THE JOURNAL OF STRATEGIC STUDIES, 2016
http://dx.doi.org/10.1080/01402390.2016.1254938

Routledge
Taylor & Francis Group

REVIEW ESSAY

Civil–Military Relations and Policy: A Sampling of a New Wave of Scholarship

Peter Feaver

Department of Political Science, Duke University, Durham, NC, USA

Zoltan Barany, **The Soldier and the Changing State: Building Democratic Armies in Africa, Asia, Europe, and the Americas**. *Princeton*: *Princeton University Press*, 2012. Pp.472. $32.95/£24.95, PB. ISBN 978-0-6911-3769-8.

Stefano Recchia, **Reassuring the Reluctant Warriors: U.S. Civil-Military Relations and Multilateral Intervention**. *Ithaca*: *Cornell University Press*, 2015. Pp.296. $39.95, HB. ISBN 978-0-8014-5291-8.

Caitlin Talmadge, **The Dictator's Army: Battlefield Effectiveness in Authoritarian Regimes**. *Ithaca*: *Cornell University Press*, 2015, Pp.320. $26.95, PB. ISBN 978-0-8014-5347-2.

Thoughtful commentary on the politics of civil–military relations is as ancient as the Greek and Roman Republics and as contemporary as the latest headlines. As a subfield of political science, the study of civil–military relations came of age during the 1950s and has experienced several waves of theoretical and empirical flourishing since that time. Currently, the field is enjoying another renaissance of engaging scholars – call them 'Young Turks' – pressing the boundaries through innovative and well-executed projects. The three books reviewed here are but a small sample drawn from a much larger corpus of new work. These three exhibit well the many strengths – and a few limitations – that the best new scholarship exhibits.

These three works are also a useful snapshot of the state of the field in one particular respect: the progress the field has made intellectually since the landmark works of Samuel Huntington and Morris Janowitz from the early Cold War. The picture is mixed but mostly positive. Some of the arguments rely more on Huntington than I think is healthy or warranted, but there is much more here that goes well beyond the established civil–military frameworks.

CONTACT Peter Feaver pfeaver@duke.edu
© 2016 Informa UK Limited, trading as Taylor & Francis Group

Each of the studies shows that it is possible to offer fresh insights on an issue as enduring as the civil–military problematique: how to make the military an effective defender of the state without also making the military a capable threat to the state. And each opens the door to further work that promises to magnify the impact and reach of the Young Turks.

How transitional governments bring the military under democratic control

I begin with Zoltan Barany's *The Soldier and the Changing State: Building Democratic Armies in Africa, Asia, Europe and the Americas* (Princeton 2012). As the title suggests, this study builds the most directly on Huntington's work – both his seminal *Soldier and the State* (Harvard University Press, 1957) and his *Political Order and Changing Society* (Yale University Press, 1968). It is also by far the most ambitious of the three – indeed, so different in scope and approach that it almost warrants a separate review. It asks how states build militaries that support democracy – i.e. support civilian control – and it considers this across multiple state-building contexts (post-war, post-colonial independence, post-regime change) and across the globe (Australia and Oceana, and, of course, Antarctica, are the only geographic regions uncovered in the case selection).

Despite such a sweeping purview, or perhaps because of it, Barany adopts the frame of mid-range theory, almost thick description. There are a few core deductive hypotheses that travel across the regions, but for the most part, Barany is just describing what he sees in the 27 cases he examines.

To be sure, Barany opens with a literature review and theory chapter that is as wide-ranging as is his empirical ambit. What results is not a theory *per se* – indeed, he takes pains to emphasize that he is not doing grand theorizing – but along the way he makes numerous claims about what does and does not constitute good civil–military relations in theory or practice. Some of these claims are well-grounded in the literature, but others seem more *ad hoc*. For instance, he claims that the military must never run for political office (p. 32); while I agree with him that this is problematic, Barany does not spell out the basis for making this an unqualified requirement. Similarly, he says the military have a right to expect 'clear and sound guidance from the state' (p. 33); again a desirable feature, but hardly a right or, if it is a right, it is one that is violated more often than it is observed. The occasional contradiction and demonstrably dubious claim add to the sense of it being an off-the-cuff listing. Thus, he claims that ordinary people do not want the military to reflect their values (p. 38) but then claims that since societal attitudes towards gender and sexual identity vary across democracies, 'popular will' determines what different military policies will be in each case – in other words, the public requires that the military reflect their values. Similarly, he claims that the 1973 War Powers Resolution (p. 31) 'settled' the issue of US presidential authority to deploy forces without explicit legislative warrant – a claim that is hard to square with the fact that every President since has refused to recognize the constitutionality of the War Powers Act and the courts have similarly given it wide berth.

Notwithstanding these minor distractions, the literature review culminates with the right question: why, given their potential power, would the military ever eschew seizing political power? Barany notes that there is no single answer that holds across all cases, but suggests that a mix of four complementary answers apply in different measure, depending on the circumstances: (i) an internalized norm of commitment to civilian rule; (ii) civilian leaders have developed mechanisms to keep the military subordinate; (iii) military leaders have seized power before and are chastened by their failure at governing; (iv) military leaders have seized power and are satisfied with what they accomplished and believe they do not need the distraction anymore.

