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Introduction by Richard K. Betts

Caitlin Talmadge. *The Dictator's Army: Battlefield Effectiveness in Authoritarian Regimes*.
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Introduction by Richard K. Betts, Columbia University

Scholars of political violence readily delve into policy and strategy but seldom below those levels of analysis. They usually consign concern with operations and tactics to military buffs. As Carl von Clausewitz argued, however, strategic success depends on and is ultimately reducible to tactical success.[1] So predicting how military success or failure affect political and strategic outcomes in war is naturally driven to the operational and tactical levels.

Contemporary work in this vein has accelerated gradually. Over thirty years ago the legendary Andrew Marshall's Office of Net Assessment in the Defense Department sponsored a major historical survey by a couple of dozen historians which was ultimately published in three volumes edited by Allan Millett and Williamson Murray under the title *Military Effectiveness*.[2] Political scientists at the time had become attentive to the question of how to measure effectiveness, in large part because of Cold-War debates about the NATO-Warsaw Pact military balance, and produced theory about it, especially in terms of strategic innovation.[3] Attention to details of what specific techniques cause success in combat was slower to develop.

After the principal theoretical landmark by Stephen Biddle a dozen years ago[4] literature on the subject picked up.[5] Caitlin Talmadge's book is an ambitious new addition to the evolving genre. Like few other studies it integrates tactical-level analysis with political analysis not only in terms of military effects on victory or defeat in interstate wars, but also in terms of the relation between choices in military organization and competence and internal political stability.[6] Talmadge takes off from Biddle's argument that the 'modern system' of tactical effectiveness is optimal for success in war and asks why so many countries fail to adopt it. She argues that authoritarian leaders compromise the competence of officers for leadership in war by promoting those

[1] Carl von Clausewitz, *On War*, eds. and trans. Michael Howard and Peter Paret (Princeton: Princeton University Press, 1984), 95-97, 130, 181-182, 363.

[2] Allan R. Millett and Williamson Murray, eds., *Military Effectiveness*: vol. I, *The First World War*; vol. 2, *The Interwar Period*; vol. 3, *The Second World War* (Boston: Allen & Unwin, 1988). This project followed on Martin van Creveld, *Fighting Power: German and U.S. Army Performance, 1939-1945* (Westport: Greenwood Press, 1982).

[3] For example: Barry R. Posen, *The Sources of Military Doctrine: France, Britain, and Germany Between the World* Wars (Ithaca: Cornell University Press, 1984) and Stephen Peter Rosen, *Winning the Next War: Innovation and the Modern Military* (Ithaca: Cornell University Press, 1991).

[4] Stephen Biddle, *Military Power: Explaining Victory and Defeat in Modern Battle* (Princeton: Princeton University Press, 2004).

[5] For example: Elizabeth Stanley and Risa Brooks, eds., *Creating Military Power: The Sources of Military Effectiveness* (Stanford: Stanford University Press, 2007); Shimon Naveh, *In Pursuit of Military Excellence: The Evolution of Operational Theory* (London: Frank Cass, 1997); Kenneth M. Pollack, *Arabs at War: Military Effectiveness, 1948-1991* (Lincoln: University of Nebraska Press, 2002).

[6] Among few other examples of this combination are Pollack, *Arabs at War* and Risa A. Brooks, "Making Military Might: Why Do States Fail and Succeed? A Review Essay," *International Security* 28:2 (Fall 2003).

whose loyalty to the regime is assured and deliberately limiting the ability of officers to exercise initiative or optimize training of their forces for combat.

The four reviews that follow all declare Talmadge's work to be impressive and find few faults in it. Most of their criticism concerns additional issues and hypotheses that should be explored rather than mistakes in the book as far as it goes. No author could reasonably hope for a set of reviews much more laudatory than these. Nonetheless, the reviews identify promising avenues for refinement and extension of the argument.

Among four issues that Jason Castillo cites for further investigation is the question of which "coup-proofing" practices need not impede battlefield performance. He notes Nazi Germany as an obviously relevant case to explore in this regard. Dan Reiter also sees that case as pointing to the need for more disaggregation of the internal regime-protection measures and notes the importance of factors that can lead to correction of ineffectiveness apart from the binary tradeoff between attention to external and internal threats. Yet Reiter compliments Talmadge for her refinement of heretofore "simplistic" coup-proofing theory and her demonstration of the need for "quality case studies in security" because of the "deeply flawed" quantitative measures of coup-proofing.

Ryan Grauer emphasizes Talmadge's focus on regime *strength* rather than regime *type* as the critical variable driving choices that limit military proficiency. (This evokes two of Samuel Huntington's main concepts about civil-military politics that bear on these questions: subjective control and praetorianism.)[7] He also emphasizes that ideology, independent of fear of disloyalty, may play a more potent role in some regimes' selection of military leadership than Talmadge indicates. Jessica Weeks finds the ability of Talmadge's approach "to generate predictions not only *across* regimes, but also within the same regime over time" to be "a particularly exciting aspect of the theory." She wishes for more about "*why* and *when* regimes perceive coups to be a greater threat than conventional war" (but also flags where Talmadge does engage that question on pages 20-23). Weeks also argues that leaders exercise a wider range of choices between the conflicting concerns about external and internal threats than the book emphasizes.

I share the enthusiasm of the reviewers for the book. My main quibble would be that it subsumes too much under the rubric of 'dictator.' In reality the continuum from dictatorship to democracy is quite wide and varied. The difference between the quasi-democratic authoritarian regime of Nguyen Van Thieu in South Vietnam and the totalitarian absolutism of Saddam Hussein in Iraq is tremendous, and might make some difference in the constraints on military performance.[8] Nevertheless, few, if any, other works have developed analysis of the interplay of international and internal politics in the development of military power in as much depth, clarity, and subtlety as Caitlin Talmadge's *Dictator's Army*.

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[7] Samuel P. Huntington, *The Soldier and the State* (Cambridge: Harvard University Press, 1957), passim; Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968), chapter 4.

[8] See for example Allan E. Goodman, *Politics in War: The Bases of Political Community in South Vietnam* (Cambridge: Harvard University Press, 1973), chapters 3, 5-7.

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Review by Jasen Castillo, Texas A&M University

Why do states vary in their ability to create and to field militaries capable of performing well on modern battlefields? In the field of security studies, few questions trump this one in terms of importance. The question gets to the heart of how to measure military power. Traditional estimates of military power focus on things we can count, like troops, tanks, ships, and aircraft. Any full picture of military power, however, requires some assessment of how well people will perform in battle with the material capabilities they possess.

Caitlin Talmadge's impressive book, *The Dictator's Army: Battlefield Effectiveness in Authoritarian Regimes*, offers a compelling answer to this important question. When militaries adopt the correct set of organizational practices (real, frequent training; merit promotion; decentralized command; and open communication), they stand a better chance of succeeding in battle. Why do all states not adopt these practices? This book argues that the balance between internal and external threats determines whether regimes allow their militaries to embrace these organizational characteristics. In particular, fear of a coup will prevent governments from doing what it takes to win on the modern battlefield.

The central argument in a nutshell is that "regimes facing significant coup threats are unlikely to adopt military practices optimized for conventional combat, even when doing so might help them prevail in conflicts against other states (or even to combat other types of internal threats, such as conventional civil wars or insurgencies)" (2). Although the book's theory could apply to any country regardless of regime type, Talmadge focuses on authoritarian states. (12).

Supporting evidence for this persuasive argument comes from two detailed and well-organized case studies. The first case study pairs Saddam Hussein's Iraq against Revolutionary Iran during their bloody conflict in the 1980s. In a second case, Talmadge examines the military performance of North Vietnam and South Vietnam from 1963 to 1975. The selection of these cases allows Talmadge to control for several potential intervening variables, while also giving her the opportunity to trace in detail the causal logic of her theory. Case studies allow Talmadge to unpack the processes at work in each country, connecting military performance to a regime's worries about internal security.

Coup prevention was a priority in most of these countries except North Vietnam, which, as the theory would predict, fielded a military with excellent battlefield performance. Iraq improved its combat effectiveness late in the war by removing coup prevention measures on key units. Similarly, only the unencumbered 1st Division of the South Vietnamese military could perform well in combat, while other Army of the Republic of Vietnam (ARVN) units suffered from not following the practices required for success in conventional combat. Finally, Iranian forces initially did well until the regime's revolutionary zeal and concern about military coups eventually hampered training and performance.

Two of the many contributions that the *Dictator's Army* makes to the study of military effectiveness strike me as particularly important. First, the book provides a comprehensive list of the organizational practices that are logically and empirically linked to skill on the conventional battlefield. These allow militaries to do well in what Stephen Biddle labels the "Modern System."[1] His work lays out the demands of modern warfare. Simply put, Biddle explains what military units must do to succeed in war. Talmadge explains why some states can or cannot follow the recipe. For scholars, knowing the precise steps a military organization must implement to do well in this 'modern system' represents a significant contribution to understanding the sources of effective combat performance. Military planners can also use this insight when they craft net assessments of potential adversaries. The next step is to see if the book's theory can apply beyond military dictatorships. This would also help us get away from the stale debate over regime type and military effectiveness.

