combat power of forward deployed Category I divisions that was assumed to have taken place in the early 1980s occurred only in part. Additional weaponry *may* have been forward deployed but the manpower to use the weaponry is not present. Thus the true day-to-day combat capability of GSFG units did not go up in the same proportion as the hypothetical improvement in firepower associated with increased weapons densities.²⁵ Nevertheless, in my analysis, I treat frontline Soviet divisions as if they were fully manned.

Fifth, Cohen argues that the Attrition–FEBA Expansion Model is artificial because it shows a battle line that stretches but never breaks (p. 80). He intimates that NATO cannot fail in my analysis, because the front never ruptures. But the utility of the model is that it indicates if and when, in the course of combat, breakthroughs become plausible. Cohen mischaracterizes the nature of the model, which does not purport to predict the detailed course of a Central European battle. Rather, the model tests the ability of NATO's force structure to cope with the plausible cumulative demands of breakthrough battles of given intensity. Once demand diverges from supply, NATO is in trouble, and I say so.²⁶ But I do not prejudge whether there will

this chart, he must get declassified all the assumptions that went into the 1976 estimate and the 1988 estimate, explain the differences in the methodologies for the measurement of combat power employed in each case, and lay all this information out for others to judge.

23. Posen, "Is NATO Decisively Outnumbered?" pp. 190–193 states this clearly. Graph 1, p. 188, the Pact-NATO mobilization curve, shows clearly that my recalculation indicates a day-to-day force ratio of 1.2:1 in ADEs. I freely admit, however, that I have not incorporated *every* claim about Soviet force structure modernization that has appeared in the open literature; I consider it my responsibility to exercise some judgment in these matters.

24. "There are now new intelligence findings that strongly suggest that forward deployed Soviet units in East Germany may be in a lower state of readiness than is commonly believed." Les Aspin, *The World After Zero INF*, news release, House Armed Services Committee, September 29, 1987, p. 12.

25. Elsewhere Cohen makes much of the possibility that publicly available information on the Pact order of battle may underestimate the threat. But here we seem to have a case of an overestimate of the threat, a kind of intelligence failure that Cohen completely ignores in his analysis, and one that is not at all uncommon.

26. See "Measuring," p. 84. It is worth reiterating that all applications of the model published

in fact be a command reaction to this trouble, or exactly when that reaction should occur, or exactly when the trouble becomes so severe that catastrophe is imminent. NATO commanders may resist too long, and find themselves defending at lower and lower densities. If so, a rupture is likely. Or they may initiate frontwide withdrawals to eliminate Pact salients, shorten the front, and free up enough reserves to prevent a rupture. Neither of these events is a happy one. Either could occur, and in no case do I pretend that the developing situation is anything but bad news.

PESSIMISTS' SHORTCUTS

Four assumptions are commonly employed by Pessimists as shortcuts in the debate about the adequacy of NATO's conventional capabilities. While none of them is completely wrong, neither are they as simple, obvious, true, or proven as is often implied. They raise as many questions (and as many article topics) as they answer.

ARGUMENT BY AUTHORITY. Cohen's essay bows in the direction of particular authorities, especially the former and current Supreme Allied Commanders Europe (SACEURs) Rogers and Galvin (pp. 50, 88). Both commanders have publicly stated, in effect, that NATO would have to use nuclear weapons within days or weeks of a Soviet conventional attack. Here Cohen should follow his own advice and undertake some political analysis. SACEUR has many roles, and one of the most important is sustaining the belief systems that hold the alliance together. If conventional forces are inadequate, and nuclear escalation is inevitable, then deterrence is strong, and no war, conventional or nuclear, will occur. This is an image of NATO's strategic situation that is very popular among some Europeans. It comes as no surprise that SACEURs make statements of this kind about the imminence of nuclear escalation. Similarly, SACEUR is one of the principal players in the intense annual fight for budget share that national security elites must wage in each of the alliance's liberal democracies. An image of permanent shortfall in conventional forces is a powerful servant of bureaucratic interests in the fight for more resources. As scholars, we are paid to investigate policy questions

thus far credit the Pact with an ability to move its breakthrough efforts in NATO's direction in three sectors simultaneously. Thus, while Cohen complains that the pace of advance never increases, it would be equally fair to protest that the pace never slackens, as described in my 1984 essay, or slackens only when Red is short of reserves, as described in my 1988 essay. Thus, these applications of the model are inherently quite favorable to the Pact, giving it credit for considerable tactical success.

from the inside out, not to stop at the declarations of involved and interested parties. A trained social scientist is not obliged to check his or her critical faculties at the door upon entering the world of military analysis. For every pessimistic authority we can find a qualifiedly optimistic one, such as Generals Philip Mallory, Glenn Otis, and Frederick Kroesen, or a contemptuously astonished one, such as former Secretary of Defense Robert McNamara.²⁷

THE TEN-FOOT-TALL RUSSIAN. Another newly popular simplifying assumption is the exaggerated deference for Soviet military experience and technique. Cohen admonishes us to pay more attention to information about the Soviet military (pp. 86–87). We should by all means learn as much as we can about Soviet forces, doctrine, training, and tactics. But we ought not slip into the assumption that because some Soviet officer wrote about a particular plan or tactic, or some Soviet unit practiced it, the Soviets can therefore execute it successfully against military resistance. Much open source Western analysis of the Soviet military (including Cohen's) too often makes this mistake: "And then the Soviets detach the Belgians and Dutch from NATO, and then they secretly concentrate large forces in small sectors, and then NATO is surprised, and then the *Spetsnaz* pounce, and then the frontal air operation smashes NATO air power, and then the breakthrough unfolds, and then allied forces are overrun." Why go to all this trouble? Just assume a defeat and close up shop. Only in his closing remarks does Cohen concede that "the complex question of Soviet ability to implement their demanding doctrine" might be critical (p. 81).

THE WORSHIP OF SECRET INTELLIGENCE. Cohen worries that those of us who operate in the open literature may not have all the information we need to support our conclusion. He asserts that only the most "highly classified" information can provide a firm foundation for analysis (pp. 58–62). This is a

27. See "AFJ Otis Interview," p. 47: "With the total NATO capability available to Central Army Group, the first echelon of the Soviet forces is going to have a whale of a time doing anything to us." Oft-quoted is General Frederick Kroesen, former CENTAG commander: "We can defend the border of Western Europe with what we have. I've never asked for a larger force." Front-matter, Senator Carl Levin, *Beyond the Bean Count*, Senate Armed Services Committee, Subcommittee on Conventional Forces and Alliance Defense, January 20, 1988, mimeo. General Philip Mallory, who has observed Pact exercises firsthand, demonstrates respect for, but not awe of, Soviet forces in his public utterances and has offered many observations about Soviet problems and vulnerabilities. See Dean and Schemmer, "Warsaw Pact Success Would Hinge on Blitzkrieg," pp. 29–36. Former Secretary of Defense Robert McNamara expressed incredulity at General Rogers's declaration of NATO weakness: "It's a disgrace, spending \$2 trillion and getting two weeks of firepower." Quoted in "The New Guns-and-Butter Battle," *New York Times*, May 22, 1988, pp. F1, 19.

statement with which one cannot argue. I don't know what I don't know. What I do know is that I rely on a host of publications to support my analysis, including publications by the executive and legislative branches of the U.S. government, such as *Soviet Military Power*, the *Department of Defense Annual Report*, the Joint Chiefs of Staff *U.S. Military Posture*, reports of the Congressional Budget Office and the Office of Technology Assessment, and dozens of volumes annually of Congressional testimony on the defense budget and other national security matters. I also exploit the lively commercial and "hobbyist" literature, reports from the North Atlantic Treaty Organization and individual European governments, the IISS's *Military Balance*,²⁸ and the fruit of conversations with active and retired senior military officers and civilian national security officials. The purpose of much of the material published by U.S. and European official sources is to attract public support for increased military spending.²⁹ Is it plausible that the threat is significantly understated in these documents? Unlikely.