These questions form a backdrop, occasionally explicit but more often implicit, to the rich narrative of how the democratization of the armed forces transpired in 27 specific historical cases. These cases are clustered into nine chapters according to their common dominant contextual feature – whether it was after major defeat, after a civil-war, after a coup, etc. For the most part, the cases draw on the secondary literature, which Barany occasionally augments with interviews and other direct engagement with area specialists. The familiar cases – Germany, Japan, Spain, Russia, Pakistan – do not necessarily break new ground, but even subject matter experts are likely to learn new details. Some of the other cases – Yemen, Slovenia, Bangladesh – are rarely considered outside of narrow area studies reports and so a particularly distinctive contribution here.

The breadth of coverage is truly impressive. While it is common for large-n studies to have specific references to cases as varied as these, and while some edited volumes approximate Barany's in coverage, I am hard-pressed to come up with a sole-authored work that goes into as much historical detail into so many cases. Each chapter ends with a table summarizing the key descriptive judgments made – e.g., how influential are civilian independent experts, or how extensive is the degree of military interference in politics, etc. – that generalists like me will rely upon for quite some time. Barany shows what can be accomplished by medium-n-sized studies, and he has persuaded me that we should continue to value such contributions.

Given such a broad scope, it is perhaps unfair to flag a case he does not study and should have. But since Barany hooks his argument on the controversies surrounding the collapse of Saddam Hussein's army and the decidedly mixed success the Allies had in rebuilding Iraq's security forces in the aftermath of the war, it is ironic how little the book offers in the way of direct insight into that particular case. It is not one of Barany's 27 cases, and by the time Barany returns to the matter at the end of the concluding chapter, his policy recommendations on how the Iraq effort could have been better managed are pretty thin gruel. First, he writes, the United States should only have purged the unacceptable members of Saddam's army. Second, it should have built the new security forces around the remaining acceptable ones. And finally, once the new army was built, it should have conducted a massive purge, if that was still desired. This potted history of the Iraq experience rather begs the question, however. According to Paul Bremer and Walt Slocombe, the US officials responsible for the actions Barany (and others) criticize, the CPA did not disband a functioning military – rather, the CPA announcement regarding the Iraqi security forces simply acknowledged what had already happened, namely that Saddam's army had simply dissolved.[1] Moreover, as Stephen Biddle, Ryan Baker, and Julia Macdonald argue,[2] US efforts to build what Barany would consider to be a democratic military were repeatedly undermined by Iraqi's own civilian democratic leaders. It is not clear the Barany approach is any more realistic than what was actually done.

Because the case selection criteria primarily turn on intrinsic importance and interest (owing to important case-specific distinctive characteristics), the research design is better suited to theory-building than theory-testing. In this sense, the argument is Huntingtonian in design as well as in substance. Barany identifies a series of plausible inductions that are sensible inferences from the impressively broad material he has gathered. The result is a new reigning set of hypotheses, but one that must await further testing or further theoretical refinement to be viewed as a new complete theory of civil–military relations under conditions of democratization. In the meantime, Barany's basic insights are as sound a launching pad for further particularized study as anything Huntington has offered.

For example, Barany concludes that a democratic army, meaning a military that is supportive of democracy and civilian control, is a necessary condition for successful democratization. Identifying necessary conditions can be an important theoretical contribution, but only if the framework avoids the tautology: the defining feature of democracy and civilian control is a military subordinate to civilian rule, so the same factor cannot be both a defining feature and a necessary prerequisite thereto. Barany is on surer theoretical ground when he can identify features of the military that are conducive to fostering a democratic army. Barany identifies some – the quality of leaders, the transparency of institutional frameworks, the

[1]Paul Bremer, 'How I Didn't Dismantle Iraq's Army', *New York Times*, 6 Sept. 2007.
[2]Biddle, Stephen, Ryan Baker, and Julia Macdonald, 'Small Footprint, Small Payoff: The Military Effectiveness of Security Force Assistance', unpublished manuscript, 14 Feb. 2016.

incrementalism of reform efforts, the growth of civilian branches of government and civilian sectors of society, the quality of professional military education, the orientation of the military to new missions, and the circumscribed role for retired military – but they are in the realm of probabilistic associations, not necessary conditions.

