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Michael C. Horowitz. ***The Diffusion of Military Power: Causes and Consequences for International Relations.*** Princeton: Princeton University Press, 2010. 286 pp. ISBN: 9780691143958 (cloth, $75.00); 9780691143965 (paper, $26.95).

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Introduction by William C. Wohlforth, Dartmouth College

While knowledgeable observers rightly discounted the geopolitical significance of China's launch of the refurbished Russian aircraft carrier *Varyag* last August, the event did underscore the salience of the topic of this H-Diplo roundtable. No student of international relations can be indifferent to the questions that Michael Horowitz addresses in *Diffusion of Military Power*. Will China ultimately develop true carrier warfare capacity? Why has U.S. supremacy in this area gone unchallenged for over half a century? How likely is it that China will emulate other aspects of U.S. military power? How many new nuclear powers are we likely to see? Will Iranian drones soon patrol American skies? Horowitz develops and tests a bold structural- and material interest-based theory to explain the propensity of military innovations to diffuse through the international system.

As all the reviewers in this symposium stress, Horowitz's theory runs against explanations that feature culture, norms, and the particular features of individual cases. Given that explanations of this type have dominated the literature in recent years, the current symposium could easily have descended into a zero-sum slugfest. Yet Horowitz managed to present his study in a way that encourages critical readers to engage his arguments productively rather than to attack. Indeed, the reviewers are unstinting in their praise. The book is "genuinely pathbreaking," says Adamsky. Grissom calls it a "brilliant new conceptual framework." For Farrell, it is an "important and carefully argued book" that "completes our understanding of military emulation." Going beyond general praise, the reviews that follow treat readers to compact assessments of the state of research in this key field of inquiry, excellent capsule descriptions of an important new work in that field, and promising arguments about policy implications and future research directions.

Needless to say, it is not a love fest. All the reviewers note areas of competition as well as complementarity between Horowitz's structural account and explanations that are more attentive to specifics. Adamsky stresses that without specific attention to a given military's intellectual capacity to "generate a new theory of victory," Horowitz's model may miss variation in adoption capacity, Mahnken is skeptical of any account that excludes strategic culture, and Farrell insists that material interests cannot fully explain Germany's and Japan's decisions not to develop nuclear weapons.

But Grissom highlights the biggest question: can standard bottom-up and context-heavy accounts be reconciled with top-down structural theories like Horowitz's? Grissom is pessimistic on this score, but I'm not sure that is warranted. After all, different approaches to explaining diffusion might be compatible even if they are never truly reconciled. If the aim is nuance, as Mahnken suggests, Horowitz still provides a great place to start an inquiry. And critical push back can lead to refinements in the theory or further empirical tests, as Farrell and Adamsky argue. In the end, the overall tenor of the reviews suggests optimism on this score. This refreshingly positive and productive outcome is a testament to the quality of the work under scrutiny, and of the assembled reviews it attracted.

Participants:

Michael C. Horowitz is an associate professor of political science at the University of Pennsylvania. He is the author of *The Diffusion of Military Power: Causes and Consequences for International Politics* (Princeton University Press, 2010). In addition to his work on military innovation and military technologies, he is working on two book projects, one i the role of leaders in international politics and one on the relationship between religion and conflict. His work has been published in *International Security*, the *Journal of Strategic Studies*, *International Organization*, *Orbis*, the *Journal of Conflict Resolution*, and elsewhere. He received his PhD from the Department of Government at Harvard University and his BA in political science from Emory University.

William C. Wohlforth is the Daniel Webster Professor at Dartmouth, where he teaches in the Department of Government. His most recent publications are "Assessing the Diffusion of Power: How Not to Evaluate Theories." *International Studies Quarterly* (forthcoming, March 2012), "Gilpinian Realism and International Relations," *International Relations* (forthcoming Spring 2012), "No One Loves a Realist Explanation: The Cold War's End Revisited" *International Politics* Vo1. 48: Nos. 4/5 (July/September 2011), and *International Relations Theory and the Consequences of Unipolarity* (Cambridge, 2011), coedited, with G. John Ikenberry and Michael Mastanduno).

Dmitry (Dima) Adamsky is an Assistant Professor at the Lauder School of Government, Diplomacy and Strategy, IDC Herzliya. His latest publications include: *The Culture of Military Innovation* (Stanford CA.: Stanford UP, 2010) and "The War over Containing a Nuclear Iran: The Morning After in Israel," *Foreign Affairs*, April, 2011. His current projects explore regional nuclear doctrines and the influence of ideational and cultural factors on nuclear weapons and strategy.

Theo Farrell is Professor of War in the Modern World at the Department of War Studies in King's College London, and Vice Chair of the British International Studies Association. He is a former Associate Editor of *Security Studies* and *Review of International Studies*. He currently holds a three-year ESRC/AHRC research fellowship (2009-2012) funded under the Research Council's UK "Global Uncertainties" programme. Professor Farrell's latest book, co-edited with Terry Terriff and Frans Osinga, is *A Transformation Gap? American Innovations and European Military Change* (Stanford, CA: Stanford University Press, 2010). His forthcoming books include a study on Army Transformation in Britain, France and the United States (co-authored with Terry Terriff and Sten Rynning), and a volume on Miltiary Adaptation in the Afghanistan War (co-edited with Frans Osinga and James Russell).

Adam Grissom is a Senior Research Fellow and Adjunct Professor at Georgetown University's Edmund A. Walsh School of Foreign Service as well as a Senior Political Scientist at RAND. His research focuses on military innovation, small wars, and special operations forces. Recent publications include *Train, Equip, Advise, Assist: The USAF and the Indirect Approach to Countering Terrorist Groups Abroad*, and "The Future of Military Innovation Studies," in the *Journal of Strategic Studies*.