My work has been before the public for several years; it is widely read. If the marketplace of ideas provides a good test of Cohen's secret information hypothesis, then it should be telling that I have never had an intelligence or military official intimate to me in even an oblique way that there are special secret things that would hugely alter my conclusions if I only knew them. Indeed, my work has often been privately commended by such people.

Finally, I would submit that the limitations on data in the unclassified world may be a positive blessing for the progress of the analytic enterprise as a whole. An old saw often attributed to the U.S. intelligence community is, "when in doubt, collect." It has some truth in it; it is important to know as much about our adversaries as we can. But the intelligence community has long been accused of suffering from an abundance of data and a dearth of good analysis. Whether such accusations are true or not, all of this academic intellectual energy focused on processing the publicly available information must be a good thing, even if the public information did constitute but a small share of the data available in the classified world. After all, this service is often free to the government, and since it is conducted by people

28. Cohen often intimates that the IISS's *Military Balance* contains too little information, much of it wrong, but as he is surely aware, the IISS relies on official NATO, European government, and U.S. sources, private as well as public.

29. For example, every year the publication of *Soviet Military Power* coincides with the delivery of the defense budget to Congress.

who have few, if any, bureaucratic axes to grind, it provides a rare source of genuinely competitive analysis and fresh perspective.³⁰

THE "CULT OF THE OFFENSIVE" AND THE "CULT OF SURPRISE." No discussion of Cohen's work would be complete without reference to the "cult of the offensive" and its close cousin "the cult of surprise." In Cohen's view, because the Soviets are the aggressors, they have the "initiative," and thus will choose the time, place, manner, and weight of an attack; they will probably achieve surprise, which Cohen deems to be quite decisive (pp. 60–61, 71, and 87). His descriptions of Soviet air and ground offensive operations paint them as unfolding with pile-driving precision and success. The relative effectiveness of offensive and defensive postures, operations, and tactics, and the ease and potential military decisiveness of surprise, are important issues for the strategic studies community. But they have not been settled in favor of the argument that these factors necessarily give the Soviets either a special advantage, or more importantly, a decisive one. Although Cohen likes to quote Clausewitz favorably, he fails to consult him on these issues: In Book Six, some 150 pages entitled "Defense" develop the idea that "defense is the stronger form of waging war." Clausewitz's brief explicit discussion of "Surprise" reminds us that surprise is just as feasible for the defense as the offense (the basic principle of the ambush), but that "by its very nature surprise can rarely be outstandingly successful."³¹

In my judgment, although Eliot Cohen's essay injects heat into the debate on the conventional balance, it does little to sharpen or clarify fundamental issues. Indeed, following his methodological injunctions would obstruct a difficult and important enterprise. It would divert analysis of the relative competitiveness of NATO and Warsaw Pact military forces into a sea of political speculation or an attic full of Soviet military literature. Cohen offers no "net assessment" of his own, nor does he shed any light at all on the main question of the public debate on the NATO-Pact conventional balance: What are NATO's prospects in a conventional war with the Pact, presuming that both coalitions do hold together, and that NATO mobilizes in time to

30. The lively debate in this issue of *International Security* between Joshua Epstein and John Mearsheimer, whose conclusions on NATO's competitiveness are quite similar, illustrates the vibrant quality of academic defense analysis and the contribution it can make. John J. Mearsheimer, "Assessing the Conventional Balance: The 3:1 Rule and Its Critics," *International Security*, Vol. 13, No. 4 (Spring 1989), pp. 54–89; Joshua M. Epstein, "The 3:1 Rule, the Adaptive Dynamic Model, and the Future of Security Studies," *ibid.*, pp. 90–127.

31. Carl von Clausewitz, *On War*, ed. and trans. Michael Howard and Peter Paret (Princeton: Princeton University Press, 1976), p. 359; 198–201.

move its forces to their combat positions along the inter-German border before combat begins?

Cohen's bottom line is clear, however: that Joshua Epstein, John Mearsheimer, Barry Posen, and any other analyst who ventures an "optimistic" judgment about the current NATO–Warsaw Pact military balance should cease work on this issue, and leave it to professional insiders whose work will be so highly classified that it will escape critical attention. From time to time they may bless us with brief sanitized summaries of their labors, which we are to treat with reverence and respect. The potential for serious error in following this advice is twofold. First, who will audit their work? Who will provide the peer review to keep them honest and ensure quality control? The answer is, no one. Second, who will train their successors? The answer is that they themselves will. Thus, whatever errors they make will, unsubjected to critical scrutiny and debate, be passed on to their acolytes. The national security business of the state is conducted this way in the Soviet Union, but the United States is a democracy, we do things differently here, and some of us believe that significant advantages flow from this fact.

—*Barry R. Posen*
Cambridge, Mass.

The author replies:

It is difficult to know what to make of the views Barry Posen and John Mearsheimer take of our disagreements. On the one hand, Posen says that the field of strategic studies advances when scholars "publish their findings; this invites criticism from their colleagues." So I always thought. And yet he and Mearsheimer maintain that my critique of their arguments is not a normal scholarly disagreement but an attempt to subvert NATO's cohesion by spreading "defeatism"; or perhaps an effort "to obstruct a difficult but important enterprise"; or even a conspiracy to stifle their literary efforts, while creating a clique of "professional insiders" who will serve as the advance party of a Soviet-style national

The views expressed here are the author's alone, and not necessarily those of the Naval War College or of any other government agency.

