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Commentary on "Victory Misunderstood"

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PREFACE

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Commentary on "Victory Misunderstood"

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 the orthodox view of the war.¹

This argument has been briefed widely to government, analytic, and academic audiences, and has attracted considerable comment.² It also has stimulated counterarguments by four analysts whose views of the war and its implications differ from mine. Daryl Press holds that the outcome was overdetermined: superior Coalition technology and skill both were sufficient, but neither was necessary.³ Thomas Keaney, though offering no specific alternative, implies that air technology was the predominant factor.⁴ Thomas Mahnken and Barry Watts advance "situation awareness" as a new explanation.⁵ These contributions enrich the debate: Press advances a view that, while 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; while Mahnken and Watts propose a wholly novel theory.

None of these alternatives succeeds, however. Nor do their critiques of my analysis hold up under critical scrutiny. I have responded elsewhere to the most salient of the points raised in these three articles; space constraints, however, precluded a complete, point-by-point rebuttal.⁶ This paper is intended to provide a full response. This

¹ *International Security*, Vol. 21, No. 2 (Fall 1996), pp.139-79. Further references are by page numbers in the text.

² See, e.g., Mackubin T. Owens, "How to Think About the Quadrennial Defense Review," *Strategic Review*, Winter 1997, pp. 5-6 at p. 6; David Evans, "Technology vs. Training," *The Washington Monthly*, March 1997, pp. 49-51 at p. 50. The article also won the Military Operations Research Society's Barchi Prize as the best paper presented at the Society's 1996 Annual Meeting at Fort Leavenworth, Kansas.

³ Daryl G. Press, "Lessons from Ground Combat in the Gulf: The Impact of Training and Technology," *International Security*, Vol. 21, No. 2 (Fall 1997), pp. 137-46.

⁴ Thomas Keaney, "The Linkage of Coalition Air and Ground Power in the Future of Conflict," *International Security*, Vol. 21, No. 2 (Fall 1997), pp. 147-50.

⁵ Thomas G. Mahnken and Barry D. Watts, "What the Gulf War Can (and Cannot) Tell Us about the Future of Warfare," *International Security*, Vol. 21, No. 2 (Fall 1997), pp. 151-62.

⁶ Stephen Biddle, "The Gulf Debate Redux: Why Skill and Technology Are the Right Answer," *International Security*, Vol. 21, No. 2 (Fall 1997), pp. 163-74.

response is organized to parallel the arguments raised in the critics' three papers.⁷ I first address Daryl Press' paper, then Thomas Mahnken and Barry Watts', and conclude with Thomas Keaney's.

Daryl Press

Press holds that the Gulf War's outcome was overdetermined, with Coalition technology and skill each providing sufficient conditions, but neither being necessary. By contrast, I argue that a synergistic interaction between the two was necessary and sufficient. He advances two battles from the war as evidence, holding that the Burqan counterattack shows that skill imbalance alone would still have produced the same outcome even without the Coalition's advanced technology, and that the battle of Wadi Al Batin shows that Coalition technology alone would have produced the same outcome even without a meaningful skill imbalance.

While overdetermination probably has been suspected by many, to date there has been no systematic case advanced for this view. Press thus strengthens the literature by articulating an explicit argument for this position, and does so with enough clarity to allow his analysis to be evaluated by exposure to evidence. He deserves credit for this. Unfortunately, the new theory does not hold up well against that evidence. In particular, there are at least two important problems with Press' analysis — his tacit redefinition of the question, and his problematic case analyses — in addition to a variety of smaller difficulties.

What to Explain (and Why)

First, Press poses a different — and much less demanding — question than mine. I argued, quite specifically, that the important outcome was the Coalition's historically unprecedented low loss rate. Press, though vague on the details, is clearly talking about something far less stringent. I am not convinced that he has answered even his own question adequately, but he certainly has not answered mine, and the latter, not the former, is the one that matters for policy and scholarship.

⁷ As a complete point-by-point response to the critics' arguments, this paper necessarily subsumes the observations made in "The Gulf Debate Redux." This paper is organized and presented very differently, however, so as to provide the most convenient format for direct comparison of the critics' arguments and my responses. Note that because the critics' points overlap in places, this organization induces some degree of repetition, but in exchange it facilitates direct comparison of the critiques and my responses. Where possible, I will note such instances of overlap when they occur.

I was explicit as to my dependent variable and the reasons I chose it. On pages 141-3, under the subheading "The War's Military Outcome and its Legacy," I defined the question as the causes for an historically unprecedented Coalition loss rate that was lower than previous mechanized attackers' by more than a factor of ten. Such a result was wholly unpredicted beforehand; the inaccurate predictions affected the parties' behavior; and the startling gap between prediction and realization is what gave rise to the war's sweeping policy legacy and its dramatic effects on military thought. The surprise here was *not* that the Coalition would win, or even that its loss rate would be much lower than the Iraqis'. The surprise was that the Coalition's loss rate would be radically lower than even the most successful historical attackers'. Prewar projections had used previous Mideast wars as analogies, with the Israelis' dramatic victory of 1967 offered by some as a model for the coming fight — yet the Coalition's loss rate proved to be less than a tenth of even the Israelis'.⁸ 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 be confronted by a groundswell of opinion that we face a revolution in military affairs: what would be "revolutionary" about re-creating a 24-year-old result?

Press, on the other hand, is rather vague as to just what he seeks to explain. He never offers a specific definition, referring to the 1991 outcome variously as "one-sided," "lop-sided," "decisive," "overwhelming," "a rout," or as "very bad for the Iraqis," but without telling us what is needed to qualify for this status.⁹ He does, however, put at least the 1967 Mideast War in the same category, and uses 1967 to corroborate his conclusion that skill imbalance alone can create such a result.¹⁰ 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. In the process, he moves the goal posts 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 or not, an historically unprecedented loss rate clearly was not. At a minimum, to explain an unprecedented

⁸ See refs. in note 10, p.142. These projections were not unreasonable given what was known at the time — that they proved so far off indicates an underlying theoretical gap, and thus an opportunity to expand knowledge by examining the reasons.

⁹ Press, "Lessons," pp. 137, 138, 139, 141, 142, 143, 145.

¹⁰ *Ibid.*, pp. 142-3.

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 (*not* present before 1991). Press' 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.

Problematic Case Analyses: Burqan

Press' case studies are also problematic — especially for addressing my question, but even for his own. He contends that the Burqan counterattack shows that skilled Americans could defeat Iraqis decisively even without advanced technology, and that this shows that the Gulf War would have come out about the same even with older weapons.

Yet Burqan is a poor case for Press' purposes. Burqan 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).¹¹ 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 Burqan. Tactical *defenders* have long been able to repel *attacks* at very low losses.¹² The ability of poorly equipped Marines at Burqan to repel an indifferently executed attack without loss thus does not imply that the same thing 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

¹¹ A third, involving elements of the U.S. 24th Mechanized Division, was fought after the cease-fire: Michael Gordon and Bernard Trainor, *The Generals' War* (Boston: Little, Brown, 1995), pp. 435-442.

¹² Illustrative examples include Israeli Gen. 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, or 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. 137-8; 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 Press, 1990), pp. 70-88 at p. 71; I.S.O. Playfair and C.J.C. Molony, *The Mediterranean and the Middle East, Vol. IV: The Destruction of the Axis Forces in Africa* (London: Her Majesty's Stationery Office, 1966), pp. 324-6.

ground combat.¹³ Burqan shows only that, unsurprisingly, easier tasks are attainable with less advanced equipment.

Press addresses “skeptics” who note as I do that defense is easier than offense (I might add, especially so against attackers who do not press the attack home) by observing that the Marines at Burqan also counterattacked into the advancing Iraqis. Because this action, too, was completed without loss, Press concludes that the rest of the Marines’ offensive would have come out similarly even without advanced technology.¹⁴ This argument, however, 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 positions, cover and concealment, interlocking fields of fire, etc., that characterize the tactical defense. The brief action Press describes involved the Marines striking Iraqi attackers who, at the time of the action, were either advancing, or staging for the attack in unprepared assembly areas. This hardly establishes that a frontal assault on prepared defenses (the modal combat action for Coalition forces in the Gulf) would have produced the same outcome. On the contrary, it is usually thought that, of all tactical forms, meeting engagements are the *most* sensitive to the skill of the combatants — who must improvise on the fly to cope with the fluidity typical of such actions. To use a meeting engagement, whose outcome could be expected to be systematically more favorable to skilled armies than other forms of warfare, to draw conclusions about the relative importance of skill in general is surely to risk a significantly biased conclusion.

