The Gulf War Debate *Redux*:

Why Skill *and* Technology Are the Right Answer

Stephen Biddle

Why was the Coalition loss rate in the Gulf War so extraordinarily low? In "Victory Misunderstood: What the Gulf War Tells Us about the Future of Conflict," I argued that a synergistic interaction between skill and technology offers the best explanation, and one that implies a very different future than does the orthodox view of the war.[1]

Daryl Press, Thomas Keaney, and Thomas Mahnken and Barry Watts disagree. Press holds that the outcome was overdetermined: superior Coalition technology and skill were both sufficient but neither was necessary.[2] Keaney, although offering no explicit alternative to my argument, implies that air technology was the predominant factor.[3] Mahnken and Watts advance "situational/friction awareness" as a new explanation.[4] These contributions enrich the debate: Press advances a view that, although probably widely held, has never been argued systematically in print; Keaney offers a further defense of one of the main current explanations that my article attacks; and Mahnken and Watts propose a wholly novel theory.

None of these alternatives succeeds, however. Nor do their critiques stand up to analysis. Space constraints preclude a detailed response to each of their arguments here; however, an extended rebuttal is available elsewhere.[5] For now, I address four central problems that recur in various forms across the

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The author would like to thank Tami Davis Biddle, Peter Feaver, Wade Hinkle, Victor Utgoff, and David Welch for their helpful comments.

1. Stephen Biddle, "Victory Misunderstood: What the Gulf War Tells Us about Conflict in the Future," *International Security*, Vol. 21, No. 2 (Fall 1996), pp. 139–179. Further references are by page numbers in the text.
2. Daryl G. Press, "Lessons from Ground Combat in the Gulf: The Impact of Training and Technology," *International Security*, Vol. 22, No. 2 (Fall 1997), pp. 137–146.
3. Thomas A. Keaney, "The Linkage of Air and Ground Forces in the Gulf War: Evaluation of the Battle of 73 Easting," *International Security*, Vol. 22, No. 2 (Fall 1997), pp. 147–150.
4. Thomas G. Mahnken and Barry D. Watts, "What the Gulf War Can (and Cannot) Tell Us about the Future of Warfare," *International Security*, Vol. 22, No. 2 (Fall 1997), pp. 151–162.
5. See Stephen Biddle, "Commentary on 'Victory Misunderstood'" (Alexandria, Va.: Institute for Defense Analyses, 1997), IDA D-2014; available at http://www.ida.org/DIVISIONS/sfrd/crp/commentary_on_vm.htm.

International Security, Vol. 22, No. 2 (Fall 1997), pp. 163–174

three articles. Two involve misunderstandings of my article (and especially of my dependent variable and methodology for explaining it); the other two involve difficulties in the critics' cases for their preferred explanations (especially problems with their case studies, and of causal attribution in their analysis of air power in the Gulf).

What to Explain (and Why)

The three critiques are often vague on just what the question is—and of course, different questions yield different answers. I was quite explicit as to the question to be addressed: Why was the Coalition loss rate unprecedentedly low? Under the subhead "The War's Military Outcome and Its Legacy" (pp. 141–143), I argued that the war's sweeping policy and intellectual importance stems from the fact that the Coalition loss rate was historically unprecedented among mechanized attackers, and that this extremum came as a major surprise. The surprise was not that the Coalition would prevail, or that its losses would be much lower than the Iraqis'—both these results were widely expected. The surprise was that Coalition losses proved far lower than even the most optimistic prewar forecasts. Coalition casualties were less than one-tenth of the most common expectation, less than one-two-hundredth of reported official projections, and less than one-third of even the most optimistic assessment on record (p. 142). They were far lower than those of even the most successful historical attackers: the Israeli loss rate in the 1967 Six-Day War was higher by more than a factor of 10; German loss rates in 1939–40 were higher by more than a factor of 20 (ibid.). By contrast, had the prewar projections of a one-sided but historically unremarkable outcome been realized, the result would hardly have been surprising, and we would not today confront a groundswell of opinion that we face a revolution in military affairs: What would be "revolutionary" about re-creating a twenty-four-year-old result?

