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Introduction by Robert Jervis, Columbia University

It was not difficult to find reviewers for *The Future of War: A History* because any book by Lawrence Freedman—really Sir Lawrence Freedman because the British know how to convey respect to their leading academics—is to be welcomed. Although one of our reviewers has serious criticisms of one part of the book, all speak highly of it. Beatrice Heuser calls it "a masterpiece," Mara Karlin judges it "a readable, thoughtful, and useful addition to the literature," Joshua Rovner calls it an "engaging and wide-ranging history," and Douglas Gibler, whose criticisms I will note below, calls it "a fun read and an excellent review of large swathes of military history." The reviews are all concise and to the point, and so I need only note a few things from them.

All the reviewers are impressed by Freedman's basic idea of looking at how scholars, leaders, and writers of fiction have imagined future wars in the past and all share his view that this is a cautionary tale about the great uncertainties that war involves and the difficulties in making predictions. Freedman's grasp of a wide range of literatures and history is daunting: it is hard to imagine even the best informed scholar coming away without learning a great deal.

Heuser, Karlin, and Rovner all note that most of the thinkers surveyed by Freedman concentrated on the first battles, often implying that they would be decisive. In fact, wars often stretch out much longer than expected, and this may be one of the crucial lessons we can learn from the past. The American efforts in Afghanistan and Iraq are only the latest cases where initial military victories did not readily translate into satisfactory political results. It is presumably easier for writers and planners to imagine the early stages of a conflict than the later ones.

Karlin points out a striking omission in a book about the use of force: Freedman says almost nothing about civil-military relations and organizational structures/roles and missions.

Gibler mounts the most sustained criticism, focusing on Freedman's treatment of the scientific study of peace and war that is much more popular in the U.S. than in the UK. What Freedman sees as disabling disagreements in how cases are to be coded Gibler sees as the sort of open-minded scientific willingness to criticize and improve previous studies that has made the current research an improvement on what has come before. He also notes that some of the findings that Freedman considers self-evident are contrary to the conventional wisdom that prevailed before quantitative studies took hold and the very fact that we now take for granted the importance of the issues at stake, the role of alliances, and the self-perpetuating dynamics of rivalries shows what even non-quantitative scholars like Freedman have learned from this body of research.

Participants:

Lawrence Freedman was Professor of War Studies at King's College London from 1982 to 2014. He was the official historian of the Falklands Campaign and a member of the official UK Inquiry into the Iraq War. In addition to the *Future of War: A History* he published *Strategy: A History* (Oxford University Press) in 2013**.**

Robert Jervis is Adlai E. Stevenson Professor of International Politics at Columbia University. His most recent book is *How Statesmen Think* (Princeton University Press, 2017). He was President of the American Political Science Association in 2000-2001 and has received career achievement awards from the International

Society of Political Psychology and ISA's Security Studies Section. In 2006 he received the National Academy of Science's tri-annual award for behavioral sciences contributions to avoiding nuclear war.

Douglas M. Gibler is Professor of Political Science in the Institute for Social Science Research at the University of Alabama. He is the author of *The Territorial Peace: Borders, State Development, and International Conflict* (Cambridge University Press, 2012) as well as over 40 articles on international relations and comparative politics. His research has been supported by a fellowship from the HF Guggenheim Foundation and numerous grants from the National Science Foundation. His current NSF-funded projects include an evaluation of issues, demand, and position change in militarized conflict since 1816 and a separate set of projects on selection issues in multi-level models.

Beatrice Heuser holds the Chair in International Relations at the University of Glasgow. She presently on leave to hold the Vincent Wright Chair at Sciences Po, Paris. Her most recent book is *Strategy Before Clausewitz: Linking Warfare and Statecraft, 1400-1830* (New York: Routledge, 2017).

Mara Karlin is Associate Director and Associate Professor of the Practice of Strategic Studies at Johns Hopkins-SAIS and non-resident Senior Fellow at the Brookings Institution. She served in national security roles for five U.S. Secretaries of Defense, advising on policies spanning strategic planning, defense budgeting, future wars and the evolving security environment, and regional affairs involving the Middle East, Europe, and Asia. Her first book, *Building Militaries in Fragile States: Challenges for the United States*, was just published by University of Pennsylvania Press. Karlin has been awarded Department of Defense Medals for Meritorious and Outstanding Public Service, among others. She received her PhD from Johns Hopkins University-SAIS.

Joshua Rovner is Associate Professor in the School of International Service at American University. He is the author of *Fixing the Facts: National Security and the Politics of Intelligence* (Cornell University Press, 2011); and co-editor of *Chaos in the Liberal Order: The Trump Presidency and International Politics in the Twenty-First Century* (Columbia University Press, 2018).

Review by Douglas M. Gibler, University of Alabama

Professor Freedman's *The Future of War* is a fun read and an excellent review of large swathes of military history. The commonalities between fiction and military planning, their impact, and their evolution are thought provoking, and I learned a great deal from the book. Despite that, I believe my role in this review is to respond to Freedman's treatment—or, rather, smiling disdain—for the scientific study of peace and war that has increasingly dominated political science for over fifty years. Freedman compares much of recent political science to the idealists who tried to reimagine war after World War I, or worse, the fanciful novelists who shaped popular thoughts on war with imagined stories of fear or heroics. We quantitative and formal scholars of conflict are just like H.G. Wells in this regard, trying to reimagine warfare for our own purposes, in ways that are not productive or perhaps even dangerous. I discuss below Freedman's misuse of the term reliability in evaluating scientific findings, noting that the examples of current debates Freedman uses actually point to good progress in the field and the reliability of concepts. We have learned much, and what we have learned has shaped Freedman's own arguments.