Finally, the simulation experiments I described in the paper (esp. pp. 170-171) address precisely the issue Press tries to address via Burqan — unequal skill but lesser Coalition technology — but do so for a Coalition attack against a prepared Iraqi defense-in-place. I thus considered a tactical context more relevant to the war’s other fighting,¹⁵ and the results I obtained directly contradict Press’ conclusions: in the experiments, U.S.

¹³ The Marines’ attacks on prepared defenses, unlike Burqan, were conducted under a high-tech air umbrella that enabled them to punish Iraqi mistakes in very lethal ways (pp. 162-3, 174).

¹⁴ Press, “Lessons,” note 22.

¹⁵ A central advantage of simulation counterfactual analysis is that it affords this level of ex ante experimental control. In Janus, I can control for skill and vary technology while holding other variables constant. In ex post facto case method, Press faces a more difficult problem of extraneous variable control, and fails to control for one of the most basic confounding variables for military theorizing: the difference between offense, defense, and meeting engagements. For a more detailed comparison of ex ante experimental and ex post facto case methodologies, see Stephen Biddle, *The Determinants of Offensiveness and Defensiveness in Conventional Land Warfare* (Ph.D. Dissertation, Harvard University, 1992), pp. 13-16.

losses increase by about a factor of 10 relative to the historical outcome. Yet Press fails to address the experimental findings, or their implications for his theory.

Problematic Case Analyses: The 1st Armored Division at Wadi Al Batin

Burqan thus fails to sustain Press' contention that skill alone can produce Gulf-like outcomes for theater attackers. Alternatively, he contends that the U.S. First Armored Division's action at Wadi Al Batin shows that even skilled defenders cannot defeat the M1A1 tank, and that this shows that technology trumps skill.

Yet Iraqi behavior at Wadi Al Batin tells us very little about the properties of a skilled defense. In suggesting otherwise, Press mis-characterizes both my theory and Iraqi behavior.

As for my theory, he boils it down to an effectively univariate relationship between attacker losses and engagement range. He then notes that range was short at Wadi Al Batin, and thus concludes that my theory must predict high attacker losses. Since losses were low, he finds that the case contradicts my explanation. This badly misrepresents my actual theory, whose condition for high attacker losses is high defender skill, not short engagement range (pp. 161-5). While 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 Press' only mention of skill, other than his mistaken inference from range, is to argue that Iraqi 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. M1 even at very close range."¹⁷ He thus implies that the Iraqis scored plenty of hits but none penetrated. Neither is the case. At least 76 T-72s in this battle recorded a total of only four to perhaps 11 hits, at point blank range, from stationary firing positions, against fully exposed targets.¹⁸ This falls rather short of crack marksmanship. Even so, the Iraqis

¹⁶ See, e.g., 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. 267-70.

¹⁷ Press, "Lessons," p. 144.

¹⁸ Seventy-six destroyed Iraqi T-72s were counted on the Wadi Al Batin battlefield following the engagement, in addition to 84 armored personnel carriers, eight howitzers, and a variety of other combat and support vehicles: Scales, *Certain Victory*, p. 270. For the entire war, the Iraqis scored only seven frontal-arc hits by T-72s on M1s: Enzo Bonsignore, "Gulf Experience Raises Tank Survivability Issues," *Military Technology*, February 1992, pp. 64-70 at 67. Their total for Wadi Al Batin is thus somewhere

managed to penetrate and kill four M1s at Wadi Al Batin.¹⁹ This is hard to square with a claim of impenetrable armor (see below). In fact, at Wadi Al Batin, short range is the *only* — even partial — indication of skill on the Iraqis' part. Even Press himself acknowledges this: "In *every* engagement, Iraqi troops were far less skilled than their U.S. and British opponents."²⁰

Press justifies coding a case of remarkably unskilled defense as just the opposite by misinterpreting the simulation experiments I use to test the theory.²¹ The theory holds that any of a variety of the Iraqis' manifold errors were sufficient to provide openings for Coalition technology to exploit (pp. 161-5). 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 spell out in note 84, 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 note 84 and in the initial subtitle of the section: "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 (and certainly much more than merely 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. 169-70). The Iraqis in the Gulf War have been called many things, but "very highly skilled" is rarely among them.²²

between zero and seven. In addition to these fewer-than-seven frontal arc hits by T-72s, the Iraqis managed another five specifically reported flank/rear hits on U.S. M1s at Wadi Al Batin (four of which were by T-72s): Scales, *Certain Victory*, pp. 269-70. These flank/rear hits penetrated the M1s that they struck. While it is possible that T-72s scored additional flank/rear hits at Wadi Al Batin that Scales does not report, this is unlikely. 115 mm rounds fired at point blank range have sufficient kinetic energy to penetrate these armor faces, as shown by the episodes Scales relates, and additional penetrations would surely have been reported. This implies that T-72s scored somewhere between a minimum of four and a maximum of 11 hits in this battle.

¹⁹ Scales, *Certain Victory*, pp. 269-70.

²⁰ Press, "Lessons," p. 143. Emphasis added.

²¹ Press ignores their findings; he misinterprets their structure.

²² Even if his inaccurate version of my theory were right, however, Wadi Al Batin would provide only a weak test of his own thesis. At most, it would enable him to dispose of one competitor by arguing that my

Moreover, the asserted causal mechanism that underlies Press' claim is very problematic. Press argues that the M1's armor protection affords it "near-invulnerability" that cannot be overcome, regardless of a defender's skills.²³ While the M1 is a fine tank, it is not "nearly invulnerable." In fact, even the Iraqis, notably poor gunners, killed four M1A1s at Wadi Al Batin — at least one of which was a "catastrophic-kill" (i.e., gutted by fire).²⁴ Whatever its frontal arc protection, the M1A1, like all tanks past and present, has much thinner flank and rear armor. Even gunners as inaccurate as the Iraqis occasionally managed to hit an M1 in the flank, and this proved sufficient to kill the tank. In fact, Press' case here contradicts his main argument and corroborates mine: Wadi Al Batin shows that even the world's most advanced tank, the M1, can be killed by defenders who get opportunities for flank shots at appropriate ranges. U.S. commanders are trained to seek flank shots against advancing hostile armor, and to employ their forces in ways that make this more likely.²⁵ The Iraqis did nothing of the kind, with the result that many of

definition of skill includes the Iraqis. Yet his theory implies at least one other obvious logical alternative: that skill by *his* definition of skill is not necessary. Theory testing is a relative business — one need only show superiority to available alternatives, not absolute accuracy — but some alternatives are so obvious they require assessment. If my version of skill were as idiosyncratic as he makes it out to be, it would be tunnel-vision at best to overlook the question of whether his own definition of skill yields the same prediction. In fact, by his definition of skill (like the reality of mine), Wadi Al Batin is an indeterminate test. Press' own view is that the Iraqis were far less skilled (Press, "Lessons," pp. 138, 143 -- in fact, the Iraqis in the Gulf War constitute such a clear extremum on this variable that subtle variations in skill definitions wash out: it is hard to imagine anyone coding Iraqi ground forces as anything but unskilled, whatever their specific criteria). Thus, whether skill imbalance were sufficient (as he holds) or necessary (as he rejects), the prediction for this battle is exactly the same — low U.S. losses. While success in an indeterminate test certainly does not overturn a theory, neither does it provide much of a corroboration.

²³ Press, "Lessons," p. 144.