The critiques, on the other hand, while vague, are often aimed at less demanding dependent variables.[6] This is especially problematic in Press's analysis. He never offers a specific definition of the outcome to be explained.

6. Mahnken and Watts, for example, sometimes note the "unprecedentedly low" nature of Coalition losses (e.g., p. 158), but at other points they appear to be focused primarily on whether the Iraqis could win the war, as when they note that air power undermined Saddam Hussein's strategy for prevailing (p. 159). Winning per se is a much less demanding dependent variable than winning with a historically unprecedented low loss rate.

He variously describes the 1991 outcome as "one-sided," "lopsided," "decisive," or as "a rout," but never tells us what is needed to qualify for this status.[7] He does, however, put at least the 1967 Arab-Israeli War in the same category, and uses 1967 to corroborate his conclusion that skill imbalance alone can create such a result.[8] Skill imbalance alone did create a "one-sided" outcome in 1967, but this was a far cry from what happened in 1991: in fact, the difference between the two is the whole point. To redefine the dependent variable so as to lump together a more than 900 percent difference in loss rates (and maybe even more: Was 1940 also "one-sided?") is to define away the very question that has driven the Gulf War's policy and intellectual legacy. The reason I defined the dependent variable as I did was to capture the unprecedented and surprising nature of the 1991 outcome, because this is what gave rise to the war's sweeping postwar influence. By substituting a much less demanding question, Press moves the goalposts close enough for his own theory to succeed (although his analysis is clouded by problems with his cases, as noted below), but this hardly contradicts my analysis. Whether "one-sidedness" was overdetermined in 1991, a historically unprecedented loss rate clearly was not. At a minimum, to explain an unprecedented outcome one must identify some important, historically novel explanatory feature. My analysis does, in the form of a synergistic interaction between skill imbalance (present before 1991) and new technology. Press's theory, by contrast, specifically insists that no new feature was necessary. This cannot be a correct answer for the question I posed, and the latter is the more important one for policy and scholarship.

Methodology

"Victory Misunderstood" uses a combination of complementary methods to assess the theories in the literature and advance an alternative. These methods include process tracing, subunit analysis, deductive argument, and counterfactual experimentation. My critics, on the other hand, focus on individual pieces, which they represent as the totality of the argument. Research designs, such as mine, that combine multiple methods are intended to overcome the shortcomings of individual techniques by integrating them, covering the weak-

7. Press, "Lessons."
8. Ibid., pp. 142–143.

nesses of some with the strengths of others to produce a stronger analysis than any single method could provide.[9] By taking pieces of that design out of context, misconstruing their function, and simply ignoring the rest, the critics thus distort the actual research. Although the result is easier to attack, it is at best a caricature.

The most serious distortion is in Keaney's, and Mahnken and Watts's papers. Mahnken and Watts assert that my "analysis of the war rests almost entirely upon a computer reconstruction of a single brief engagement between the U.S. 2nd Armored Cavalry Regiment (ACR) and the 18th Mechanized Brigade of the Republican Guard's Tawakalna Division at 73 Easting on February 26, 1991."[10] Similarly, eight of Keaney's thirteen paragraphs concern 73 Easting and the Janus simulation of the battle, while the other five address only Keaney's own views on air power's effects. I can only conclude that they did not read my article very carefully. The simulation experiments that allegedly constitute the whole basis of my analysis take up about eight of the article's forty pages, and occur near the end of the piece, following more than twenty-six pages of process tracing, subunit analysis, and deductive argument that apparently escaped the attention of my critics. The role of the article's eight pages of experimentation, moreover, is certainly *not* to provide some kind of minute random sample from the population of all possible combat outcomes so as to induce therefrom a general theory of conflict, as Keaney, and Mahnken and Watts apparently believe. Such a procedure would face far greater epistemological problems than those they describe, were that actually what I had done.[11]

Instead, I use experimentation as one of several methods for testing theories that are external to the tests. Different methods are used to allow each to do what it does well without requiring individual methods to address issues for which they are unsuited. Theories of air power's effects, for example, were thus tested using process tracing (pp. 149–152). The evidence for this assessment is drawn in part from the Battle of 73 Easting's historical database (*not* the