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security state (pre-*glasnost*).¹ I regret these and other intemperate suggestions in their replies. Nonetheless, and despite my reservations about the theoretical and evidentiary bases of their work then and now, I continue to think that “these analysts deserve full credit for raising and exploring an interesting set of issues concerned with the problem of net assessment.”²

I must, however, caution readers of this issue of *International Security* that in order to sort out this debate they may have to reread my article and those of Mearsheimer and Posen to which it referred. This is so for four reasons. First, their letters either ignore or make scant reference to many of the criticisms I made in “Toward Better Net Assessment.” For example, in that article I questioned Posen’s and Mearsheimer’s attempt to evaluate Soviet prospects for a quick win in Central Europe without looking at the vital matter of ammunition and supply stocks—an area in which NATO has grave deficiencies.³ I challenged Mearsheimer’s unsupported assertion that NATO’s artillery had significant technical advantages over its Soviet counterparts, when the evidence points the other way.⁴ I noted that Posen was incorrect in saying that NATO was warned and reacted in a timely fashion to the Soviet invasion of Czechoslovakia (which implied, in his view, that successful Soviet surprise of NATO was unlikely).⁵ I further observed that Posen’s historical evidence—no small part of his argument—contained not a single examination of a Soviet offensive operation in World War II, which is surely

1. Similarly, although both gentlemen believe that authors may legitimately “choose to focus their own work on particular questions,” they fault me for not demonstrating that the Soviets *would* win a war in Central Europe quickly. And yet I wrote at the beginning of “Toward Better Net Assessment”: “This article is *not* about ‘why the Soviets *would* win quickly in Europe.’ Rather, it discusses problems of net assessment and uses as its point of departure a critique of important views about the conventional balance in Europe.” “Toward Better Net Assessment: Rethinking the European Conventional Balance,” *International Security*, Vol. 13, No. 1 (Summer 1988), p. 53. Emphasis in the original. References to Posen and Mearsheimer replies are to their letters in this issue, *International Security*, Vol. 13, No. 4 (Spring 1989), pp. 128–160.

2. *Ibid.*, p. 52.

3. Whereas the Warsaw Pact is reliably reported to have 60 to 90 days’ worth of ammunition, fuel, and spare parts forward deployed, “NATO is far short of its 30-day munitions objective.” Moreover, these shortages are most severe with respect to “smart weapons, and are much worse in some corps areas than others.” U.S. Congress, Senate Committee on Armed Services, *NATO Defense and the INF Treaty: Report of the Committee on Armed Services of the United States Senate*, 100th Cong., 2nd sess. (Washington, D.C.: U.S. Government Printing Office [U.S. GPO], 1988), p. 7.

4. Cohen, “Toward Better Net Assessment,” p. 76. For other examples of a similar kind, see Jonathan Alford’s review of John J. Mearsheimer, *Conventional Deterrence* (Ithaca: Cornell University Press, 1982), in *Bulletin of the Atomic Scientists*, Vol. 40, No. 5 (May 1984), pp. 41–42.

5. Cohen, “Toward Better Net Assessment,” p. 60.

relevant to the case before us.⁶ I indicated that Posen mismeasured the size of headquarters on either side by counting specialized combat units as command elements, and expressed my doubts about a model in which under “plausible” assumptions the Soviets were never allowed to advance more than 2000 meters a day. I mentioned Mearsheimer’s misreading of Soviet strategy in World War II, his view that the Soviets relied on slow advances across a broad front rather than deep strategic penetrations—a historical error that casts doubts on his claim to “pay close heed” to the workings of the Red Army.

These omissions in their rebuttals—and there are others, some of which I discuss below—are important. In some cases they weaken key links in the chain of reasoning holding together their arguments. In other cases they reveal a careless handling of the facts, which is always important because no strategic theory, no matter how simple and elegant, has much utility if it lacks a foundation in accurate data. This concern is particularly important when Posen, for example, continues to withhold from his readers the detailed workings of the Attrition–FEBA Expansion Model, when he proclaims his reliance on a modified version of a fourteen-year-old firepower scoring system (WEI/WUV I), and when he avows his determination to exercise “judgment” about the real extent of Soviet force modernization, i.e., to modify standard published descriptions of the Soviet order of battle in unspecified ways.

A second reason for rereading the original articles is that Posen and Mearsheimer have, in a number of cases, misrepresented “Toward Better Net Assessment,” or fabricated arguments to which, they claim, I subscribe. Mearsheimer, for example, makes a great deal of my “seven scenarios” for how the Soviets would win quickly in Europe. What he appears to have done is taken a criticism of his work (e.g., the failure to consider the impact of chemical weapons on the battlefield) and turned it into a full-blown “scenario” (e.g., the Soviets launch an attack on NATO that succeeds because of their use of chemical and other unconventional weapons).⁷ In point of fact,

6. His response (p. 150 n. 9) would suggest that neither air power nor artillery played much of a role in breakthrough battles on the Eastern Front. This is simply untrue.

7. General Frederick J. Kroesen, former commander of U.S. Army forces in Europe, declared in an interview that: “If I were a Soviet commander, I would have great difficulty not recommending the use of chemical weapons in support of my operations. [No commander here] thinks that we wouldn’t have a major problem if we were subjected to chemical attack.” “GI’s in Europe: ‘Good Now, Going to Get Better,’” Interview with Gen. Frederick J. Kroesen, Commander in Chief, U.S. Army, Europe, and Seventh Army, *U.S. News & World Report*, August

there was no enumerated list of scenarios in “Toward Better Net Assessment,” which was an essay about various aspects of net assessment, not a collection of scenarios for successful Soviet blitzkrieg. Posen similarly presents a false picture of my article when he claims that I “worship secret intelligence,” and reports that I declared his analysis faulty because he does not know “special secret things that would hugely alter my [Posen’s] conclusions if only I knew them.” What I wrote in the article, however, was something quite different. I said that in some cases important data (e.g., on Soviet tank armor) is indeed highly classified.⁸ In general, however, I said that the issue is *not* that “the ‘real’ numbers exist in government—some do, some do not.” Rather, I argued, one can only judge the accuracy of data (classified or not) by knowing in some detail the sources of intelligence that produced it, and I offered evidence that on a number of recent occasions Western *intelligence agencies*—not just academics—have misgauged the Soviet order of battle by substantial amounts. In other words, “Toward Better Net Assessment” argued that the data problem exists in government as well as outside it. Furthermore, I observed that published information on Soviet forces fluctuates—usually upward—from year to year. The most recent issue of *The Military Balance* provides fresh evidence of this. In the 1988–89 version, the International Institute for Strategic Studies (IISS) has, at a stroke, credited the Group of Soviet Forces in Germany with five independent tank regiments—some 750 tanks previously absent from the annual volumes.⁹ I concluded that although “academic analysts have no choice but to work with the data available to them,” they should show some modesty about their

9, 1982, p. 24. This interview with Kroesen is frequently cited by Posen and Mearsheimer, but in an exceedingly selective fashion.

8. Some important things really do stay secret for long periods of time—think of the revelations about Anglo/American cryptographic successes against Nazi Germany, which only came out in the early 1970s. As for Posen’s declaration that he has never heard hints from government officials that he may be missing some important pieces of data, all I can say is that I have rather different recollections of a conversation at a conference at the Kennedy School of Government this past year. Finally, I call his attention to the observations of Philip Morse, the founder of American operations research in World War II, who warned fellow modelers that: “Problems of secrecy, encompassed in the military term ‘security’ will nearly always obtrude in operations research in any field, and the worker must be prepared to cope with them intelligently and not emotionally.” Philip M. Morse and George E. Kimball, *Methods of Operations Research* (Los Altos, Calif.: Peninsula Publishing, 1970), p. 7.

9. International Institute of Strategic Studies (IISS), *The Military Balance, 1988–89* (London: IISS, 1988), p. 31. Other increases in both the quantity and quality of Soviet armament in Eastern Europe are also now accepted by the IISS—and as relatively long-standing facts, not the products of this year’s Soviet arms buildup.

conclusions.¹⁰ The same should be said of governmental analysts as well. How a plea that scholars understand the fallibility of their data bases becomes evidence of a cult of classification escapes me.