Second, the book's theory puts flesh on the bones of James Quinlivan's original insights into the role of coup proofing on the effectiveness of militaries in the Middle East.[2] Quinivan wrote his article because Iraq's poor performance in the First Persian Gulf War seemed puzzling. Building on this work, *The Dictator's Army* presents a complete theory with scope conditions and causal logic explaining the effects that different coup proofing practices have on battlefield performance. Because she spends time building a coherent theory, Talmadge can offer interesting insights into the relationships between regimes and their armed forces. For example, she distinguishes between the detrimental interference of Saddam Hussein on Iraq's battlefield performance from the less harmful political meddling of Germany's dictator, Adolf Hitler, on the Werhmacht.

Like any good work in the social sciences, this book also raises a number of questions that could drive future research. I see four issues worth further investigation. None of these detract from the volume. On the contrary, the clarity of the book allows us to think about where the debate should go next.

First, how much coup proofing impedes performance? Put another way: how many of these organizational practices do militaries need in order to do well on the battlefield? Talmdage gives us a list of necessary practices but it seems possible that some of them are more important than others when it comes to battlefield performance. A government's fear of a coup might lead it to adopt some protection practices but not others. Regimes likely cross some threshold where their interference begins to impede military effectiveness. Instances of 'mild' coup protection would likely result in a mixed performance on the battlefield. Similarly, there is likely a threshold where too much coup protection has taken place for a regime to reverse course and to undo the damage in a time of war, like Saddam Hussein's decision during the Iran-Iraq War. Perhaps testing the theory beyond dictatorships—in more mixed regimes-- could shed light on some of these threshold issues.

Second, what is the role of motivation, morale, and unit cohesion in the organizational practices outlined in the book's theory? The book focuses on the skills required for success on the conventional battlefield, but what about will? A large literature exists pointing to the importance of morale on the battlefield. Some militaries can often compensate for a lack of material by fighting with more determination in combat.[3] Biddle

[1] Stephen Biddle's *Military Power: Explaining Victory and Defeat in Modern Battle* (Princeton: Princeton University Press, 2004).

[2] James T. Quinlivan, "Coup-proofing: Its Practice and Consequence in the Middle East," *International Security* 24:2 (Fall 1999), 131-165.

[3] Hew Strachan, "Training, Morale and Modern War," *Journal of Contemporary History* 41:2 (2006); 211-227; and, Nora Kinzer Stewart, *Mates and Muchachos: Unit Cohesion in the Falklands/Malvinas War* (Washington: Brassey's Inc., 1991). John A. Lynn, The Bayonets of the Republic: Motivation and Tactics in the Army of Revolutionary France,

argues that small-unit cohesion is a key ingredient to success in the modern system. My suspicion is that the organizational practices Talmadge identifies as essential for fighting effectively also contribute to morale and unit cohesion, but these are not the only sources of military motivation. Separating skill from will is a tricky business, but nevertheless, how coup proofing might effect combat motivation is a crucial question.

Third, and related to the previous question, what other coup protection practices might regimes adopt to safeguard their survival without undermining battlefield performance? The history of Nazi Germany suggests three practices that a dictator might choose. One simple strategy would entail coopting the armed forces by developing a common set of interests. The German officer corps may not have loved Hitler's populist message, but they embraced his goals of national rearmament and territorial expansion.[4] Inculcating the military and the society with the regime's ideology is another way of increasing loyalty to the regime, while also generating combat power.[5] Finally, creating parallel military organizations can both protect the regime and allow the military to fight with great effectiveness on the battlefield. Although the Waffen SS varied in its fighting performance early in the Second World War, by the later years of the conflict it represented some of the best equipped, determined, and proficient units of the German military. Parallel military organizations also excel at applying coercion to the regular armed forces as well as the general population to keep both fighting, instruments that ensured the survival of the Soviet state against the German onslaught of 1941-1942.

Last, what are the other hurdles that might prevent a military from adopting what Talmadge identifies as the key organizational practices to succeed on the modern battlefield? The book takes it as axiomatic that all militaries would adopt these practices if the regimes let them do so. Still, I wonder if all military organizations are up to the challenge. *The Dictator's Army* concedes that militaries might make bad choices. I can think of a few possibilities that would prevent military organizations from adopting the right practices. Consider, for example, the Italian military before World War II as described by the historian MacGregor Knox. He lists all kinds of reasons for its subpar performance, but they key ones include class divisions, poor relations between the military and society, and tensions between officers and enlisted personnel.[6] Most of these issues had little to do with coup protection.

1791-1794, 2nd. ed. (Boulder: Westview Press, 1996); Paul C. Stern, "Why Do People Sacrifice for Their Nations?" *Political Psychology* 16:2 (1995), 217-235.

[4] MacGregor Knox, *Common Destiny: Dictatorship, Foreign Policy, and War in Fascist Italy and Nazi Germany* (Cambridge: Cambridge University Press, 2000); Matthew Cooper, *The German Army, 1933-45: Its Political and Military Failure* Robert J. O'Neill, *The Germany Army and the Nazi Party, 1933-1939* (New York: James H. Heineman, Inc., 1966).

[5] This is one of the central arguments I make in Jasen Castillo, *Endurance and War: The National Sources of Military Cohesion* (Stanford: Stanford University Press, 2014). A good discussion of ideology and the German armed forces is Omer Bartov, *Hitler's Army: Soldiers, Nazis, and War in the Third Reich* (Oxford: Oxford University Press, 1992).

[6] MacGregor Knox, "Conquest, Foreign, and Domestic, in Fascist Italy and Nazi Germany," *Journal of Modern History* 56:1 (March 1984), 1-57; Alistair Horne, *The French Army and Politics, 1870-1970* (New York: Peter Bedrick Books, 1984).

These organizational pathologies also plagued the French armed forces from 1870 to 1940. At the same time, the politicians of France's Third Republic also worried that the military might intervene in politics. Although not a dictator's army, the French example is instructive since Talmadge's theory should apply beyond authoritarian regimes. Despite ever-present coup fears, and a steady stream of problems within the organization, the French military put in a mixed performance: fighting persistently in the Franco-Prussian War, fighting well in the First World War, but fighting terribly in in 1940.

Despite these questions, this Talmadge has written a terrific book that advances our understanding of military effectiveness. And it was a pleasure to read, something that is no small feat these days.

Review by Ryan Grauer, University of Pittsburgh

Battlefield effectiveness, given its impact on the course and nature of world politics, is arguably one of the most understudied topics in international relations. Despite a literature that has burgeoned impressively over the past several years, there are a vast number of questions to which we simply do not yet have good answers. Caitlin Talmadge has identified a number of such questions and, in this book, makes a signal contribution to scholarly understandings of why some militaries are better able to translate the men and materiel they possess into actual combat power.

One of the most prominent questions in the literature on battlefield effectiveness is whether, and how, particular regime characteristics might influence armed forces' performance in combat. To date, investigations of this question have largely focused on the role that regime type plays in this regard. Some scholars argue that, for a variety of institutional and sociopolitical reasons, armed forces fielded by democracies tend to fight better than do those mustered by non-democracies.[1] Others contest the point, suggesting that particular case codings or other variables that are often correlated with democratic governance may be driving the finding.[2] No matter which argument is correct, this focus on regime type as a driver of martial capability is inherently limited. Even if the militaries of democratic regimes do tend to perform better on the battlefield, that knowledge does not help us understand variation in the capabilities of belligerent forces in the more than 50 percent of wars fought between 1816 and 2007 in which no democracy was involved.[3] Nor does it assist analysts who are interested in forecasting the likely strength of non-democratic belligerents in potential future wars. Understanding whether regime characteristics condition martial performance writ large requires knowing more than how political elites gain and lose power.

Therein lays Talmadge's primary contribution to the literature on battlefield effectiveness. Though her examination of the way in which different types of authoritarian regimes generate martial capability is both welcome and needed, Talmadge's insistence that we focus on regime strength rather than regime type when thinking about drivers of battlefield effectiveness is much more important. Ruling elites in all types of regimes can be more and less firmly ensconced; some face almost no internal or external threats to their positon and place, others must fend off assaults from multiple quarters, and many fall in between. Democratic and non-democratic elites alike, when facing relatively more significant challenges to their authority (real or imagined),

[1] Dan Reiter and Allan C. Stam, *Democracies at War* (Princeton: Princeton University Press, 2002).

[2] Stephen D. Biddle and Stephen Long, "Democracy and Military Effectiveness: A Deeper Look," *Journal of Conflict Resolution* 48:4 (August 2004): 525-546; Michael C. Desch, *Power and Military Effectiveness: The Fallacy of Democratic Triumphalism* (Baltimore: Johns Hopkins University Press, 2008); Alexander B. Downes, "How Smart and Tough Are Democracies? Reassessing Theories of Democratic Victory in War," *International Security* 33:4 (April 2009): 9–51; Michael Beckley, "Economic Development and Military Effectiveness," *Journal of Strategic Studies* 33:1 (February 2010): 43-79.

[3] Meredith Reid Sarkees and Frank W. Wayman, *Resort to War: 1816-2007* (Thousand Oaks: CQ Press, 2010); Monty G. Marshall and Keith Jaggers, *Polity IV Annual Time Series 1800-2011*, version p4v2012 (College Park: Center for Systemic Peace, 2012), http://systemicpeace.org/polity/polity4.htm.

will naturally seek to do two things: bolster institutions that can be used to protect themselves and weaken those that could be employed by would-be usurpers.