9. For a more detailed defense of such "syncretic" research designs, see Donald Polkinghorne, *Methodology for the Human Sciences: Systems of Inquiry* (Albany: State University of New York Press, 1983), pp. 252–256, 258; see also Jack Snyder, "Science and Sovietology: Bridging the Methods Gap in Soviet Foreign Policy Studies," *World Politics*, Vol. 40, No. 2 (January 1988), pp. 169–193.
10. Mahnken and Watts, "What the Gulf War Can (and Cannot) Tell Us," pp. 154–155.
11. For a more detailed discussion of methodological issues in the use of computer simulation models for counterfactual experimentation, see Stephen Biddle, "The Determinants of Offensiveness and Defensiveness in Conventional Land Warfare," Ph.D. dissertation, Harvard University, 1992, pp. 216–223.

computer simulation), in part from the broad secondary literature on the Gulf War (eight separate sources are referenced, and only space constraints prevented a more extensive listing of additional works—none of which concerns 73 Easting), in part from the non–Gulf War military historical literature, and extensively from the Gulf War Air Power Survey, or GWAPS (which I am puzzlingly held to have overlooked). Other existing theories were tested using a similar variety of evidence as befitted the nature of the theories themselves (pp. 152–157). Computer simulation, by contrast, was used only to test a new theory whose plausibility was first established via an argument that it could overcome the identified limitations in existing explanations, and whose specific content was then motivated by an extended deductive argument (pp. 157–165). This argument—and the resulting theory—includes the effects of air power as well as ground weaponry, *both* of which it treats as extremely important. Because the new theory is specified in terms of technological sophistication generally, however—either advanced air technology or advanced ground technology (or both) is sufficient to meet its terms—it can thus be tested by examining variations in any of several technologies and still provide a challenging test. This enabled me to exploit Janus's particular strengths (especially its ability to represent the effects of mistakes in ground-force employment). It hardly means that the theory ignores air power, however, and it certainly does not mean that a theory and a test are the same thing, as Keaney, and Mahnken and Watts appear to believe.

Press, on the other hand, overlooks the experimental findings. Simulation cases E and F test directly his thesis that lesser technology was unnecessary for the 1991 outcome, yet do so under conditions more relevant for the majority of the war's close combat than Press's case study (see below). The counterfactuals' findings directly contradict his argument: in the experiments, U.S. casualties rise by a factor of 8 to 20. Similarly, case A deals directly with his argument that a skill differential was unnecessary, and again contradicts his position: U.S. losses in the counterfactual increase by more than a factor of 20. Press acknowledges parts of this in two footnotes to his introduction (fns. 2 and 3), then simply ignores the implications for his analysis and proceeds as though the experiments did not exist.

The Critics' Case Analyses

Two of the articles offer short case studies that my critics hold to contradict my conclusions, and that they use in varying degrees as evidence for their own theories. Press, for example, argues that the al-Burqan counterattack shows that skill alone is sufficient to produce "one-sided" outcomes, and that the U.S. 1st Armored Division's attack at Wadi al-Batin shows that technology alone is sufficient. Both conclusions are highly problematic—either for Press's dependent variable or for mine.

PRESS: AL-BURQAN AS EVIDENCE THAT SKILL IS SUFFICIENT

Al-Burqan is a poor case for Press's purposes. It was one of only two episodes in the Gulf War in which Coalition forces fought on the tactical defense against a large-scale Iraqi attack (the other being al-Khafji).[12] By contrast, the great bulk of the war's close combat involved U.S. assaults on prepared Iraqi defenses-in-place (as at 73 Easting). Attack and defense are very different in many respects, among these being likely casualty levels. Frontal assaults on prepared defenses are normally very costly for attackers who press the issue home, as U.S. forces did in the Gulf. Defenders, by contrast, enjoy important tactical advantages that substantially reduce their casualty exposure. Coalition attackers' ability to prevail with radically low losses in repeated, ordinarily very costly frontal assaults was remarkable and historically unprecedented. If this had happened without access to high-technology weapons, it would contradict my explanation. This is not what happened at al-Burqan. Tactical defenders have long been able to repel attacks with very low losses.[13] The ability of poorly equipped Marines at al-Burqan to repel an indifferently executed attack without loss thus does not imply that the same result could be achieved in the much riskier and more demanding mission of tactical assault on prepared defenses—which made up the great majority of the Coalition's ground combat.[14] Al-Burqan shows only that, unsurprisingly, easier tasks are attainable with less advanced equipment.