Thomas G. Mahnken is Jerome E. Levy Chair of Economic Geography and National Security at the U.S. Naval War College and a Visiting Scholar at the Philip Merrill Center for Strategic Studies at The Johns Hopkins University's Paul H. Nitze School of Advanced International Studies (SAIS). He served as the Deputy Assistant Secretary of Defense for Policy Planning from 2006-2009. He is the author of *Technology and the American Way of War Since 1945* (Columbia University Press, 2008), *Uncovering Ways of War: U.S. Intelligence and Foreign Military Innovation, 1918-1941* (Cornell University Press, 2002), and (with James R. FitzSimonds) of *The Limits of Transformation: Officer Attitudes toward the Revolution in Military Affairs* (Naval War College Press, 2003). He is editor of *The Journal of Strategic Studies*.

Review by Dmitry (Dima) Adamsky, Lauder School of Government, Diplomacy and Strategy, IDC Herzliya

Actors throughout the international system respond differently to major military innovations. These differences may have significant impact on the key issues of international relations (IR). When the 'debut' of a major military innovation reveals its significance, actors in the international system start to respond. Why do actors' responses vary? Why are some actors more successful than others in adopting major military innovations? What are the consequences for international politics? These are the central questions which Michael C. Horowitz explores in his book. The author examines the driving forces underlying the spread of military innovations and explores the consequences of this diffusion for international politics.

The book is a genuinely path-breaking endeavor. While most of the military innovations' literature stays at the invention stage of military innovation, Horowitz goes beyond it. The book distinguishes itself from the mainstream by exploring how states respond to military innovations and how the subsequent spread of responses influences distribution of power in the international system. By explaining this major, yet overlooked and little understood issue, Horowitz makes a significant contribution to the military innovations and IR literature. Horowitz builds on previous research and by adding his own theoretical contribution, and generates a coherent analytical framework for understanding the spread of military power.

To explain the patterns of military innovations' spread and its influence on the systemic level, Horowitz introduces adoption-capacity theory. The theory explains the reality from the point when states are compelled to respond to a certain military innovation that emerges elsewhere in international system. According to Horowitz, following the demonstration phase, potential state responses include: adoption, offsetting by a counter measure, allying with likely adopter, or shifting towards neutrality. Horowitz focuses on innovation adopters and asks what factors determine the paths that particular states will adopt and how military power spreads throughout the international system.

Horowitz argues that two key factors determine a state's capacity to adopt an innovation: financial intensity and organizational capital. His adoption-capacity theory postulates that once states have registered an innovation, the diffusion of military power will be mostly governed by the level of financial intensity and the amount of organizational capital required for adoption of a military innovation. Financial intensity stands for the investments in procuring military hardware associated with the innovation - per unit cost of the particular tool of war. Organizational capital stands for the bureaucratic capacity to transform along the lines dictated by the innovation and to adapt to the emerging strategic and operational reality. Organizational capital, according to Horowitz, includes: focus on the critical organizational task, institutional commitment to experimentation, and organizational age.

Horowitz's theory argues that the financial and organizational capital required for the adoption of a particular innovation will determine the choices of individual states that in their turn will eventually govern the system level distribution of responses and balance of power. "The greater the financial intensity and the organizational capacity required to implement the innovation, the slower its spread at the system level and the lower the probability that a state will attempt to adopt the innovation." According to the author, "the key determinant of success is whether the financial and organizational requirements for implementing the innovation 'match' the capabilities of the state pursuing an adoption strategy." (41-42)

In terms of theory building, Horowitz capitalizes on and moves beyond both the neo-realist and constructivist schools of thought. His analytical framework encapsulates arguments about the critical role that threats and strategic competition, international and domestic norms and cultural factors play in governing diffusion process. His model explains why different types of military innovations spread differently among different types of states, enabling him to formulate hypotheses about states' responses to innovations and predictions about the systemic level distribution of responses.

Four chapters test the adoption-capacity model against the following empirical cases studies: mid-twentieth century carrier warfare, nuclear weapons, early twentieth century battle-fleet warfare and suicide terrorism. Each chapter outlines the nature of the innovation, the actors' responses, the impact of diffusion on the international system, and then tests the findings against predictions. The concluding discussion of each chapter explores how specific innovations influenced power balances, alliance patterns and duration of warfare or its probability.

Horowitz's work is an example of rigorous social science research. His case selection focuses on both state and non-state actors, goes beyond the Western world, spans a period of a century, and maximizes variation on financial intensity and organizational capital – two key independent variables. Specifically, according to the author, carrier warfare demanded high levels of financial and organizational capital to adopt. Nuclear weapons required high levels of financial intensity and low levels of organizational capacity. Suicide terrorism required low levels of financial investment but high levels of organizational intensity. Finally, battle-fleet innovation required, according to Horowitz, medium level of adoption capacity both financially and organizationally. To increase confidence in the accuracy of the results while establishing causality and to ensure that the micro- and macro levels of analysis are consistent with his theory, Horowitz skillfully utilizes a multi-method approach. In the nuclear weapons and suicide terrorism chapters he employs quantitative statistical analysis, and for exploring the battle-fleet and carrier warfare he relies on qualitative process tracing.

The book's findings and its theoretical contributions are most impressive. Horowitz convincingly shows that his theory is useful for explaining changes in military power on the micro level and also for explaining the pace of the rise and decline of states on the systemic level of IR. Empirical cases explain success and failure in strategic behavior of both state and non state actors and offer an analytical scheme for making predictions about changes in warfare and the attractiveness of alternative strategic choices at the unit level. The spread of the innovation or the lack thereof, and its diffusion patterns determine the distribution of power in international politics, thus accounting for the power transition among major international actors. On the system level, his theory explains the shifts in relative power, accounts for the military power distribution disparities and, thus enables more accurate hypotheses about prospective trends in international politics. Horowitz's theory demonstrates what powers are more likely to rise and to decline, given that the military component indeed accounts for power transitions. The book offers important insights beyond the IR theory for national security policy experts. Analyzing the emerging transformations in the nature of warfare during the information age through the lenses of his theory, Horowitz makes thought-provoking predictions about strategic and operational behavior of state and non-state actors in the context of the three main trends of the emerging battlefield: precision warfare, robotics and cyber-warfare.