Freedman's Two-Part Critique of Science

Freedman's arguments against science when it comes to predicting war rest on two pillars. First, the methods of science are ill-served for the purposes of understanding war, and, second, even the best scientific studies produce only conclusions we already know. As Freedman alludes, J. David Singer's Correlates of War Project, Johan Galtung's peace institute, and Tedd Robert Gurr's Minorities at Risk project, all constitute poorly done science that produces only trite observations.[1] This is due to the nature of the endeavor, of trying to apply science to the study of conflict, and is not endemic to the persons involved. Be mindful that my quick characterization is much less charitable here than Freedman's prose, but the conclusions are the same. While the goals of the science movement are laudatory according to Freedman, the abstraction from history is unwarranted and misses what is really important in the study of war, which is apparently "context." Science seemingly can never understand context and is, hence, doomed to fail in its exploration of the causes of war.

Freedman's review of the scientific practice of international relations research is almost always well stated, but he misstates basic disagreements about the reliability and validity of our measures. For example, Freedman describes my recently-published work in *International Studies Quarterly* with Steve Miller and Erin Little in which we documented errors in the coding of numerous cases of Militarized Interstate Disputes (MIDs) by the Correlates of War Project.[2] We were stunned by the number of cases that should be dropped, merged with other cases, and recoded; we also could not find about twenty other cases that scholars have been using in their quantitative studies. Freedman concludes from our work that "a team of researchers going through the individual cases meticulously found the MID database to be unreliable, although that was not a word they

[1] The Correlates of War Project (website), http://www.correlatesofwar.org/; "John Galtung," Galtung-Institut for Peace Theory and Peace Practice (website), https://www.galtung-institut.de/en/home/johan-galtung/; University of Maryland, Center for International Development and Conflict Management, Minorities at Risk (MAR) Project (website), http://www.mar.umd.edu/

[2] Doublas M. Gibler, Steven V. Miller and Erin K. Little, "An Analysis of the Militarized Interstate Dispute (MID) Dataset, 1816–2001," *International Studies Quarterly* 60:4 (2017): 719-730.

used." That was indeed not the word we used, and Freedman's supplantation of unreliable suggests that he missed the entire point of our study.

Reliability has a specific definition in social science. A measure is reliable when it produces similar results *under consistent* conditions. Our case evaluation was the first-ever comprehensive review of the early dispute cases between 1816 and 1992. Most cases were collected before personal computers and with little or no access to search engines that could systematically identify and build cases of interest. We rebuilt the histories of those cases and showed that early coders had often relied on superficial historical treatments of the events in introductory textbooks of various regions and time periods. The histories themselves did not present consistent evidence, so we relied on primary sources such as newspapers that allowed adequate coding of the events during the course of each case. That new evidence led us to reevaluate the earlier codings, but this hardly implies the conclusion that the concept of a dispute cannot be reliably coded. Indeed, the coding procedures for MIDs are mostly quite clear, and that is why the Correlates of War Project has now accepted most of our suggestions for dropped cases. Given the same evidence, both sets of researchers can now *reliably* code the cases, and that is what has happened. Our data on international conflict is much better now, and this is great progress.

Freedman does not mention our empirical conclusions from the data. Though over 70% of the cases had coding changes, the results analyzing previous models were mostly the same. In fact, the previous coding errors had muddied the waters in several instances, and the proper coding of cases *strengthened* key relationships in separate studies. In only one study of the three we chose to replicate were results overturned, and we speculated on how our theories can be reevaluated following those results. We thought the important takeaway from our research was clear: political science has done an excellent job of identifying the structural conditions that make conflict between states likely.

Freedman further notes that separate work of mine has questioned the utility of lumping together all forms of international conflict into militarized disputes.[3] This is a criticism of the concept of a MID but for all the wrong reasons. Indeed, Erin Little and I do note that there is ample heterogeneity in the data, as fishing disputes, attacks on shipping, and rebel-chasing border violations sit alongside the wars of the last two decades. However, Freedman again misunderstands the thesis. Our work does not suggest the data itself is either unreliable or not valid—far from it. We argue instead that scholars need to re-examine the concept of a militarized dispute and note that there are various types of cases within the dataset. This heterogeneity across disputes implies that different causal paths may explain different types of cases. So, once we control for the various types, our understanding of the dangerous international conflicts will increase. That is also exactly what we find. We demonstrate that fishing disputes and similar cases of protest-only disputes are not well predicted by standard models of international conflict, which of course is to be expected since these cases were never militarized by the home states of attacked citizens. However, once we control for or omit these cases, the predictive ability of commonly-used independent variables perform better when used to estimate *militarized* disputes. Freedman is critical of coding errors, but scholars have learned from them and produced better explanations.

[3] Douglas M. Gibler and Erin K. Little, "Heterogeneity in the Militarized Interstate Disputes (MIDs), 1816–2001: What Fatal MIDs Cannot Fix," *Political Science Research and Methods* 5:1 (2017): 189-199.

A Short Note on "Context"

Freedman blames political scientists for omitting context from their models, which he believes is all-important in understanding the causes of war. But Freedman misstates how we use context to build and test models. If key factors systematically affect the likelihood of conflict, then that context can be defined. Does the model, for example, fail to include differences across alliance types? Does it omit the importance of how much leverage the leader has with his or her domestic population? These and most other variables can be operationalized and tested, whether using quantitative or qualitative methods. If the context cannot be measured and is unique to the case, it is unrelated to the underlying theory we are trying to test. A useful model will not explain the peculiarities of every case. Similarly, peculiar cases should never be used for post hoc theorizing. Truly unique historical moments are no basis for theory; hard cases make bad law.

I can understand a historian bemoaning the loss of history in the study of war. However, historians implicitly rely on theory, making generalizations from partial evidence even in single cases. Indeed, I will paraphrase from the first chapter of an old introductory political science methods text: the level of abstraction between reality and narrative is much, much greater than between that narrative and operationalization.[4] Historians abstract from reality almost as much as political scientists do. Of course, the irony of this argument in the book is that it rests within a manuscript in which Freedman is suggesting that there is a certain process that happens in each time period before a conflict. Contemporaries talk about the future of war and are influential. These events are not unique to one particular conflict or time period but rather indicate a trend in the whole. For Freedman's arguments, the context of the particular time period has little effect on the underlying trend suggested by his thesis.