²⁴ Scales, *Certain Victory*, pp. 267-70. Press also implies that destroying an M1A1 may not kill any soldiers, thanks to the M1's advanced crew protection devices (e.g., blowout doors over the ammunition compartment, or sophisticated fire suppression systems). Such advances certainly help prevent fires and protect against secondary explosions, but they cannot save crew members from any other form of damage. A kinetic energy penetration forward of the ammunition compartment, for example, would cause serious casualties even for an M1 crew. Second, even when hit in a way that these technologies can affect, this only buys the crew the chance to bail out before the vehicle burns. They must then fend for themselves against hostile small arms and artillery without the benefit of armor protection until making their way to safety. This is a lot better than burning to death inside a less well-protected tank, but it is a long way from invulnerability (especially if fighting an opponent skilled enough to coordinate artillery support with its antitank fires). While the M1 is an important advance in crew protection, serving in one is hardly a risk-free undertaking. Finally, even an M/F kill (loss of mobility or firepower) without crew loss removes a tank from the battle. This reduces the attacker's combat capability overall, and conduces to higher losses elsewhere.

²⁵ See, e.g., Headquarters, Department of the Army, *FM 7-91: Tactical Employment of Antiarmor Platoons, Companies and Battalions* (Washington, D.C.: U.S. Government Printing Office, Sept. 1987), esp. section 2-3, "Strive for Flank Shots;" *idem.*, *FM 71-123: Tactics and Techniques for Combined Arms Heavy Forces* (Washington, D.C.: U.S. Government Printing Office, Sept. 1987), e.g., pp. 4-1 to 4-3, 4-39, 4-41, 4-59, etc.; also Daniel P. Bolger, *Dragons at War: 2-34 Infantry in the Mohave* (Novato, Calif.: Presidio Press, 1986), e.g. pp. 185, 190-192.

the few hits they did obtain struck the M1s where their armor is heaviest. One of the many ways that defensive skill mitigates technology's effects is by minimizing the need for frontal arc engagement of opposing armor; had the Iraqis been capable of this, there is every reason to expect that U.S. losses would have been much higher.²⁶

Nor would Press' view of the M1 be sufficient to sustain his case, even if it were correct (which it is not). No army consists solely of M1s. Even if tanks could be made invulnerable — which they cannot — there still would be literally thousands of thinner-skinned combat vehicles in harm's way. In 1991, the U.S. Army's VII Corps, for example, put about as many thinly-armored M2 and M3 Bradleys into close combat as it did M1s, not to mention the hundreds of British Warrior armored personnel carriers, U.S. FST-Vs, LAVs, and AAV7s that fought in the Gulf.²⁷ If all the Iraqis had done was to destroy, say, 10-20 percent of these — without damaging a single M1A1 — they would have caused radically higher Coalition casualties. In response, Press dismisses "skeptics" who counter that tanks are only part of the modern battlefield's population, by proposing that only tanks be exposed to enemy fire.²⁸ Having been, I think, the original skeptic who pointed the problem out to Press, I must say I am still skeptical. The existence of the M2, the M3, the Warrior, and the others is not merely accidental or gratuitously wasteful. Armies field such vehicles because they are needed: modern combined arms tactics require the integrated use of tanks with complementary weapon types that trade armor thickness for other virtues.²⁹ Unsupported tank charges sometimes succeed, but only

²⁶ Flank shots are not rare or bizarre occurrences, especially for skilled defenders. Analyses of U.S. Sherman tank losses in World War II showed that fully 70 percent of all hits by German gunfire were against the Shermans' flank or rear armor faces: Alvin D. Coox and L. Van Loan Naisawald, *Survey of Allied Tank Casualties in World War II* (Ft. Leslie McNair: Operations Research Office, Johns Hopkins University, 31 March 1951) ORO-T-117, declassified 27 March 1978, Figure 12. The frontal arc was hit more often than any other single surface (30 percent of hits, vice 16 percent for either side of the hull above the track line, the next most-hit surface), but the cumulative hits to non-frontal surfaces overall provided the majority of the total. Since the frontal arc is the modal — though certainly not the exclusive — target, tank designers typically allocate it a disproportionate share of the design's total weight of armor. This requires that other areas receive lighter protection. The tank's net expected survivability is higher for this allocation than for a uniform distribution, but this produces "invulnerability" only if opponents strike *only* the heavier frontal arc (and if the frontal arc itself is so heavy as to be impenetrable). This has never been the case historically.

²⁷ FST-Vs are armored vehicles used to carry artillery forward observers into range of enemy fire; LAVs (Light Armored Vehicles) and AAV7s are lightly armored land and amphibious vehicles used by the U.S. Marines in the Gulf. For numbers deployed, see U.S. Department of Defense, *Conduct of the Persian Gulf War*, Final Report to Congress Pursuant to Title V of Public Law 102-25 (Washington, D.C.: U.S. Government Printing Office, April 1992), pp. 736, 739, 747, 750.

²⁸ Press, "Lessons," p. 145.

²⁹ I have elaborated on this point elsewhere; for a more detailed discussion see Biddle, *The Determinants of Offensiveness and Defensiveness in Conventional Land Warfare*, pp. 102-121; for an application to future warfare in particular, see idem, "Assessing Theories of Future Warfare," paper presented to the

where defenders oblige by leaving themselves exposed, as the Iraqis did at Wadi Al Batin. To throw unsupported tanks against a defense which conceals its infantry to U.S. standards (and whose infantry can hit its targets) is extremely costly, given tanks' inherent visibility limitations.³⁰ This is the main reason why infantry exists in offensive formations, and is the whole *raison d'être* for the M2 Bradley. Press' proposal to leave the Bradleys behind would thus work only against opponents as unskilled as the Iraqis (though not all U.S. commanders in the Gulf were prepared to assume even this: Bradleys saw extensive close combat in 1991).³¹ No technology has ever been without exploitable weaknesses (though some have been harder to exploit than others); combined arms techniques and skill in their use have thus been necessary throughout the 20th century, and I see nothing coming in future technology that is likely to change this. Contrary to Press' argument, Wadi Al Batin does not show otherwise.

Other Difficulties

Press' argument simply ignores a variety of other evidence advanced in my paper that contradicts his thesis that technology is sufficient and skill imbalance is unnecessary. Simulation Case A, for example, deals directly with this contention, and contradicts his position: with no change in technology, a highly skilled Iraqi defense caused U.S. losses to increase by more than a factor of 20. By Press' theory, a change in Iraqi skill should have been irrelevant in the face of superior U.S. equipment. While Press notes the simulation results as an aside in a footnote to his introduction, he ignores their implications for his argument and proceeds as though they did not exist. Alternatively, I argue that hundreds of exercises fought at the U.S. National Training Center (NTC) in the Mohave Desert show that superior technology is not sufficient for radically low attacker losses. At the NTC, regular U.S. Army units with the full panoply of M1A1 tanks, M2

Security Studies/U.C. Davis/Office of Net Assessment Conference on the Revolution in Military Affairs, Monterey Calif., August 1996, and to the 1997 International Studies Association Annual Meeting, Toronto, March 1997.

³⁰ In fact, this can be very costly against even moderately well-trained infantry, as the Israelis discovered in the Sinai in 1973: see, e.g., Herzog, *The War of Atonement*, pp. 182-96; Anthony H. Cordesman and Abraham Wagner, *The Lessons of Modern War, Vol. I: Arab-Israeli Conflicts, 1973-1989* (Boulder, Colo.: Westview, 1990), pp. 57-61. On the skill level of Egyptian infantry in 1973, see *ibid.*, pp. 56-7; also George W. Gawrych, "The Egyptian High Command in the 1973 War," *Armed Forces and Society*, Vol. 13, No. 4 (Summer 1987).

³¹ It should also be noted that the First Armored Division at Wadi Al Batin did not actually leave its Bradleys out of close combat as Press implies — the Third Brigade (on which Press focuses) simply rearranged its formation to put tanks in the lead, with the M2s assigned to lay down a base of fire to cover the tanks' advance. This is far from removing the Bradleys from harm's way; it merely increases somewhat the range at which they would be engaged. On Bradley employment at Wadi Al Batin, see, e.g., Scales, *Certain Victory*, pp. 267-9.