To verify Horowitz's analytical framework I applied it to another major military innovation - the Information Technology Revolution in Military Affairs (IT-RMA) in Russia, the U.S. and Israel. Adoption-capacity theory indeed elegantly explains the puzzling variation in the paths taken towards this innovation by military communities in three countries. However this test also revealed a minor flaw of the book. *The Diffusion of Military Power* does not differentiate clearly between a state's aptitude to envision what Stephen Rosen calls a "new theory of victory"[1] and a state's institutional capacity to implement this vision. Ability to formulate the "new theory of victory" refers to capacity to think systematically about the changing character of warfare, to register the emerging military regime and to envision how innovative trends in warfare can be incorporated into a state's strategic and operational practices. Crafting a "new theory of victory" is a separate conceptual-theoretical endeavor. It is the first stage of adopting foreign innovation. In the subsequent stage this abstract vision transforms through bureaucratic-institutional steps into new military doctrine and concept of operations. Horowitz does not designate it as a separate variable and merges these two endeavors under the rubric of *organizational capacity*.

This sub-division does not seem to be purely semantic. Ability to register the relevance of the foreign military innovation for the emulating a state's operational environment demands an investment of significant intellectual capital. Intellectual rigor of a military community enables it to register exposition of the major military innovation and to realize that it can be beneficially co-opted in the domestic military posture. In principle, this endeavor chronologically precedes financial and organizational investments. The vision of the new theory of victory is usually the driving force behind the financial and organizational steps. This conceptual ability depends on staff work in the field of military thought and operational art. Existence of this capacity should not be taken for granted across the military communities. One also should not assume equal intellectual capacity of every military community to generate a new theory of victory.

[1] Stephen Peter Rosen, *Winning the Next War: Innovation and the Modern Military* (Ithaca: Cornell UP, 1991).

Since institutional commitment and capacity to think systematically about the emerging trends in warfare and to grasp the potential embedded in the foreign military innovation vary from one military organization to another, this variance may influence the spread of the innovation. States may possess financial and organizational capacity but still fail to adopt the innovation due to the low institutionalization of the systematic military thinking. As the intellectual history of the Blitzkrieg and IT-RMA demonstrate, variance in the capacity of military thought among professional communities explains the gaps of time between familiarity with technology and understanding of its potential impact on the nature of warfare.

Horowitz's earlier works suggest acceptance of this argument. Several passages in this book also attest to it, particularly where he mentions military thought and doctrine development as components of the organizational endeavor, where he discusses the relationship between technology and military innovation and also where he explores the issue of the "critical task focus." (pp. 35-37). However in all these places the discussion does not deal explicitly with the conceptual capacity to envision future war. This may leave a reader with the impression that the author assumes this capacity to be equal across the cases. More explicit discussion about the investment of institutional-intellectual capital aimed at developing a new theory of victory, and its variation across the cases, might have increased the analytical rigor of adaption-capacity theory and its ability to make predictions about the spread of military innovations.

Moreover, theories of victory originate differently in various ideational environments. Horowitz clearly acknowledges cultural factors as an enabler and conditioner of military innovations. However, lengthier discussion of that topic would be welcome. Under the impact of cultural factors, different states may have varying propensities for innovations on different levels of warfare. As some states are doing better on the strategic level, some excel on the level of operational art and others stand out on the tactical level. The same may be true about their ability to emulate innovations on various levels of warfare. If innovation demands significant investment on a particular level of warfare, different states may manifest different levels of adoption capacity.

These small oversights aside, Horowitz has written an outstanding book that offers a more accurate explanation for the causes and consequences of transformations in military regimes than any previous account. *The Diffusion of Military Power* - a major contribution to IR literature - should be well received as an excellent reference work for scholars, teachers, students of international security and military innovations, and for practitioners of security policy planning.

Review by Theo Farrell, King's College London

This fine book hammers home the essential point that military power rests on emulation as much as innovation. Indeed, the development of warfare is mostly a story of the pack following a few trailblazers. So how can we explain the diffusion of military innovations? Realists posit a process of natural selection. States emulate those innovations that have proven successful in war. Those states that fail to innovate or emulate, will eventually go out of business.[1] Constructivists posit a process of social selection. States emulate the innovations of the powerful because this confers status.[2] These theories focus on the reasons for emulation. Horowitz elaborates and tests a theory of state capacities for emulation.

Horowitz seeks to explain both individual state behaviour and the international pattern of military diffusion. Why do some states emulate and not others? And why are some military innovations widely emulated and not others? These questions matter because successful military innovations are potential game-changers; key innovations can win wars and disrupt the balance of power. Since the late 1980s, a sizeable literature has grown on military innovation.[3] This has fed a policy appetite to learn how to out-innovate the enemy. But for scholars and students of world politics, military emulation is arguably the more important phenomenon. Yet it is far less studied.[4]

Horowitz proposes a new "adoption-capacity theory" of military emulation. The theory seeks to explain both individual state choices about, and the international pattern of, military emulation. It does so in terms of the "resource mobilization challenges and organizational changes required to adopt the new innovation" (3). In essence, Horowitz argues that ease will determine extent of emulation. Military innovations that have high financial intensity (cost per unit), and require high organisation capital (to undertake change) are less likely to be emulated than innovations which are cheaper and less disruptive for military organisations. Equally, states that have high financial and organisational capital are more likely to adopt the expensive and troublesome innovations, than states with limited financial and organisational resources. Often cases present variations along both vectors, i.e., high/low cost and high/low organisational capability.

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[1] Joao Resende-Santos, *Neorealism, States, and the Modern Mass Army* (Cambridge: Cambridge University Press, 2007).

[2] Dana P. Eyre and Mark C. Suchman, "Status, Norms, and the Proliferation of Conventional Weapons: An Institutional Theory Approach," in Peter J. Katzenstein, ed., *The Culture of National Security* (New York: Columbia University Press, 1996), 79-113.

[3] For a literature review, see Adam Grissom, "The Future of Military Innovation Studies," *Journal of Strategic Studies* 29: 5 (2006): 905-34.

[4] Emily O. Goldman and Leslie C. Eliason, eds., *The Diffusion of Military Technology and Ideas* (Stanford, CA: Stanford University Press, 2003).

The theory is tested using four case studies of military innovations – battlefleet warfare, carrier warfare, nuclear weapons, and suicide terrorism.