12. A third, involving elements of the U.S. 24th Mechanized Division, was fought after the cease-fire. See Michael Gordon and Bernard Trainor, *The Generals' War: The Inside Story of the Conflict in the Gulf* (Boston: Little, Brown, 1995), pp. 435–442.

13. Illustrative examples include Israeli Major General Dan Laner's loss-free 1973 defense against an Iraqi armored division attack, the German defense against the abortive British tank charge at Ruweisat Ridge in North Africa in 1942, and the British defense against the Germans' dithering attack in the Battle of Medenine in 1943. See Chaim Herzog, *The War of Atonement: October 1973* (Boston: Little, Brown, 1975), pp. 137–138; P.G. Griffith, "British Armoured Warfare in the Western Desert, 1940–43," in J.P. Harris and F.H. Toase, eds., *Armoured Warfare* (New York: St. Martin's, 1990), pp. 70–88, at p. 71; and I.S.O. Playfair and C.J.C. Molony, *The Mediterranean and the Middle East*, Vol. 4: *The Destruction of the Axis Forces in Africa* (London: Her Majesty's Stationery Office, 1966), pp. 324–326.

14. The Marines' attacks on prepared defenses, unlike al-Burqan, were conducted under a high-tech air umbrella that enabled them to punish Iraqi mistakes in very lethal ways (pp. 162–163,

PRESS: WADI AL-BATIN AS EVIDENCE THAT TECHNOLOGY IS SUFFICIENT

Press's analysis of Wadi al-Batin suffers from a number of problems, among the more serious being inaccurate characterization of my theory's predictions for the case.[15] He boils down my explanation to an effectively univariate relationship between attacker losses and engagement range, notes that the range was short here, and thus concludes that my theory must predict high attacker losses. Because losses were low, he finds that the case contradicts the theory. This badly mischaracterizes the theory, whose condition for high attacker losses is high defender skill, not short engagement range (pp. 161–165). Although a skilled defense sometimes reduces range, this does *not* mean that any time range is short, the defender must therefore be highly skilled. The Iraqis at Wadi al-Batin displayed, inter alia, the same poor marksmanship, poor vehicle maintenance, poor fire control, inability to coordinate different combat arms, and slow crew evolutions of Iraqi troops elsewhere.[16] In fact, at Wadi al-Batin, short range is the only—even partial—indication of skill on the Iraqis' part. Press himself acknowledges this: "In every engagement, Iraqi troops were far less skilled than their U.S. and British opponents."[17]

Press justifies coding a case of remarkably unskilled defense as just the opposite by misinterpreting the simulation experiments I use to test the theory.[18] My theory holds that any of a variety of the Iraqis' manifold errors were

174). Note that Press's counterargument—that a Marine advance into the oil field also succeeded without loss—confuses a meeting engagement with an assault on a prepared defense. In meeting engagements, two forces on the move collide, with neither enjoying the advantages of prepared defensive positions. The Iraqis in the oil field were either moving or in unprepared assembly areas; success with this easier problem hardly establishes that a frontal assault on prepared positions (the modal combat action in the Gulf) would have produced the same outcome.

15. Other problems include his arguments that the M1A1 is "nearly invulnerable" and that only tanks need be exposed to enemy fire: neither is the case. See Biddle, "Commentary."