Thirdly, one has to go back to the original articles because in their replies to "Toward Better Net Assessment" Posen and Mearsheimer appear confused about some of their *own* earlier arguments. Mearsheimer, for example, declares above that in his original (1982) article he examined a scenario in which NATO's mobilization lagged that of the Warsaw Pact by up to six days. In fact, "Why the Soviets Can't Win Quickly in Central Europe" says that it looks at a situation in which "NATO's mobilization begins *immediately* after the Pact starts to mobilize."¹¹ A look at Mearsheimer's numbers suggests that by "immediately" he meant just that—and indeed, a few pages after the sentence quoted above, Mearsheimer seriously raised the possibility, in another scenario, of NATO mobilizing *before* the Warsaw Pact.¹² Posen does something similar when he suggests that in his original essay he described a fifty kilometer sector for a Soviet Army as the *overall* sector occupied by such a force, rather than the *breakthrough* sector on which it would launch its main effort to penetrate NATO's forward line. Yet, in fact, in "Measuring the European Conventional Balance," Posen wrote of a "50 km breakthrough sector" for a Soviet Army.¹³

Fourthly, one must return to the original articles because Posen and Mearsheimer misrepresent some of the sources they advance in their defense, such as the interviews with Generals Glenn K. Otis and Frederick Kroesen, former commanders of the U.S. Army in Europe and NATO's Central Army Group. Posen correctly quotes Otis as saying that the "first echelon of the Soviet forces is going to have a whale of a time doing anything to us," but neglects to mention the rest of the interview, in which Otis pointed out that the Soviets would have eleven echelons behind the first, and that his forces could only handle the first two or three.¹⁴ Mearsheimer misconstrues that

10. Cohen, "Toward Better Net Assessment," p. 59.

11. John J. Mearsheimer, "Why the Soviets Can't Win Quickly in Central Europe," *International Security*, Vol. 7, No. 1 (Summer 1982), repr. in Steven E. Miller, ed., *Conventional Forces and American Defense Policy: An International Security Reader* (Princeton: Princeton University Press, 1986), pp. 121–127, at p. 124. Emphasis added.

12. *Ibid.*, p. 127. I remain unable to understand how and why this might occur.

13. Barry R. Posen, "Measuring the European Conventional Balance: Coping with Complexity in Threat Assessment," *International Security*, Vol. 9, No. 3 (Winter 1984/85), repr. in Miller, *Conventional Forces*, pp. 79–120, at p. 108.

14. Charles D. Odorizzi and Benjamin F. Schemmer, "An Exclusive AFJ interview with: General

article altogether, saying that General Otis “argued that NATO is likely to do well in a conventional war with the Pact.” In fact, Otis said that unless the United States could send ten divisions in ten days to Europe during a crisis, NATO would probably have to resort to the use of nuclear weapons. He concluded: “I need ten divisions in ten days, and that’s our promise to NATO. We can’t do it today.”¹⁵

All this said, some interesting points of difference between us, surfaced in their letters, merit discussion here. Let us turn first to particular points of contention and then to the more important, and more deeply-rooted differences about method and approach.

POSEN’S FORCE MULTIPLIER

Posen continues to defend the propriety of his decision to multiply his measurements of NATO firepower by one hundred fifty percent to account for NATO’s nominally superior logistics, command and control, and intelligence. When one looks at his evidence, one finds that it is shaky (e.g., the calculation of the relative size of NATO and Warsaw Pact headquarters mentioned above, and discussed at some length in “Toward Better Net Assessment”). In his response he falls back on his original argument: if we devote lots of money and resources to something, it must be because we think it has a payoff, therefore it has a payoff, and since it has a payoff, we should take credit for it. Waste, duplication, and lack of coordination do not intrude into this tidy world of NATO military procurement and administration. To disagree with this contention is, in his words, to “show contempt for the organizational skills of the thousands of professional officers who have designed the NATO armies of the past thirty years.”¹⁶

This is a curious argument coming from someone who believes that military organizations “frequently behave in ways that are inimical to the interests of the state,” and are, in fact, often incapable of keeping up with changing

Glenn K. Otis, Commander-in-Chief of the U.S. Army in Europe and Commander of NATO’s Central Army Group,” *Armed Forces Journal International*, January 1987, p. 46.

15. *Ibid.*, pp. 47, 50. See also note 5 above, and note 26 below. It should be noted that Kroesen’s and Otis’s optimistic remarks refer to the forces under their immediate command, i.e., NATO’s Central Army Group (and, in some cases, U.S. forces alone), not to all of NATO and its subordinate commands.

16. Posen, reply, p. 153. Posen shows no similar deference to Soviet officers, however, and his examination of this issue neglects certain Soviet advantages in this area. See Cohen, “Toward Better Net Assessment,” pp. 76–77.

times without the benefit of “a good kick from the outside.”¹⁷ More to the point, it runs contrary to the view of any known participant in NATO procurement and logistics programs, and every major study of the subject since NATO’s inception. The former chairman of the Independent European Program Group recently discussed “the waste of astronomic amounts of money because we fail to harmonise requirements and time schedules and continue to develop too wide a variety of equipment in a relatively small series,” as well as “the extremely wasteful duplication in the development and the production of defense equipment.”¹⁸ Does NATO really deploy six different main battle tanks firing four different kinds of ammunition because its professional advisers want it that way? Is NATO’s use of half a dozen incompatible communication systems really the result of the high “organizational skills” of a unified NATO military? If Posen wants us to believe NATO has the benefits of logistics, command and control, and intelligence as unified as those of the Warsaw Pact, he will have to provide evidence to support his contention. Thus far, he has not, and his multiplier remains questionable.

RATIOS AND DIAGRAMS

Mearsheimer clings to his belief that ready Warsaw Pact and NATO forces stand in a 1.2:1 ratio, a ratio that has held since 1976 or even 1973, according to his first article. He argues that the graph that he has reprinted does not undermine his confidence in the current or historical validity of the 1.2:1 ratio, even though the two bands indicate pre-hostilities ratios ranging between 1.5:1 and 3:1. This is so, he maintains, because it reflects a new Defense Department firepower measure. In recent Senate testimony he discussed this new firepower measure, which he identified as the DEF (Division Equivalent Firepower). At that time (October 1987) Mearsheimer said that he had learned that “the raw balance of forces changes little when you move from ADEs to

17. Barry R. Posen, *The Sources of Military Doctrine: France, Britain, and Germany between the World Wars* (Ithaca: Cornell University Press, 1984), pp. 241, 226.

18. Jan van Houwelingen, “Defence co-operation is Europe’s future,” *Jane’s Defence Weekly*, February 13, 1988, p. 273. See the testimony of Thomas A. Callaghan, in U.S. Congress, Senate Committee on Armed Services, *Alliance and Defense Capabilities in Europe: Hearings before the Subcommittee on Conventional Forces and Alliance Defense of the Committee on Armed Services, United States Senate, 100th Cong., 1st sess.* (Washington, D.C.: U.S. GPO, 1988), pp. 89–99. Henceforth cited as *Alliance and Defense Capabilities*.