Horowitz provides a compelling account of military emulation in naval warfare. The big-gun, heavily armoured British battleship, HMS *Dreadnought*, transformed naval warfare in the early twentieth century. Commissioned in 1906, *Dreadnought* threatened to blow all opposition out of the water. Within a matter of years, all the major naval powers were acquiring their own battleships. In World War II (WWII), aircraft carriers overtook battleships as the most powerful naval platform, a position that continues to this day. Yet only the United States has maintained an effective carrier force, currently with 11 large-deck carriers and 12 small carriers for short take-off and vertical landing (STOVL) jets. France and Brazil each have one large-deck carrier (both inclined to break down), and India, Italy, Russia, and Spain each operate one small STOVL carrier. In 2010 Britain scrapped its two STOVL carriers and committed to building two large-deck carriers by 2020, thus leaving the Royal Navy without a carrier capability for ten years.[5] Moreover, incredibly, the Royal Navy will be given only enough resources to operate one carrier, and so the other will be put into storage. One carrier does not provide a continuous at-sea capability. If carriers are the capital ships of naval warfare, why is only the U.S. navy equipped for carrier warfare? For starters there is the unit cost; carriers, like battleships before them, are awfully expensive. But more than that, as Horowitz correctly argues, carrier warfare presents an enormous organisational challenge given the "complicated systems integration tasks" required (91). This is what stumped the Soviet navy when it tried to build a carrier force from the mid-1960s on. Horowitz neatly draws out the policy implications. The US Navy should not get too excited about China's attempt to build a couple of carriers. Given the organisational challenges involved, greatly compounded by Chinese military culture, the effort to construct a carrier fleet is likely to be long, costly and ultimately fruitless for the Chinese, just as it was for the Soviets.

For me, less compelling is Horowitz's analysis of the diffusion of nuclear weapons. Horowitz points out that mere possession of a nuclear weapon is sufficient to deter, suggesting that a state does not need to bother developing a complex nuclear force structure (105-107). This leads him to argue that nuclear weapons require high financial and low organisational mobilisation. Unfortunately, building a basic nuclear bomb is not as expensive, nor as difficult, as one might hope.[6] More to the point, what we know of

[5] A recent UK parliamentary report concludes that the new carrier may not be operational until 2030, leaving Britain without a carrier for twenty years. Public Accounts Committee, 56th Report, *Providing the UK's Carrier Strike Capability*, 23 November 2011, at http://www.publications.parliament.uk/pa/cm201012/cmselect/cmpubacc/1427/142702.htm .

[6] Anna M. Pluta and Peter D. Zimmerman, "Nuclear Terrorism: A Disheartening Dissent," *Survival* 48: 2 (2006): 55-69.

emerging nuclear powers suggests that the covert acquisition and safe operation of nuclear weapons present enormous organisational challenges.[7]

Horowitz's puts down to cost the slow and limited diffusion of nuclear weapons.. He finds that few states can afford such weapons, and even for wealthy states, like Germany and Japan, it is far more cost-effective to rely on the US nuclear umbrella. Surely a more convincing explanation has to do with norms and identity. Germany and Japan have not weaponised their civilian nuclear programmes because to do so would be an anathema to their respective anti-militaristic strategic cultures.[8] At the same time, notwithstanding the "nuclear taboo" with respect to the use of nuclear weapons, there is no evidence of an international norm against the possession of nuclear weapons.[9] Thus when Horowitz concludes that the decreasing cost over time of nuclear weapons "has opened the door to new adopters" (133), I would simply add 'whose strategic cultures are comfortable with acquiring nuclear weapons."'

The suicide terrorism case presents something of a puzzle. Suicide attacks are very effective at killing large numbers of people. Given this, Horowitz wonders why the older terrorist groups were slow (PLO), or indeed did not (ETA, IRA), adopt this tactic. One obvious answer is that some terrorist groups don't want to cause mass casualties for fear of alienating their social base. Horowitz responds to this argument by pointing out that suicide attacks need not necessarily involve high civilian casualties; the Tamil Tigers used suicidal bombing most effectively against military targets. The Provisional Irish Republican Army (PIRA) case is especially intriguing. Irish terrorists did commit suicide for their cause. In 1981, seven PIRA prisoners in a Northern Ireland jail starved themselves to death in protest at being treated as ordinary criminals instead of prisoners of war. So why not engage in suicide attacks against military targets? Horowitz's explanation is pretty convincing. PIRA operations "placed a premium on preserving the expertise and dedication of trained volunteers" (202). PIRA had a conservative and clunky organisational structure, with several layers of command above its Active Service Units, and this prevented the scale of organisational change that would have been required to adopt suicidal tactics.

In this important and carefully argued book, Horowitz completes our understanding of military emulation. Constructivists have argued that the pace and extent of emulation may

[7] See, for example, David Albright and Corey Hinderstein, "Unraveling the A. Q. Khan and Future Proliferation Networks," *The Washington Quarterly* 28: 2 (2005): 111-128; Peter R. Lavoy, Scott D. Sagan, and James J. Wirtz, eds., *Planning the Unthinkable* (Ithaca, NY: Cornell University Press, 2000).

[8] Thomas U. Berger, *Cultures of Antimilitarism: National Security in Germany and Japan* (Baltimore, MD: The Johns Hopkins University Press, 1998).

[9] William Walker, "The Absence of a Taboo on the Possession of Nuclear Weapons," *Review of International Studies* 36: 4 (2010): 865-76; Nina Tannenwald, *The Nuclear Taboo* (Cambridge: Cambridge University Press, 2007).

be explained by the cultural fit between the military innovation and the adopting state.[10] Horowitz focuses on the material fit. He offers a powerful theory and through meticulously researched cases, demonstrates its wide applicability. The new fashion in Security Studies is to synthesise theory to produce more powerful explanations of social outcomes, instead of marshalling history to engage in competitive theory testing.[11] A recent study of military emulation within the North Atlantic Treaty Organisation demonstrates the virtue of such analytical eclecticism. The evidence suggests that variation in European emulation of U.S. military innovations this past decade has been shaped by the interaction of cost and cultural constraints.[12] A major strength of Horowitz's book is that he shows how his theory complements as well as challenges existing theories of emulation.