What We Know about the Future of War, with Social Science and with History

Freedman rightfully suggests that we judge the prospects of a field by its contributions and he cites a 2012 review compendium, *What Do We Know About War?*, in which John Vasquez argued that science was the best means of solving the puzzle of war.[5] However, Freedman dismisses the volume by suggesting that "on the main conclusions of the scientific school there was not a lot that went beyond what would be obvious to any serious observer of international affairs. He noted the importance of the [sic] 'the issue at stake', how alliance formation and military buildups could be mutually reinforcing, and that 'rivals have a much higher probability of going to war than other types of states"' (113).

In some sense it is fantastic that a historian as renowned Freedman takes all these facts as accepted knowledge today. That was not always the case. In 1993 Vasquez was arguing against the main of historical perceptions of the causes of war, and there was even a previous debate in the *American Political Science Review* regarding

[4] Gary King, Robert O. Keohane, and Sidney Verba, *Designing Social inquiry: Scientific Inference in Qualitative Research* (Princeton: Princeton University Press, 1994).

[5] John A. Vasquez, et al., *What Do We Know About War?* (Lanham: Rowman & Littlefield 2000).

his view of the balance of power concept, so central to historical thinking.[6] Indeed, as Freedman himself points out earlier in the book, much historical scholarship—for thousands of years according to realists reading Thucydides—believed that deterrence worked, that the content of issues was trivial, and that an equal likelihood of conflict remained among all states in the system. The "intuitive" acceptance now of Vasquez's argument simply documents further progress.

Really, though, there is little that is intuitive in the knowledge we have gained over the past fifty years. One example is the growing literature on the Democratic Peace, which Freeman gets horribly wrong. Freedman starts by suggesting that political scientists simply catalogued events and noticed that democracies did not fight each other, at least not during the Cold War. This point is fair—democratic peace theory has always been a finding that is in search of a theory. However, Freedman pushes further and trots out familiar arguments that states transitioning to democracies are war-prone, citing evidence from the years since the Cold War. This is somewhat beside the point, and in any case Freedman is mistaken. He criticizes political science for not understanding that transitions will lead to violence, but political science has spent years testing this theory. Transitions to democracy do not make conflict more or less likely, and these arguments appear nowhere in the review of causes of war research cited because the early arguments were amended, amended again, and finally debunked.[7]

The empirical observation that democracies do not fight each other is based on political science research. Historians never noticed this. As Freedman points out, I disagree with the conclusion that regime type influences this,[8] but it is an empirical truth that democracies have few disputes between them and none or very few wars.[9] My argument is that territorial settlement enables liberalization within the state, and democracy becomes more likely; without territorial issues, future conflict is less likely as well. My causal argument is different from others, but the finding is the same.

I have focused above on the chapters engaging political science over the last few decades, but I am going to close by returning to one more irony in the overall theme of the book, especially given the arguments against science within it. Freedman's main argument is broad and suggests that humans have always written about the future of war, have mostly been influential doing so, but have also been wrong most of the time. Fine, but how do we know this? For Freedman, the evidence is provided in the argument—look at all the examples. As a social scientist, though, I want to know why he chose those examples, and how consistently he analyzed

[6] See John A. Vasquez, *The War Puzzle* (New York: Cambridge University Press, 1993), and "The Realist Paradigm and Degenerative Versus Progressive Research Programs: An Appraisal of Neotraditional Research on Waltz's Balancing Proposition." *American Political Science Review* 91:4 (1997): 899-912.

[7] See, for examples, Vipin Narang and Rebecca M. Nelson, "Who are these belligerent democratizers? Reassessing the impact of democratization on war." *International Organization* 63:2 (2009): 357-379, and Bear F. Braumoeller, "Hypothesis Testing and Multiplicative Interaction Terms." *International Organization* 58:4 (2004): 807-820.

[8] Doublas M. Gibler, *The Territorial Peace: Borders, State Development, and International Conflict* (New York: Cambridge University Press, 2012).

[9] Bruce M. Russett, John R. Oneal, and Michaelene Cox, "Clash of Civilizations, or Realism and Liberalism *Déjà Vu*? Some Evidence," *Journal of Peace Research* 37:5 (2000): 583-608.

them. I want to know how we can avoid a clear selection effect, since the most influential writers were also the ones least likely to be forgotten by history. Freedman's provocative book begs these and other questions. The same political science methods of which he is so skeptical may prove to be the best hope for finding answers.

Review by Beatrice Heuser, University of Glasgow and Sciences Po

The witty title of this book points to the crucial paradox that our future is in part unforeseeable, but will in part at least be what we make it. And what we make of it depends on how we expect it to be, for better or worse.

The debates in the U.S. and Britain of the last quarter century as to whether the peaceful time has come to get rid of nuclear weapons have often seen the argument that the future is unforeseeable, and that this is why we need to hold on to nuclear weapons. Well, the one thing we *can* predict with certainty is that the future will contain nuclear weapons if we decide to hold on to them. Out of fear of uncertainty is born a new—albeit partial—certainty. And thus it is with regard to the prediction of future wars that this book passes review.

A story of attempts to insert such certainties into the future is also a story of worst-case scenarios which become real as prophets of future war and war planners focused excessively on them rather than on alternatives. This is the story of the First World War, foreseen by all but a very few as a war of mass armies. Most writers of the age lacked the imagination to conceive of alternatives or to see what sort of war would result from the application of these mass armies in conjunction with unprecedented firepower and supplies, and how, unleashed indiscriminately over battlefields of previously unimaginable dimensions, they would create a war which prior to 1914 all sides should have worked above all to prevent rather than win. Why was this the case? Because this was a world in which 'Notions of arbitrating disputes were not merely dismissed as naïve in their idealism but as actually "immoral'," as Freedman remarks about the Social Darwinists of the time like German General Friedrich von Bernhardi (44). Before 1914, those whom I.F. Clarke called "the voices prophesying war" failed to predict what total war would look like on the battlefield and to develop alternatives, either through diplomacy, or through limited war scenarios.[1]

After the First World War, several military writers in Britain, France and indeed in Germany made it their top priority to find ways of fighting the next war in ways that would make it less cruel than the previous war.[2] The intention was good, the outcome a complete failure. That some of them argued for appeasement in the interest of avoiding a rerun of World War I (the *acte manqué* which would have been appropriate in 1914 but was not appropriate when faced with a Hitler), they were after 1939 condemned for playing into Hitler's hands. What the J.F.C. Fullers and the André Magionts and the Raoul Castexes and the Basil Liddell Harts had failed to predict was the extremes to which an ideology would drive an advanced industrial society with its perfected war machine. The failure of imagination which Margaret Macmillan diagnosed for the eve of the First World War[3] can equally be found both outside and inside Germany in the 1930s: even German dictator Adolf Hitler's generals, hard-wired to obey, were flabbergasted at the audacity of his plans, and one or two

[1] I.F. Clarke, *Voices Prophesying War: Future Wars, 1763-3749*, 2nd ed. (New York: Oxford University Press, 1993).