DEFs, which is hardly surprising.”¹⁹ And in fact, knowledgeable Defense Department officials inform me that the new scoring system uses the same analytical base (an up-to-date version of the WEI/WUV scoring system) as the ADE (armored division equivalent), and hence produces similar results—a point with which Posen concurs.²⁰

Both Posen and Mearsheimer insist on reading only the lower line of the lower of the two bands on the 1988 Department of Defense (DoD) graph, saying, for example, that “the Pentagon suggests that the current initial force ratio would be 1.6:1,” when in fact the lower band shows force ratios ranging between 1.6:1 and over 2:1.²¹ This bottom band reflects standing forces, the top the situation after a month of peacetime mobilization on each side: why the second band (showing ratios ranging between 2.5:1 and 3:1) is irrelevant to the question of the conventional balance is unclear to me. Posen says that it is “plausible” that the 1988 *Annual Report* chart reflects ratios that assume a lag of several days in NATO mobilization. In point of fact, neither band reflects *any* lag in NATO mobilization. The first band deals with standing forces alone, the second with *simultaneously mobilized* forces on either side.²²

In “Toward Better Net Assessment” I pointed out that the 1976 ratio was highly dubious back then, for a number of reasons.²³ What was and remains untenable, however, is Mearsheimer’s belief that force ratios in Europe have remained unchanged for a decade and a half. In fact, as Phillip Karber has recently pointed out, over the last twenty years there “has been a major conventional arms race in Central Europe. It is an arms race that NATO has not been winning in terms of active forward deployed forces.”²⁴ Karber’s careful study of the balance recently led Senator Carl Levin to issue a new version of his *Beyond the Bean Count* report. The first report frequently cites,

19. *Alliance and Defense Capabilities*, p. 236. In the same hearings (p. 137) he declared that “if you look at the ADE balance of standing forces in Europe, you see that the Warsaw Pact has an advantage over the United States and its allies of approximately 1.2 to 1. I repeat, 1.2 to 1.” He advanced then, and has advanced since, no evidence to support this ratio, save for his 1982 article, which in turn rested on the 1976 DoD report.

20. Posen reply, note 22.

21. *Ibid.*

22. Personal communication from knowledgeable DoD official.

23. Posen appears to agree, and even Mearsheimer has prepared a fall-back position in note 15. Among other things, the 1976 ratio may not have counted non-divisional Soviet units.

24. Testimony of Phillip A. Karber in U.S. Congress, Senate Committee on Armed Services, *NATO Defense and the INF Treaty: Hearings before the Committee on Armed Services of the United States Senate*, 100th Cong., 2nd sess. (Washington, D.C.: U.S. GPO, 1988), Part I, p. 227. For an up-to-date survey of the numerical basis of balance estimates, see all of his testimony, pp. 225–271. Henceforth cited as *NATO Defense Hearings*.

and is frequently cited by, Posen and Mearsheimer. It was withdrawn—much to Senator Levin’s credit—when he became aware of factual omissions and inaccuracies in it similar to those still defended by Posen and Mearsheimer. Thus, in its latest incarnation *Beyond the Bean Count* acknowledges a Warsaw Pact advantage of 2.4:1 in tanks overall, 3:1 in modern tanks, and 3.5:1 advantage in artillery.²⁵ Karber estimates that the theater-wide Warsaw Pact/NATO ADE ratio went from 1.5:1 in 1967, to 1.8:1 in 1977, to 2.2:1 in 1987, and he has provided rather more evidence than Mearsheimer’s original thirteen-year-old graph to support this view.²⁶

Mearsheimer attempts to extrapolate trends from the latest graph in the DoD annual report. I do not see how he is able to do this, because of the wide range of unstated assumptions that determine the upper and lower bounds of the two bands. All that one can hope to get from the chart is some sense of where the rough ratios may lie today and in the past, according to a range of assumptions—and in no case do those support the notion of an unalterable 1.2:1 ratio. Nor is it difficult to see why the balance should have deteriorated over the last ten years or so. It is generally acknowledged that Soviet military spending, much of it directed at Europe, rose at a steady rate (between two and four percent per annum, according to the IISS) during this period. Much, though by no means all, of the corresponding increase in American military spending went to improvements in nuclear and maritime forces (a fact often deplored in print by Posen and Mearsheimer). At the same time, European NATO spending on conventional forces has grown very little. The Belgian budget is less in real terms than it was in 1980, the German and Danish budgets are perhaps three percent larger than they were then, and even the French and British budgets are barely ten percent larger than they were in 1980.²⁷ Even if Soviet defense spending went up by only two percent a year in the 1980s that would be enough to increase spending faster than NATO, and to do what has been done, namely, to modernize

25. See Scott D. Dean, “Levin Releases Revised, ‘Value Added’ Conventional Balance Assessment,” *Armed Forces Journal International*, October 1988, pp. 12–16. The differences between the Karber and IISS figures are depicted in Anthony H. Cordesman, “Fatal Flaws in Presenting the NATO/Warsaw Pact Balance,” *Armed Forces Journal International*, July 1988, pp. 60–68.

26. Senate Committee on Armed Services, *NATO Defense and the INF Treaty*, pp. 57–70. The ratio refers to active forces (i.e., excludes reserves, war stocks, and non-operational equipment) in the NATO Guidelines Area—Belgium, the Netherlands, the Federal Republic of Germany, Poland, East Germany, and Czechoslovakia.

27. Spending figures taken from IISS, *The Military Balance, 1988–89*, pp. 250–251.

Soviet forces in Eastern Europe, deploy them forward, and build up their war stocks.

SOVIET ATTACK FRONTAGES

As I observed above, the muddle on this subject seems to be of Posen's and Mearsheimer's own making. The key points to bear in mind are as follows:

- Mearsheimer has argued that the Soviets can never get much better than a 1:1 ratio on the battlefield, that is, at the level of the NATO brigade or the Soviet equivalent. I demonstrated in "Toward Better Net Assessment" that this is simply not true: the Soviets can, and in their exercises do, throw a full tank or mechanized regiment at fronts as narrow as two or three kilometers—compared to the seven to fifteen kilometers held, according to Mearsheimer, by a NATO brigade. Mearsheimer has ignored this issue.
- In no case do either Posen or Mearsheimer discuss the massing of forces in the most common-sense way, namely, incorporating the effects of massed artillery fires (which might come from artillery in neighboring forces or from higher echelons, as well as the assaulting unit). Rather, they pretend that the battlefield is best understood as tank butting into tank, soldier into soldier, all charging straight ahead, supported by artillery pieces that only fire straight forward. In their defense they claim that this bizarre simplification of the battlefield benefits neither side. The superior range and vastly superior quantities of Soviet artillery, and the fact that the Soviets, not NATO, choose where the Warsaw Pact attacks, makes this argument untenable.
- In their original articles, Posen and Mearsheimer both assigned three corridors, fifty kilometers wide, to three Soviet Armies trying to batter their way into Central Europe. They called these corridors "breakthrough sectors." They now accept that the *breakthrough sector* for a Soviet Army, as opposed to its width in its rear areas, or the zone in which it launches diversionary attacks, can be considerably narrower than fifty kilometers.
- Posen and Mearsheimer have never adduced any *evidence* to suggest that the Soviets actually plan to attack along only three (at the outside, four) fifty kilometer avenues.²⁸ Rather, the three corridor/fifty kilometer rule

28. Indeed, all the evidence—including many of the sources cited by Posen and Mearsheimer—suggests something very different. See, for example, the discussion in Cohen, "Toward Better Net Assessment," pp. 64, 70, notes 44 and 62.

reflects their judgment—and *only their judgment*—of how the Soviets should (or must) understand the problem.