Focusing on regime strength should inform our understandings of belligerents' battlefield effectiveness because, as Talmadge convincingly argues, the means by which ruling elites under threat strengthen their own hand and weaken that of their most powerful opponents have different implications for their militaries' capabilities. Elites facing strong external threats but few or no internal challenges are likely to work to increase their armed forces capacities to use men and materiel effectively in combat. Specifically, she notes, they will work to ensure that officers are promoted on merit rather than political connections; soldier training is thorough and realistic; command and control structures are unified and rationally organized; and information management protocols facilitate the swift movement of data and news throughout the force. The relatively pacific domestic environment allows such elites to rest easy in the knowledge that their militaries' resultant tactical and operational strength – so necessary to ensure the national defense – is unlikely to be turned against them.

By contrast, Talmadge argues, elites facing strong internal threats and few significant external challenges are likely to undermine their militaries' tactical and operational capabilities by privileging political loyalty over competence in the selection and promotion of officers, minimizing soldier training, creating multiple and tangled chains of command, and complicating intra-military communication. Doing so makes it harder for militaries to launch coups on their own or serve as an effective tool for oppositional civilian elites in their efforts to depose those in power. That the resultant forces are likely to be ineffective and weak in traditional combat is relatively unimportant, since the paucity of external threats reduces the chances that they will be called on to fight in such a manner. Elites facing mixed internal and external threats are likely to pursue one of two tracks: they may either work to tailor the military such that it addresses what they deem the most pressing (usually internal) threat and hope for the best with the respect to the other (typically external) challenge, or create a mixed force in which the bulk of the military is structured so that it poses little internal threat and some select units are bolstered so that they are capable of performing effectively on conventional battlefields.

Talmadge tests her argument in two paired comparisons: a series of battles fought by North and South Vietnam between 1963 and 1975, and a series of battles fought between Iran and Iraq during their 1980-1988 war. The case studies are well-chosen. They offer considerable variation on the dependent variable – battlefield effectiveness – and, within the paired comparisons, hold relatively constant a variety of factors which are often cited as potential drivers of combat performance, including national population and wealth, regime type, and culture. The cases also cover some of the lesser-examined forces and battles in the battlefield effectiveness literature and, as such, add to scholars' collective stock of empirical knowledge on the topic. Finally, the studies are very well-executed. Talmadge draws on an array of primary and secondary sources, some only newly available, to present detailed, nuanced descriptions of events and causal processes.

The evidence marshaled generally offers strong support for Talmadge's theory. In Vietnam, for example, the North's ruling elites feared little domestic challenge, having consolidated their one-party rule over the decade preceding the first of the examined battles, while the South's various ruling elites could not be sure that theirs would not be the next in a long line of governments to fall to a coup. The North's internal stability permitted president Ho Chi Minh and his coterie to promote conventional warfighting capabilities in their various military organizations and, as expected, the resultant meritocratic promotion policies, rigorous training, rationalized command and control, and relatively easy intra-military communication helped those forces perform effectively on a consistent basis. The South's domestic turmoil, by contrast, led President Ngo Dinh Diem and his successors to largely undermine their army's conventional warfighting capabilities. The majority of units in South Vietnam's army were poorly trained, led by officers installed for their political rather than military qualities, subject to direction through tangled and centralized chains of command, and relatively isolated from contact with other units. They consequently performed abysmally in the field. An exception, however, was 1st Division. That unit, stationed furthest from Saigon and closest to North Vietnam, posed relatively little threat to the regime and needed to be strong to serve as a first line of defense. It was afforded many of the advantages North Vietnam gave its forces and, very often, fought well.

The Iran-Iraq War similarly bolsters confidence in Talmadge's claim, though it does so with a twist. Neither the Iranian nor the Iraqi leadership felt secure during the war and, as a consequence, both generally adopted policies and practices that undermined their militaries' conventional warfighting effectiveness. Iran performed better than Iraq at the outset of the war, however, because many of its regular forces still enjoyed the lingering benefits of the leadership and training the Shah afforded them prior to the 1979 revolution. That competence eroded over time, and by 1982, the Iranians regularly failed to exhibit basic tactical proficiency or execute complex operations. Iraq, by contrast, exhibited marked tactical and operational improvement late in the war after having struggled to fight well for six years. Talmadge shows that this change can be traced to Iraqi President Saddam Hussein altering his threat calculus. In particular, Saddam increasingly feared growing domestic threats stemming largely from the military's dissatisfaction with how the external threat posed by Iran was being managed and, to alleviate the pressure, grudgingly permitted improvements in officership, training, command arrangements, and information management in the Republican Guard and some regular army units.

Talmadge's insightful theoretical move and compelling empirical work combine to make a powerful argument. Strong as it is, however, a few questions remain. First, with respect to Talmadge's dependent variable, I am not fully persuaded that the conceptualization of battlefield effectiveness presented in the book is the most appropriate one. Talmadge follows what increasingly seems to be standard practice in the literature by defining her dependent variable as a "property intrinsic to a particular military or military units" measurable in terms of how well armed forces perform basic tactics and carry out complex operations (4).[4] At the risk of seeming pedantic, this understanding seems much more akin to battlefield *efficiency* than battlefield *effectiveness*. As the term is conventionally used, an actor operating in a social setting is described as effective when it achieves its intended objective vis-à-vis others. An effective teacher, for example, is one who successfully helps his or her students learn. An effective advertising company is one that successfully convinces the public to purchase its clients' products. What are militaries' intended battlefield objectives? Few, if any, armed forces are tasked with simply to mastering basic tactics and capably conducting complex operations. Rather, they are meant to achieve a wide array of things on the battlefield, from waging a fighting withdrawal to complete destruction of enemy forces. An effective military would thus be one that succeeds in carrying out its assigned tasks and, ideally, preventing its adversary from doing the same. Tactical and operational proficiency are often required to do so, but such capabilities are better thought of in terms of efficiency, like a teacher's skill in stretching his or her ever-declining budget when purchasing classroom supplies or an

[4] This kind of operationalization is premised on the argument advanced in Allan Reed Millett, Williamson Murray, and Kenneth H. Watman, "The Effectiveness of Military Organizations," in *Military Effectiveness* 1 (Boston: Unwin Hyman, 1988), 2-3.

advertising company's capacity to find inexpensive and innovative ways to boost brand recognition, rather than effectiveness: factors that are helpful in achieving, but not quite the same as, the intended end.[5]

Accordingly, I would propose that battlefield effectiveness is better understood as a property of the relationship between engaged forces and is measurable in terms of whether or not those forces achieve their intended objectives in combat. Functionally, substituting this understanding of battlefield effectiveness for tactical and operational proficiency does not change Talmadge's analysis or conclusions in any real way; by my reading of the evidence presented, assessments of effectiveness in North Vietnamese, South Vietnamese, Iraqi, and Iranian forces would stay largely the same, save perhaps in the fighting at Hue in 1968. It would, however, draw the conceptualization of battlefield effectiveness closer to conventional understandings of the term, preclude conflating efficiency and effectiveness, and, to the extent that tactical and operational proficiency are thought to be significant achievements in their own right, provide a clearer way of thinking about such capabilities moving forward.

Second, setting aside questions about how the dependent variable is operationalized, it remains somewhat unclear how we should think about the four causal mechanisms posited to connect regime strength with battlefield effectiveness. As noted, Talmadge argues that variation in the quality and nature of threats to ruling elites will shape their policies regarding militaries' promotion patterns, training regimens, command arrangements, and information management, and the specific policies adopted in these issue areas will then condition forces' battlefield effectiveness. Why these four specific issue areas should be the primary mechanisms through which elites condition effectiveness, and how they might interact with and relate to one another, is not immediately clear. Talmadge notes they are "most logically central to combat capability" and "tell us a great deal about the likely tactical and operational fighting power a given army is likely to generate" (13). This may be true, but is each equally important? Are policies facilitating the smooth functioning or impeding the operation of all four necessary for excellent or poor battlefield effectiveness? Even if deductively ideal policies are needed in all four areas to ensure high levels of battlefield effectiveness in conventional combat operations, could insecure elites manipulate policies in only one or two areas and both eliminate the threat the military poses to their regime and undermine its conventional effectiveness? Can good or poor policies in one or two issue areas trump the effect of good or bad policies in the other areas?