16. See, for example, Robert H. Scales, Jr., *Certain Victory: The United States Army in the Gulf War* (Washington, D.C.: Office of the Chief of Staff, U.S. Army, 1993), pp. 267–270. Press's only mention of other aspects of skill is to argue that inaccuracy was not "the principal problem" because "the Iraqis scored direct hits on U.S. tanks, but their T-72s were simply unable to penetrate the advanced armor of the U.S. M1s even at very close range" (Press, "Lessons," p. 144). He thus implies that the Iraqis scored plenty of hits but none penetrated. Neither is the case. At least seventy-six T-72s in this battle recorded a total of only four to perhaps eleven hits, at point-blank range, from stationary firing positions, against fully exposed targets (Biddle, "Commentary"). This falls rather short of crack marksmanship. Even so, the Iraqis managed to penetrate and destroy four M1s at Wadi al-Batin, one of which was gutted by fire. See Scales, *Certain Victory*, pp. 269–270. This is hard to square with a claim of impenetrable armor. For a more detailed discussion of Press's claim that the M1 is simply too tough for skill to overcome, see Biddle, "Commentary."

17. Press, "Lessons," p. 143.

18. Press ignores their findings; he misinterprets their structure.

sufficient to provide openings for Coalition technology to exploit (ibid.). The experiments focus on two representative examples (poor position preparation and poor tactical warning). The experiments, however, explicitly assume *error-free Iraqi performance in all respects other than these two mistakes* (as I state in fn. 84 on p. 166). Janus assumes U.S.-level troop performance unless errors are explicitly introduced. By adding two such mistakes, I show that these alone were sufficient to offer the needed openings. When these two are removed, however, the result is not merely a slightly less error-ridden performance, as Press assumes, but U.S.-quality troop skill with Iraqi equipment (as I state explicitly, both in fn. 84 and in the initial subhead of the section: "Technology against an Error-Free Defense" means just what it says). For the Iraqis to get anywhere near such performance levels would require vastly more than just better position preparation (much less shorter range, which is only one of several advantages from proper preparation), and would require much more than merely progressing from dozens of serious errors to one fewer than that. I argue explicitly and at some length that partial improvement buys a defender little—very high levels of defensive skill are required to offset the lethality of modern weapons (pp. 169–170). The Iraqis in the Gulf War have been called many things, but "very highly skilled" is rarely among them.

MAHNKEN AND WATTS ON AL-KHAFJI AND AL-BURQAN

Mahnken and Watts, by contrast, are rather vague on just what their cases (al-Khafji and al-Burqan) are supposed to show. Their purpose is described only as highlighting "aspects of the war that an exclusive focus on 73 Easting obscures" and as illustrating "the importance of [friction and situational awareness]."[19] It is difficult to know what to make of such purposes. My theory is not simply a generalization on 73 Easting; for Mahnken and Watts to observe that some aspects of some other events differ from those of an event used to test a theory is hardly grounds for overturning the theory. Absent a more systematic argument, such an observation has very little meaning as evidence. They imply that al-Khafji and al-Burqan were more representative of the war's ground combat than 73 Easting. Yet this strains credulity: Do they really mean that the war's only two large-scale Iraqi counterattacks are more representative of the ground war than a Coalition offensive? As for their cases and their own theory, mere illustration is among the weakest uses of case studies.[20] Yet this

19. Mahnken and Watts, "What the Gulf War Can (and Cannot) Tell Us," pp. 155, 156.
20. At most, Mahnken and Watts's cases approximate what Harry Eckstein terms "plausibility probes"; see Harry Eckstein, "Case Study and Theory in Political Science," in Fred I. Greenstein and Nelson W. Polsby, eds., *Strategies of Inquiry* (Menlo Park, Calif.: Addison-Wesley, 1975),

is the only specific evidence advanced in defense of their situational awareness/friction thesis. Moreover, the illustration is at best an awkward one: in spite of atypical lapses in U.S. "situational awareness" (the al-Khafji attack initially took Coalition forces by surprise, as did al-Burqan),[21] the Coalition nevertheless defeated the Iraqis with heavy losses to them in both cases. Does this really illustrate the importance of situational awareness for military success? Perhaps net "situational awareness" still favored the United States even at its least-favorable moment; however, as we are given no basis for characterizing anyone's level of this variable, it is hard to know. In any event, Mahnken and Watts themselves provide no reasons to explain this unusual strategy of presenting only evidence that appears to contradict their own theory.