Again, the reader is best advised to go back to the original articles to come to grips with this discussion: for example, in “Toward Better Net Assessment” I noted how Posen and Mearsheimer made selective use of old editions of various reference works to give more favorable frontages for attacking Soviet forces when new (and from their point of view, less favorable) editions were available.²⁹ If I made an error, it appears to have been in thinking that when, in their original articles, Posen and Mearsheimer talked about “Soviet Army breakthrough sectors,” they used the term in its accepted and proper sense. It now appears that they realize that they did not, although they fail to see the consequences this has for their previous line of argument.

But the difficulty does not end here. Posen, in particular, cites a number of sources to bolster his position. Leaving aside the confusion created by his shifting use of “attack sectors,” “breakthrough sectors,” “zones of attack,” and “main efforts,” one must turn to those sources to verify his use of them.³⁰ For example, he cites Department of the Army *FM 100-2-1, The Soviet Army: Operations and Tactics*, as saying that a division “zone of attack” is fifteen to twenty-five kilometers wide, which would seem to support his original contention that the Soviets could not mass much more than 1.5 ADEs against a twenty-five kilometer section of front. The field manual in question does indeed talk about a zone of attack that wide. It goes on to say that this deployment pattern describes a case in which a Soviet division is advancing with three of its regiments abreast, a formation used “if enemy defenses are not well prepared.”³¹ If those defenses *are* well-prepared, i.e., if the scenario resembles that of Posen’s and Mearsheimer’s rapid and effective NATO counter-mobilization, “a probable array for the division in these circumstances would be two regiments in the first echelon massed across a frontage as narrow as 6 to 10 kilometers, followed by two second echelon regiments

29. Cohen, “Toward Better Net Assessment,” p. 70, especially notes 61–64.

30. One must do so, in part because of other errors in his use of sources. In *ibid.*, p. 69, note 60, for example, I noted that Posen said that Soviet breakthrough efforts would create only small “wedges” in NATO lines. The source he cited for this latter observation said no such thing.

31. Headquarters, Department of the Army, *FM 100-2-1, The Soviet Army: Operations and Tactics* (Washington, D.C.: Department of the Army, July 16, 1984), p. 5-20. Posen omits this phrase in his letter.

and a small reserve." The field manual does not "concede," as Posen puts it, that higher densities are possible but undesirable and indeed unlikely: rather, it asserts that the Soviets would reduce their frontages sharply if they faced a well-prepared defense. Posen's Attrition-FEBA Expansion Model, *which assumes well-prepared NATO defenses*, should be tested with this kind of breakthrough effort, rather than the easier one he has chosen. He admits in his letter that he uses in his model parameters that do not allow the Soviets to concentrate below seventeen kilometers to a divisional front.³² That is more or less *double* the frontage that *FM 100-2-1* considers a likely Soviet option *in the sort of scenario conceived by Posen*.

WHERE CAN THE SOVIETS ATTACK?

In "Toward Better Net Assessment" I noted that Posen and Mearsheimer resolutely exclude from their analyses Soviet operations along NATO's flanks, and Mearsheimer responds to this criticism. He believes that the Soviets could not attack through Austria because of the formidable, presumably Alpine, terrain they would find. A quick look at the map (let alone investigation on the ground) would reveal that the northern portion of Austria is no more difficult than any other part of the inter-German border. Terrain would, no doubt, be a problem if the objective of a Soviet thrust lay in the resort town of Innsbrück, but that is not the case under consideration. The Austrian army is not much of an obstacle either, Mearsheimer's view notwithstanding, and indeed it has been estimated that even with stiff Austrian resistance Soviet forces could march through the northern reaches of that country in forty-eight to ninety-six hours.³³ One should remember that we speak of a country that, throughout the time Mearsheimer has been setting forth his views, has had no anti-tank or surface-to-air missiles, no first-class aircraft, and no modern tanks in its inventory. Its citizen soldiers do six months of initial training and sixty days during the next fifteen years—an average of four days a year. To talk about the Danish army offering a formidable resistance to the Soviets is similarly exaggerated, at best, given the continued stagnation of the Danish defense budget, which has provoked at least one high-level resignation from the Danish military. In both cases Mearsheimer does not account for the overall strategic context of a war in

32. Posen, reply, p. 151.

33. Conversation with knowledgeable DoD official.

central Europe, in which the Soviets would have a number of operational objectives, including unhinging NATO's air defenses, diverting or pinning down first-rate NATO forces, and increasing the risk to NATO's rear areas.

If we confine the argument to Germany alone, we must contend with Mearsheimer's assertion that the areas he denies the Soviets "are covered by formidable obstacles—mountains, forests, marshes, rivers, and urban sprawl—that would greatly impede the movement of large armored forces." But the three (or at most four) attack corridors he permits the Soviets also have many of these terrain features. Moreover, the most formidable obstacle to an army is not terrain but another army, a point confirmed often by military history. Among many other examples mentioned in my article, consider the German thrust through the hilly, wooded, obstacle-ridden Ardennes in 1940. In one sector three German armored divisions crossed not only this difficult terrain, but a major river, in order to attack the unsuspecting French on a *ten*-kilometer front (an interesting and instructive bit of concentration for a breakthrough!). This, apparently, is a "dubious historical analogy" which Mearsheimer considers irrelevant to the European conventional balance and hence inadmissible to our discussion. In that case, one must ask why he has decided to use it as a major piece of evidence to support his arguments in his article "Assessing the Conventional Balance: The 3:1 Rule and Its Critics," which appears in this issue of *International Security*.

THE AIR BALANCE

Mearsheimer continues to think air power an "exogenous factor" in the military balance. Posen, on the other hand, says that I challenged his average kill-rate calculations for NATO sorties, i.e., the number of vehicles one might expect each NATO aircraft to destroy on an average flight. In fact, I questioned not the absolute size of those kill-rates but the fact that he credited NATO with twice as many sorties as the Warsaw Pact, and three times as many hits on Warsaw Pact as opposed to NATO vehicles. In his defense he describes the fire power of an A-10. One should, in that case, note the maximum speed of an A-10 (a mere 423 mph, roughly the same as a 1944 single-engine P-51), and that the A-10 first flew some sixteen years ago, which means that its design parameters were shaped very largely by the air defense threat of the late 1960s and early 1970s. One might as well also note the capacities of Soviet air defense systems, such as the venerable ZSU-23-4, a system still unequalled in the U.S. Army, which can spew out 3,400

rounds per minute, and which has been succeeded by even more lethal weapons.³⁴ In the words of General Kroesen (in a portion of the interview with him that Posen and Mearsheimer choose to ignore): “We have few if any qualitative advantages left. The Soviet weapons-modernization program of the past 20 years has been so comprehensive that it has enabled them to catch up with us and, in fact, surpass us in many ways.”³⁵

I did express some doubt about Posen’s five percent attrition figure for NATO aircraft, a number which is indeed open to question in several ways. I noted that the number does not reflect the much higher attrition rates that air forces have encountered when first encountering dense, first-rate, and unfamiliar defensive systems. Responsible NATO commanders agree: at a conference in Brussels on April 1988 Major General Jörg Bahnemann, Vice Chairman of NATO’s Air Defense Committee, estimated that attrition rates would range between ten and twenty percent in the initial period of war.

