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Review Essay

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Review Essay

William Rose, Andrew Majkut, Michelle Strayer, and Christopher Chen

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M.L.R. Smith and David Martin Jones, *The Political Impossibility of Modern Counterinsurgency: Strategic Problems, Puzzles, and Paradoxes*. New York: Columbia University Press, 2015. 272 pp. \$50 (Hb). ISBN 978-0-231-17000-2

Austin Long, *The Soul of Armies: Counterinsurgency Doctrine and Military Culture in the US and UK*. Ithaca: Cornell University Press, 2016. 275 pp. \$30 (Pb). ISBN 978-1-5017-0319-5

Christopher Paul, Colin P. Clarke, Beth Grill, and Molly Dunigan, *Paths to Victory: Lessons from Modern Insurgencies*. Santa Monica: RAND, 2013. 328 pp. \$40 (Pb). ISBN 978-0-8330-8054-7

Just over half a century ago, David Galula wrote the seminal book on the ‘population-centric’ approach to counterinsurgency (COIN): *Counterinsurgency Warfare: Theory and Practice* (1964). Four decades later, in the wake of an uncontrollable insurgency following the US-led invasion of Iraq, a flurry of manuscripts in this tradition appeared. A small number of books published between 2002 and 2010 stand out for their systematic, general analyses on the topic.

Initiating this wave of scholarship was John Nagl’s *Counterinsurgency Lessons from Malaya and Vietnam: Learning to Eat Soup with a Knife* (2002). More widely read is his paperback version that reversed the order of title and subtitle (2005). Nagl elaborated on the population-centric approach and how and why its presence or absence affects prospects for success. His theoretical contribution is the thesis that to succeed in a COIN campaign, a military institution must be a ‘learning organization’ able to adapt to circumstances on the ground. Furthermore, he argues, military culture is a major determinate of an institution’s ability to adapt. Nagl, a US Army Lt. Colonel, joined US Army General David Petraeus, Marine Corps Lt. General James Amos, and other senior military officers to produce *Counterinsurgency*, US Army Field Manual [FM]3-24 / Marine Corps War Fighting Publication 3-33.5 (2006). In the University of Chicago Press edition, Nagl’s Forward said that of the many books that were influential in writing the new manual, Galula’s was most important (2007, p. xix). David Kilcullen, also writing in the population-centric tradition, wrote *The Accidental Guerrilla* (2009) and published a compilation of most of his other writings in *Counterinsurgency* (2010).

Over the past half dozen years, few books have come out that provide new general analyses of when, how, and why counterinsurgency can succeed. More common have been critiques that focus on problematic counterinsurgency efforts in contemporary Iraq or Afghanistan. Several general critiques of COIN have also been published. In this latter tradition our essay reviews the book by M.L.R. Smith and David Martin Jones (2015), a volume in the

series Columbia Studies in Terrorism and Irregular Warfare. The title and subtitle of the book indicate their primary purpose, to prove *The Political Impossibility of Modern Counterinsurgency* by highlighting *Strategic Problems, Puzzles, and Paradoxes*. The authors challenge the utility of disaggregating the concept of war to include subcategories such as unconventional war, guerrilla war, and insurgency. Missing from COIN discourse, they argue, is attention to strategy as conceived by Carl von Clausewitz: the conscious linkage between a state's major political goals and appropriate means to accomplish them.

The authors of the second and third books reviewed do not aim to prove a point, but rather to discover new insights about the uses and limits of various approaches to counterinsurgency.

The title of Austin Long's book – *The Soul of Armies: Counterinsurgency Doctrine and Military Culture in the US and UK* – references the centrality of military culture for explaining important observations about counterinsurgency. Long describes and explains the sources and consequences of different military cultures, particularly archetypes of continental and maritime armies. An original finding is that sometimes military doctrine does not guide counterinsurgency operations, and the cultures of particular organizations explain their actual behavior. He also finds that while being a learning institution increases prospects for successful counterinsurgency, it does not guarantee it. The book is part of the series Cornell Studies in Security Affairs.

The third book is authored by Christopher Paul et al. (2013a). It differs from most other books on counterinsurgency, including the other two reviewed here, because the authors do not conflate a specific operational method or approach to local governance with the concept of counterinsurgency. The plural nature of 'paths' in *Paths to Victory* conveys that successful approaches to countering an insurgency exist along a spectrum from population-centric to enemy-centric. The subtitle, *Lessons from Modern Insurgencies*, alludes to the two companion volumes that contain detailed case studies of all 71 insurgencies begun and completed from 1944-2010; the cases are summarized in the main book. Paul et al. develop 24 diverse hypotheses about practices and circumstances that might favor a successful campaign, both from the scholarly literature and from practitioners. The hypotheses are tested against the case studies, producing findings about correlations of successful COIN campaigns. The book is published by the RAND Corporation.