Bradleys, thermal sights, modern navigation gear, and all the rest are regularly *defeated* by OPFOR opponents with Iraqi-type equipment but very high levels of skill in its use (p. 153). This, too, directly contradicts Press' argument, yet again he simply ignores it, offering no reasons why this should not be taken as disconfirmatory evidence on his thesis.

Finally, Press argues that only "relative technological advantages" can be "predictors of military success," whereas I am held to be indifferent to the effects of the next 100 years of Iraqi weapons development as a result of focusing on "absolute levels of technology."³² I am not sure I fully grasp his meaning (e.g., "relative technological advantages" — can an advantage *not* be relative?), but I am sure that 100 years of anyone's weapon development is not irrelevant, and I certainly fail to see how my article could be interpreted to suggest otherwise. Press may have gone astray by misinterpreting the minimum technological conditions needed for the 1991 loss rate. Although this was not addressed explicitly in my article, I do not think it was necessary that Iraqi technology was inferior to the Coalition's — I suspect that if the Iraqis had had U.S. equipment but used it with no more skill than they displayed in 1991, the results would have been quite similar. However, this does not mean that an Iraqi army of 2091 would suffer the same fate against a U.S. force armed with 1991 weapons. To say that a technology *advantage* is unnecessary is not to imply that gross *inferiority* is irrelevant. Rough parity at the 1991 U.S. state of the art probably would have been sufficient given the skill differential between the combatants and the performance of 1991 weapons. Rough parity at the 1967 state of the art would not have been. Gross inferiority in either era would surely have been disabling. None of this is inconsistent with anything I argued in "Victory Misunderstood."

Given these difficulties, Press' proposed policy implications are without foundation. Some, however, would be problematic even if the rest of his paper had been sound. For example, he holds his analysis to show that we will be successful in future conflicts if we have at least parity in either skill or technology, as long as we have superiority in the other.³³ This would not follow from his analysis even if his analysis were correct. First, his discussion says nothing about what might happen if we were actually inferior in one category but superior in the other. He claims to have shown that parity in one is *sufficient* if we enjoy superiority in the other, but now he asserts that it is *necessary* even if we enjoy superiority in the other (we require "at least parity"). Nothing

³² Press, "Lessons," note 7.

³³ *Ibid.*, p. 145.

in his analysis even speaks to the matter. Second, his standard for “superiority” is fuzzy at best. As stated, he requires only that our technology be better than our opponents’ — no magnitude of supremacy or overtrump is specified. Yet his causal mechanism as articulated in the Wadi Al Batin case accorded the M1 an extraordinary degree of superiority: effective invulnerability to enemy fire. Is this necessary, or does the M1 guild the lily? The remainder of the paragraph muddies the waters: he implies that “sophisticated Soviet equipment” is enough to cancel U.S. technological advantage. This could be read to imply a need for much more than mere superiority per se (U.S. equipment is clearly “superior” to even “sophisticated” Soviet gear, and is likely to remain so, yet this is evidently not margin enough). As written, however, it also appears to contradict his own case analysis: the Iraqis had bought all the “sophisticated Soviet equipment” Saddam’s oil money could buy, yet Press argues that Iraqi weapons were still decisively inferior to ours at Wadi Al Batin. In the case study, skill plus Soviet equipment was not enough; in the conclusion, it is. Third, his formulation here, unlike my theory, implies that superiority in either skill or technology at any level of technological sophistication is sufficient.³⁴ Yet nothing in his analysis even speaks to any level of sophistication other than that of 1991. At best, this aspect of his conclusion is thus not shown; at worst, it is at odds with substantial historical evidence. As a single example, German *stosstruppen* in March 1918 enjoyed superior skill and at least equality in equipment with their British opponents; by Press’ theory as articulated here, that should presumably have produced “one-sidedness” on a par with 1967 or 1991. The Germans did break through, but their loss rate was at least an order of magnitude higher than the Israelis’ in 1967, and at least two orders of magnitude higher than ours in 1991.³⁵ For me, the level of sophistication matters (this is what Press appears to misinterpret in his discussion of “absolute” technology); Press’ conclusion as given apparently rejects this, yet this either implies consequences seriously at odds with observed experience, or makes his dependent variable even more elastic.

Thomas Mahnken and Barry Watts

Thomas Mahnken and Barry Watts, by contrast, combine their critique of my paper with a new theory of their own, which holds that “situation awareness” and/or

³⁴ Here this is implicit, elsewhere it is explicit: in his notes 2 and 7 he goes out of his way to specify that it is only the *relative* performance of the two sides’ weapons that counts. Twice the range thus has the same effect whether it embodies 100 yards vs 50 yards or 3000 meters vs 1500. He explicitly counterposes this to my formulation in “Victory Misunderstood:” *ibid.*, note 2.

³⁵ Biddle, *Determinants of Offensiveness and Defensiveness*, pp. 241-311.

Clausewitz' concept of friction, offer a superior explanation of the Gulf War outcome. Their critique includes seven specific allegations. They claim I neglect key sources of information, creating a strawman version of the Gulf War debate;³⁶ that I rely too little on the Gulf War Air Power Survey (GWAPS), which I misquote;³⁷ that I rest my analysis of future warfare "almost entirely" on a computer analysis of a single, brief battle (73 Easting);³⁸ that I omit Coalition air power from my analysis;³⁹ that I mis-characterize the state of net assessment;⁴⁰ that I create a strawman of the RMA literature;⁴¹ and that I "dismiss the RMA out of hand."⁴²

None of this, however, is the case. Nor does either "situation awareness" or friction offer a reasonable alternative to the explanation I advance. To show why, I will first address each critique in turn, after which I will evaluate the case for Mahnken and Watts' preferred "situation awareness" thesis.

Do I Neglect Key Sources of Information?

Mahnken and Watts claim I made "selective use of sources,"⁴³ yet their two-page discussion fails to provide even a single example of an appropriate source that I failed to consult. The discussion they hold to substantiate this claim in fact consists of nothing more than their own disagreements with some of the authors cited in my article.⁴⁴ Of

³⁶ Mahnken and Watts, "What the Gulf War Can (and Cannot) Tell Us," pp. 152-3.

³⁷ Ibid., pp. 153-4.

³⁸ Ibid., pp. 154-5.

³⁹ Ibid., pp. 158-60.

⁴⁰ Ibid., p. 154.

⁴¹ Ibid., pp. 160-61.

⁴² Ibid., p. 162.

⁴³ Ibid., p. 152.

⁴⁴ In particular, they are apparently unhappy with Greg Easterbrook, John Mueller, and Norman Schwartzkopf: *ibid.*, p. 152-3. We are left to wonder what they think of the many other histories of the war I cite at length on, *inter alia*, pp. 139, 147, 148, and 153 of my article. All, one must suppose, are likewise deficient. Apparently no adequate history has yet been written, or at least none that Mahnken and Watts bring to our attention.

I might also point out that my article contains no reference to Schwartzkopf. Undeterred, Mahnken and Watts nevertheless judge his book to have been central to my literature review. In fact, they devote much of their discussion to Schwartzkopf's apparent failings, and their argument rests heavily upon their critique of his book as one of just three examples of the deficient histories that they insist I singled out for attack — and the only one they trouble to discuss in any detail: "What the Gulf War Can (and Cannot) Tell Us," pp. 152-3. Scholars sometimes overlook references in others' articles; creating footnotes that do not actually exist is a bit more unusual. Building an argument around a fictitious footnote is, to my knowledge, an original contribution to the literature. Ironically, the Schwartzkopf book, which Mahnken and Watts think I depend upon in spite of its shortcomings as a history and its inconvenient absence from my article, is the *only* piece of Gulf literature mentioned anywhere in their

course, I share Mahnken and Watts' view that the existing literature is incorrect — in fact, the opening sentence of my article states quite unambiguously my conclusion that the literature is wrong — but the literature is what it is. If they are aware of some actual source that makes a different or better argument than the works I cite, then I invite them to identify it. Barring that, it can hardly be claimed that by surveying the major existing work on the war I have somehow created a strawman.