[10] Theo Farrell, "Transnational Norms and Military Development: Constructing Ireland's Professional Army," *European Journal of International Relations* 7: 1 (2001): 63-102; Emily O. Goldman, "The Cultural Foundations of Military Diffusion," *Review of International Studies* 32: 1 (2006): 69-91.

[11] Peter J. Katzenstein and Rudra Sil, eds., *Beyond Paradigms: Analytical Eclecticism in World Politics* (New York: Palgrave, 2010).

[12] Terry Terriff, Frans Osinga, and Theo Farrell, eds., *A Transformation Gap? American Innovations and European Military Change* (Stanford, CA: Stanford University Press, 2010).

Review by Adam Grissom, Georgetown University

Michael Horowitz's *Diffusion of Military Power* offers a brilliant new conceptual framework for understanding how military innovations spread across borders. Moreover, in so doing the book cuts against the grain of a decade of military innovation scholarship in terms of subject matter, method, and style of argumentation. The end result is that all-too-rare find: an innovative book on innovation.

In recent years military innovation research has gravitated toward micro-level phenomena. One line of research, led by Theo Farrell and James Russell, explores bottom-up processes of innovation and adaptation among tactical units.[1] Another line, exemplified by Dima Adamsky's *Culture of Military Innovation*, examines the impact of cultural lenses on innovation, armed forces in particular.[2] A third focuses on the problem of learning.[3] The common thread linking these three lines of research is the conception of innovation as a highly contingent process best understood, and studied, within the peculiarities of a given context.

Refreshingly, the *Diffusion of Military Power* cuts directly in the opposite direction, marking a return to the roots of the field by making bold arguments about the structural characteristics of the international system, states as more-or-less unitary actors, and the generalizability of positivist research.

Horowitz begins by noting, correctly, that the diffusion of military innovations throughout the international system is only very partially understood. Enquiry into this particular aspect of innovation studies has stagnated since Goldman and Eliason's 2003 edited volume on *The Diffusion of Military Technology and Ideas*.[4] Goldman and Eliason stopped short of developing a generalized conceptual framework for understanding why some states adopt military innovations from elsewhere and why others refuse. The purpose of Horowitz's volume is to develop such a framework.

[1] See for example, Theo Farrell, "Improving in War: Military Adaptation and the British in Helmand Province, Afghanistan, 2006-2009," *The Journal of Strategic Studies*, volume 33, number 4, 2010, 271-296; James Russell, *Innovation, Transformation, and War: Counterinsurgency Operations in Anbar and Ninewa Provinces, Iraq, 2005-2007* (Palo Alto, CA: Stanford University Press, 2010).

[2] See for example, Dima Adamsky, *The Culture of Military Innovation: The Impact of Cultural Factors on the Revolution in Military Affairs in Russia, the US, and Israel* (Palo Alto, CA: Stanford University Press, 2010); Eitan Shamir, *Transforming Command: The Pursuit of Mission Command in the U.S., British, and Israeli Armies* (Palo Alto, CA: Stanford University Press, 2011); Theo Farrell and Terry Terriff (eds), *The Sources of Military Change: Culture, Politics, Technology* (New York: Rienner, 2001).

[3] See for example, Robert Foley, Stuart Griffin, and Helen McCartney, "Transformation in Contact: Learning the Lessons of Modern War," *International Affairs*, volume 87, number 2, 2011, 253-270.

[4] Emily Goldman and Leslie Eliason, *The Diffusion of Military Technology and Ideas* (Palo Alto, CA: Stanford University Press, 2003).

Horowitz seeks to answer the question of "why some military innovations spread and influence international politics while others do not…" (3) by conducting four case studies. These are the dreadnought (the first 'all big gun' battleship type, developed by Britain at the turn of the century), the aircraft carrier, nuclear weapons, and suicide terrorism. The cases are based on a mix of primary and secondary sources, tilting toward the latter in all except the dreadnought. The cases are concise and well presented. While they do not necessarily make fundamental new contributions to the historiography on each individual innovation, the cases will be valuable to many H-Diplo members as solid capsule histories of interesting and important innovations.

Moreover, what Horowitz's case studies do explore, and what most conventional histories do not, is how each innovation subsequently did or did not diffuse throughout the international system. In each of the four cases the individual story of the innovation itself is well known but the subsequent diffusion story is not; partly because it generally didn't happen. The dreadnought was the dominant warship type of its time, yet most countries did not acquire them. The same is true for the aircraft carrier today. The widespread proliferation of nuclear weapons has repeatedly been predicted over the past fifty years, but has not yet occurred. In more recent years, the tactical effectiveness of suicide terrorism led many to worry that the technique would spread to many terrorist and insurgent groups. The actual pattern of diffusion has been modest. Horowitz thereby engages in that most difficult of historiographical tasks, explaining why some things happened and others didn't. It is to his credit that all four cases offer plausible explanations for the non-occurrence of diffusion, though inevitably the evidence is sparser than it is for events that did happen.

Horowitz derives three major findings from the cases. First, at the level of the international system, the diffusion of a military innovation is governed by two variables: the "financial intensity" of the innovation and the "organizational capital" required to adopt it. (209) Horowitz uses single unit cost as a proxy for financial intensity (e.g. the cost of a single aircraft carrier) and he measures organizational capital requirements in terms of the degree of change implied to personnel recruitment, training, and doctrine. He argues that any particular military innovation can be characterized in terms of these two variables. The higher the financial intensity of an innovation, the slower and more limited diffusion will be throughout the international system. Likewise, the higher the requirement for organizational capital, the slower and more limited the diffusion. The interaction of these two variables yields four generalized possibilities for any given innovation, captured in the social scientist's old standby, the four-cell table. Notably, only one of the four combinations is compatible with quick diffusion – innovations that are both low cost and require little organizational capital to implement. All others will diffuse less broadly and quickly. In a broad sense, then, the study of diffusion will most often be a study of non-diffusion.