In the opening stages of a European war one must consider the likelihood of massive attrition on both sides—a fact the Soviets fully understand, and for which they plan.³⁶ Posen’s assertion to the contrary notwithstanding, it is by no means clear that the close air support balance favors NATO, particularly when one considers the density of Soviet forward air defenses, their steady advances in the quality and quantity of their attack helicopters, and the advantages they would gain in a first strike. Here as elsewhere, Posen and Mearsheimer make the mistake of thinking that all front-line NATO forces are as well-equipped as those of the United States. They are not, particularly in the area of attack helicopters and long-range SAMs. Moreover, here, as elsewhere, a sound net assessment requires dynamic operational analysis, and not merely static bean counts. The consequences of pin-down attacks on opposing air bases, the potentialities of anti-aircraft systems as well as dogfighting fighter planes, the role of surface-to-surface missiles—all require careful examination of a kind lacking in Posen’s analysis.

34. See Ken Brower, *The Warsaw Pact–NATO Military Balance: Quality of Forces*, Report No. C66 (Sandhurst, U.K.: Soviet Studies Research Centre, 1988).

35. “GI’s in Europe,” p. 24.

36. See Phillip A. Petersen and John R. Clark, “Soviet Air and Antiair Operations,” *Air University Review*, Vol. 36, No. 3 (March/April 1985), p. 50. Petersen and Clark consider that the Soviets would be willing to expend as many as a thousand aircraft in the initial air operation. This willingness to take heavy losses affects Soviet doctrine at all levels, and is quite different from the approach of Western armies.

THE BROADER ISSUE: "SCIENTIFIC METHOD"

The above does not exhaust the list of particular differences between myself and Posen and Mearsheimer, although it may exhaust the patience of those who have read thus far. There are larger differences of method and approach that must be addressed, and which are possibly of greater interest to readers of *International Security*. Of these the most interesting is the implicit claim of Posen and Mearsheimer to be the partisans of scientific method, debating the acolytes of a pre-scientific world view shaped by Sovietological fads and "cults" of the offensive, of surprise, or of arcane knowledge. In their view, only the scientifically-inclined participate in the "real debate," which is carried on chiefly in the pages of this journal. Let us examine this claim to scientific rigor, and to an unassailable orthodoxy derived therefrom.

Most of us would agree that the study of war, like the study of human affairs more generally, is quite different from the study of quantum mechanics, botany, or astronomy.³⁷ But by no means should this lead us to think that social scientists, particularly those interested in strategy, cannot learn from their colleagues in the hard sciences. To consider how they may, let us ask what are those methods of the natural sciences that one can apply to the study of war. In doing so we should look at the views of successful practicing physical scientists, and, in particular, scientists who know something about war. Being mindful of Posen's opinion that I misunderstand "military analytic activity, and the associated activity of military modeling," I suggest that we turn for guidance to the men who founded the field.

Let us start, then, with the views of P.M.S. Blackett, a Nobel laureate in physics who, in fact, pioneered the application of scientific method to the study of war. Blackett led Britain's Operations Research (O.R.) effort during World War II, beginning with his appointment to Britain's Anti-Aircraft Command in the summer of 1940, and continuing in leading O.R. positions in the Royal Air Force Coastal Command and the Royal Navy's anti-submarine warfare organization. He was perhaps the foremost operations researcher of World War II. In his view, O.R. was the "application of scientific method to war."

A broad but eminently reasonable view is that scientific method consists of a systematic method of learning by experience. . . . Scientific method may

37. For a lengthy discussion of this subject, see Herbert Storing, ed., *Essays on the Scientific Study of Politics* (New York: Holt, Rinehart, and Winston, 1962). We could test my hypothesis, I suppose, by submitting these three letters and the articles that preceded them to an impartial board of prominent natural scientists: I, for one, am willing to try that experiment.

be defined as that combination of observation, experiment and reasoning (both deductive and inductive) which scientists are in the habit of using in their scientific investigations. . . . It may be noted that it is the use of appropriate and precise methods of observation and reasoning which make an investigation scientific. . . . One of the initial approaches to any problem must be to examine the material, consider what conclusions can be drawn from it and decide what further information is required and how it can be obtained. . . . The most important qualification [of the operations researcher] is ability to take a broad view of a problem, so that important factors will not be missed.³⁸

Note Blackett's emphasis on accurate observation and data collection, which he places prior to theorizing or modelling—and indeed, without which he deems either activity pointless. Note secondly his insistence that researchers must be able to grapple with the big picture, “so that important factors will not be missed.”

It should be clear by now that Posen's and Mearsheimer's procedures do not meet the first criterion of scientific method, namely, the energetic collection and rigorous examination of data. But do they at least “take a broad view of the problem,” as Blackett requires? In point of fact, they not only admit that they have excluded large areas from their inquiry and stripped much that is important out of their analyses, but present these actions as evidence of their scientific rigor.³⁹ Posen goes further, saying that the only question one may legitimately ask of him is whether his analysis “captures an important, central aspect of the plausible Soviet war effort.” By this he refers to his observation that the Soviets will probably try to break through NATO's front-lines, and that they will use armored forces to do it. Now, one cannot consider this a major, let alone a new and unexpected insight. One cannot regard it, in fact, as anything more than the very beginning of an effort to understand the problem. In their search for simplicity Posen and Mearsheimer, like others who carry their models too far, turn to constructions that caricature reality, rather than represent it. One critic of such analysts

38. P.M.S. Blackett, “The Scope of Operational Research,” in *Studies of War: Nuclear and Conventional* (New York: Hill & Wang, 1962), pp. 199–200, 203. His essay was initially published in the first issue of *Operational Research Quarterly* (March 1950).

39. If they adopted my views, they say, “net assessment would indeed be extremely difficult” (Mearsheimer, reply, p. 131)—which does indicate one area of agreement among us, since I think that *good* net assessment, no matter where it is done, is indeed quite difficult. I find it hard to imagine either Blackett or his American counterpart, Philip M. Morse, suggesting that one should discard important data because they would make a problem “difficult”—quite the opposite, in fact.

has observed that their single-minded quest for simplifying models ends by substituting “methodological tools and the question ‘Are they useful or not?’ for the assertion of propositions about the world and the question, ‘Are they true or not?’”⁴⁰

There are many other dangers in such an approach. In the first instance, it has led Posen and Mearsheimer (and many others) to the wrong starting point in their analyses: it has made them feel free to refrain from a sophisticated political analysis of how a war in Europe might break out. Yet examination of that issue is, as Clausewitz put it, “the first and most comprehensive of questions.” Posen tells his critics that they may ask only whether his political assumptions are “naively favorable” to one side or the other. The proper question, however, is whether those assumptions are simply naive. And it *is* naive to assume, for example, that both coalitions will hold together firmly in a war. Coalitional fragility can, as I have argued, cut either way, but that does not mean that we can refrain from analyzing the problem altogether. When one forgoes serious political analysis, one falls back on “theoretical” axioms. Mearsheimer, for example, takes this course when he asserts the theoretical proposition that “when states are threatened by an aggressor, they usually do not bandwagon with their foe, but balance against it.”