Attributing Outcomes to Air Power

Keaney, and Mahnken and Watts see an impressive array of effects from Coalition air power. Some of these are simply wrong, as in Keaney's assertion that air power denied the Iraqis the ability to move or be reinforced.[22] The entire Iraqi blocking force moved from its pre-ground-war locations to occupy the positions from which they met the VII Corps attack (pp. 146, 151). The al-Burqan counterattack, as Press also points out, represented a multibrigade Iraqi movement that reached close combat with the Marines without being thwarted by Coalition air forces en route. The Iraqi battalion that occupied al-Khafji obviously moved to reach it, and did so unscathed.[23] Although some Iraqi movements suffered heavily from air attack, others did not.

Other parts of their argument are hard to square with the facts on the ground. Keaney declares that air power rendered the Iraqis incapable of "even tactical success," and Mahnken and Watts claim that "those Iraqi units that did survive [the air campaign] to fight were irrelevant to the outcome of the conflict."[24] Yet GWAPS's own figures indicate that more than 4,000 Iraqi armored vehicles survived the air campaign. At least 1,200 of these occupied dug-in positions directly across VII Corps' axis of advance and are known to

pp. 79–137. On the weakness of multiple plausibility probes as evidence, see Alexander L. George, "Case Studies and Theory Development," in Paul G. Lauren, ed., *Diplomacy: New Approaches in History, Theory, and Policy* (New York: Free Press, 1979), pp. 43–68, at p. 52.
21. See, for example, Gordon and Trainor, *The Generals' War*, pp. 278–279, 363–369.
22. Keaney, "The Linkage of Air and Ground Forces," p. 148.
23. Gordon and Trainor, *The Generals' War*, pp. 276–279.
24. Keaney, "The Linkage of Air and Ground Forces," p. 148; and Mahnken and Watts, "What the Gulf War Can (and Cannot) Tell Us," p. 159.

have fought back when attacked. More than 100 fought at 73 Easting alone (pp. 149–152). Not even Keaney, or Mahnken and Watts dispute the 73 Easting computer simulations as a representation of 73 Easting itself; the simulation findings, however, strongly suggest that the 18th Brigade of the Tawakalna was technically capable of defeating the elements of the U.S. 2nd ACR that struck it, and defeating them with very heavy U.S. losses—perhaps 70 percent of the U.S. forces engaged (p. 167). That sounds like a "tactical success" to me, whatever the 18th's fate might have been in subsequent fighting. Alternatively, to assert that 1,200–4,000 intact, actively resisting armored vehicles were "irrelevant" seems rather a stretch—the whole Israeli army in 1967 had only 1,000.[25]

Many of the effects they cite, although true enough, are unlikely to have been caused by Coalition air attack. Keaney, and Mahnken and Watts attribute the Iraqis' lack of military proficiency essentially to the air campaign. The Iraqis were certainly inept, as I point out at some length (pp. 159–160), and there was certainly an extended air attack—but this does *not* mean that the latter caused the former. In the Iran-Iraq War, the Iraqis faced no meaningful air threat at all, yet their professional skills were hardly better than in 1991.[26] Was Coalition air attack really responsible for Iraqi engineers not knowing how to prepare a vehicle fighting position? Did air power really render Iraqi commanders unable to arrange radio protocols with regular check-in times for brief messages from scouts?[27] Was it really air power that made the Iraqis launch counterattacks without bothering to call for artillery support? Was air power the reason the Iraqis proved unable to operate stalled vehicles that U.S. troops subsequently drove off the battlefield as prizes following minor maintenance? Keaney, and Mahnken and Watts assume that prior to January 17, 1991, the Iraqis would have been free of these (and many other) shortcomings, which appeared only as a result of Coalition air attack; yet the Iraqis' comparably poor performance in a decade-long, nearly air power–free war contradicts this assumption. Far more likely is that the Iraqis simply lacked the necessary professional skills. Although air power surely exacerbated these shortcomings (as I argue on pp. 162–163, 174), it did not create them.