Horowitz's second major finding applies at the individual state level. He argues that the capacity of individual states, in terms of financial resources and organizational flexibility, will influence whether they adopt an outside innovation. In Horowitz's view, great powers will tend to be wealthy but not as organizationally flexible because of internal vested interests, particularly in military organizations. Lesser powers will tend to have fewer financial resources but also fewer vested interests constraining their flexibility.

Horowitz derives his third major finding from the interaction of the previous two. The diffusion of an innovation throughout the international system, and its impact on that system, will be governed by the interaction between the financial and organizational intensity of the innovation, on one hand, and the financial and organizational capacity of states on the other hand. Innovations that are high-cost but require little organizational change will tend to favor existing great powers (Horowitz puts nuclear weapons in this category). Innovations that are low-cost but require considerable organizational change will benefit lesser powers (suicide terrorism). Low-cost innovations requiring little organizational capital will spread rapidly to all powers, producing little net change in the system. However, those innovations with the largest potential impact on the international system are the fourth kind: high-cost innovations requiring a great deal of organizational capital to implement. According to Horowitz, such innovations are difficult for any power to adopt. They spread very slowly, if at all, through the international system. Any powers that manage to acquire the innovation, however, will possess capabilities that many others will find it difficult to emulate – a "first mover advantage" with potential to profoundly disrupt the existing international system (49).

Horowitz achieves a conceptual breakthrough by integrating key variables at the international system level, the individual state level, and the individual innovation level into a single coherent framework for understanding the likely pattern of diffusion of new military innovations and the implications thereof for the stability of the international system. This is a brilliant accomplishment that will change how the field approaches the subject of diffusion.

That does not mean, however, that *The Diffusion of Military Power* is the last word on the subject. Some will debate the characteristics of the four case studies offered in the book and where they fit in the framework – each is a complicated story and there is room for argument. Others will use the framework as a heuristic device to begin asking questions about the likely diffusion of an unfamiliar innovation, and in so doing will understand that innovation more completely and, perhaps, identify anomalous cases that do not fit the predictions of the framework. The anomalies provide the basis for supplementing the framework, moving the research program forward. This final role is, after all, the true purpose of such a framework – something Horowitz makes abundantly clear throughout the book.

I would raise a different question for Professor Horowitz, however. As I read the *Diffusion of Military Power*, I was struck by the degree to which innovation is reified as "the unit of analysis." (5) That is to say, innovations are essentially "things" in terms of discrete phenomena around which clear boundaries can be drawn. They have enduring qualities that are intrinsic to their character in all contexts. Their military value, financial intensity, and organizational intensity are objective values – "battlefleet warfare" is a distinct thing with a given cost and organizational requirement that is the same across the international system. I gather this is necessary in order to portray diffusion in structural terms – there must be a distinct thing to diffuse (the innovation) and it must have objective characteristics. On its face, this view of innovation appears to leave little room for the bottom-up processes and contextual factors that have dominated military innovation research for the last several years. Indeed I find the two perspectives difficult to reconcile. Does *The Diffusion of Military Power* mark the genesis of a new and truly incommensurate line of research, or does Professor Horowitz see a locus of interaction between these very different conceptions of innovation and causality?

Review by Thomas G. Mahnken, U.S. Naval War College

Michael C. Horowitz's *The Diffusion of Military Power* is a welcome addition to the literature on military innovation. In particular, Horowitz builds on, and complements, Emily Goldman and Leslie Eliason's excellent edited volume on the diffusion of innovations.[1] Whereas the strength of the Goldman and Eliason volume lies in a set of rich historical case studies, Horowitz provides greater methodological rigor and a more parsimonious theoretical construct. He also contributes to the literature on revolutions in military affairs. As I have argued elsewhere, military revolutions can be characterized as a cycle of innovation, diffusion, and refinement.[2] Horowitz illuminates that second, crucial step. In particular, he has important things to say about why some innovations result in long-lasting military advantage, while others yield only transitory benefit.

Horowitz builds upon previous theories of military innovation "to explain how, given the distribution of geopolitical interests and national attributes, the financial and organizational requirements for adopting an innovation determine how it spreads." (21) His explanation centers on the "financial intensity" of an innovation and the "organization capital" of the military that seeks to adopt it. (9) In so doing, he draws an important distinction between interest in responding to the advent of an innovation and the substance of that response. He tests his theory by looking at the spread of four innovations: aircraft carriers, nuclear weapons, the modern battle fleet, and suicide terrorism. These he explores through a mixture of qualitative case studies and large-*n* statistical analysis. Although my bias is heavily toward the former approach, Horowitz demonstrates the utility of the latter method.

This is a book about the spread of innovations rather than the process of innovation itself. By Horowitz's own admission, he "sidesteps the traditional debate about whether strategic competition, cultural factors, or norms best explains emulation." (9) This is understandable, but at times his search for broad explanatory power reduces the richness of the story he tells. One of Horowitz's two key variables is "organizational capacity," which he further sub-divides into critical task focus, experimentation, and organizational age. I agree that the organizational dimension of the adoption of innovations is crucial; however, I don't believe that organizational capacity, at least as Horowitz has outlined it, fully captures it. Indeed, organizational capacity has less power than organizational culture in explaining why military organizations may choose to adopt some innovations and not others. As I have shown elsewhere, organizational culture serves as a powerful lens for understanding why the U.S. armed forces, to take one example, developed the weapons

[1] Emily O. Goldman and Leslie C. Eliason, eds., *Adaptive Enemies, Reluctant Friends: The Impact of Diffusion on Military Practice* (Stanford: Stanford University Press, 2003).

[2] Thomas G. Mahnken, "Weapons: The Growth & Spread of the Precision-Strike Regime," *Daedalus* 140, no. 3 (Summer 2011).

they did.[3] By sidestepping the cultural dimension, Horowitz misses an opportunity for an even more detailed and nuanced understanding of the diffusion of military innovations.

That having been said, this is an excellent book. It does what good theory should do: provide a useful lens through which we can comprehend reality. Whereas structural realists tend to assume that innovations will be countered rather quickly and effortlessly, Horowitz shows how the adoption of expensive and complex innovations is likely to be difficult and thus how they may therefore yield lasting advantage.