Intuitively, it seems that focusing on training patterns and command arrangements would tell us virtually all we need to know about militaries' capacities to perform basic tactics and conduct complex operations. To wit, it is hard to imagine a situation in which a military with promotion patterns, command arrangements, and information management structured to facilitate conventional warfighting would score well in terms of basic tactical competence if training regimens were not similarly rationalized. Conversely, with thorough and rigorous training, even politically connected but incompetent officers, centralized control, and inferior information management capabilities should not significantly hamper forces' capacities to handle weapons and use terrain for cover and concealment. Similarly, command arrangements seem key to capacities to carry out complex operations; if competent commanders leading well-trained troops that can communicate easily are not permitted the authority to act on their own initiative, then it will be quite difficult for units to rapidly

[5] On the distinction between efficiency and effectiveness in organizations, see Martin Landau, "Redundancy, Rationality, and the Problem of Duplication and Overlap," *Public Administration Review* 29:4 (August 1969): 346-358; Charles Perrow, *Complex Organizations: A Critical Essay* (New York: Random House, 1986); Karlene H. Roberts, "Some Characteristics of One Type of High Reliability Organization," *Organization Science* 1:2 (May 1990): 160-176.

execute context-sensitive combined and joint actions with one another. It may be, and likely is the case, that policies regarding and the quality of militaries' arrangements in the four issue areas that Talmadge identifies tend to covary. But that does not necessarily mean that they are equally important in conditioning levels of battlefield effectiveness. Especially for scholars and policymakers seeking to apply the important insights of this book more widely, is it imperative that they engage in the kind of laborious, detailed information-gathering process underpinning the empirical work presented in this book, or are the values of some indicator variables more important to get right than others?

Finally, especially in the Iran-Iraq War case, it seems that at least one factor beyond the balance of internal and external threats – ideology – may play an important role in shaping elites' policies on military matters. Talmadge's interpretation of the dynamics of that conflict are convincing in explaining why, even though elites in both states feared internal threats, Iran was more effective on the battlefield early in the war and Iraq fought better toward the end. One question not fully addressed, however, is why Iranian leadership did not make the same calculated gamble that Saddam did in order to bolster its capabilities and win the war early on when it was already advantaged. Iran, as Talmadge points out, benefited from the lingering impact of the Shah's more rational military policies in the early part of the war, but that edge waned over time. Talmadge attributes this change in Iranian battlefield effectiveness to a plane crash that killed Iran's few competent commanders and the fact that the leadership in Tehran learned the wrong lessons from the first two years of the war. The first factor could not be helped, but the reasons why the Iranian regime credited the newly created revolutionary forces with the decisive effort rather than the regular forces merits some additional consideration. It is possible that the Iranian leadership made this choice because they felt less threatened by a force more obviously loyal to the regime, but the logic underpinning such calculus is fundamentally ideological at its core; the congruence in the regime's and the revolutionary forces' religious and political fervor would have driven the decision. It is also possible that ideology played a more naked role, and that Tehran believed that the revolutionary forces, by virtue of their passion and motivation, were necessarily superior to regular forces in spite of promotion, training, command arrangement, and information management policies that depressed their conventional warfighting capabilities. In either case, something beyond the nature and intensity of threats posed to the regime drove its policymaking vis-à-vis the Iranian military. Even if the role of ideology in shaping elite decision making in Iran is a substantive outlier, a clearer understanding of how to handle the many non-rational factors that influence decision making would help readers define the set of cases to which Talmadge's model might be usefully applied.

These questions are, on the whole, relatively minor quibbles with what is in the end a very good book. Talmadge's insightful and careful work offers a fresh and needed perspective in the literature on battlefield effectiveness, and it will deservedly play a key role in shaping the direction and substance of future work on the topic.

Review by Dan Reiter, Emory University

The Dictator's Army by Caitlin Talmadge is a welcome and constructive contribution to our knowledge of how different regimes fight their wars. Its focus on dictatorships is especially timely, joining a new and growing body of work on dictatorships and war,[1] and complementing older work on democracies and war.[2]

The book builds on the coup-proofing hypothesis that when dictators fear being overthrown by military coups d'état, they take steps to reduce the risk of coups, and these steps have the unintended effect of reducing military effectiveness. Coup-proofing measures include promoting officers on the basis of political loyalty rather than merit, restricting training programs, creating centralized and convoluted command structures, and restricting horizontal and vertical information flows within the military.

Talmadge builds out on this relatively simple theoretical structure. She proposes that dictators facing levels of external threat that exceed their levels of internal threat will forgo or reverse coup-proofing measures in order to maximize military effectiveness. This argument is related to the realist dismissal of domestic politics, positing that international competition pressures (in this case, higher levels of external threat) moot the effects of regime type on foreign and military policy (here, the tendency of dictators to coup-proof).

Her argument links to other ideas about military practices and adaptation. Coup-proofing is one of many possible factors, such as perverse domestic political dynamics or organizational inertia, which might push a state to adopt inferior military practices. Once states adopt inferior practices, the next question is, when do these states self-correct and adopt more effective practices? Talmadge's answer is that states self-correct under conditions of sufficiently high external threat and sufficiently low internal threat. There are other theories that also aim to explain self-correction, proposing that factors such as changes in quantitative performance indicators or possession of sufficient amounts of organizational capacity can facilitate self-correction.[3]

The Dictator's Army contains case studies of four states, North and South Vietnam from 1963-1975, and Iran and Iraq from 1980-1988. It uses English-language primary and secondary sources. Some of the most novel materials are primary documents from Iraqi sources obtained after Saddam Hussein fell from power in 2003. Usefully, the book explores variation across time within a country, and then also variation within the military of a single country.

The cases provide support for the theory. There are some striking, novel findings. Though the coup-proofed South Vietnamese military generally performed poorly, one of its divisions, based far from the capital and hence posing less of a coup threat, mostly escaped coup-proofing and enjoyed higher combat performance. The coup-proofed Iraqi military performed poorly up through the 1986 disaster at the al-Faw peninsula, but

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[1] See Jessica L.P. Weeks, *Dictators at War and Peace* (Ithaca: Cornell University Press, 2014).

[2] See Dan Reiter and Allan C. Stam, *Democracies at War* (Princeton: Princeton University Press, 2002).

[3] Scott Sigmund Gartner, *Strategic Assessment in War* (New Haven: Yale University Press, 1997); Michael C. Horowitz, *The Diffusion of Military Power: Causes and Consequences for International Politics* (Princeton: Princeton University Press, 2010).

the higher level of external threat created by the Iranian victory at al-Faw pushed Saddam to reverse some coup-proofing, leading to better Iraqi combat performance in the last stages of the war. There is also some interesting variation in Iranian coup-proofing as the influence of the Shah in the Iranian military receded in the years following the 1979 revolution.

The book advances our knowledge of the important topic of how dictators' militaries operate. It highlights how simplistic current coup-proofing theory is, and how greatly we need new ideas, such as those developed here, which explain variance in coup-proofing behavior. The book also serves as an important reminder of the ongoing and deep need for quality case studies in security. Most quantitative measures of coup-proofing are deeply flawed, and careful case studies can provide much better measures.

There are some elements of the book's empirics that encourage further discussion. Consider first the South Vietnam case, especially during the last years of the Vietnam War. This case supports the theory, though in a somewhat different manner than as Talmadge describes. The post-January 1973 withdrawal of American military support, coupled with the clear, ongoing external threat from North Vietnam (a threat culminating in the conquest of South Vietnam in April 1975), should have pushed Saigon to rollback at least some of its coup-proofing, according to the theory. The author's take is that efforts at coup-proofing occurred too late to help, and that reluctance to coup-proof was driven at least in part by a South Vietnamese belief that America would come to Saigon's aid in the face of renewed North Vietnamese aggression, thereby diminishing Saigon's sense of external threat. Talmadge also posits that Saigon was still concerned about an internal coup threat (56).

However, the central source Talmadge cites for this time period, George Veith's book *Black April*, comes to different conclusions on two of these points. Specifically, Veith argues that Saigon understood the reality of diminished American support, and more importantly that South Vietnam did start to roll back coup-proofing in response to a higher level of external threat.[4] Regarding American support, Congress slashed the White House's aid requests in 1974, to the point that South Vietnam began to restrict combat operations for want of material support. President Gerald Ford's August 1974 restatement of America's commitment to South Vietnam notwithstanding, by fall 1974 Saigon complained to the White House that American aid was "utterly inadequate."[5]

President Richard Nixon had also in late 1972 told the South Vietnamese that the United States would "react vigorously" to any North Vietnamese violations of a Peace Accord, a statement that perhaps was understood to mean American air strikes. The political feasibility of renewed American military involvement aside, any

[4] George Veith, *Black April: The Fall of South Vietnam, 1973-1975* (New York: Encounter Books, 2012). Talmadge cites pages 5 and 52.

[5] Veith, 83.

promise of such support was soon rendered moot by the passage of legislation banning American combat activity in South Vietnam after August 15, 1973.[6]

In short, the decline of American military support should have heightened South Vietnam's perceived level of external threat. Somewhat consistent with the theory, and contravening Talmadge's assessment of South Vietnamese inaction, however, South Vietnam during this period did take steps to reform its military and improve its effectiveness. It did this for two reasons, according to Veith. First, the Nixon policy of Vietnamization had started to work, in the sense of improving South Vietnamese fighting abilities. Second, the spring 1972 North Vietnamese Easter Offensive had served as a wake-up call for Saigon, serving a similar function as the 1986 Iranian al-Faw victory did for Baghdad, and Saigon responded by shaking up its command structure, including replacing older, incompetent leadership with younger, battle tested leadership. The result was the at least somewhat improved South Vietnamese combat performance in 1974 and 1975.[7] This is the chain of events predicted by Talmadge's theory: higher external threat (Easter Offensive) causing (some) coup-proofing reversal (improved personnel appointment policies), in turn improving combat performance.