[2] See Beatrice Heuser: *The Evolution of Strategy* (New York: Cambridge University Press, 2010), 178-190.

[3] Margaret MacMillan, *The War that Ended Peace: How Europe Abandoned Peace for the First World War* (London: Profile Books, 2014).

were even shocked by their cruelty. Before 1939, many voices of future war failed to predict that there was no alternative to total war against an enemy as wicked as Hitler-led Germany.

Others, however, contributed to make the next war a total war of city bombing and indiscriminate killing. Take this passage from the famous memorandum produced by the Royal Air Force's (RAF) chief Sir Hugh Trenchard in 1928:

> There can be no question, whatever views we may hold in regard to it, that [aerial bombardment] will be used [in the next war]. There may be many who, realising that this new warfare will extend to the whole community the horrors and suffering hitherto confined to the battlefield would urge that the Air offensive should be restricted to the zone of the opposing armed forces. If this restriction were feasible, I should be the last to quarrel with it; but it is not feasible. In a vital struggle all available weapons have always been used and always will be used. … Whatever we may wish or hope, and whatever course of action we may decide, whatever be the views held as to the legality, or the humanity, or the military wisdom and expediency of such operations, there is not the slightest doubt that in the next war both sides will send their aircraft out without scruple to bomb those objectives which they consider the most suitable.[4]

Apologetically and defensively packaged, this was to be a self-fulfilling prophecy.

In a variation of the theme of committing suicide for fear of dying, war planners to this day are mesmerised by worst-case scenarios and feel compelled to prepare for them, even in times of relatively robust peace, for fear of being caught out at a later stage. Keeping in mind the long procurement cycles and the defence acquisition decisions of today which determine what will be available in 2030, 2040, and 2050, they will argue that it is safer to hedge bets than to forego options and use any 'peace dividend' for non-military purposes.

Freedman rightly draws attention to the multiple intentions that might be lurking behind the respective works of prediction – from persuading the government to make major investments in some new weapons systems or to reapportion defence spending in favour of particular sectors to the attention-seeking behaviour or some academics, policy advisors, or officials in or out of uniform. There is the temptation to credit any and every technological break-through as a dangerous possibility to be outdone by an adversary, or as providing the long-sought magic bullet to be able to achieve defence on the cheap. And there are extrapolations from the recent past that show blindness to longer-term developments. Yet there are also continuities in war and in hot spots or war-drivers the world over which make some predictions as to what areas to watch quite plausible. Such predictions or rather warnings of possible developments, Freedman argues, deserve to be taken seriously (277-287).

There is a message of hope: the world since 1945 seems to have benefitted from the many works of fiction, faction, and 'the grim branch of knowledge which is sometimes called the science of war' (Liddell Hart, quoted on 54). Not least thanks to these, consensus was established within a decade of the first use of nuclear weapons that these were not 'normal' weapons, and that the purpose even of planning elaborately how to use

[4] Quoted in Jeremy Thin, "The Pre-History of Royal Air Force Area Bombing," MA thesis, University of Canterbury (2008), 144.

them should be put, above all, in the service of deterrence of war. Perhaps the closest the world has come to nuclear war was on 26 September 1983 when aerial surveillance systems in the USSR malfunctioned, signalling incoming missiles, and Soviet officer Stanislav Ye. Petrov disregarded orders to launch a retaliatory strike on NATO. We do not know if he had read any novel or seen any film of the *Failsafe* or *Doctor Strangelove* variety to stop him short of destroying the world. But many other decision-makers or advisors in the West were sensitised to the possibilities of accidents through the massive and widely circulated literature and films on the subject (68-80). Thinking the unthinkable and popularizing it through novels and films may have helped mankind survive in the nuclear age – so far.

This is another side of prediction – while one part leads to the bringing on of a particular future, another prevents it. This was certainly the main purpose of the collaborative work presented as fiction by ex-NATO General Sir John Hackett under the title of *The Third World War: A Future History*[5] (94-98). By showing what NATO might do in response to Warsaw Pact Aggression, Hackett and his collaborators wanted to show that such aggression might well be stopped in its tracks without uncontainable nuclear escalation, which was definitely not worth it. Indeed, by the time the Warsaw Pact was dissolved and the Soviet Union fell apart, World War III in the way in which NATO planners anticipated it and prepared themselves had not taken place. Having just seen out 2017, the year which General (ret.) Sir Richard Shirreff predicted to be the time for a war between (parts of) NATO and Russia (269f.), we may draw a little comfort from the idea that spelling out dangers in some contexts might lead to their avoidance.

Overall, *The Future of War: A History* is a masterpiece on which much other research will build: we have here the first comprehensive overview of how specialists and (often very well informed) novelists imagined future war. The next research question arising from this goes back to the first point raised above: to what extent have predictions of future war got it right, to what extent have they turned into self-fulfilling prophesies, and what can we do to avoid bringing on the worst-case scenario, by neglecting alternatives? These questions are truly important, perhaps vital for our survival, as we see a return of thinking about major war in Russia, China, and the United States.