One of the strengths of Horowitz's book is his willingness to extend his theory to the spread of contemporary innovations, including precision weaponry, unmanned/robotic systems, and cyber warfare. In these cases I find his theory quite useful, even if he and I disagree on the specifics. For example, he argues that the expense of modern precision weapons will limit their diffusion. I would counter that the broad availability of space-based precision navigation and timing from satellites like the U.S. Global Positioning System (GPS) constellation has made precision guidance rather inexpensive. And while precision-guided long-range missiles may be expensive, that has more to do with the cost of a missile than its guidance system. Precision-guided rockets and mortars are much less expensive and are hence likely to spread much wider than Horowitz predicts. The ultimate value of *The Diffusion of Military Power*, however, may lie in its ability to highlight the issue and set the terms of such a debate.

[3] Thomas G. Mahnken, *Technology and the American Way of War since 1945* (New York: Columbia University Press, 2008).

Response by Michael C. Horowitz, University of Pennsylvania

It is an honor and a pleasure to have my book, *The Diffusion of Military Power: Causes and Consequences for International Politics*, reviewed in this H-Diplo/ISSF forum by such a distinguished set of scholars. I am indebted to Tom Maddux for organizing the roundtable as well as the notable group of participants: Dima Adamsky, Theo Farrell, Adam Grissom, Tom Mahnken, and William Wohlforth. Each is an expert in the area of military innovation and military power, and I have read (and cited) all of their work.

In *The Diffusion of Military Power*, I argue that the spread, or diffusion, of military innovations represents an important area of inquiry for international politics. I introduce a concept called adoption capacity theory that shows how financial and organizational factors play a crucial role in influencing not only the rate and extent to which innovations diffuse, but the way those innovations end up affecting international politics, especially the balance of power and international conflict. I use this framework to address innovations ranging from carrier warfare to nuclear weapons to suicide bombing, which I view as a military innovation that has primarily served to enhance the capabilities of non-state actors. My goal was to build on existing research and rigorously show how financial and organizational factors, specifically the financial intensity and organizational capital required to adopt innovations, have played an important role across space and time in influencing international politics. Rather than thinking about the spread of innovations (which are not just new technologies, but the combination of new technologies with organizational practices for effectively employing those technologies) as something that generally happens reasonably quickly, as Robert Gilpin argues[1], I explore the variance in the diffusion of innovations and the effects of that variance on international politics.[2] While realists who believe strategic competition plays a primary role in driving diffusion might view this as automatic, I show that the adoption capacity of states and non-state actors is not entirely fungible, even under threatening conditions, meaning that we need to know more about militaries to understand the patterns of behavior they adopt.

There is much that the reviewers and I agree on, but I will focus here on those areas where we do not necessarily agree. One of the biggest areas of disagreement, which Adamsky and Mahnken raise, is the importance of culture, especially organizational culture. In somewhat different ways, each suggests that my explanation of adoption capacity, which focuses on financial and organizational factors, may risk minimizing the importance of culture. National or especially organizational culture might influence both whether or not a military considers adopting a military innovation and the ability of that military to adopt the innovation. Elites might block consideration or adoption of a military innovation because it violates cultural norms or threatens their position in a society. I address this issue in *The*

[1] Robert Gilpin, *War and Change in World Politics* (New York: Cambridge University Press, 1981).

[2] My book also explicitly builds on Gilpin (1981, p. 63) with its focus on how the ability of militaries and economies to capitalize on innovations can have major implications for the international security environment.

Diffusion of Military Power on pages 58-60, but it is worth expanding that discussion here. Culture is an incredibly charged term, often used differently by different scholars, and part of the issue here might be one of terminology. I agree with Adamsky when he writes that "Under the impact of cultural factors, different states may have varying propensities for innovations on different levels of warfare." What he calls "culture," however, I would generally consider part of organizational capital.

Essentially, I agree that cultural factors should influence the propensity for states to consider adopting innovations and to succeed if they decide to adopt them. When you peel back the layers of "culture", however, what often exists at the bottom are organizational behaviors driven by financial necessity or, more likely, organizational routines designed to do something other than maximize the ability of the organization to produce military power (for example, to sustain the power of an individual group within the organization). I tend to think of these behaviors as evidence that the organizational capital of a given military is therefore low, meaning that even in a relatively threatening environment, that military may prove unable to adopt a complicated innovation. In this way of considering the role of organizational culture, it becomes an input into measuring the overall organizational capacity of a military. I identified objective indicators of a military's organizational capital prior to the debut of an innovation – its spending on experimental research & development, the breadth of its critical task focus, and its organizational age – as a first cut at providing a way to quantify those processes that also might be described as organizational culture by some scholars.

Even if one finds that explanation unpersuasive, the reason I believe, that my argument sidesteps traditional debates over strategic competition, norms, and cultural factors (as Mahnken notes) has to do with the level at which my argument operates. When considering how to respond when another country debuts a new military innovation, after all, a country has many options, as I describe in Chapter 2. It could attempt to form alliances to deal with this new challenge, attempt to adopt all of the innovation, attempt to counter the innovation in some ways, or adopt another response strategy. One of the factors that influences the way the debate over those options occurs is undoubtedly the culture of the organization and what choices might seem socially acceptable.

Thus, it could be true that culture frames the set of acceptable responses a country might enact in response to the debut of a military innovation, but it is adoption capacity that drives the specific choice it makes. Adoption capacity theory also allows us to understand whether that culturally-driven choice is likely to succeed or fail in terms of implementation. This cultural impact on decision making is therefore compatible with my argument about organizational capital.

Of course, we want to be careful with cultural explanations and the risk of *post-hoc* assessment. We observe a state not adopting a military innovation and then infer that this has occurred due to "culture." To be fair, this is not what any of these authors have done in their own work. Indeed, their work is among the finest to employ cultural explanations.[3] The authors are clearly correct, however, that I could have done more to address this issue in my work. I think it is an important area in which overlapping research should occur moving forward.