As a work of political science, the argument *The Dictator's Army* is intended to apply to dictatorships in general, not just the dictatorships discussed in the book. How well does the theory export to other cases? Consider the Soviet Union from 1939-1945. The Soviet Union engaged in coup-proofing in the 1930s, including purging the Red Army officer corps. In a passing reference, Talmadge claims that as the Nazi threat to the Soviet Union peaked in 1942, Soviet dictator Joseph Stalin began to reverse coup-proofing, leading to improved military effectiveness in 1943-1945 (25). However, the reversal of coup-proofing began earlier, in the wake of the 1940 Soviet victory over Finland in the Winter War. Stalin recognized that the Red Army performed poorly in the Winter War, and initiated a process of military reforms, including the reversal of earlier coup-proofing measures. For example, Stalin depoliticized the Red Army by diminishing the power of political commissars. He also ordered military training to be overhauled.[8]

It is not clear if this case would support the theory or not. On one hand, Stalin's post-Winter War actions were motivated by a sense of threat, poor Soviet military performance against Finland coupled with a sense that war with Germany was somewhere on the horizon. On the other hand, one could argue that external threat at this stage was only moderate. The Soviet Union was neutral from March 1940 to June 1941, reducing its level of external threat. Germany was its ally. Right up until Germany's June 1941 invasion, Stalin did not expect an imminent German attack. Indeed, when Stalin received a wave of intelligence

[6] "Vietnam Background: Congress and the war: Years of Support," in *CQ Almanac 1975*, 31st ed. (Washington, DC: Congressional Quarterly, 1976), 296-99, available at http://library.cqpress.com/cqalmanac/cqal75-1213972.

[7] Veith, esp. 5-6.

[8] Carl Van Dyke, *The Soviet Invasion of Finland, 1939-1940* (London: Frank Cass, 1997), 210; John E. Jessup, "The Soviet Armed Forces in The Great Patriotic War, 1941-5," in Allan R. Millett and Williamson Murray, eds., *Military Effectiveness* 3, new ed. (Cambridge: Cambridge University Press, 2010), 261, 272.

warnings in 1941 of imminent German attack, he refused to believe them.[9] That is, one could argue that Stalin began to reverse coup-proofing during a time period when external threat to the Soviet Union was not high, a pattern perhaps unexplained by the theory.

The case of Nazi Germany also provides an interesting application for the theory, especially towards possibly expanding on the theory. Coup-proofing scholars such as Talmadge sometimes lump all coup-proofing measures together, implicitly assuming that when dictators coup-proof, they adopt all coup-proofing tools at the same time. However, dictators sometimes adopt some tools and not others. Importantly, adoption of some coup-proofing tools may enable dictators to avoid adopting others, and this may have more nuanced consequences for military effectiveness.

Consider Nazi Germany under the rule of Chancellor Adolf Hitler. Hitler adopted some tools of coup-proofing, such as limiting the authority of some commanders to act autonomously, and paying gigantic bribes to high level military leaders to maintain their loyalty.[10] But, Hitler also avoided adopting other coup-proofing tools, such as wide-spread purges of his officer corps, promotion on the basis of loyalty rather than demonstrated effectiveness, interference with training, and so forth. Forgoing some of these coup-proofing measures perhaps facilitated the high levels of operational and tactical military effectiveness the German military enjoyed during World War II.[11] New theory should explore how dictators choose which coup-proofing measures to adopt and which to eschew, how different coup-proofing measures might be able to substitute each other, and, critically, how some coup-proofing measures (such as bribery) might have lesser deleterious effects on military effectiveness as compared with other, more damaging measures (such as promoting officers on the basis of political reliability).

But, these critiques should not distract from the central point that *The Dictator's Army* provides important theoretical and empirical contributions to our understanding of how tyrants fight their wars. The book will push forward important scholarly debates in international relations.

[9] David M. Glantz and Jonathan House, *When Titans Clashed: How the Red Army Stopped Hitler* (Lawrence: University Press of Kansas, 1995), 41-44.

[10] Albert Seaton, *The German Army, 1933-1945* (New York: St. Martin's Press, 1982), 143-144.

[11] Jürgen E. Förster, "The Dynamics of *Volksgemeinschaft*: The Effectiveness of the German Military Establishment in the Second World War," in Allan R. Millett and Williamson Murray, eds., *Military Effectiveness* 3, new ed. (Cambridge: Cambridge University Press, 2010), 180-220.

Review by Jessica L.P. Weeks, University of Wisconsin-Madison

Caitlin Talmadge has written an important book about an intriguing puzzle: why do some non-democracies produce militaries that are effective on the battlefield, while others fail – or choose not – to translate their national assets into competent military organizations?

Talmadge argues convincingly that the answer lies in the balance of internal and external threats faced by the incumbent regime. Governments fearing military coups have incentives to organize their militaries in ways that minimize that threat, with severe costs to conventional military effectiveness. Regimes whose bigger concern is an external enemy (whether foreign, or in the form of a mass domestic uprising) will instead tend to pursue best practices in military organization: they will adopt promotion patterns that prioritize competence over political loyalty, allow their units to train extensively, implement decentralized and clear command arrangements, and encourage extensive vertical and horizontal communication within the military. By focusing on the balance between internal and external threats, Talmadge is able to generate predictions not only *across* regimes, but also within the same regime over time or even across different units in the same country; this focus on within-regime variation is a particularly exciting aspect of the theory. The book is a major addition to the burgeoning literature on the behavior of not just states, but the political actors (regimes and leaders) within them when it comes to decisions bearing on military effectiveness and foreign policy more generally.[1]

The book's empirical focus is on two sets of carefully paired cases: North and South Vietnam during the Vietnam War, and Iran and Iraq during their bloody eight-year conflict. Talmadge argues that North Vietnam did not face coup threats, and therefore adopted conventional war practices that maximized its battlefield effectiveness. This is particularly interesting given that North Vietnamese civilian officials did nonetheless meddle extensively in military affairs. The succession of South Vietnamese governments, in contrast, faced significant risk of a military coup, and therefore were pressured to adopt organizational practices that led them to perform poorly in the war. The exception, Talmadge argues, was the 1st division of the South Vietnamese Army, which was far enough away from Saigon that it posed little coup threat. This case nicely suggests that even within the same country, military units that pose a smaller coup threat to the ruling regime are organized with more emphasis on conventional military effectiveness rather than being hobbled by coup-proofing.

In her analysis of the Iran-Iraq war, Talmadge is able to highlight another form of intra-regime variation: changes within the same country over time. For example, in Iraq, Saddam Hussein initially adopted coup-proofing practices, but focused increasingly on conventional capacity as the Iranian threat started to eclipse

[1] See, e.g., Jack L. Snyder, *Myths of Empire: Domestic Politics and International Ambition* (Ithaca: Cornell University Press, 1991); Risa Brooks, *Political-Military Relations and the Stability of Arab Regimes* (Adelphi Paper 324. London: International Institute for Strategic Studies, 1998); Dan Reiter and Allan C. Stam, *Democracies at War* (Princeton: Princeton University Press, 2002); Bruce Bueno de Mesquita, Alastair Smith, Randolph M. Siverson, and James D. Morrow, *The Logic of Political Survival* (Cambridge: MIT Press, 2003); Risa Brooks, *Shaping Strategy: The Civil-Military Politics of Strategic Assessment* (Princeton: Princeton University Press, 2008); Giacomo Chiozza and Hein E. Goemans, *Leaders and International Conflict* (New York: Cambridge University Press, 2011); Jessica Chen Weiss, *Powerful Patriots: Nationalist Protest in China's Foreign Relations* (Oxford: Oxford University Press, 2014); Sarah E. Croco, *Peace at What Price? Leader Culpability and the Domestic Politics of War Termination* (New York: Cambridge University Press, 2015); Michael C. Horowitz, Allan C. Stam, and Cali M. Ellis, *Why Leaders Fight* (Princeton: Princeton University Press, 2015).

the domestic one. Each of the four case studies uses and array of translated documents and other sources, and carefully puts those sources into context. Talmadge's book is particularly strong in detailing how coup-proofing practices (its intervening variable) affected military performance, and Talmadge's command of the individual battles she analyzes is impressive.

Where the book leaves the reader wanting more is on the question of *why* and *when* regimes perceive coups to be a greater threat than conventional war, particularly among the cases of interest here, all of which involve countries that were fighting conventional wars and thus already faced a significant external conventional threat. The book's central argument – that regimes tailor their military organizations to whichever threat they perceive to be greater – has a somewhat functionalist flavor. Regimes organize their militaries to prevent coups when they are most concerned about the threat of coups, and they organize their militaries to fight conventional conflicts when they believe that they require conventional capacities. While this is a reasonable and plausible argument, it leaves the reader wondering: when and why do the threat of coups loom large, and when and why do states prioritize external military effectiveness?