Barany concludes with a set of partial generalizations (pp. 343–5) that are mostly sound – though I was struck by the fact that the generalization that it is better to have a sweeping, crushing defeat evidently did not hold in the book's motivating case, Iraq. Barany reached this inference for the obvious and mostly sound reason that the two greatest successes among his 27 cases – Germany and Japan – fit that pattern, and it is easy to see how the post-defeat dominance/occupation gave the Allied powers maximum leverage to impose lingering reforms and gave the target countries maximum incentive to make the reforms stick. Of course, reasoning on those same lines is why members of the Bush Administration thought they had good reason to be optimistic about what Iraq might one day become. With hindsight we can see that there are several omitted variables that may trump the context variable Barany focuses on: first, the degree of politically relevant cleavages within the society and second, the staying power of the US commitment. Germany and Japan were crushingly defeated countries that also happened to be relatively homogenous and that enjoyed a seven decade US security guarantee backed up by tens of thousands of US forces; by contrast, Iraq was a crushingly defeated country that happened to have a deep sectarian split and that was effectively abandoned by the United States barely a decade after the war (only within a year or two of achieving something resembling a cessation of hostilities). With that one exception, I did not find any partial generalizations that I would object to. They are very sensible rules-of-thumb.

But they are sensible rules-of-thumb that invite further testing. For instance, Barany also finds other patterns of success and failure across the cases he has studied. The other cases where states were able to build democratic armies more readily were the cases after military rule in Europe (Spain, Portugal, and Greece), and after communist rule in Europe (Slovenia, Russia, and Romania). But these are contingent patterns, since he could have easily picked cases of failed democratization after communist rule and, of course, there are many cases in Asia and Latin America where military rule beget more military rule. Barany's arguments are well-positioned for further testing against the universe of cases.

Indeed, *The Soldier and the Changing State* makes a great set up for a future large-n study designed to test the applicability and generalizability of Barany's inferences. Notwithstanding the limitations of the Iraq arguments, it also is a handy resource for those looking for historical examples to shed light on current policy challenges. The policy question animating Barany's study – identifying the conditions that foster the development of democratic armed forces – is a high priority for policymakers today. Barany shows that this is a daunting assignment, but not an impossible one.

How civil–military bargaining affects use of force decisions

Stefano Recchia's study of post-Cold War decision-making, *Reassuring the Reluctant Warriors: U.S. Civil-Military Relations and Multilateral Intervention* (Cornell 2015), has a far narrower scope and offers a clearer, potentially more rebuttable argument. Recchia posits an enduring post-Cold War American civil–military divide on the use of force: the reluctant military consistently pushes for multilateral endorsements as a way of sharing the military burden with allies whereas civilians are more inclined to intervene unilaterally in order to have greater freedom of action. Of course, some civilian leaders also embrace the multilateralism option from the outset, but enough do not to create persistent civil–military conflict in case after case of post-Cold War interventions. Crucially, sometimes military reluctance is strong enough to compel even unilateralist-inclined civilians to pursue multilateral endorsements as a way of buying military acquiescence.

This study builds on the empirical foundation of Richard Betts' *Soldiers, Civilians, and Cold War Crises* (Columbia University Press, 1991), and the theoretical foundations of the civil–military bargaining framework of Agency Theory (my own *Armed Servants*, Harvard University Press, 2003). Recchia also draws heavily from other studies evaluating post-Cold War civil–military conflict in the United States, as well as the related civil–military gap literature. He combines these to form a compelling narrative in which generals, skilled in the dark arts of bureaucratic politics, use those skills to push civilian leaders to seek UN or other multilateral endorsement even when civilian leaders believe such multilateralism is neither needed nor wise. In particular, military leaders use their quasi-veto power – if military leaders object publicly to a military operation then it is hard for civilian leaders to build the requisite political support to launch it – to set conditions for their support. If civilian leaders get UN authorization, military leaders will not object to this intervention but if civilians do not, the military will publicly object. An interesting problem with this argument is why the military would consistently insist on something – UN endorsement – that yields at best dodgy burden-sharing, and at worst the kind of convoluted command arrangements that hamstrung the Somalia operation. Rechhia side-steps the issue.

Recchia draws some distinctions that are not obvious and I wonder if they are even necessary. For instance, he counts as 'multilateral' only those operations that are *qualitatively* multilateral, i.e. blessed by an explicit authorization from a standing International Organization; he rejects operations that are merely *quantitatively* multilateral, in the sense of having sizable force contributions from more than one country. This allows him to code the 2003 Iraq war as 'not-multilateral,' despite the substantial contributions of British and Australian troops, let alone the meaningful contributions from other NATO allies, not to mention the rest of the coalition of the willing. This is a familiar convention in the partisan debates around the Iraq war, but it is an odd choice for a theory that purports to have as its causal mechanism the military's desire for burden sharing. By Recchia's rules, British forces did not do any burden sharing in the Iraq 2003 war.

This points to an important uncertainty left unresolved by Recchia's argument and evidence. Are American generals asking for multilateral endorsement because they genuinely want and expect burden sharing? If so, then Recchia should not care about *qualitative* multilateralism; *quantitative* multilateralism will suffice. But if generals are insisting on *qualitative* multilateralism, then why are they doing so? Perhaps generals have internalized the legitimation argument; a theoretical possibility, but like Recchia, I find this implausible enough to dismiss, especially since there is scant evidence to support it. But there is another argument that Recchia does not consider that does seem plausible: perhaps generals are insisting on the higher bar of *qualitative* multilateralism because they are grabbing for any roadblock to throw in front of the policymaking train to slow down hawkish civilians. This explanation, which would liken generals to obstructionist trial lawyers, would have the added virtue of accounting for the earlier puzzle: why do generals insist on something that does not yield much tangible benefit?