Do I Rely Too Little on GWAPS?

The main basis for this claim appears to be a non sequitur on p. 4, in which Mahnken and Watts assert that two alleged mis-references to items in GWAPS “confirm that the main source of his reinterpretation of the Gulf War is the IDA/DARPA/Army simulation of the engagement at 73 Easting.”⁴⁵ Why two mis-references would confirm that a completely different document provided a main source for an analysis is a puzzle — but either way, I make extensive use of GWAPS. It is cited no fewer than 10 times, and provides the key data for much of my assessment of the effects of air power in the war (pp. 149-52). Mahnken and Watts clearly interpret these data differently than I do, but my disagreement with their analytical conclusions certainly does not mean that I overlooked their work.

They do have one point, though. While 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 note 42, but not in note 31, where Guard tank attrition is first described. It should have appeared in both notes. On the other hand, Mahnken and Watts' assertion that my figures for surviving Iraqi armor are CENTCOM's and not GWAPS' (the second alleged mis-reference) 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 less 300, which is *GWAPS'* conclusion — not CENTCOM's. The final figures of 2000 surviving tanks and 2100 other armored vehicles are the difference between these GWAPS attrition figures and the CIA's photo-intelligence-derived figures for tanks

critique that I do *not* reference. They provide no actual examples of anything I did not reference that they think I should have.

⁴⁵ Mahnken and Watts, “What the Gulf War Can (and Cannot) Tell Us,” p. 154.

originally in the theater, again as reported in GWAPS on the pages cited. These figures are correct as I described them.

Does My Analysis Rest on a Computer Simulation of 73 Easting?

Mahnken and Watts assert that “His analysis of the war rests almost entirely upon a computer reconstruction of a single brief engagement between the American 2d Armored Cavalry Regiment (ACR) and the 18th Mechanized Brigade of the Republican Guard’s Tawakalna Division at 73 Easting on February 26, 1991.”⁴⁶ This attempt to “generalize from a single tactical engagement” is held to be an inadequate basis for building sound theories.⁴⁷

I can only conclude that they did not read my article very carefully. The simulation experimentation that allegedly constitutes the whole basis of my analysis takes up about eight of the article’s 40 pages, and occurs near the end of the piece, following more than 26 pages of process tracing, subunit analysis, and deductive argument that apparently escaped their attention.

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 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.⁴⁸

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 the effects of air power, for example, were thus tested using process tracing (pp. 149-152). The evidence for this assessment is drawn partly from the 73 Easting historical database (*not* the computer simulation); partly 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 concern 73 Easting); partly from the non-Gulf-War military historical literature; and extensively from the Gulf War Air Power Survey (which I am held to have overlooked).

⁴⁶ Ibid., at pp. 154-5.

⁴⁷ Ibid., at p. 159.

⁴⁸ For a more detailed discussion of methodological issues in the use of computer simulation models for counterfactual experimentation, see Biddle, *The Determinants of Offensiveness and Defensiveness in Conventional Land Warfare*, pp. 216-223.

Other existing theories were tested using a similar variety of evidence as befitted the nature of the theories themselves (pp. 152-7). 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. 157-65). 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. Since 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 Mahnken and Watts imply.

Research designs, such as mine, that combine multiple methods are intended to overcome the shortcomings of individual techniques by integrating them, covering the weaknesses of some with the strengths of others to produce a stronger analysis than any single method could provide.⁴⁹ By taking pieces of that design out of context, misconstruing their function, and simply ignoring the rest, Mahnken and Watts thus distort the actual research. While the result is easier to attack, it is at best a caricature.

Do I Omit Coalition Air Power From the Analysis?

Among the more puzzling aspects of Mahnken and Watts' critique is their conviction that my article denies any important role to the air campaign.⁵⁰ 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). Air power was certainly an important contributor to the Coalition's radically low loss rate, and my analysis certainly does not say otherwise.

⁴⁹ 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. 252-6, 258; also Jack Snyder, "Science and Sovietology: Bridging the Methods Gap in Soviet Foreign Policy Studies," *World Politics*, Vol. XL, No. 2 (January 1988), pp. 169-193.

⁵⁰ Mahnken and Watts, "What the Gulf War Can (and Cannot) Tell Us," esp. pp. 158, 160.

This is not to say that the *nature* of its contribution is necessarily as Mahnken and Watts suppose, however. Mahnken and Watts are prepared to attribute a remarkable array of effects to Coalition air power. While they chide me for "omitting Coalition air power,"⁵¹ their own analysis much more closely approximates an omission of Coalition *ground* forces: "By the beginning of the ground campaign, Coalition air power had blinded, bloodied, pinned down, and frustrated a force of some fifty divisions. Those Iraqi units that did survive to fight were irrelevant to the outcome of the conflict"⁵² Mahnken and Watts go on to quote favorably "several" unnamed Iraqi prisoners who "stated openly that the ground campaign was unnecessary, and that, had the air campaign continued two or three weeks longer, the Iraqi army would have been forced to withdraw due to logistical strangulation."⁵³

Yet the "blinded" Iraqis somehow saw enough to be able to reposition a roughly five-division blocking force directly across VII Corps' axis of advance after the ground war began.⁵⁴ Some Iraqi intelligence and communications pathways were blocked, but others were not. The "pinned down" Iraqis managed not only to move this force of some 600 tanks and 600 other armored vehicles into position to meet VII Corps, but they also somehow moved a multi-brigade force into close combat with the Marines at Burqan without significant losses to air attack en route, as well as moving a battalion into Khafji unscathed.⁵⁵ Some Iraqi movements suffered heavily from air attack, others did not. The "logistically strangled" Iraqis managed to place ample food, water, and ammunition in the bunkers occupied by Republican Guard ground forces, and to establish major, untouched munitions dumps within easy reach of the blocking force that opposed VII Corps.⁵⁶ Some Iraqi divisions were denied supplies, others were not. At a minimum, Mahnken and Watts' blanket assertions to the contrary are overstated.

⁵¹ Ibid., at p. 158.

⁵² Ibid., at p. 159.

⁵³ Ibid., pp. 159-60.

⁵⁴ See, e.g., Scales, *Certain Victory*, pp. 232-6, 266; Gordon and Trainor, *The Generals' War*, p. 387, as referenced in "Victory Misunderstood," p. 146n.

⁵⁵ See, e.g., Mahnken and Watts own case studies: "What the Gulf War Can (and Cannot) Tell Us," at pp. 155-6; also Gordon and Trainor, *The Generals' War*, pp. 267-88, 363-9.

⁵⁶ See, e.g., U.S. News and World Report, *Triumph Without Victory* (New York: Random House, 1992), p. 384; *Operation Desert Shield/Desert Storm, Hearings Before the Committee on Armed Services, United States Senate* (Washington, D.C.: USGPO, 1991), S. Hrg. 102-326, p. 114; Lieutenant Colonel G. Chesley Harris, "Operation Desert Storm: Armored Brigade in Combat," *Infantry*, May-June 1992, pp.13-19; Steve Vogel, "Hell Night: For the 2d Armored Division, it was no Clean War," *Army Times*, October 7, 1991, pp.8ff at 24; Col. Michael D. Krause, *The Battle of 73 Easting, 26 February 1991: A Historical Introduction to a Simulation* (Washington, D.C.: U.S. Army Center for Military History and the Defense Advanced Research Projects Agency, August 27, 1991).

Alternatively, to conclude that some 1200-2100 surviving, actively resisting Iraqi armored vehicles were “irrelevant” to the outcome is a bit of a stretch: the entire Israeli army in 1967 had only 1000.⁵⁷ Perhaps all this armor would simply have been withdrawn voluntarily in a couple of weeks even without a ground invasion, but the war as actually fought offers no evidence for this, and a testimonial from an unknown number of unnamed prisoners of war who apparently served in the harder-hit parts of the Iraqi military is a long way from proof for such a sweeping assertion.⁵⁸

More broadly still, Mahnken and Watts attribute the Iraqis’ lack of military proficiency essentially to the air campaign.⁵⁹ The Iraqis were certainly inept, as I point out at some length (pp. 159-60), 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 1991.⁶⁰ 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?⁶¹ 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? 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’ prior 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. While air power surely exacerbated these shortcomings (as I argue on pp. 162-3, 174), it did not create them.