Unlike books from the previous decade that analyze and promote COIN, these three recent books are explicit about limitations of all approaches to counterinsurgency. They vary considerably on their degrees of pessimism, diagnoses of problems, and implications for theory and policy. This review essay starts with the pessimistic view offered by Smith and Jones. It then explores Long's book and its focus on the sources and consequences of military cultures. Finally, the more general and more complex analysis by Paul et al. is analyzed. The concluding section explores promising connections among the three works.

COIN AS A FLAWED IDEA

M. L. R. Smith is Chair in Strategic Theory in the Department of War Studies at Kings College, University of London. David Martin Jones is associate professor in the School of Political Science and International Studies at the University of Queensland, Brisbane. In their book's introduction they state their purpose of analysis: '...Few studies have asked the simple but puzzling question: What precisely is COIN? Is COIN a strategy, a doctrine, a theory, a military practice, or something else? In this respect, this analysis presents COIN as a set of intellectual puzzles that require both unpacking and skeptical deconstruction. This is the principal objective of this volume: to uncover the hidden thinking that obscures the contemporary understanding of COIN' (p. xv). The introduction includes their statement of 'strategic theory', the one theory they believe has utility for properly understanding the subject matter. Unlike the contemporary narrative surrounding COIN, which Smith and Jones argue has an apolitical emphasis on techniques learned from post-World War II anti-colonial struggles, strategic theory is explicitly attentive to politics – both the local politics of a conflict and especially the political interests of states contemplating intervening in such conflicts.

Controversial Concepts

Addressing the conceptual underpinnings of counterinsurgency first requires an exploration of the concept of insurgency. Smith and Jones begin chapter 1 with a definition drawn from FM3-24: insurgencies are 'organized movement[s]' that seek to overthrow a 'constituted government through the use of subversion and armed conflict' (2007 cited Smith and Jones 2015, p. 1).

They assert that seeking any definition of 'insurgency', or any other subcategory of war, is self-defeating. Doing so suggests 'a type of war imbued with its own distinctive character and practices', one that involves 'the violent contestation for civil control' (p. 3). This term and its companion 'guerrilla war' are seen as forms of 'asymmetric warfare' involving a weaker side confronting a more powerful actor. This assumption leads to the view that insurgency is practiced by nonstate actors employing guerrilla methods in conditions of civil war.

The authors argue that such thinking is flawed. In particular, distinguishing 'insurgency' as a category of war leads to five problems: exceptionalizing war (e.g., believing that an 'unconventional war' does not require attention to conventional warfare); depreciation of the philosophy of war (e.g., believing that actors are motivated by primordial urges and not rational political calculations); the decontextualization of war (e.g., the tendency to focus only on insurgent tactics and not the broader Clausewitzian concepts of passion, chance, and reason); overprescription in war (that follows from overoptimism that a set of techniques can prevail against all or at least most insurgencies); and destrategization of war (as a predetermined COIN response removes strategic judgment).

Smith and Jones consider important issues. They make a good case for many of these supposed consequences of thinking solely in terms of particular subcategories of war. Sometimes they overstate their case, however, leading the reader to conclude that it is never useful to give credence to subcategories. On the 'exceptionalizing war' thesis, for example, we agree it makes sense to understand that during war strategies and tactics may vary over time or space. However, we believe that having subcategories of war is useful analytically and practically; it helps both to know what you are dealing with and to recognize potential shifts in forms of warfare.

Chapter 2 turns to the concept of counterinsurgency, asking if COIN is a strategy, a military doctrine, or a series of techniques. According to FM3-24, it is the ‘military, paramilitary, political, economic, psychological, and civic actions taken by a government to defeat an insurgency (2007 cited in Smith and Jones 2015, p. 33). Smith and Jones warn that the field manual ‘seems to maintain that counterinsurgency is a universal strategy...that can be applied across time and space, regardless of contingent factors’ (p. 33). They assert that counterinsurgency does not fit their definition of ‘strategy’ or its necessary link between the ends and means of policy. Rarely asked are the central questions: ‘For what is one fighting, and is that object likely to be worth the fight?’ (p. 34). These questions highlight a major contribution of the book, the only one of the three to give extensive credence to issues of ‘grand strategy’; we return to this theme below.

Might COIN be a doctrine? The authors assert, ‘Doctrine seeks to develop a set of agreed-upon methods by which the armed forces will conduct their operations and establish a common language for doing so. In this manner, it seeks to render military action predictable’ (p. 35). Perhaps COIN is a doctrine, but Smith and Jones argue that doctrines are problematic because they assume uniformity of situations and proper responses – ignoring that ‘all wars are exceptional in their origins, shape, and direction’.

The association of COIN with strategy and doctrine is explained by the so-called ‘technocratic’ writings of practitioner-scholars starting with Galula (1964) and extending into the rest of the decade initiated by the 9/11 attacks. Smith and Jones are highly critical of the new wave of scholarship on COIN, the ‘signpost text’ of which is Nagl’s book (2015, p. 188, n.8). With Nagl’s attention to only two case studies, the successful British experience in Malaya and the failed US experience in Vietnam, Smith and Jones claim that Nagl and like-minded scholars construct a faulty narrative – about how British military culture favored small wars and the flexibility that came with the experiences, which led to a successful ‘hearts and minds’ COIN campaign in Malaya (p. 39). They claim that Nagl and his cohorts ignore critical aspects of the British campaign, such as the conditions of colonial governance that provided more leverage to get results.