[5] General Sir John Hackett: *The Third World War: A Future History* (Sidgwick & Jackson, 1978)

Review by Mara Karlin, Johns Hopkins University

No shortage of books with the words "future" and "war" in their title exists, yet eminent scholar of security affairs Lawrence Freedman has found a way to distinguish his newest book, *The Future of War: A History*. Freedman approaches the question that wracks the mind of all national security practitioners and scholars with a historic eye, offering a readable, thoughtful, and useful addition to the literature. He examines how a wide range of observers thought about future conflict when it was just that—the future. And while he suggests that patterns of conflict are more continuous than not, the book's bumper sticker easily could be a mash-up of Cool Hand Luke and Margaret MacMillan: "what we've got here is… (a) failure of imagination."[1]

As a former senior defense official who spent years cogitating over which wars the U.S. military would fight in the future and guiding the allocation of the 600+ billion-dollar defense budget, Freedman's book resonates deeply. More broadly, he speaks to a number of audiences in this work, including the academic and policy communities, historians and futurists alike. Freedman's writing style reads effortlessly and betrays the mountain of effort (particularly sourcing) that this book no doubt required.

Part one is easily the most rigorous and compelling portion of the book. In it, Freedman explores the history of conflict as seen from the mid-1900s until around 1990. Like any meditative work on conflict, it is invariably read through the lens of today. Reading about Barbarossa and about Pearl Harbor, and the notion that, "war was bound to come, and therefore it should be started on the best possible terms," one can't help but reflect on the current debate over how to respond to North Korea's nuclear weapons stockpile (66). Just two pages later, the reader is left chilled by Orwell's reminder that, as Freedman describes, "creatures out of the Dark Ages have come marching into the present" (68). Yet again, I could not help but recall ISIS's recent propagation of slavery, destruction of universities, libraries, and antiquities as an attempt to neutralize Iraqi thought. Simply put, the first part of this book presents one riveting chapter after the next.

Part two—rather in line with the period of time it covers—is somewhat less so. While the sections on fragile and weak states present broad overviews on how to think about states that cannot exert a monopoly on violence contra the German sociologist Max Weber—namely chapters 13, 15, 18, and 19—the entire part nevertheless reads like a history of the post-1990 period rather than a history of how thinkers and observers viewed it at the time. It is a missed opportunity as the reader briefly encounters revealing conflicts like Russia's involvement in Chechnya in the 1990s and in Syria since 2015, but only for a page or so, precluding any meaningful exploration of lessons learned (205-207). For the latter, a failure of imagination on the part of many U.S. and European policymakers was overwhelming, as they had never imagined that Russia would intervene in Syria's conflict to use its territory as a tactical and operational training battleground.[2]

[1] technomage116, "Cool Hand Luke - Failure to Communicate," YouTube video, 0:09, posted 15 April 2010, https://www.youtube.com/watch?v=V2f-MZ2HRHQ. Margaret MacMillan, *The War That Ended Peace: The Road to 1914* (New York: Random House, 2014), 605.

[2] Leonid Bershidsky, "Putin's Goals in Syria Went Beyond Saving Assad" (column), *Bloomberg*, 4 January 2018, https://www.bloomberg.com/view/articles/2018-01-04/putin-s-goals-in-syria-went-beyond-saving-assad.

Part three presents a useful overview of today's challenges and is particularly digestible for the layperson dipping into the question of conflict's future trajectory. Freedman is diligent in exploring crucial issues like the changing character of war; newer tools like cyber capabilities, robots, and drones; and shifts in the security landscape including mega-cities and climate change. Some of these topics would particularly benefit from a deeper look, including going beyond the reflexive comparison of robots to the "Terminator" films which has largely characterized discussion of lethal autonomy to date (245). Perhaps if R2D2 or Short Circuit were considered as models, the dialogue would be less alarmist. Stretching this examination to include debate over which types of conflict these tools would be most useful for would be helpful. And, like the rest of the book, part three is layered with gems that could undoubtedly serve as tomes in and of themselves. To take just one example, Freedman casually remarks on the lure of the first move in thinking about conflict, explaining that "far less was written on second and third blows, and still less about those later years when an impasse had been reached and the fighting ticked over, with casualties but no breakthroughs" (278).

To be sure, this book is not a manual. Indeed, Freedman preemptively takes on this potential criticism by explaining—quite rightly—that "it would be against the spirit of this book to predict the incidence and form of future wars" (286). Nevertheless, the reader seeks illustrative and plausible approaches to inform the study of future war. It is in this vein that the reader would do well to recognize how Freedman indirectly reminds us of tools that are helpful for those who must attempt the dangerous art of prediction. Most notably, Freedman demonstrates the value of fictional writing and film as tools that help explode reigning paradigms. He points to *Ghost Fleet* as one piece of writing that resonates with today's national security scholars and practitioners, and outlines how an earlier generation was influenced by *The Battle of Dorking* which centered around a fictional invasion of Britain and was so compelling that it "triggered a national debate on the state of Britain's preparedness for war" (4-7 and 250-252).[3] In chapter 7 ("The Balance of Terror") alone, he discusses a wide range of work including books by authors such as George Orwell, H.G. Wells, Nevil Shute, and T.S. Eliot, as well as the films "Red Alert" and "Dr. Strangelove" (68-80).

Scholars and practitioners of national security affairs point to an unsettled feeling that is permeating the contemporary state of affairs. While Freedman gives it a nod, he also reminds us of how confused and nascent our understanding of conflict can be—including its parameters, permutations, and rules. His deft dissection of professor Steven Pinker's work pairs nicely with the history of other efforts to track conflict, primarily the Correlates of War Project,[4] and underscores how definitions can impede analytic attempts to track patterns (xi-xv and 115-122). Similarly, he reminds the reader that just a century or so ago, rules of warfare remained foreign. In one compelling example, he underscores that the notion of attacking civilian ships was anathema thus offering a sobering reminder of how young the law of armed conflict is (39-40).

There are a few valuable topics whose absence in this book raises questions. Most notably, civil-military relations and organizational structures/roles and missions are largely left unaddressed. Both invariably influenced the historic debates on the future of war and almost surely offer insights for the next generation of

[3] P.W. Singer and August Cole, *Ghost Fleet: A Novel of the Next World War* (Boston: Houghton Mifflin Harcourt, 2016).