The tight empirical focus raises some scope issues. On the one hand, Recchia explicitly examines only the post-Cold War era – i.e. the era when the Security Council was a semi-responsible actor and not deadlocked by superpowers wielding a Cold War veto. Of course, the rise of Russian aggression under Vladimir Putin and Chinese adventurism under Xi Jinping raise the question of whether the argument has already lost (or shortly will lose) a good deal of its traction. On the other hand, the underlying causal mechanism that Recchia posits – namely a desire by generals to get others to join them in the fight – should have operated during the Cold War (and in the future as well). Why, then, weren't generals able to demand similar burden-sharing devices from their hawkish civilians in those earlier settings?

In terms of argument and evidence, the Recchia book is not as persuasive as it could be. Recchia relies too much on labels the precision of which has been lost because of overuse in partisan debates. For instance, Recchia talks about 'wars of choice' as if that were both synonymous with humanitarian missions and the antonym of 'wars of necessity.' In fact, all wars, regardless of the mission category, are wars of choice – there is even a debate among certain circles whether US involvement in World War II was 'necessary'[3] – and beyond its use as a rhetorical brick to throw at partisan enemies, it is not clear how it adds much of scholarly value. Or consider his use of the 'neoconservative' label, which Recchia uses liberally to denote Iraq War supporters such as Secretary of Defense Donald Rumsfeld, who in fact argued against some of the aspects of the war most precisely associated with neoconservatism, namely the promotion of democracy. Recchia stretches the term to encompass Rumsfeld's preference for light footprint operations, which had little to do with neoconservatism *per se*. More problematically, Recchia attributes motivations and then 'proves' them with quotes not to the individuals themselves but to critics of the individuals. Thus, Recchia supports the claim that Rumsfeld wanted a light footprint in Iraq as a way of killing off the Powell Doctrine with a quote not to Rumsfeld or one of his close advisors but to Undersecretary of State Marc Grossman, a sharp Rumsfeld critic (p. 202).

Part of this may be due to a noticeable skew in the slate of civilian leaders and military officers he interviewed. While it is possible that the few interviewees who remain anonymous provide more balance, it is striking that he interviewed none of the senior military officers responsible for Iraq war planning and policymaking – Generals Richard Myers, Peter Pace, and Tommy Franks – but repeatedly characterized their views through the eyes of two more junior officers who gained fame participating in the later partisan debate over the war known as the 'revolt of the retired generals': Maj. Gen. John Batiste and LTG Greg Newbold. Similarly, he interviewed very few advocates of the Iraq War (civilian or military) on the Bush team: not Douglas Feith, Scooter Libby (or anyone on Vice-President Cheney's staff), Donald Rumsfeld, or Paul Wolfowitz, to name just the most prominent. He did interview Stephen Hadley, then Deputy National Security Advisor, but relies on that interview primarily to dispose of rival explanations. While the potential skew is more evident in the Iraq case, there are some noteworthy omissions from the Clinton years: Madeleine Albright, Rand Beers, Sandy Berger, General Wesley Clark, Richard Clarke, General Hugh Shelton, and others.

To be fair, this critique itself needs to be heavily caveated. Recchia's interview list is impressive and a considerable empirical base on which to mount an argument. I am not suggesting he did not try to reach these other interview subjects and it would be wrong to fault a scholar for failing to interview former policymakers who refuse to be interviewed. But in these cases, scholars need to be especially attuned to their own biases and filters and the way that the empirical record they have access to might itself be

[3]Eric Nordlinger, *Isolationism Reconfigured* (Princeton: Princeton University Press 1995).

biased and skewed and then take steps to balance against that. It is not clear how hard Recchia leaned to balance against this skew.

The net result is that the Iraq case study has the somewhat imbalanced feel of a just so story. Advocates of the use of force are painted as incautious and insensitive to the costs of action. Opponents of the use of force are painted as carefully weighing all the pros and cons. None of this morality tale is essential to make the core causal argument Recchia is seeking to make. For instance, the basic model (and most of Recchia's hard evidence) would just as easily support the opposite 'just so' narrative that has overly cautious/timorous generals who have inadequately weighed the costs of doing nothing using their veto power to create the obstacle of insisting on an International Organization-sanctioned burden-sharing arrangement before supporting a military intervention advocated by civilian leaders who have carefully weighed the costs of action against the costs of inaction. It is telling that the charges of recklessness and over-optimism are levied only against the Iraq hawks and not the Haiti, Bosnia, and Kosovo hawks. And, of course, it is quite telling that in Recchia's account the doves are never called out as wrong.