On the question of coup threat, Talmadge argues that coups pose a particularly significant risk when a country has weak political institutions (with personalist regimes and military regimes being particularly coup-prone), and/or has a poor history of civil-military relations (20-22). External threats rise in importance when a regime faces "impending conflict with another state, or if the regime has foreign policy objectives that affirmatively require territorial revision." (23)

What Talmadge could do more to acknowledge is that many of these factors, on both the domestic and external side, are influenced by choices of the incumbent regime and leader, rather than being foisted on them. Some leaders (such as Iraq's Saddam Hussein), for whatever reason, aspire to create highly personalized dictatorships. That kind of regime gives the leader great domestic power, but increases the threat of coup attempts and therefore requires a coup-proofing strategy of military organization, which in turn hobbles military effectiveness. Other leaders (such as North Vietnam's Ho Chi Minh) choose to develop stronger, if more constraining, political institutions, which provide them with firmer civilian control of the military and lowers the risk of coups. This allows them to prioritize the country's conventional military power and further expansionist foreign policy goals, but undermines the leader's personal control over policy.

Put differently, leaders and regimes face significant tradeoffs in their choices about how to organize their country politically and militarily, and they have agency over these tradeoffs. This agency is particularly great when it comes to the decision to fight a conventional foreign war, which requires significant conventional military capacity. War usually does not come out of thin air: regimes can choose whether to escalate a conflict to the point of war rather than reaching a peaceful bargain, and even more importantly, governments have substantial discretion in whether to pursue expansionist foreign policy objectives requiring conventional strength. But in order to be successful in their goals, these leaders must forgo coup-proofing strategies. What do they choose?

Viewed this way, the balance between coup threat and external threat is not an exogenous independent variable causing the type of military organization. Rather, different leaders and regimes weigh the costs and benefits of different goals, and prioritize the goals – including battlefield effectiveness – that best suit them. Leaders cannot have it all. They can (try) to pursue domestic preeminence in a personalist regime, but then must accept some degree of external weakness due to the necessity of coup-proofing their military. Alternatively, a leader can try to pursue a successful expansionist foreign policy, but because that goal precludes coup-proofing, the leader will need to either run the risk of a military coup, or share power in the form of a single-party regime. Except for the rare instances in which the threat environment is foisted upon a leader, the threat environment is thus something to be *explained* rather than to be taken as given.

Related issues bear on the historical case studies. Much of the book's most persuasive and detailed evidence concerns not on how the threat environment causes leaders to choose specific patterns of military organization, but rather how military organizational practices affected performance on the battlefield. While Talmadge illustrates this effectively in her detailed case studies, this is also the part of the book whose findings are most established in the existing literature on the deleterious effects of coup-proofing on military effectiveness.[2]

When it comes to assessing the effect of her primary independent variable – the threat environment – the book is less detailed. Talmadge helpfully provides ten indicators that she uses to determine whether the dominant threat to the state is coups or an external threat requiring conventional military capacity (35-36); seven of those factors increase the salience of coup threats, while three of those factors focus on external threats that create pressures to generate conventional capacities. But how does one know, *ex ante*, which threat is dominant in a particular case, and why? In the case of South Vietnam, for example, there are only two pages devoted to describing and coding the primary independent variable, the threat environment (52-53). It is not entirely clear where South Vietnam would stack up on the list of ten indicators, particularly given the domestic insurgency (which would mitigate in favor of conventional war-fighting capacity, according to her checklist). And the book does not discuss how the threat environment could have been a product of choices by the regime's leaders. The same is true in the book's one-page assessment of the threat environment of North Vietnam (63-64). The descriptions of the threat environment facing Saddam Hussein (150-155), and Iran (166-170) are longer, but they highlight the many choices leaders made in terms of how to organize their regimes politically and institutionally, thus creating their own domestic and international threat environments.[3]

In sum, Talmadge has written an important book that shows persuasively that military organizational practices affect battlefield effectiveness. Her argument that the balance between internal and external threats constrains the regime's choice of military organizational strategy raises many intriguing questions for future research. There is no doubt that scholars will take up that challenge.

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[2] E.g. Stephen Biddle and Robert Zirkle, "Technology, Civil-Military Relations, and Warfare in the Developing World," *Journal of Strategic Studies* 19:2 (June 1996): 171-212; Brooks, *Political-Military Relations;* and James T. Quinlivan, "Coup-proofing: Its Practice and Consequences in the Middle East," *International Security* 24:2 (Fall 1999): 131-165.

[3] Relatedly, the discussion of the differences between the South Vietnamese 1st Division and other units is intriguing in making the case that this division was less threatening to the regime, and therefore was permitted to institute better conventional war-fighting practices. While Talmadge admits that the historical record does not allow her to establish that practices were different in the ARVN 1st because this division did not threaten a coup, but since this is one of the significant examples of intra-regime variation, it would be nice to see more evidence that the central government actually chose organizational practices that were different in the ARVN 1st, rather than simply sending their best soldiers to that division.

Author's Response by Caitlin Talmadge, The George Washington University

It is a privilege to engage in a discussion of my book with the distinguished participants in this roundtable. I have learned a great deal from the scholarship of each and am delighted to have the opportunity to respond to their thoughtful and detailed reviews, which I hope will help spark further exploration of the ideas in *The Dictator's Army*. In this essay I will briefly recap my book's argument before addressing the reviewers' comments regarding the book's theory, empirics, methodology, and implications for future research.

In *The Dictator's Army* I sought to make sense of variation in military performance that is puzzling from the perspective of existing theories—particularly variation in military effectiveness across different units of the same military or in the same military over time. Such variation is hard to explain if military power is a function of fairly static factors such as national wealth or culture. I am pleased that the reviewers generally agree that this effort is one of the book's strengths and that it helps us account for more fine-grained differences in the operational and tactical fighting power of armies. The argument and evidence show that militaries' organizational practices—their key policies with respect to promotions, training, command and information management—stem fundamentally from the threat environment facing a given regime and powerfully modulate states' abilities to use their material resources in the generation of fighting power on the battlefield.

Ultimately, my goal was to offer a framework that could help scholars studying military performance, as well as policymakers seeking to assess or influence it in the real world. For methodological and substantive reasons I chose to focus my empirical work on authoritarian regimes. These states display significant variation in the internal and external threats that they face, the military organizational practices that they adopt, and the battlefield power that their armies generate. For example, authoritarian regimes almost always engage in significant politicization of the military, but my work shows that this politicization can take dramatically different forms, with some maximizing combat effectiveness and others hindering it. Understanding these differences is both a pressing practical concern as well as an important part of improving our general understanding of the role of political institutions, civil-military relations, and threats in shaping states' behavior in war.

Theoretical Questions

Jasen Castillo, Ryan Grauer, and Dan Reiter all raise variants of the same theoretical issue, which is whether all of the coup protection practices I emphasize in my theory are equally important and necessary. For example, can states choose to adopt only some of these measures and not others? Might certain combinations of coup protection measures be less damaging to conventional military performance than others? In other words, is the trade-off between coup protection and conventional military effectiveness as stark as I portray it to be? Both Castillo and Reiter point to Nazi dictator Adolf Hitler in particular as a leader who seems to have managed to engage in creative forms of coup-proofing that did not hinder battlefield effectiveness.

I strongly endorse the reviewers' call for further research in this area, especially on parallel militaries, which as Castillo notes may provide a way for leaders to check the regular military. The challenge with that approach, as I note in the book, is that leaders have to be able to ensure that the parallel military is both highly loyal *and* that it has enough combat capability to protect the regime against the presumably larger and highly capable regular military (253, note 8). This is theoretically possible but practically difficult in many cases. For this reason, many regimes with parallel forces, such as the People's Commissariat for International Affairs (NKVD) in the Soviet Union or the Islamic Revolutionary Guard Corps (IRCG) in Iran, still engage in additional coup-proofing measures that do hinder combat performance.[1]

The Nazi regime had some unusual, perhaps unique, advantages that eased this trade-off, or at least delayed it. Chief among these were the Prussian martial tradition, which endowed the military with an enduring underlying competence, and the historically unprecedented Nazi ideological apparatus, which directed that competence toward the regime's enemies and rendered coups nearly unthinkable.[2] As I note in the book, Hitler generally "did not have to choose between competence and loyalty in his officer corps—there was an ample supply of men who possessed both, significantly reducing the coup risk that Hitler faced from building an externally effective army" (256, note 42). For example, even as Hitler built the SS, he generally did not interfere with the *Wehrmacht's* long-standing adherence to rigorous training, and he initially devolved significant tactical and operational authority to military officers on the battlefield (256, notes 41, 42, 43).

Interestingly, despite this extraordinary margin for maneuvering against both internal and external dangers, as the war went on Hitler did eventually confront the trade-offs identified in my theory. Growing more distrustful of his military, for example, he "commented that he often bitterly regretted not having purged his officer corps the way Stalin did."[3] In 1942-1943 he actually began to assume personal command of his field armies, with disastrous consequences for battlefield effectiveness, as the theory would predict (256, notes 41, 42, 43). Nevertheless, the Nazi case and others point to the need for further research about the conditions under which regimes do and don't adopt particular constellations of coup-proofing measures, and with what effects.[4]

Empirical Questions

Reiter also thoughtfully probes the applications and potential limits of the theory by raising two sets of empirical questions. One relates to the theory's predictions in the South Vietnam case I examine. Reiter argues that the 1972 Easter Offensive losses and the decline of American military support should have heightened South Vietnam's sense of external threat, leading to a reduction in coup-proofing and an improvement in military performance. The thrust of his criticism is that I understate the degree to which South Vietnam actually did this, and also perhaps that the theory predicts they should have done it even more vigorously than they did.