⁵⁷ Anthony H. Cordesman and Abraham R. Wagner, *The Lessons of Modern War Vol. 1, The Arab-Israeli Conflicts, 1973-1989* (Boulder, Colo.: Westview, 1990), p. 15.

⁵⁸ Elsewhere, Mahnken and Watts retreat to: “While Coalition air forces certainly did not win the war on their own” “What the Gulf War Can (and Cannot) Tell Us,” at p. 158. Which is it? Do they tacitly disagree with the quotation they subsequently introduce, do they really mean only that air forces could have won the war alone but were not given the opportunity, or do they simply not mean the initial observation at all?

⁵⁹ See, e.g., “What the Gulf War Can (and Cannot) Tell Us,” pp. 158-60.

⁶⁰ See, e.g., Anthony H. Cordesman and Abraham R. Wagner, *The Lessons of Modern War, Volume II: The Iran-Iraq War* (Boulder, Colo.: Westview, 1990), pp. 412-55.

⁶¹ Even one-word messages would prevent tactical surprise, yet be too short to be jammed or intercepted: 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 “frustrated,” or that an important military outcome can be traced to “shattered preconceptions.”⁶² 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.

Do I mis-characterize the state of net assessment?

Because I do not “discuss the kind of work Andrew Marshall had conducted and sponsored,” I am held to have “mis-characterized the state of the net assessment art.”⁶³ Yet the activity of net assessment and the Department of Defense’s Office of Net Assessment, directed by Andrew Marshall, are not one in the same. In Defense Department hallway shorthand, “Net Assessment” often refers to the specific office of that name, but in academic usage, “net assessment,” “balance assessment,” and “campaign assessment” all refer broadly to the attempt to project the results of possible armed conflict.⁶⁴ Of this broader activity, the overwhelming majority of analyses focus on the numbers and types of the two sides’ equipment. A small minority, sponsored mostly by Marshall’s office, attempt to address a wider set of considerations; whatever one thinks of these efforts, much remains to be done. Either way, though, even Mahnken and Watts agree they constitute a tiny minority of the whole. As they themselves put it in their opening paragraph: “... most analytical methodologies confine themselves to simplistic quantitative comparisons.”⁶⁵ Elsewhere they explicitly contrast Marshall’s work with “most” assessments, which “concentrate mainly on the quantifiable features of military forces.”⁶⁶ Yet this characterization — their words not mine — is almost

⁶² Mahnken and Watts, “What the Gulf War Can (and Cannot) Tell Us,” pp. 158, 159.

⁶³ Ibid., p. 154.

⁶⁴ See, e.g., Williamson Murray and Allan R. Millett, eds., *Calculations: Net Assessment and the Coming of World War II* (New York: Free Press, 1992). In fact, even the Defense Department formally acknowledges that the activity of net assessment is undertaken far more broadly than simply the work of the Office of Net Assessment per se. See Defense Department Directive DODD 5111.9, “Director of Net Assessment,” March 22, 1995, esp. paragraphs D.2, D.3, D.4, E.1.b.

⁶⁵ Mahnken and Watts, “What the Gulf War Can (and Cannot) Tell Us,” p. 151.

⁶⁶ Ibid., p. 154.

identical to my own, which they apparently find so objectionable: “Most current net assessments and force planning methodologies focus on the numbers and technical characteristics of the two sides’ weapons.” (p. 140). They apparently view this passage as fundamentally inconsistent with theirs — but a difference over whether to cite a minority exception to the general trend in the literature hardly suggests this. I cite multiple, specific examples so as to avert any misunderstanding of just what literature I mean (p. 140, note 5); I refer explicitly to the majority tendency of this literature; this majority is just as I describe it; and my characterization of this majority is not even challenged by Mahnken and Watts, who provide an extended testimonial to Andrew Marshall, but do not actually controvert anything in my analysis.

Do I create a strawman of the RMA literature?

Mahnken and Watts hold that I provide “an excessively narrow interpretation of the debate about a prospective revolution in military affairs (RMA).”⁶⁷ They subsequently describe the version I address as “simply the two most plausible ideas put forward,”⁶⁸ leaving me in the apparently deficient position of not addressing the *implausible* ideas. I plead guilty.

By contrast, they provide a few, if not implausible, at least ethereal versions in their paper. Mostly, they seem satisfied to treat the RMA as something more akin to a state of mind than an analytical argument. It is a “hypothesis [elsewhere a ‘conjecture’] that new, far more powerful methods of warfare may be in the offing,” but “Today, however, we can perceive only dimly the future course of this transformation and the features of the military regime it might produce.”⁶⁹ As a result, it is inappropriate for me to conclude that the RMA thesis has problems merely because its “most plausible” ideas do not hold water. After all, maybe it will turn out to be something else altogether. Who knows?

This position is actually a far more damning critique of the RMA debate than any I have offered. Mahnken and Watts strive mightily to defend the possibility of a coming RMA against what they see as my “dismissal” of the idea (see below), yet by denying it any content they destroy the village in order to save it. They reduce the future warfare debate to intellectual quicksilver: no actual argument is worth analyzing against evidence, because the future could be anything — to discard any one projection (or any

⁶⁷ Ibid., at p. 160.

⁶⁸ Ibid., at p. 161.

⁶⁹ Ibid., at p. 161.

several) by analysis is meaningless. If true, however, this would also imply hopelessness for any policy making they would have us adopt in response, and reduce the whole enterprise, both pro and con, to pointless speculation.

I am actually much less dismissive of the enterprise than this. I believe it is both possible and important to do a relatively better rather than a relatively worse job of anticipating future warfare and its demands. The only way to do this, though, is to take ideas seriously. That means, in part, holding analysts accountable for producing analyses that stand up against evidence. A theory of RMA that is so formless that it cannot possibly be overturned, even by contradicting its “most plausible” arguments, is too vague to be of any value either. It is much closer to tautology (or possibly art for art’s sake) than to useful policy analysis or scholarship. I do not think most authors in the RMA literature are so undemanding of their work as this. I am inclined to credit them with the worthy aspiration of making meaningful claims about the future on the basis of testable propositions about the present and the past. If Mahnken and Watts really disagree, then our disagreement goes far deeper than the RMA debate per se.

Do I Dismiss the RMA Out of Hand?

I am held to have “dismissed the RMA out of hand” on the flimsy basis of “a computer simulation of the encounter at 73 Easting.” This is partly a serious misreading of my methodology, as I argued above, and partly a rather dismissive treatment itself as an assessment of a 40 page analysis — how much more is required to qualify as a considered counterargument? In the process, however, a number of problematic observations are offered that merit brief response.

First, Mahnken and Watts attribute to me the argument that the Gulf outcome is unlikely to be repeated because of its idiosyncratic features.⁷⁰ Actually, this is the opposite of what I argue — they apparently have mistaken the explicitly labeled arguments of Jeffrey Record, Bobby Inman, Joseph Nye, and others, with whom I disagree, for my own (p. 175 and note 90).

They further argue that “Biddle’s construct of combat cannot refute the *possibility* that technology-enabled changes in warfare will occur in coming years.”⁷¹ Of course, this is just a somewhat differently worded version of my own text: “The possibility cannot be excluded that future weapons might change the dynamics of battle in ways that render

⁷⁰ Ibid., at p. 161.

⁷¹ Ibid., at p. 161, emphasis in the original.