More concerning still is the fact that Recchia's model captures only part of the prevailing military mindset on the use of force. While there are always prominent outliers that might play an outsized role in a given case, Recchia is on solid ground to rely on the findings of existing research that documents some general patterns in the way the military approach the use of force. Recchia is right that military officers tend to be cautious about initiating the use of force, particularly on missions that can be characterized as primarily humanitarian in objective. Similarly, he is right that the officers usually ask for 'exit strategies,' and almost always prize the 'clear objectives' those strategies seem to offer while fearing the 'mission creep' they believe the absence of an exit strategy invites. Moreover, he is right that the US military would prefer to hand off any post-war stabilization mission to some other force, say a blue-helmeted US peacekeeping force staffed by non-Americans. All of these push in the direction captured by Recchia's model: a preference for qualitative multilateralism.

However, the same body of empirical work has also shown that the military prefer two other desiderata that would seem, in theory, to cut against qualitative multilateralism: the military tend to prefer as much operational autonomy as possible in the actual execution of the military mission, and the military tend to prefer larger, more decisive force if the decision to use force has been made. Adding a formal UN dimension to the operation would complicate both of those operational goals, as shown in a variety of post-Cold War missions, particularly the Somalia and Kosovo operations. Perhaps including these omitted preferences will not change the overall argument. Perhaps the military weighs them all and nets out a grudging preference for qualitative multilateralism as the one most likely to maximize burden sharing, net-net. But it seems also possible that factoring in these other known desiderata would yield a somewhat different account: that reluctant generals demand hard-to-get qualitative multilateralism on a subset of cases even though it would compromise their desire for autonomy and decisive force not because they harbor optimistic hopes it will yield burden sharing (which painful historical experience has convinced them is unlikely anyway), but because it offers them their best shot at thwarting the mission at the outset, without resorting to overt political insubordination. I do not know whether this alternative explanation is more right than Recchia's, but I do know that he has not adequately rebutted it.

The unevenness of the argument in the empirical chapters may help explain why Recchia's concluding chapter, where he lays out some normative and policy prescriptive implications, is not as compelling as it might be. Based entirely on his reading of the decision to intervene in Iraq – and ignoring other crucial decisions with significant civil–military overtones, such as the Surge of troops in 2007 – Recchia comes down siding with the 'revisionists' who advocate that the military should aggressively push back against civilians who fail to heed their advice (p. 241). Similarly, since in Recchia's telling apparently only hawks experience cognitive pathologies of over-optimism, he does not address how wishful thinking led to repeated failures to act decisively to forestall the catastrophic civil war in Syria. For instance, it is plausible that President Obama issued his now-infamous 'Assad must go' red-line while simultaneously refusing to authorize significant support for the rebels seeking to accomplish that red-line because he was overly optimistic it would happen within a short window even without US material support.[4] Similarly, the warnings of hawks about how letting the civil war in Syria drag on would have multiple deleterious second and third order effects on US national interests seem prescient 5 years later as we struggle to deal with the rise of the Islamic State, the crisis in the EU caused by massive migration from the Middle East, and the erosion of US credibility across multiple regions. The picture of Obama decision-making on Syria is still murky, pending a Recchia-style empirical analysis, and I can not rule out the possibility that Recchia's bottom-line model will hold up well enough with this new case. But I do not think that is likely. Enough is known about cases Recchia did not examine to warrant a more caveated approach to policy prescriptions.

The foregoing has dwelt perhaps overlong on quibbles with the book. There is, in fact, much to like. Overall this is a worthy contribution to the new stream of mid-range civil–military relations theorizing. Recchia's

[4]Steve Mufson, '"Assad Must Go": Those Three Little Words are Huge Obstacle for Obama in Syria', *Washington Post*, 19 Oct. 2015.

analysis of how bureaucratic politics plays out in the civil–military context (pp. 34–62) is as good as anything written on the topic since Richard Betts' classic study. Moreover, his interviews clearly unearthed evidence that demonstrated the functioning of sub-components of his argument – for instance, how objections from senior officers essentially vetoed military options in Sudan during the Bush years – and one wishes he had the space and the research design to include more of that evidence, thus maximizing his already considerable contribution to the growing literature on how civil–military relations shape strategy.

The case on Haiti is an impressive interweaving of secondary and primary sources, including a broad range of original interviews. It is the best researched case and, not coincidentally, the case that best fits Recchia's argument. Indeed, it might even be the motivating case, since it is the one optimally situated to test the dynamics: the intervention with the least plausible threat/interest-based rationale, the intervention soonest after a military disaster (the ill-starred Ranger raid in Mogadishu) that empowered the military vis-à-vis their civilian counterparts, the intervention where a formal UN endorsement, while difficult to get, would not be impossible to get, and the intervention where the alternative explanations were inherently the weakest.

The Haiti case is as close to a slam-dunk for Recchia's argument as the empirical record is likely to offer up, and he slams it home impressively. The Bosnia case is also quite strong. The Kosovo case does not work quite as well. Oddly for a book about civil–military relations, General Clark's travails and bureaucratic political skullduggery is only cursorily addressed. And while *qualitative* multilateralism was achieved, it produced somewhat uneven burden sharing since the United States shouldered the Kosovo combat load at roughly comparable levels to what they carried later in Iraq – though, crucially for Recchia's argument, NATO shouldered much more of the Kosovo stabilization burden than they did in Iraq. Finally, for the three non-Iraq cases, Recchia quite convincingly shows that his preferred explanation is more plausible than two prominent alternatives – norm internalization and preventing negative issue linkage – and for Haiti he also convincingly rebuts a third alternative explanation, namely the possibility that the administration pursued multilateralism as a way of increasing public support.