I fully agree with Reiter that South Vietnam lessened its coup-proofing measures during and after the devastation of the Easter Offensive, and that these changes produced some improvements in South Vietnam's

[1] See also Caitlin Talmadge, "Different Threats, Different Militaries: Explaining Organizational Practices in Authoritarian Armies," *Security Studies* 25:1 (spring 2016), 121, fn 23.

[2] On this ideology, see Castillo, *Endurance and War*, especially chapter 3.

[3] Quotation from Joachim Fest, *Plotting Hitler's Death: The Story of the German Resistance* (New York: Henry Holt, 1996), 332. Noted in Talmadge, *The Dictator's Army*, on p. 256.

[4] For work in this direction, see Dan Reiter, "The Coup-Proofing Toolbox: Dictator's Choices, Military Effectiveness, and the Puzzle of Nazi Germany," draft paper, May 2016.

effectiveness. I document this process extensively in the book (108, 113-116, 137). However, I also argue that these changes were never as widespread as they would have needed to have been in order to maximize South Vietnamese battlefield effectiveness, for two reasons.

First, I disagree with Reiter about how Saigon viewed the external threat environment. The balance of evidence indicates that Saigon believed right up until the end that some U.S. assistance might be forthcoming in the event of a truly catastrophic threat from the North, and that this hope muted the incentives South Vietnam might otherwise have faced to move more rapidly toward conventionally oriented military organizational practices. This is a running theme of my analysis of South Vietnam prior to the Easter Offensive (53, see also 40, 50), and the source that Reiter and I both reference for the period after that, George Veith's *Black April: The Fall of South Vietnam, 1973-75*, shows that this reasoning continued to hold sway in Saigon.[5] Beyond Nixon's promise to "react vigorously" in the event of an attack from the North (which I cite, 56), Veith documents that even in September 1974, well after the 1973 legislation Reiter cites banning U.S. combat activity inside South Vietnam, President Ford was still providing backchannel reassurances of forthcoming aid.[6] In fact, even amidst the actual North Vietnamese invasion in early 1975, Ford continued to send South Vietnamese President Nguyen Van Thieu "vague promises of support," and according to Veith "Thieu still clung to a vain hope that B-52s would return."[7] All of this points to the fact that the signal from the threat environment was perhaps not as clear as Reiter suggests: yes, the conventional threat to the regime was growing, but the regime might still have outside help in dealing with that threat, so the external danger didn't provide enough of a reason to wholly reorganize the military away from the enduring emphasis on coup protection—especially since coups remained a significant threat.

That leads to my second point. My theory emphasizes that even when external threats are pressing, leaders often will still prioritize coup prevention for a variety of reasons (18-23). Furthermore, the theory argues that the process of moving from coup prevention practices toward conventional war practices is likely to be difficult, requiring very strong and unambiguous signals from the external threat environment, and also likely to be slow in military regimes whose factionalism is an obstacle to rapid decision-making (23-27, especially 25). In fact, this is consistent with what we see in the South Vietnamese case. Even as external, conventional threats grew stronger starting in 1972 (although we can debate by how much), "Thieu remained fearful of a coup," as both Veith and I document (56).[8] Given this lingering fear and the theory's emphasis on the primacy of coup threats and the difficulty of change, I believe that South Vietnam's half-hearted moves toward conventional warfare practices are in fact exactly what the argument would predict. In fact, the theory helps us make sense of South Vietnamese behavior that can otherwise seem puzzling for exactly the reasons Reiter identifies. In any event, Reiter's criticism usefully shows how the connections posited in the theory—from threats to military organizational practices to effectiveness—can help us rigorously examine empirical evidence in order to understand why states behave as they do.

[5] George Veith, *Black April: The Fall of South Vietnam, 1973-1975* (New York: Encounter Books, 2013).

[6] Veith, *Black April*, 18 and 84-5.

[7] Veith, *Black April*, 223.

[8] Veith, *Black April*, 26.

Reiter also asks to what extent the theory can explain Soviet dictator Joseph Stalin's behavior from 1939-1945, which I mention briefly in the book (25). Again, Reiter and I agree that the overall pattern in the case is broadly consistent with the theory: in the pre-war environment, Stalin's coup fears led to military organizational practices that hindered the generation of conventional, externally oriented military power, but as World War II posed an increasingly grave danger to the survival of Stalin's regime, he pushed the military to adopt organizational practices geared toward defeating the Nazis. Reiter's key question concerns the timing of these changes, and he argues that they may have come earlier than my theory expects. Specifically, he dates them to the period immediately following the disastrous Winter War of 1940 and argues that such a turn-about may confound my theory given that external threats to the Soviet Union were only "moderate" at this stage, before the Nazi betrayal of June 1941.

I read the case somewhat differently. Although Stalin did implement some changes to military organizational practices during and immediately following the war in Finland, these were limited, in part because although the Winter War was a debacle, the invasions of Poland and the battle for Khalkin-Gol were seen as having been adequate.[9] It is true that in 1940 Stalin made some changes, for example, releasing many officers who had been imprisoned in the purges and promoting General Semion Timoshenko, who then initiated some military reforms, especially with respect to training.[10] But according to Roger Reese, "inspections… in 1941 revealed that none of the lessons of the Finnish war had been implemented in the training or organization of the divisions…. Nothing had changed."[11] Indeed, this is part of why the surprise Nazi invasion in June 1941 was so devastating.

It was that onslaught, which, unlike the Winter War, actually had the potential to unseat the regime in Moscow, that seems finally to have galvanized Stalin to undertake a much more vigorous effort at military re-organization than had been the case in 1940.[12] For example, Stalin ruthlessly began to weed out commanders who performed poorly in battle, and he improved the ability of different military units to coordinate their actions, even as he retained an ironclad personal grip on their activities.[13] These changes did not transform the Soviet behemoth overnight, but, combined with the earlier efforts and Allied aid, they eventually resulted in

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[9] Roger Reese, *The Soviet Military Experience: A History of the Soviet Army, 1917-1991* (New York: Routledge, 2000), 98.

[10] On the releases, see Gabriel Gorodetsky, *Grand Delusion: Stalin and the German Invasion of the Russia* (New Haven: Yale University Press, 1999), 120. On Timoshenko, see Reese, *The Soviet Military Experience*, 99.

[11] Roger Reese, *Red Commanders: A Social History of the Soviet Army Officer Corps, 1918-1991* (Lawrence: University of Kansas Press, 2005), 146. See also Reese, *The Soviet Military Experience*, 97.

[12] See David Glantz, *Colossus Reborn: The Red Army at War, 1941-1943* (Lawrence: University of Kansas Press, 2005), especially chapters 1, 10, and 12; and David Glantz and Jonathan House, *When Titans Clashed: How the Red Army Stopped Hitler* (Lawrence: University of Kansas Press, 2015), especially chapter 5.

[13] Reese, *Red Commanders*, pp. 158-163, 175; and Glantz*, Colussus Reborn*, especially 370.

improved battlefield effectiveness by 1943-1944.[14] As such, I believe the case conforms to the theory's predictions fairly tightly, though readers will have to examine the evidence for themselves to judge.

Methodological Questions

Grauer raises an important methodological point, which is whether the book's basic conception of effectiveness is the most useful one. Is effectiveness really a property intrinsic to particular units, reflecting whether they can perform certain tasks such as basic tactics and complex operations, or is this notion better thought of simply as efficiency? In other words, Grauer wonders, for assessments of true effectiveness, do we also have to assess whether forces achieve their ultimate objectives in combat, which might include broader goals such as the destruction of adversary forces? Although Grauer cautions that this questioning may be "pedantic" and acknowledges that adopting his alternative conceptualization of effectiveness wouldn't alter my book's basic findings, his question is one I considered closely.

Ultimately I chose to define effectiveness as I did because I wanted to focus specifically on battlefield effectiveness, that is, operational and tactical fighting power (4-8). As Betts notes in his introduction, scholars have paid relatively less attention to these levels of warfare, so I sought to make a contribution there. I was particularly interested in trying to understand which armies could conduct the key tasks envisaged in Stephen Biddle's highly influential concept known as the "modern system" of warfare, which is indeed more narrowly focused on specific operational tasks.[15] Grauer's alternative definition starts to shade closer to strategic effectiveness, which of course also merits inquiry but is distinct. Furthermore, armies usually have to be tactically and operationally effective in the ways I describe in order for them to achieve the broader effectiveness that is of interest to Grauer. Hence in many ways I see my book as a prerequisite to doing the type of analysis he envisions, and which I agree is important.

Last, I tried to stay away from the emphasis on efficiency that Grauer mentions because, to me, efficiency always implies some reference to the resources that an army uses to generate a given level of fighting power. It is, in essence, grading on a curve—asking whether we think an army fought well given the resources available to it, rather than according to some objective standard (4). For example, even though my book rates elite Iraqi units as effective in the closing battles of the 1980-1988 Iran-Iraq War, I would not rate the Iraqi military as being efficient. Given Iraq's material resources, it arguably should have performed at least that well years earlier than it did and on a much wider scale. Nevertheless, Iraq did get eventually get the job done on the battlefield, albeit at great cost (230-2). By contrast, I would rate the North Vietnamese as being both effective and efficient—performing the key tasks of modern battle and managing to do so with minimal material resources. This contrast suggests that efficiency and effectiveness, while both important, are distinct.