[my] conclusions invalid and bring about a revolution, perhaps of the type so often discussed.” (p. 179). The main difference is in the meaning they attribute to the observation. Mahnken and Watts apparently believe that the burden of proof lies not on those who would have us believe that the future will be radically different from the present — an RMA — but rather on those who remain skeptical. It is up to the latter to prove that a revolution cannot possibly occur. This is a curious twist on the normal obligations of both scholarship and policy, in which the usual assumption is that radical transformation is uncommon enough to require positive demonstration before warranting full acceptance. My argument was that a major piece of the evidence underpinning the RMA position (the Gulf War) has been misinterpreted. By showing that the evidence is thus weaker than widely believed, I am inclined to conclude that we should reduce our confidence in the RMA as a projection of the future. Mahnken and Watts, on the other hand, are prepared to accept the RMA view of the future (whatever they think that is) until and unless it is somehow proven null and void. For me, inability to *prove* that the future cannot take some given form is a normal fact of social science; it should be acknowledged to avoid potential misunderstanding, but is hardly a decisive argument in favor of some specific projection. The purpose of scholarship is to evaluate the relative weight of positive evidence, not to give up unless the future can be proven or disproven.

The Case for Friction and “Situation Awareness”

Mahnken and Watts add a variety of other, less extended mis-characterizations of my article, but the above treats the main lines of their critique.⁷² In addition to this critique, however, Mahnken and Watts also advance a new explanation of their own: that the Gulf War outcome is best understood as the result of Clausewitzian friction and/or “situation awareness.”⁷³

⁷² They claim, for example, that I “neglect the human dimensions of combat interactions,” (ibid., p. 160), yet such interactions are the whole focus of my argument — what are Iraqi mistakes and Coalition skill if not “human dimensions” of the conflict? Elsewhere they take me to task for “neglecting the strategic and operational dimensions of the war” (ibid., p. 160) and for somehow failing to live up to the standard of Gerhard Weinberg’s recent history of World War II (ibid., p. 162). The latter is, at best, an odd comparison — what is the relationship supposed to be between my 40 page analysis of one aspect of the Gulf War (the radically low Coalition loss rate) and Prof. Weinberg’s 1200 page general narrative history of a completely different conflict? *A World at Arms* (New York: Cambridge Univ. Press, 1994) is a wonderful book, but if all proper analyses now require 1200 page treatments, then the journal industry is in deep trouble. As for strategic and operational issues, Mahnken and Watts again confuse a general narrative history and a focused analysis of a particular cause and effect relationship. As I state explicitly in note 63, I focus on Iraqi tactical mistakes because these are sufficient for the outcome I seek to explain, not because their operational and strategic behavior (much less ours) were unimportant. Different analyses have different purposes, and not all purposes require extended general narrative histories.

⁷³ Mahnken and Watts, “What the Gulf War Can (and Cannot) Tell Us,” pp. 151-2, 156-8.

In general, I am quite sympathetic to approaches, such as theirs, that seek to incorporate variables other than equipment per se into projections of combat outcomes. One useful way to look at my own article is as a first step in the development of a better-balanced theory of warfare that interrelates technical and behavioral variables in ways that current theories do not. I am inclined to think that accurate perception of the battlefield and the ability to cope with the unexpected are important consequences of combining skilled human performance with the right equipment. While I doubt very much that "situation awareness" and the ability to cope with the unexpected are the *only* important issues, they certainly seem important enough to be worthy of consideration in a wider context — and I certainly do not find such ideas fundamentally inconsistent with anything I argued in "Victory Misunderstood."

Given this, one wonders why Mahnken and Watts insist that "situation awareness" and friction have no important relationship to anything I discuss. They present these ideas as mutually exclusive with mine: as "fundamental aspects of combat that the author's model neglects," creating a "major gap in the understanding of war presented in 'Victory Misunderstood.'"⁷⁴

If they insist on treating these ideas as a wholly independent theory, however, then they must be evaluated by the same standards as any other independent theory. Yet the Mahnken/Watts formulation falls woefully short by such a standard. Perhaps unsurprisingly for such a brief exposition, it is badly underdeveloped. They provide no real causal mechanism, their terms are vague, and they offer no guidance on what sorts of observations might either sustain or contradict this theory in actual application. On the contrary, their articulation approaches unfalsifiability even in principle. They assert, for example, that "In-depth campaign histories invariably corroborate the potential of Clausewitzian friction to shape, if not drive, combat outcomes."⁷⁵ This combines extraordinary reach with remarkable imprecision. To claim that "in-depth" histories "invariably" corroborate anything is quite remarkable, especially when made without any explicit reference to any actual history. Alternatively, how would one know whether a given campaign had been "shaped, if not driven" by something or not? Can one infer

⁷⁴ Ibid., at pp. 156, 157.

⁷⁵ Ibid., at p. 157.

from this any possible observation that could falsify this theory, even in principle? The net result is more truism than meaningful theoretical argument.⁷⁶

They offer very little substantive evidence on behalf of this formulation. Their two, very brief, case studies of Khafji and Burqan are described only as “illustrations.”⁷⁷ Mere illustration, however, is among the weakest applications of case method, and is almost never sufficient to establish real confidence in a theory — especially when the theory is intended to supersede one, such as mine, which has been more extensively tested.⁷⁸

Moreover, most of the little evidence they do advance appears to contradict their own argument. In spite of atypical lapses in U.S. “situation awareness” (the Khafji attack initially took Coalition forces by surprise, as did Burqan),⁷⁹ the Coalition nevertheless defeated the Iraqis with heavy loss to them in both cases. Does this really illustrate the importance of situation awareness for military success? The theory’s vague formulation makes a conclusive assessment impossible — perhaps not “situation awareness” still favored the U.S. even at its least-favorable moment, or perhaps the “shapes” of these battles, if not their outcomes, were as Mahnken and Watts would predict. It is impossible to know for sure, since they give us no basis for characterizing anyone’s level of “situation awareness,” or for knowing a “shaped” from an “unshaped” battle when we see one. At best, however, the cases as they present them here offer a good deal less than a ringing endorsement — as actually written, they appear to offer disconfirming evidence. While Mahnken and Watts’ vague formulation prevents this from actually disconfirming their theory, their strategy of presenting only evidence that appears to contradict their own argument is certainly not the customary approach.

⁷⁶ This is a criticism of Mahnken and Watts, not Clausewitz. The latter offers great insight into the nature of war, and can be used as a corrective to some, overly reductionist theories, but is *not* appropriately used as a direct tool for net assessment, as Mahnken and Watts have attempted to do here.

⁷⁷ Mahnken and Watts, “What the Gulf War Can (and Cannot) Tell Us,” p. 156.

⁷⁸ At most, Mahnken and Watts’ cases approximate what Harry Eckstein terms *plausibility probes*: 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), pp. 79-137. On the weakness of multiple plausibility probes as evidence, see Alexander 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.

⁷⁹ See, e.g., Michael Gordon and Bernard Trainor, *The Generals’ War* (Boston: Little, Brown, 1995), pp. 278-9, 363-9.

Thomas Keaney

Keaney's paper closely parallels Mahnken and Watts'. As with them, he ignores all parts of my article except the 73 Easting Project, which he prefers to regard as the entire basis of my analysis.⁸⁰ As with them, he insists on treating 1200-2100 surviving, actively resisting Iraqi armored vehicles, organized in prepared defensive positions across the Coalition's major axes of advance, as "isolated pockets of resistance."⁸¹ And as with them, he credits the air campaign with a wide variety of sweeping effects on the Iraqi military, ranging from denying the Iraqis the ability to "move, be reinforced, or achieve even tactical success," to inducing their poor military proficiency (e.g., their inability to coordinate their actions).⁸² These are serious flaws in Keaney's analysis, and are discussed in detail above.

The only major substantive difference is the absence of either friction or situation awareness from Keaney's analysis. While I am unpersuaded of the merits of Mahnken and Watts' particular formulation, they view its absence from my paper as a "major gap in the understanding of war;" I thus leave it to the reader to assess the gravity of Keaney's apparent oversight.

While there are thus no important differences, there are a number of more modest issues that are unique to Keaney's paper and that warrant at least brief attention. First, Keaney asserts that the air campaign caused "the unwillingness or inability of Iraqi soldiers to respond when the ground campaign began." He then goes on to emphasize that this was so "throughout the theater."⁸³ In fact, he is wrong — the Republican Guard and at least some Iraqi Army heavy divisions fought back when attacked, they did not "fail to respond." I argue this at some length in my paper (pp. 149-51). Keaney seems a bit confused as to how, exactly, to respond to this argument. He variously denies it⁸⁴ (albeit without supporting argument or evidence); accepts it⁸⁵ (but allows as how it was true only for battles "named and referenced in U.S. accounts of the ground war" — it is unclear why naming a battle makes it less important); and dismisses it as mere "anecdotes

⁸⁰ See, e.g., Keaney, "Linkage," pp. 147, 148, 149. In all, eight of Keaney's 13 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.