[4] The Correlates of War Project (website), http://www.correlatesofwar.org/.

thinkers. Of course, future analysis could look at how other countries have judged the future of war, but Freedman wisely refrains from any attempt to do so in an already fulsome work.

In conclusion, today's national security policymakers and scholars should read Freedman's book, *The Future of War: A History*, with a sober and disciplined eye… and perhaps a strong glass of whiskey.

Review by Joshua Rovner, American University

There is something irresistible about predicting the future of war. Pessimists are forever warning that new technology will lead to new horrors. Optimists are forever searching for signs that humanity is evolving peacefully, and that war is becoming an anachronism. Both sides are routinely wrong, writes Lawrence Freedman in his engaging and wide-ranging history, yet they continue to prophesize with great enthusiasm. Everyone is guilty. Politicians, military officers, journalists, novelists, scholars, and pundits are strangely confident about predicting the future, even if they acknowledge the dubious track record of past prognosticators. And they always seem to have a receptive audience. We know the limits of soothsaying, but we cannot help ourselves.

While there is typically little agreement among futurists, a few common themes emerge from Freedman's survey. One is the obsession with the first move. Nineteenth-century novelists imagined horrible short wars. Military and political leaders agreed that future conflicts would not be prolonged and exhausting. Instead, "All could be won or lost in a short time" (6). As a result, everything rested on preparing for an intense spasm of violence, and strategists spent far too little time on what would happen next. This assumption ran through the twentieth century, despite extraordinary political and technological change. The first move preoccupied Europeans before World War I (Chapters 1-2); Japanese before World War II (Chapter 3); and Americans in the Cold War (Chapters 8-9). Today's strategists wonder if offensive cyberattacks will immediately render conventional forces impotent (Chapter 21).

This pattern is puzzling, given the rarity of quick and decisive wars, although it will be familiar to political scientists who have studied the "cult of the offensive."[1] Freedman does not offer a simple explanation for why observers were obsessed with the first move. Some were clearly reacting to Prussia's rapid victories in the 1860s. Others believed that industrial-age technology permanently shifted the balance to offensive forces, meaning that dug-in defenders would not last long. Military officers liked to map out the opening campaigns in future conflicts, which they believed they could control, as opposed to subsequent moves, which they could not. And reformers played up the drama of decisive campaigns to force reforms upon the military.

Above all else, the first move obsession reflected what Cathal Nolan calls "the allure of battle" (10).[2] The idea contains a curious mix of hope and fear. Quick and decisive campaigns imply clear outcomes and durable political settlements, a future in which wars are brief and peace is lasting. Short wars also appeal to strategists because they can maintain the link between policy objectives and organized violence before war takes on a life of its own. No one wants to get stuck in a war of attrition in which the costs of fighting overwhelm the value of the political object. The fear, however, is of quick and decisive defeat. National security is precarious if the armed forces are vulnerable to sudden catastrophe.

This kind of strategic schizophrenia is present today, most notably in the current U.S.-China competition. Doctrine on both sides emphasizes the importance of seizing the initiative through rapid attacks on enemy

[1] Stephen Van Evera, "The Cult of the Offensive and the Origins of the First World War," *International Security* 9:1 (Summer 1984): 58-107.

[2] Cathal J. Nolan, *The Allure of Battle: A History of How Wars Have Been Won and Lost* (New York: Oxford University Press, 2017).

communications, up to and including cyber attacks. Both sides hold out the hope that they can win by forcing the enemy into operational sclerosis, avoiding the need for a bloody, drawn-out fight. But the tactics and tools that might enable a quick victory also make it possible to imagine losing fast. Strategists understandably focus on preventing such a disaster, but as a result they might overlook plausible scenarios about what could happen after the first stage of fighting. These include the risk of nuclear escalation, on the one hand, and protracted conventional conflict on the other.[3] Freedman's book is a reminder that confident prewar predictions about the course of combat are routinely wrong. Leaders in Washington and Beijing should take his warning to heart.

The book's other major theme has to do with the desire to return war to the professionals. Freedman chronicles the stylized romanticism of pre-modern battle, which supposedly spared civilians, and the evergreen hope that new technologies will allow armies to return war to its proper place. The classical model of combat, which goes back to antiquity, took hold in the nineteenth century and never really let go. It confined wartime violence to organized battlefields, where the test of battle would determine political outcomes. "Military defeat would equal political disaster," Freedman writes, "but the war itself would not be so bad" (7).

The classical model must have been especially appealing to those who lived through ferocious post-World War II insurgencies. It is easy to understand the reverie for structured conventional battle, even if the reality of the past never really lived up to the classical ideal. It is also easy to understand the enthusiasm for the so-called revolution in military affairs, which supposedly heralded an era of warfare in which high-tech militaries regained their earlier advantages, and made war less terrible for civilians (186-192).

The wars in Iraq, Afghanistan, and Libya all began with the belief that modern technologies would enable something like the classical model. The precise application of force by highly professional militaries would enable quick victories. Armed forces, sufficiently trained and equipped, could target their counterparts with amazing precision. Wars would return to contests among armies, where the relative balance of training, tactics, and technology would determine the result. None of these conflicts went according to plan, however. It proved impossible to translate early military gains into lasting political results, and grotesque civil wars followed. Freedman shows that we have seen the same pattern of technologically driven optimism before, with similarly disappointing results.

These issues deserve more discussion. Unfortunately, Part II of the book turns away from the central question about how past observers have imagined future wars. Instead, it delivers an elaborate critique of quantitative political science, followed by a review of U.S. foreign policy in the 1990s and 2000s. While I sympathize with many of Freedman's arguments, most of them will be familiar to international relations scholars who have grown up in the midst of intense disciplinary fights over methodology. Moreover, much of the literature he criticizes is not about the conduct of war itself. It is about the causes of war, the frequency of fighting, the fraught efforts to count casualties, and the nature of international politics. Freedman's critique suggests that a

[3] On escalation, see Caitlin Talmadge, "Would China Go Nuclear? Assessing the Risk of Chinese Nuclear Escalation in a Conventional War with the United States," *International Security* 41:4 (Spring 2017), 50-92. On protraction, see Joshua Rovner, "Two Kinds of Catastrophe: Nuclear Escalation and Protracted War in Asia," *Journal of Strategic Studies* 40:5 (2017): 696-730.

historically nuanced approach would have yielded more reliable answers to these questions. This is a hugely important issue for scholars, but it feels like material for another book.