Moving on from the relationship between adoption capacity theory and organizational culture, Grissom raises the question of whether my emphasis on "innovation" as the unit of analysis draws boundaries that are too neat and clean. Focusing on "innovation" might make it harder to assess the way militaries adapt over time and often develop ideas from the bottom-up. Grissom's own research demonstrates how military changes often result from bottom-up developments at the small unit level.[4] Grissom is also correct that some of the field has turned in recent years away from considering innovations and towards questions of "adaptation", or smaller, more continual changes.[5] Theo Farrell, for instance, has written a very interesting piece on adaptation in the context of British operations in Helmand province in Afghanistan.[6] I view these lines of research as complimentary. Most of the changes that occur within militaries are adaptations, after all – they are small changes that might either bubble up as best practices from a small unit in the field or come down from above in response to changing priorities. Major military innovations, as I define them in *The Diffusion of Military Power*, are somewhat different. They are major changes in the conduct of warfare relevant to leading military organizations. They always involve significant organizational change and often involve the incorporation of new technologies as well. The level of change is somewhat different than that studied in the literature on bottom-up innovation.

A bottom-up innovation could certainly reach the level where it would enter into my framework if the level of change was large enough and it became common practice throughout a military. Then we could study the rate and extent of its diffusion to other actors. Another, more direct, way to merge these lines of research is to think about the relationship between organizational capital and micro-level innovation. Though not a topic of inquiry in my book, it is possible, for example, that countries that score lower on organizational capital indicators might be less likely to allow bottom-up innovations to occur and permeate an organization. The unit costs of relevant technologies required to

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[3] Another very interesting piece that attempts to rigorously study cultural issues related to innovation adoption is Emily O. Goldman, "Cultural foundations of military diffusion," Review of International Studies 32:1 (2006): 69-91.

[4] Adam Grissom, "The Future of Military Innovation Studies," *Journal of Strategic Studies* 29:5 (2006): 905-934.

[5] For an example of work in this vein by a prominent military innovation scholar, see Williamson Murray, *Military Adaptation in War* (Alexandria, VA: Institute for Defense Analysis, 2009). Also see Chad C. Serena, *A Revolution in Military Adaptation* (Washington, DC: Georgetown University Press, 2011).

[6] Theo Farrell, "Improving in War: Military Adaptation and the British in Helmand Province, Afghanistan, 2006-2009" *Journal of Strategic Studies* 33:4 (2010): 567-594.

implement more micro-level adaptations might also influence the way senior officers evaluate those changes, influencing their probability of adoption. In these ways, we could consider ways to merge my claim about adoption capacity with ongoing research into military adaptation and bottom-up innovation. This is potentially a very fruitful endeavor and I hope that Grissom and I have an opportunity to continue exploring.

Since some of the reviewers raised questions about my discussion of particular innovations, I will now turn to the case studies identified in my book. Farrell suggests that my interpretation of nuclear weapons as financially expensive but requiring low levels of organizational capital is misplaced. Instead, nuclear weapons might not be that expensive, but they are very complicated to build. While I agree that the cost of acquiring nuclear weapons has declined over time, much of that has to do with the unique properties of nuclear weapons. I know of no other weapons system where possessing a single version of even a very old and outdated design is enough to significantly raise your military capabilities. Yet a country that duplicated the Manhattan Project designs and built a rudimentary nuclear bomb would be viewed very differently than a country that built a World War II-vintage *Yorktown*-class aircraft carrier. The unique power of nuclear weapons means that, even though they are actually extremely expensive from a unit cost perspective, countries can invest in them little by little over time. While it is true that those costs have been declining somewhat, the overall costs are still extremely large. Countries also often need external assistance to get them going along the path to either uranium enrichment or plutonium production.

On the organizational side, our disagreement revolves around how you consider what constitutes the state of being "organizationally complicated." Farrell is right that "covert acquisition and safe operation of nuclear weapons present enormous organizational challenges." My claim has to do with the way I measure the organizational capital level required for adoption. Innovations requiring high levels of organizational capital and those that require large changes in how organizations already operate – those are the cases where bureaucratic barriers or failures of creativity can lead to the stifling of innovation adoption. Nuclear weapons are different, however, for two reasons. First, there are many different organizational pathways towards nuclear weapons. There are several developmental options, whether it is focusing on enriched uranium or plutonium, gun assembly versus implosion designs, and many other ways that the weapons can vary. This means that a military or country that might not be able to implement its plans through one path has many others to choose from. Second, while interest groups within a military might argue over who will control those weapons, incorporating nuclear weapons does not necessarily disadvantage any particular entrenched group. Compare the production of nuclear weapons to the introduction of aircraft carriers. In the carrier case, the vision of advocates – using carriers as mobile strike platforms across great distances – necessarily involved a decrease in the relative bureaucratic power of those in the battleship community.

Moreover, countries can choose to place nuclear weapons within new military units specially designed to incorporate them, such as the Second Artillery in China, or even outside of the military itself. Thus, a country seeking to adopt nuclear weapons is not forced down one particular developmental path. A country could therefore score very low on all of the organizational capital metrics I outline and still have a high probability of adopting. Therefore, I think the organizational capital required to adopt is low even though I acknowledge that it is organizationally complicated.

Mahnken concludes by pointing out a disagreement between us concerning the spread of precision weaponry and other Information Age systems. Here I actually suspect we agree. The point I attempted to make was that the high unit costs associated with the platforms needed to utilize precision weaponry up to this point (primarily manned fighters, bombers, and submarines) placed many of these systems outside the reach of most actors. The challenge today and moving forward is that those unit costs are rapidly declining, especially as the platforms are changing towards less financially-intense systems such as unmanned aerial vehicles (UAVs). Thus, Mahnken and I both would predict a relatively wide spread of these systems over the next generation, though we would do so potentially for slightly different reasons.

To conclude, I would like to thank the participants in this roundtable once again. This has been an extremely stimulating discussion from my perspective. I think that each of these contributions touches on areas where I could have presented my claims more persuasively or perhaps where my argument about adoption capacity requires modification. These contributions also lay out an ambitious agenda for continuing research on change, or the lack thereof, within military organizations. That is something I look forward to continuing to write about, and hope that some of these fine scholars may join me.