⁸¹ See, e.g., Keaney, "Linkage," p. 147.

⁸² See, e.g., *ibid.*, pp. 148-9.

⁸³ *Ibid.*, pp. 148-9.

⁸⁴ *Ibid.*, p. 148.

⁸⁵ *Ibid.*, p. 147.

of large volumes of small arms fire rattling off vehicles.”⁸⁶ So does Keaney think the Iraqis resisted, or not? And if he thinks not, then why are we to disregard the evidence to the contrary that I present on pp. 149-51? Merely labeling inconvenient evidence as “anecdote” does not establish a point. In fact, most of the evidence I present, Keaney does not address in even this much detail (e.g., large caliber fire received by U.S. forces; counterattacks by Iraqi units; refusal of uncommitted Republican Guard units within earshot of the battle to surrender rather than being destroyed; Iraqi casualties removed from the battlefield after the fight; etc.). His argument is thus neither supported by the evidence nor even internally consistent.

Second, he argues at some length that 73 Easting was not representative. Of course, since my theory is not, as he claims, a generalization on 73 Easting, his argument is of unclear relevance.⁸⁷ For the record, however, the battle was not the outlier that Keaney makes it out to be. Keaney justifies his claim by arguing that “73 Easting stands out as one of the few cases in which the Iraqis had massed in a defensive front of any significant size.”⁸⁸ In fact, the Iraqi blocking force that met the VII Corps attack consisted of at least 1200 armored vehicles (p. 152), arrayed in depth on a more than 80 kilometer front.⁸⁹ The battle of 73 Easting involved only about 100 of these vehicles and 15 kilometers of this front.⁹⁰ VII Corps’ other engagements against the rest of this force produced very similar results (pp. 146-7, 151). Even if we ignore all other Iraqi Guard and heavy division activity, the blocking force alone thus contained more than enough armor on a dense enough frontage to have caused radically higher Coalition casualties. This is precisely what my article argues. Whether one chooses to call this “representative” or not depends on what one compares it to — 73 Easting was quite similar to other blocking force actions, but quite unlike the overrunning of the Iraqi conscript infantry at the frontier, as I point out (pp. 146, 149). To argue, as Keaney does, however, that 73 Easting is too unrepresentative for its result to be germane to the question posed in my article is simply wrong.⁹¹

⁸⁶ Ibid., p. 147.

⁸⁷ Tests of theories are often designed to be unrepresentatively challenging to the theory under test, for example. See, e.g., Eckstein, “Case Study and Theory in Political Science,” pp. 79-137.

⁸⁸ Keaney, “Linkage,” p. 147.

⁸⁹ Scales, *Certain Victory*, p. 266.

⁹⁰ 73 Easting Database.

⁹¹ Keaney also contends that whatever its fate against 2nd ACR, the 18th brigade of the Tawakalna would have been overrun by follow-on U.S. forces; given this, an initial defeat of the 2nd would have been unrepresentative because it would have been followed by a crushing U.S. success: Keaney, “Linkage,” p. 2. There are at least two major problems with this argument. First, my dependent variable is U.S.

Third, Keaney argues that because the Iraqis could not “coordinate their actions,” the ensuing combat outcome “cannot lead to meaningful conclusions on the relative values of skill and technology for either side.”⁹² This is a non sequitur. A battle in which one side is unskilled (their actions are uncoordinated) cannot tell us anything about the effects of skill? How does Keaney propose to examine the effects of skill if he restricts analysis to highly skilled armies? To do this is to insist on indeterminate research designs.⁹³

Finally, Keaney holds that I “stack the deck against revolutionary change” by focusing on ground weapon vs. ground weapon exchanges, whereas real revolutions usually require “asymmetrical operations” in which “dissimilar weapon types or forces are brought to bear against one another.”⁹⁴ Here, too, Keaney ignores all parts of my article other than the 73 Easting/Janus analysis. My theory explicitly addresses a variety of “dissimilar weapon” interactions — one of which in particular (the indirect effects of air power) I discuss at some length as an illustrative example (pp. 162-3). It takes an extremely selective reading of my article to conclude that it addresses nothing but the direct exchange of ground fire.

Keaney’s reading of my article aside, however, his proposition that revolutions usually occur when dissimilar weapons interact is not particularly useful. When have dissimilar weapons ever *not* interacted? With rare exceptions, battlefields have always been populated by a variety of weapon types, some of which typically focused their

casualties, not Iraqi victory or defeat in the war. If every initial contact between U.S. forces and Republican Guard defenses had caused the lead U.S. units to suffer 70 percent losses (as the Janus experiments imply that 2nd ACR might have suffered), then U.S. losses would have been radically higher — whether follow-on U.S. units subsequently overran the Iraqis without cost or not. But second, why should we believe that Iraqi defenses good enough to hurt lead U.S. units so badly would suddenly become helpless against follow-on attacks? U.S. armored cavalry regiments are heavily armored units with a close-combat punch per capita not radically different from that of their follow-on forces. If the 18th brigade of the Tawakalna could inflict 70 percent losses on an armored cavalry unit, then it is hard to understand why it (or its comrades deployed to its rear and as yet untouched by 2nd ACR) would not be able to put up a meaningful resistance when attacked by the U.S. First Infantry Division in its turn.

⁹² Keaney, “Linkage,” p. 149.

⁹³ Alternatively, Keaney claims elsewhere that the Iraqis’ poor proficiency was due to air attack; perhaps his argument here is meant to suggest that the Iraqis’ inability to coordinate should be attributed to Coalition air attack, rather than to their skill. Of course, if this is his intent, his statement is at best imprecise. Even if this is the case, however, and even if Keaney’s causal attribution of Iraqi ineptness to Coalition air attack were right (which it is not), this would still not justify his contention here. Whatever the deeper cause of unskilled performance, an observation of unskilled performance in action still permits valid analysis. Only if one were to use a lack of skill which resulted from air attack to conclude that air attack was unnecessary would the results be invalid — and I make no such argument.

⁹⁴ Keaney, “Linkage,” pp. 149-50.

efforts on unlike opponents. Artillery has always focused mainly on enemy infantry (or fortifications). Heavy cavalry sometimes fought other cavalry, but was mainly intended to engage hostile infantry after the artillery had disrupted their formation. Air defense units rarely engage other air defense units. Keaney's contention here is a bit like arguing that ships usually sink when the sea is salty.⁹⁵

Concluding Observations

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

Second, the critics' two new theories offer less satisfactory explanations of the war's key military outcome. Press' case for overdetermination falls afoul of inappropriate case selection and inaccurate analysis of case evidence, which clouds its ability to explain Press' own, less demanding, dependent variable, much less mine. Mahnken and Watts' "situation 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 than to corroborate on the basis of the partial argument presented. While the literature is richer for their inclusion and assessment, neither theory can thus yet be judged a success.

⁹⁵ Alternatively, perhaps Keaney really meant to say that revolutions are more likely when a *new* weapon's introduction creates a *new* type of dissimilar interaction. He lists submarines vs. surface ships, and aircraft vs. ground forces; perhaps he means that the initial introduction of the submarine and the airplane were revolutions (even if their subsequent employment was not), and that by allegedly focusing only on direct-fire interactions I thus miss the best opportunity for a new revolution today. If so, however, then what is the new dissimilar-weapon interaction in the Gulf War, whether I missed it or not? Aircraft vs. ground targets? This has been going on since World War I. Artillery vs. infantry? This one is older still. Scuds vs. cities? This is newer, but neither original nor terribly revolutionary in its effectiveness in 1991. All effort to the contrary notwithstanding, I am thus unable to articulate a version of this argument that seems at all plausible.

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