The book's conclusion is a plea for humility about the future, at a time in which predictive analytics are in vogue. Industry, academia, and government are captivated by the idea that more data, better data processing, and clever analytical collaborations can yield more accurate predictions. The U.S. intelligence community, for example, is investing heavily in different kinds of forecasting. The National Intelligence Council produces the unclassified Global Trends series, and the Intelligence Advanced Research and Projects Activity is currently managing several forecasting projects.[4] Freedman describes the Global Trends project but offers no analysis, and he does not discuss the other projects at all (273-276). This section is begging for commentary, especially given Freedman's long history of writing about strategic intelligence estimates.[5] It is not clear whether he thinks rigorous forecasting is useful, whether it could be improved, or whether it should be abandoned. Intelligence officials could surely use a frank assessment, even if it throws cold water on the project.

[4] Office of the Director of National Intelligence (ODNI), "Global Trends" (website), https://www.dni.gov/index.php/global-trends-home; for a catalog of IARPA-sponsored research, see: ODNI, "Anticipatory Intelligence," https://www.iarpa.gov/index.php/about-iarpa/anticipatory-intelligence.

[5] Lawrence Freedman, *U.S. Intelligence and the Soviet Strategic Threat* (London: Macmillan, 1986).

Author's Response by Lawrence Freedman, Kings College London

I am grateful to all the contributors for taking the trouble to review my book. It is a tradition in these responses not to comment about the kind words, but that does not mean to say that they are not appreciated and give me some confidence that, by and large, the book has something to offer. I welcomed Beatrice Heuser showing how my ideas could be developed and taken further. It may also be a tradition that there should be a vigorous defence in the face of all criticisms, but you cannot welcome criticism and then insist that all of it is invalid. This was a difficult book to write for reasons I'll explain. I struggled over some of the decisions regarding structure and scope. It would be odd if readers accepted all my decisions.

For example I take Joshua Rovner's point that I should have looked more at forecasting and provided more of a commentary on the National Intelligence Council's Global Trends series. I had assumed that the paragraphs on successive versions of this publication spoke for themselves by describing the struggle to get the methodology right and the gradual awareness that one of the challenges to a stable world order might lie within the United States, but I can see why this was insufficient. On the other hand I cannot see how a book on the future of war could concentrate on questions of conduct and set aside questions of causation and incidence. This is a book about the future of war, and not just warfare.

It is the case that up to 1990 these issues of causation and incidence were less prominent than they became. This was because the focus was on the next big great power war. As had been the case since the 1870s, when the book opens, the challenge was assumed to be one of imagining how it might be fought and what might determine victory or defeat. The likely belligerents were known, and while it was often hard to come up with a credible triggering crisis that was not something to worry too much about because the drama and excitement lay in how the war would unfold. During the Cold War the natural preoccupation was whether a future war, and indeed everything else, would end with massive nuclear exchanges. Then came the abrupt end to the Cold War. Only one super-power left was left standing. Suddenly nobody was sure where the next big war would come from, or if there would ever be one again, while remarkably few among the commentariat paid much attention to civil wars, despite the number that were then underway. Remarkably quickly, Western countries began to intervene in some of these civil wars, and the interventions became more ambitious and demanding. One thing led to another. Mix in the impact of 9/11 and soon there were major campaigns underway in Iraq and Afghanistan that were difficult to bring to a successful conclusion.

The security agenda therefore changed dramatically after 1990. One set of issues evaporated while a quite different set took its place. The challenge for me was how to write about this period. In the face of such discontinuity there could be no continuity in my narrative. This is the problem of the book's second section and it caused me a lot of difficulty. The first section was largely concerned with anticipations of wars between major powers. There were some persistent themes, for example whether war's effects could be contained and the developing fixation with knockout blows. Section Three to some extent returned to the focus on big wars and picked up on some of the earlier themes, and also allowed for an analysis of many of the current forward projections that are now shaping thinking on future war (cyber and information warfare, artificial intelligence, resource wars etc). But Section Two is all about discontinuity. And this in part reflects a prior failure of imagination. There was little written before 1990 that prepared policy-makers or the commentariat for the new situation. The possible collapse of state socialism in Europe, let alone the actual break-up of the Soviet Union, did not really come in to view until late 1988. While academics and commentators were trying to get their heads around the new situation, events moved on, and the situation became different again. They were continually trying to catch up with events rather than prepare for what might come next. I saw my task as capturing this perplexing period. Of course, as Mara Karlin observes, this became more descriptive because the 'future war' literature did not really exist so I was looking instead at waves of theorising that attempted to draw the first lessons from events.

To add to the challenge, this was a period in which the academic community, especially in the fields of international relations/ security studies, was also going through major change – expanding in numbers, broadening in interests, and increasingly embracing quantification. The limited research on matters such as ethnic conflict, civil wars, humanitarian intervention, mass casualty terrorism, nation building, counter-insurgency and so on prior to 1990 was more than matched by the surge of activity after 1990, with issues being picked up as they hit the headlines. I thought it worthwhile to assess the performance of the academic community in such demanding circumstances. It was, after all, the main provider of new ideas to the wider policy community. How well did they do from the perspective of a consumer? This is the issue at the heart of Section Two. It is about how theorists and practitioners alike struggled to come to grips with this state of affairs.

Rovner suggests that the material on social science methodology and data in this section fits awkwardly and disrupts the flow of the book, which to a degree was unavoidable once I had decided to address this issue. Gibner, most seriously, complains that the social science endeavour has been misrepresented and that I have drawn incorrect conclusions. My analysis is not, however, yet another round in the battle between historians and social scientists but if anything points more to how the academy adapted. In this respect I think Gibner is defending what is increasingly recognised to be a limited and dated approach by younger academics, including those who regularly use data sets and statistical analysis.