This view would, I think, have come as a surprise to President Eduard Beneš on the dark day fifty years ago when his nation was bound and delivered to Adolf Hitler, or when he learned a few days later that his East European neighbors—soon themselves to fall victim to the same foe—would each carve their preferred slices from the rump state that remained. It would have surprised French generals who wished to engage in cooperative force deployments with Belgian forces in the months and years before May 1940. It would have surprised those English statesmen who pleaded with Irish authorities to provide ports and air bases to a hard-pressed Britain in the first year of World War II, or who begged the Swedes to deny the Wehrmacht transit rights across their nominally neutral land.

40. Hedley Bull, “International Theory: The Case for a Classical Approach,” in Klaus Knorr and James N. Rosenau, eds., *Contending Approaches to International Politics* (Princeton: Princeton University Press, 1969), pp. 20–38. Nor is this the view simply of old-fashioned scholars. See Hugh J. Miser, “Science and Professionalism in Operations Research,” *Operations Research*, Vol. 35, No. 2 (March–April 1987), p. 315.

There is a seeming moderation in Posen's and Mearsheimer's initial claims about their models, in their warnings about the dangers of reification and the like. But as Hedley Bull observed:

The freedom of the model-builder from the discipline of looking at the world is what makes him dangerous; he slips easily into a dogmatism that empirical generalization does not allow, attributing to the model a connection with reality it does not have, and as often as not distorting the model itself by importing additional assumptions about the world in the guise of logical axioms. The very intellectual completeness and logical tidiness of the model-building operation lends it an air of authority which is often quite misleading as to its standing as a statement about the real world.⁴¹

And indeed, what Posen and Mearsheimer portray as a modest endeavor ends up as something quite different. Thus, Posen concludes one of his analyses with the declaration that those who disagree with him are motivated at best by an "exaggerated manifestation" of normal uncertainty about combat outcomes, or by what he considers equally likely, "a disingenuous public relations campaign stimulated by annual budget battles," or by "a confession of strategic, organizational, and tactical incompetence of a higher order." This is hardly scientific modesty about the powers of a "simple model."⁴²

This overconfidence goes further. Posen and Mearsheimer have inaccurately described my references to the previous and current SACEURs as advocacy of unthinking deference to their views. In fact, I did not and do not "bow" to their authority. Rather, I noted their rather substantial disagreements with Posen and Mearsheimer, and suggested, at the end of my article, that: "practitioners of net assessment must commit themselves to analytic eclecticism. This requires a willingness to factor in not only many varieties of academic analysis but reasoned and intuitive judgments made by men like General Rogers. We must seek many windows into an intellectual and practical problem of staggering complexity and obscurity."⁴³ Posen and Mearsheimer, however, take the view that "[General] Rogers' views lie outside

41. Bull, "International Theory," p. 31.

42. Barry R. Posen, "Is NATO Decisively Outnumbered?" *International Security*, Vol. 12, No. 4 (Spring 1988), p. 202. I mention similar confidence expressed by Mearsheimer in Cohen, "Toward Better Net Assessment," p. 51.

43. Cohen, "Toward Better Net Assessment," p. 88.

the boundaries of the real debate about the balance."⁴⁴ This, very simply, is hubris. One should not accept uncritically the views of past and present SACEURs, but to deny them *locus standi* in discussions of the NATO–Warsaw Pact military balance reflects a failure to understand the real world, let alone “the real debate.”

Both Posen and Mearsheimer take a rigid view of military balances and warfare, which they see as governed by immutable laws (the 3:1 rule), models (the Attrition–FEBA Expansion Model) and ratios (1.2:1, from 1976 to 1989). If one believes Mearsheimer’s statements about Field Manual 100-5 (the 1976 version, which, as I observed in my article, was rejected root and branch by the Army in the early 1980s), one would think that it contains in the 3:1 rule the essential truth about warfare at all levels, from a thousand-man infantry battalion to a million-man Army Group, and at all times. If one believes Posen’s description of the Attrition–FEBA Expansion Model (whose workings remain, to this day, opaque), it captures the eternal verities about armored breakthroughs, although he has not reported testing it against any historical case such as May 1940 or June 1967.

The pity of this is that they and analysts who use similar methods—be their conclusions optimistic or pessimistic—fail to understand the balance today, and run the risk of failing to see a qualitative transformation in the European conventional balance that has begun already, and looks likely to dominate the scene for the next twenty years. That transformation is, first and foremost, political: it involves the pressures boiling up in Eastern Europe and the Soviet Union itself for fundamental changes in Communist political systems. At the same time, one sees in Western Europe a diminishing consensus on conventional defense, and, because of demographic and budgetary pressures, a likely stagnation or decline of resources devoted to defense. The second transformation is military-technical in character; indeed, the Soviets describe it as nothing less than a “revolution in military affairs.” New military technologies are emerging which greatly enhance the range, precision, and lethality of conventional munitions; these technologies reinforce the growing importance of information—assembling it, manipulating it, denying it to the enemy, and communicating it to one’s own forces. The result looks to be forms of warfare in which operations are conducted simultaneously at great

44. They argue this in part because General Rogers and his successor are alleged not to have written or spoken at length about this subject, which is hardly true. Most recently see *NATO Defense Hearings*, Part II, pp. 100–214.

depths on both sides, in which loss rates are very high, and in which the initiative can swing back and forth between attacker and defender. To understand such battles, most of our current concepts—driven by an essentially linear picture of war—are inadequate, and our measures of effectiveness problematic.⁴⁵

A very considerable challenge awaits students and practitioners of net assessment in general, and of the military stand-off in central Europe in particular. I suspect that in ten or even five years' time much of the current debate will look hopelessly obsolete, of interest as intellectual history, perhaps, but not as a depiction of the real world. One hopes that analysts—in government and outside it—can proceed to new and better assessments. This will not occur, however, unless they think hard and self-critically about their methods before they begin battling about their conclusions.

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45. The demographic, social, and military-technical changes are discussed in Andrew W. Marshall and Charles Wolf, eds., "Sources of Change in the Future Security Environment: A Paper by the Future Security Working Group, submitted to the Commission on Integrated Long-Term Strategy" (Washington, D.C.: U.S. GPO, 1988). A much longer paper is forthcoming shortly. On the "revolution in military affairs," see Notra Trulock, Kerry Hines, and Anne Herr, *Soviet Military Thought in Transition: Implications for the Long-Term Military Competition*, PSR Report No. 1831 (Arlington, Va.: Pacific-Sierra Research Corporation, 1988).