While it is certainly the case that the Bush administration pursued more formal multilateralism prior to the invasion of Iraq than Recchia credits, he rightly observes that Bush did not make securing additional UNSC authority a prerequisite for action. Thus Iraq becomes a contrary case that Recchia has to explain, which he does primarily by blaming the silent generals – Myers, Pace, and Franks – who were in Reechia's view derelict in not forcing the multilateralism issue. Curiously, Recchia omits one crucial fact about the prewar planning for the Iraq invasion: that the Rumsfeld-Franks plan called for a rapid withdrawal coupled with and facilitated by a hand-off of the operation to the United Nations. There is no question that the Bush administration severely underestimated the costs and difficulty of the post-conflict stabilization mission, as Recchia (and everyone else who writes on the issue) duly observes. There is also no question that the Bush administration was overly optimistic about the role that Iraq's own security forces would play in the mission, as Recchia (but too few of the other critics) duly observes. But Plan A for Iraq *did* involve a substantial UN mission and this raises the obvious question – since they expected the UN to step up, why didn't the Administration lock that in before the conflict? Recchia offers one tantalizing quote from Kori Schake who suggests that the administration simply 'didn't have an extended stabilization period in mind' (p. 201). And this may explain it.

But there is an alternative explanation, one that is more parsimonious because it also explains other aspects of Iraq war policymaking: the Bush administration over-learned lessons of Afghanistan. The Administration had just successfully toppled the Taliban, using a jury-rigged light-footprint war plan, as contrary to the off-the-shelf existing war plan as was the eventual Iraq invasion plan, and in defiance of critics who had claimed the plan would fail.[5] And then, even though the Administration launched the war without first securing formal UN authorization and while eschewing NATO offers of assistance, once the Taliban was toppled the Administration was able to secure the necessary UN authorization to convert this to a formal multilateral peacekeeping operation, precisely the kind of qualitative multilateralism that is Recchia's focus. In other words, perhaps the generals understood all along that the plan was to rapidly turn over the operation to multilateral forces, as they understood was happening in Afghanistan, and they, along with Bush administration hawks, were over-optimistic about the success of that plan because it had worked better than expected in Afghanistan – defying the predictions of the same critics who were predicting problems in Iraq. Recchia hints at such an explanation in a brief paragraph in the concluding chapter (p. 246) but fails to explore it carefully or to see how it might provide an alternative explanation for the Iraq case.

The several critiques mentioned in the foregoing evaluation underscore a positive aspect of Recchia's project, and thus a fitting place to close out this section. At every turn, Recchia is making strong claims that are interesting and, at times, even provocative. They invite critical scrutiny and suggest fruitful lines of follow-up testing or exploration. The fact that I find fault with some of them should not obscure the deeper fact that Recchia has made an

[5]John Mearsheimer, 'Guns Won't Win the Afghan War', *New York Times*, 4 Nov. 2001.

important contribution to knowledge and, in particular, a vital addition to the current renaissance in civil–military relations.

How patterns of civilian control in authoritarian regimes affect military effectiveness

Caitlin Talmadge's *The Dictator's Army: Battlefield Effectiveness in Authoritarian Regimes* (Cornell 2015), is the tightest and most compelling of the three books chosen for review. It is at the same time both classic and innovative. Classically, it is squarely in the tradition of civil–military relations that examines the civil–military problematique of how to have a military that is strong enough to provide protection from external threats without itself becoming a threat to civilian rule. Talmadge hearkens back to one of the central claims of classical civil–military relations scholarship – that patterns of civil–military relations matter not just for what they mean for health of democratic political practices but also for effective policy. Innovatively, it answers an all-too-often-unheeded call to make patterns of civil–military relations the explanatory variable and other concepts of interest the dependent variable – and it does so looking not at democracies but at dictatorships, too often viewed narrowly through the lens of coups.

Talmadge advances an argument that lies at the cross-section of work from two other important scholars from an earlier wave – Stephen Biddle and Risa Brooks. Biddle argues that different patterns of military practice yield different levels of combat effectiveness.[6] Brooks argues that different patterns of civil–military relations yield different quality strategic assessments.[7] Talmadge brings these two arguments together to forge her own: different patterns of civil–military relations yield different levels of combat effectiveness.