Jessica Weeks raises a second methodological concern, expressing a desire to know more about "*why* and *when* leaders perceive coups to be a greater threat than conventional wars." In her view, the threat environment, both domestically and internationally, is not an exogenous variable. Rather, "these factors," in her view, "are influenced by choices of the incumbent regime and leader, rather than being foisted on them….. They have

[14] See also Talmadge, "Different Threats, Different Militaries," 119-20, fn 21.

[15] Stephen Biddle, *Military Power: Explaining Victory and Defeat in Modern Battle* (Princeton: Princeton University Press, 2006).

agency over these trade-offs." Weeks is right that we can always know more about why particular states make particular choices, and I share her hope that future researchers will take up this challenge.

I chose to treat the threat environment as exogenous for two reasons. First, even if we believe that leaders "create" their own threat environments, it isn't clear that knowing how and why they make these choices gives us additional leverage over understanding the battlefield power that their armies will generate. In other words, we don't have good reason to believe that these choices systematically confound the causal relationships emphasized in the book in ways that would substantially change the findings (indeed, Weeks does not make this claim). Given that the threat environment is acknowledged to be analytically powerful, no matter whether we think it is foisted upon leaders or engineered by them, I chose in the interests of parsimony to begin my analysis there.

Second, the leaders in all four of the states I examine clearly did face significant constraints in their ability to shape their threat environments. In South Vietnam, for example, leaders inherited a hobbled post-colonial army and faced a severe internal legitimacy challenge, followed soon by a foreign-backed insurgency and eventually an invasion, all of which they no doubt happily would have avoided if given the option (43-53). But as I discuss extensively in chapters 2 and 3, it isn't clear that alternative patterns of military organization were really available to them given the prominence of coup threats that plagued the regime from its inception (54-62). Indeed, my framework expects these coup threats to dominate leader concerns even where other threats are very serious (22), which is exactly what happened in South Vietnam. Similarly, leaders in Tehran in 1980 were taken by surprise when the Iraqis invaded and clearly had not designed Iran's military forces for dealing with such a threat. Nor is it clear that they could have or should have, given the institutional chaos of the revolution and the multiple other dangers facing the nascent regime, including coups (139-150, 165-177).

As the initiators of their wars, North Vietnamese and Iraqi leaders clearly did have more agency over their threat environments, although even there it is important not to overstate the extent to which Ho Chi Minh and Saddam truly had the ability to build the domestic institutions and external conditions that they would have preferred. For example, Saddam launched the Iran-Iraq War impulsively, and, in his own mind, defensively, as a means of pushing back against revolutionary Iran's potential threat to his Sunni Ba'athist regime (142-146). Moreover, Saddam's persistent prioritization of coup protection measures both before and during most of this war was rooted in a sadly accurate understanding of deeply rooted features of Iraqi political life that pre-dated him (150-55) and that persist even today.

It is notable, for example, that even after overthrow of the Ba'athist regime and tremendous U.S. pressure to build democratic institutions, the succession of Shi'a leaders in post-2003 Iraq all have read their domestic threat environment very similarly to how Saddam and his predecessors did. As a result, these new leaders have adopted military organizational practices that are strikingly familiar and that have produced similarly costly battlefield outcomes.[16] These continuities suggest that leaders may be more constrained and reactive than is often recognized, and that treating their threat environments as exogenous is analytically reasonable.

[16] Keren Fraiman, Austin Long, and Caitlin Talmadge, "Why the Iraqi Army collapsed (and what can be done about it," Monkey Cage blog, *Washington Post*, 13 June 2014; Cailtin Talmadge and Austin Long, "Why the U.S. (Still) Can't Train the Iraqi Military," Monkey Cage blog, *Washington Post*, 22 September 2015; and Caitlin Talmadge and Joshua Rovner, "Why Victory in Mosul Won't Solve America's Iraq Conundrum," Lawfare blog post, 10 April 2016.

Nevertheless, Weeks is right to note that more theorizing on this subject is warranted, and her review points to a host of promising additional research questions that I hope will receive attention.

Toward a Future Research Agenda

The reviewers outline several other ideas for future research as well, most of which I heartily endorse. First, as Castillo notes, there are good reasons to wonder if my framework offers insight into the military performance of democracies as well as well as dictatorships, and I agree with his suggestion that future research should explore this possibility. Indeed, my focus on regime strength—rather than regime type—as a major indicator of the threat environment points in this direction, as Grauer notes as well. Presumably, as he explains, regime institutional strength can vary within democracies as well as autocracies, as can civil-military relations, the other major indicator I develop. I briefly touch on this possibility in the book's first chapter, noting that just as some stable autocracies can be quite well institutionalized, some nascent democracies can be quite coup-prone (21), and as such we might expect them to perform poorly on the battlefield (32-3). Hence some of the most critical variation in military performance probably does cut across regime type, as the reviewers note.

Betts makes a related point that even within autocratic regimes, there is important additional variation to explore, and I agree. I tried to get at some of these distinctions in *The Dictator's Army*, taking my cue from Barbara Geddes's insight that "different kinds of authoritarianism differ from each other as much as they differ from democracy," as well as from Weeks's scholarship on varieties of authoritarianism.[17] One of my central findings was that we should not expect uniform patterns of military organization in authoritarian states, because the threat environments facing these regimes can be so different. In general, however, Betts's comments and those of the other reviewers shore up the need to continue to refine our understanding of how political institutions ultimately shape military performance—a challenge that one of the other reviewers, Dan Reiter, has powerfully addressed as well and that I hope my book will help future researchers continue to engage.[18]

Castillo and Grauer also both raise important questions about the role of ideology in explaining military performance, which I agree future research should explore more closely than I did in *The Dictator's Army*.[19] My framework sought to explain two major aspects of battlefield performance: basic tactical proficiency and the ability to conduct more complex operations that require both initiative and coordination across different combat arms or units. I did not incorporate ideology as a primary driver of these components of military performance because I think they are mostly a function of military organizational practices that regimes adopt in response to the internal and external threats that they face. Nevertheless, Grauer notes that in some cases ideology itself may shape how regimes understand their threat environments, and Castillo suggests that regimes also can use ideology as a tool for managing the threat environment. These are both intriguing

[17] Barbara Geddes, "What Do We Know About Democratization After Twenty Years?" *Annual Review of Political Science* 2 (June 1999), 121; and Jessica L.P. Weeks, *Dictators at War and Peace* (Ithaca: Cornell University Press, 2014).

[18] Dan Reiter and Allan C. Stam, *Democracies at War* (Princeton: Princeton University Press, 2002).

[19] Promising work in this direction includes Jason Lyall, *Paths of Ruin: Explaining Military Effectiveness in Conventional War*, unpublished book manuscript, 2016, which explores the role of exclusionary regime ideologies in shaping military power.

possibilities that scholars should explore further in other cases, and in fact I mention them briefly in my discussion of the role of nationalism in the Vietnam cases (134).

Furthermore, Castillo's own work has shown that ideology is a critical driver of a different and very important component of military performance: cohesion.[20] He correctly notes in his review that I largely bracket the question of cohesion in my book, although I agree with him that it is a prerequisite to the components of military performance that I examine (7) and that the military organizational practices I emphasize are likely to reinforce or undermine cohesion in ways the book does not explicitly address. For example, good training probably builds soldiers' confidence in their leaders and in their units' proficiency, which should strengthen soldiers' willingness to stand and fight under difficult conditions. In short, the 'will' and 'skill' components of military effectiveness are likely interrelated, as Castillo suggests.

Despite this connection, however, I do think that my book highlights the often distinctive origins and effects of these two components of military performance. For example, I note in my case study of Iran that Iranian cohesion was the one strong point in the military performance of Iran's revolutionary forces and that this cohesion probably had ideological sources (228-230). Still, Iran's persistent difficulty with tactical proficiency and complex operations suggests to me just how distinct cohesion can be from these other aspects of battlefield performance. Iran's army was extremely ideological and extraordinarily cohesive but still not very tactically and operationally effective due to the internal threats facing Iranian leaders and the military organizational practices that those threats engendered.

Iran's experience stands in contrast to that of the North Vietnamese, for example, whose units were cohesive as well as tactically proficient and capable of complex operations. Clearly, North Vietnam was able to harness the military power of ideologically motivated cohesion, while Iran squandered it. Future research should build on both Castillo's work and my own to try to develop a more integrated understanding of this sort of variation. The relationship between will and skill is clearly important, and Castillo is also right that fully understanding it may require exploration of additional hurdles that face some militaries, such as class divisions in the society from which these militaries draw recruits.

I thank him and the other reviewers for bringing all of these issues into the conversation about *The Dictator's Army*, and I look forward to seeing where the discussion goes next.

[20] Jasen Castillo, *Endurance and War: The National Sources of Military Cohesion* (Stanford: Stanford University Press, 2014). See also the H-Diplo roundtable on the book, forthcoming.