25. Anthony H. Cordesman and Abraham R. Wagner, *The Lessons of Modern War*, Volume 1: *The Arab-Israeli Conflicts, 1973–1989* (Boulder, Colo.: Westview, 1990), p. 15.
26. See, for example, Anthony H. Cordesman and Abraham R. Wagner, *The Lessons of Modern War*, Volume 2: *The Iran-Iraq War* (Boulder, Colo.: Westview, 1990), pp. 412–455.
27. Even one-word messages would prevent tactical surprise, yet be too short to be jammed or intercepted (see Biddle, "Victory Misunderstood," p. 160).

Some of what they argue, however, is not really testable. Scholarship requires that causal claims be articulated with enough clarity to allow evidence of some kind to sustain or refute them. Yet I am not sure how to assess a claim that any given Iraqi unit was in "disarray," or that an important military outcome can be traced to shattered "Iraqi armed forces' preconceptions."[28] I evaluated all available claims for the effects of air power that were either clear enough to be testable, or for which I could reasonably infer testable implications. If air power advocates feel there are important effects that I have not addressed, then I invite them to articulate causal mechanisms that enable an objective test. Absent that, we are left to accept or reject propositions with enormous consequence, and considerable controversy, as articles of faith.

Of course, this is *not* to argue that air power did not play a pivotal role in the Gulf. Among the more puzzling aspects of Keaney's, and Mahnken and Watts's critiques is their conviction that my article denies any important role to the air campaign. Air power was clearly an important form of the advanced technology that I argue was a necessary condition for the Coalition's radically low losses. It played an especially important role in enabling the less well equipped Marines to defeat actively resisting Iraqi armor with such low losses (as I state explicitly on p. 174). While those who attribute the Gulf outcome solely or primarily to the air campaign overstate their case, it was certainly an important contributor and my analysis does not say otherwise.

Concluding Observations

The above review points to two conclusions. First, the analysis in "Victory Misunderstood" is sound. The critics' attacks fall short in part because of misreadings of the article; in part because of faulty case analyses, redefinitions of the question, and mistaken causal attribution; and in part because of simple factual errors.[29]

28. Keaney, "The Linkage of Air and Ground Forces," p. 149; and Mahnken and Watts, "What the Gulf War Can (and Cannot) Tell Us," p. 158.
29. They do have one point, however. Although my figure of 24-percent attrition for Republican Guard armor *is* correct—and cited to the correct page in GWAPS—the citation does not appear the first time the information is used. The source for the number is *GWAPS, Summary Volume,* p. 106, which appears in my documentation for Guard tank attrition when discussed in footnote 42, but not in footnote 31, where Guard tank attrition is first described. It should have appeared in both notes. On the other hand, Mahnken and Watts's assertion that my figures for surviving Iraqi armor are U.S. Central Command's (CENTCOM's) and not GWAPS's is simply wrong. The referenced text in GWAPS provides the GWAPS authors' conclusion that the CENTCOM figures for Iraqi armor losses were high by 300. My figures for Iraqi losses are the CENTCOM estimates

Second, the critics' two new theories offer less satisfactory explanations of the war's key military outcome. Press's case for overdetermination falls afoul of inappropriate case selection and inaccurate analysis of case evidence, which clouds its ability to explain Press's own less demanding dependent variable, much less mine. Mahnken and Watts's "situational awareness/friction" theory is too underdeveloped to assess conclusively. Perhaps unsurprisingly for such a brief exposition, they adduce too little evidence to evaluate it properly, and the theory's causal argument is at best incomplete. Perhaps more surprising, the evidence they *do* provide appears more apt to disconfirm their explanation than to corroborate it on the basis of the partial argument presented. Although the literature is richer for their inclusion and assessment, neither theory can thus yet be judged a success.

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less 300, which is GWAPS's conclusion—not CENTCOM's. The final figures of 2,000 surviving tanks and 2,100 other armored vehicles are the difference between these GWAPS attrition figures and the Central Intelligence Agency's photo-intelligence-derived figures for tanks originally in the theater, again as reported in GWAPS on the pages cited. These figures are correct as I described them.