Specifically, following Biddle, Talmadge says there is a generally accepted set of best practices to produce a military optimized for combat effectiveness in conventional war. This requires promotions based on merit; training that is rigorous, realistic and frequent; command that is decentralized, unified and clear; and information sharing that is active on both horizontal and vertical dimensions. The problem is that such a military could pose a threat to a leader, if that leader's hold on power was itself tenuous because it was based on personalistic authoritarianism. A regime that does not fear coups but does face external conventional threats will invest in such a military. But a regime that has reason to fear coups will have a strong incentive to make contrary choices: to select commanders on personal

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

[7] Risa Brooks, *Shaping Strategy: The Civil-Military Poltics of Strategic Assessment* (Princeton: Princeton University Press, 2008).

loyalty not merit; to restrict training; to have a centralized and convoluted command; and to restrict information sharing and to hobble the military with widespread counter-intelligence efforts within the ranks. Talmadge argues that these different practices yield different degrees of two critical components/determinants of combat effectiveness: tactical proficiency (the capacity to use weapons accurately) and competence in complex operations (the capacity to aggregate effectively from individual, to small-unit, to combined arms operations). She brackets off a third feature that is often considered essential: unit cohesion.

Talmadge is not the absolute first to make an argument of this sort. Biddle and Zirkle have made a similar argument and, of course, the downsides of coup-proofing techniques have long been a staple of civil–military relations.[8] But she is the first to subject the argument to a carefully designed and rigorously applied empirical test based on a close examination of the Vietnam War and the Iran–Iraq War.

She has proven to my satisfaction and probably to most others, that the deficiencies in combat performance by the South Vietnamese, as compared with the North Vietnamese, owes a great deal to the steps successive South Vietnamese leaders took to try (unsuccessfully, as it turned out) to ensure that their large and well-armed military would not pose a coup threat. The North Vietnamese, who did not need to worry as much about coup-proofing, could direct their military to optimize for the waging of a conventional war. Similarly, Iran's poor battlefield performance owes, at least in substantial part, to the deleterious effects of coup-proofing steps the regime took. Iraq's military similarly suffered early in the war until Saddam Hussein realized that he might have more to fear from military defeat and so allowed a portion of his force – the Republican Guard – to develop more conventionally optimal practices and thus develop greater battlefield effectiveness.

Of necessity, the case studies are just that – cases – rather than exhaustive analyses of combat effectiveness in the two wars. It might have been preferable for Talmadge to be more explicit about the research design that led to the selection of these battles for close examination (she does have a convincing research design explaining why she chose these wars).

The case studies are masterful examples of how to use military history effectively to inform deeper political science debates. Talmadge demonstrates a command of the battles and an even-handedness in dealing with ambiguous evidence. I am sure military historians will quibble with

[8]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. On coup proofing more generally, see: James T. Quinlivan, 'Coup-Proofing: Its Practice and Consequences in the Middle East', *International Security* 24/2 (1999) 131–65; Ulrich Pilster and Tobias Bohmelt, 'Coup-Proofing and Military Effectiveness in Interstate Wars, 1967–99', *Conflict Management and Peace Science* 28/4 (September 2011) 331–50; and Jonathan Powell, *Coups and Conflict: The Paradox of Coup-Proofing*, Ph.D. dissertation, University of Kentucky, 2012.

interpretations of this or that phase of any given battle, but then she offers effective quibbles of her own for why certain battle accounts have missed key aspects (see, for example, her critique of US Marine-centric accounts of the Hue battle during the Tet Offensive, pp. 94–6).

Several nuances in Talmadge's argument deserve to be emphasized. First, she is at pains to emphasize that her DV is combat effectiveness not war outcomes. Combat effectiveness is a potentially important contributor to war outcomes, but war outcomes might be heavily determined by other factors that are not part of the causal mechanism on which she is focusing. She is right about this, but it has profound implications for her research design. Her argument is harder to scale to the large-n analysis that other civil–military-related theories lend themselves to until we get better databases of combat effectiveness. Databases on war outcomes are well-established but databases of combat effectiveness are still very much works in progress.

Second, she carefully distinguishes between her focus on the *type* of civilian control and what other scholars have focused on, the *degree* of civilian control, specifically degree of civilian micro-management. She notes that you can have very different types of civilian control with the same extent of civilian micro-management: both Hitler and Hussein micro-managed and interfered in military operations to a roughly equivalent extent, but Hussein's was far more corrosive of battlefield combat effectiveness because he disposed of generals without regard to battlefield performance and greatly limited realistic training.

Third, she carefully considers alternative explanations and shows where they fall short or, more importantly, where they are better considered as complementary rather than alternative explanations. It is rare that we political scientists advance arguments in which we are right and everyone else is wrong. It is enough to do what Talmadge has done, show how even if other people are partly right she is still also making a useful contribution to the debate.

Where Talmadge is most vulnerable is likely on questions of scope. An ungenerous way of summarizing her argument is that Talmadge shows that efforts to inoculate their regimes against coups hurt the battlefield effectiveness of many ground units in South Vietnam (1960s), Iraq (1980s), and Iran (1980s). She does not prove that this problem afflicts other militaries at other times, nor that it would affect air and naval units or wars where air and naval forces played a more critical role. Nor does she prove whether this same factor explains the collapse of Iraqi forces in 1991 and 2003 (though she suggests it likely did) let alone in 2014 (though again she suggests it did). Nor does she weigh in one way or the other on whether the Iranian military today is still as ineffective as she judged it to be 30 years ago. As empirical findings go, that is not nothing, but clearly Talmadge aims to make a bigger contribution.