Gibner caricatures my position as trying to dismiss political science by trying to show that it is no different from the idealists who tried to re-imagine war after World War One. I am in fact interested in looking at how academic contributions (and not only from political science) are shaped by the historical setting in which they emerge and are developed. I am interested in political science qua politics, not qua science.

The early pioneers in the 1950s and 1960s who urged a science of international relations comparable to the natural sciences did believe strongly that this would improve the chances of peace. This meant that their original questions, and data collection, were shaped by the challenge they sought to mount to the realists, with their more tragic view of international affairs. They were thus caught up in the same debates about great power relations as the realists. This meant, as was widely acknowledged after 1990, that they were unprepared to deal with a sudden surge of interest in civil wars. Moreover, close examination of the main data set—the correlates of war (COW)—revealed numerous problems with the data itself. I spent a whole chapter on the problem of counting casualties not just to show that COW regularly got the numbers wrong, but more importantly to show how difficult it is to get the numbers right however hard you try, and that the decisions you make on what to count, for example excluding civilians or consequential deaths through famine and disease, skew the analysis. One reason, incidentally, why I stuck with this chapter is that ours is a field that deals in death yet rarely addresses it directly (somebody commented that this was one of the bleakest book chapters they had read).

It was because of this interest in the quality of the original data set that I picked up on the article Gibler co-authored for ISQ on problems with the militarised-interstate dataset (MID). His response to my use of this article illuminates what I think is a fundamental difference in approach. It starts with the word 'reliability.' He says a "measure is reliable when it produces similar results *under consistent* conditions." Well that is one definition that might serve a scientific purpose, especially if you believe that in our field there can ever be consistent conditions, but I think I am entitled to use a more common definition. Here is the *Oxford English Dictionary* definition: "Consistently good in quality or performance; able to be trusted."

When historians, not being scientists, come across a piece of research that for whatever reasons keeps on using dubious evidence we find it very difficult to trust the conclusions even if we might suspect that better evidence might support them. Gibner , however, finds it reassuring that even after finding so many mistakes the results from the improved MID come out largely the same. ('Though over 70% of the cases had coding changes, the results analyzing previous models were mostly the same'). At any rate his research clearly improved this data base so hopefully it can now support more trustworthy research. I note in the book that the number as well as the quality of the data bases has generally improved (although for those worried about consistency this produces a different sort of problem). But my main interest lay in the use being made of the research.

This is relevant to the discussion of the Democratic Peace. Gibler thinks I got this "horribly wrong." I think he completely misses the point of my discussion. I was interested in the topic because it helped demonstrate how easy it was to complicate an apparently simple proposition, especially once researchers had to grapple with the conceptual issues raised by the meaning of democracy and its relation to liberalism (a staple for political theorists). I concluded that the theory was in effect generalising from Cold War Western Europe and North America, which was full of democracies at peace with each other. Giving the theory that context helped explain its power but also its limitation. For the purposes of my book what was important was how much the idea that democracies do not fight with each other had been picked up and cited regularly by political leaders. The Democratic Peace turned into a slogan, and war-torn societies were given democratic forms, without the benefit of the necessary social conditions and political institutions, with unfortunate consequences.

Which brings us to another contentious word—'context.' First, we can agree that historians (and I should note I began life as political scientist and consider myself a bit of a hybrid) also theorise and also that we want to generalise. We all look for patterns and recurrent forms of causation. We resist getting stuck on individual episodes and we look for parallels and comparisons, and sometimes even counter-factuals. But this is a matter of degree. At one extreme there is an individual case, from which it is dangerous to generalise, and at another there is a collection of episodes from the last 200 years that have some similarities (sufficient at least for the purposes of coding) though they come from completely different parts of the world and moments in international history. That is, they have very different contexts. Somewhere between the two there are propositions based on instances which share contexts—for example they come from the same period and share many significant features.

Second, I also agree with Gibler's statement that: "A useful model will not explain the peculiarities of every case. Similarly, peculiar cases should never be used for post hoc theorising. Truly unique historical moments are no basis for theory; hard cases make bad law." But here we come to a difficulty. Inter-state wars are actually quite rare. Everyone of them is peculiar. Each is an outlier. Though Gibler writes that "political science has done an excellent job of identifying the structural conditions that make conflict between states likely," no model could have predicted that Russia would annex Crimea or the U.S. invade Iraq (or can now tell us what will happen with Iran). These episodes required consideration of the peculiarities of the situation. This is because war is a matter of choice, whatever the structural conditions, and to understand choices you need to understand the personalities involved and the variety of factors that impinge on them at a particular moment.[1] If everything is determined by structure then there is little point in bothering to produce policy-relevant research. To me the subject only comes alive when studying how choices were made in the past and how they might be made in the future.

The rarity and peculiarity of inter-state wars helps explain the limited results, given the amount of effort expended, achieved by social scientists when it came to explaining their causation and incidence. More positively, I was increasingly struck by the much more impressive results achieved by those working on civil wars (which is why I why ended up citing a lot of their research). The great advantage of the work on civil wars was that (sadly) there have been far more cases to compare and contrast even within a relatively short time period. In addition, the best scholars did not pretend that analysing whatever could be quantified was ever sufficient. Their statistical analysis was usually combined with thorough case studies, and a real effort to get to know the countries and regions they were studying. They took account of context and peculiarity. Let me conclude by quoting myself. I note that all studies of IR (whatever school) remained state-centric in the early years after 1990 but then improved: "Over time the best studies were those that kept the statistical work on tap rather than put it on top, combining it with field work and archival research. As a result their conclusions were often less clear-cut, but they were more reliable" (147).

[1] Gibler says that prior to 1993 realists "believed that deterrence worked, that the content of issues was trivial, and that an equal likelihood of conflict remained among all states in the system." Well this may have been true of a few dogmatic neo-realists but emphatically not so when it comes to other scholars and obviously historians. Most of us (and I was 44 by 1993) did not believe such nonsense (and how could you during the Cold War). I am happy to provide personal citations on request.