How big is open to debate. As her book title implies, her argument extends most readily to other dictatorships but perhaps not to advanced industrialized democracies. For instance, how much do civil–military factors explain variations in combat effectiveness in American units? And if such variation can be traced back to civil–military factors, are they through causal processes Talmadge identifies or others? Put another way, is the critique that is so popular among current generations of American military officers – namely, that battlefield effectiveness varies inversely with civilian micromanagement – correct and, if so, is that support for or against the Talmadge theory?

Nevertheless, I believe she has succeeded in that bigger contribution, namely demonstrating how one can deduce mid-range civil–military relations theory and then empirically test it where there is an abundant secondary sources literature. This shifts the civil–military lens far from the 'civilian control' focus and, hopefully, is a model and harbinger of more to come.

Conclusion

None of these books upends a major argument in the civil–military relations field, but collectively they, along with a flock of other projects, attest to the liveliness of the field. While each advances original theoretical arguments in greater or lesser measure, all of them are well-grounded in the empirical wing of the sub-field. But even more, all are grounded in the small-to-medium-n empirical wing of rich qualitative methods approaches.

None of the scholars took the next step, but two, and perhaps all three, of the arguments invite it: testing the insights against larger-n databases. Barany's approach to democratization is, I would argue, ready now for such testing. Talmadge's arguments will be as databases of combat effectiveness – vice, combat outcomes – are refined. Some observable implications of Recchia's work could be tested in this fashion if a case could be made that some version of the dynamics he has identified should have operated, in theory, during the Cold War. Of course, quantitative testing is not an end to itself, but, given the parallel renaissance in the quantitative study of civil–military relations, this might be a fruitful area of mutual leverage.

Importantly, all of the books speak to the community that most cares about civil–military relations: the policy community for whom the civil–military problematique is not an academic exercise but a daily practical challenge. At a time when the field laments the gap between the labors of academics and of policymakers, it is refreshing to read cutting edge scholarship operating comfortably at the intersection. Graduate students looking for research questions that will both utilize the hard-won tools of political science analysis and contribute to real-world concerns, should find all the inspiration they need in the current renaissance within the subfield of civil–military relations.

Disclosure statement

No potential conflict of interest was reported by the author.

Notes on contributor

Peter Feaver (Ph.D., Harvard, 1990) is a professor of political science and public policy at Duke University. He is Director of the Triangle Institute for Security Studies (TISS) and also Director of the Duke Program in American Grand Strategy (AGS). From June 2005 to July 2007, Feaver served as Special Advisor for Strategic Planning and Institutional Reform on the National Security Council Staff at the White House, where his responsibilities included the national security strategy, regional strategy reviews, and other political–military issues. Feaver is the author of *Armed Servants: Agency, Oversight, and Civil-Military Relations* (Harvard Press, 2003) and *Guarding the Guardians: Civilian Control of Nuclear Weapons in the United States* (Cornell University Press, 1992). He is co-author, with Christopher Gelpi, of *Choosing Your Battles: American Civil-Military Relations and the Use of Force* (Princeton University Press, 2004); and co-editor, with Richard H. Kohn, of *Soldiers and Civilians: The Civil-Military Gap and American National Security* (MIT Press, 2001).

Bibliography

Biddle, Stephen and Robert Zirkle, 'Technology, Civil-Military Relations, and Warfare in the Developing World', *Journal of Strategic Studies* 19/2 (June 1996) 171–212. 10.1080/01402399608437634

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

Biddle, Stephen, Ryan Baker, and Julia Macdonald, 'Small Footprint, Small Payoff: The Military Effectiveness of Security Force Assistance', unpublished manuscript, 14 February 2016.

Bremer, Paul, 'How I Didn't Dismantle Iraq's Army', *New York Times*, 6 September 2007.

Brooks, Risa, *Shaping Strategy: The Civil-Military Poltics of Strategic Assessment* (Princeton: Princeton University Press 2008).

Mearsheimer, John, 'Guns Won't Win the Afghan War', *New York Times*, 4 November 2001.

Nordlinger, Eric, *Isolationism Reconfigured* (Princeton: Princeton University Press 1995).

Pilster, Ulrich, and Tobias Bohmelt, 'Coup-Proofing and Military Effectiveness in Interstate Wars, 1967-99', *Conflict Management and Peace Science* 28/4 (September 2011) 331–50. doi:10.1177/0738894211413062

Powell, Jonathan, 'Coups and Conflict: The Paradox of Coup-Proofing', Ph. D. dissertation, University of Kentucky, 2012.

Quinlivan, James T., 'Coup-Proofing: Its Practice and Consequences in the Middle East', *International Security* 24/2 (1999) 131–65 doi:10.1162/016228899560202
Steve Mufson, ''Assad Must Go': Those Three Little Words are Huge Obstacle for Obama in Syria', *Washington Post*, 19 October 2015.