In order to assess the validity of such criticisms, we reread Nagl’s book. One puzzle that we haven’t solved is determining which edition of Nagl’s book Smith and Jones used. Their bibliography indicates the 2002 version, but uses the title and subtitle in the order not published until 2005 (i.e., *Learning to Eat Soup with a Knife: Counterinsurgency Lessons from Malaya and Vietnam*). Regardless, Smith and Jones’s first accusation is that Nagl ignores crucial aspects of the British campaign. Our reading of Nagl suggests otherwise. For example, the 2005 version includes an addition, ‘Preface to the Paperback Edition: Spilling Soup on Myself’ (pp. xi-xvii). Nagl writes,

...I hope to point out omissions and missteps from that first edition, written before my own physical immersion in counterinsurgency...’ (p. xi). ...In the following pages, I note that the British did a better job gaining the trust of the Malay population, but I didn’t properly emphasize that when the insurgency began they had been in the country for well over a century, developing long-term relationships and cultural awareness that bore fruit in actionable intelligence’ (p. xiii). ...The task was also made much easier by the ethnic

composition of the insurgency and by Malaya's geography; external support is almost always a prerequisite for a successful insurgency (p. xvi, n.1).

Moreover, in the introduction to both the 2002 and 2005 versions of the book, Nagl describes what he does with some humility – a perspective missed by Smith and Jones. ‘This book does not attempt to provide the definitive answer as to why the United States “lost” the war in Vietnam, nor why the British “won” in Malaya – although the conclusions it will draw about the organizational culture and learning abilities of the two armies certainly demonstrate some of the reasons for the differing results of the two conflicts’ (2005, p. xxii). He adds, ‘The organizational learning approach has not previously been applied to explain cases of military adaptation during the course of a conflict’ (p. xxiii). Smith and Jones's second accusation is that Nagl pays little or no attention to the role of coercion in the British campaign. Yet he is attentive to coercive measures; see his quotation by R. J. W. Craig (2005, p. 98). Thus Smith and Jones overstate their case and do not give a fair account of important parts of Nagl's book. This situation detracts from the objectivity of their critique.

As Smith and Jones continue their discussion of doctrine, they note that COIN ideas have contributed to doctrine but ‘must nevertheless be subordinated to political considerations’ (p. 46). Prudential application of judgment, rather than simply following the dogma of the COIN, is always necessary. This makes sense.

Smith and Jones conclude the chapter with discussions of what COIN is, if it is not a strategy or a useful doctrine. First is ‘that COIN is a narrative. It is a story about triumph over adversity’ (p. 49). With the right generals and the right doctrine, the story goes, success is likely. This mythical narrative can also help garner popular support for foreign interventions (p. 51); this latter idea gains elaboration in the fourth function, COIN as political spin (pp. 53-54). Second is COIN as the subtext that modern armies must be flexible. Here the authors reference Nagl's thesis that success is more likely when a military institution is flexible and adaptable (pp. 51-52). They do not criticize Nagl's idea, but rather his use of the Malayan case study as its source. Third, and directly related to the second function, is COIN as a statement of the obvious: ‘It is self-evident that any military organization should be subtle, learn from mistakes, and jettison old ways of doing things as the unique circumstances of war demand’ (p. 52). Referring to these and related ideas, they add that ‘[s]uch advice applies to all wars. In the sense that this advice outlines nostrums of positive practice, it offers rules of conduct that any professional armed forces would adopt, regardless of context’ (p. 53). However, this third point has a serious problem: the authors state it as a fact rather than a hypothesis. They assert that all militaries would act appropriately if the situation called for a certain approach. Yet only some military organizations act that way in practice, as Nagl claims and as we'll see in Long's book.

Chapter 3 is ‘Counterinsurgency and the Ideology of Modernization’. The authors have a reasonably full and clear summary of the comparative politics literature and ideas associated with modernization theory as well as the US foreign policy tendency toward exceptionalism. They argue that COIN ‘has an ideological dimension imbued with a distinctly American liberal philosophical and political self-understanding’. In effect, a ‘cult of democratic modernization’ informs US understandings of COIN (p. 57). To illustrate these ideas they quote FM3-24: ‘Success in counterinsurgency...requires establishing legitimate government supported by the people and able to address fundamental causes that insurgents use to gain support’. Smith and

Jones continue, saying that ‘the Field Manual again avers that the “primary aim of any COIN operation” is to foster “legitimate government”’ (2007 in Smith and Jones 2015, p. 57).

Yes, the idea of ‘legitimate government’ is an important part of this field manual. Unfortunately, problems arise that question their reading of the primary source. The authors wrongly suggest that the manual associates ‘legitimate government’ only with ‘democracy’ or ‘democratic modernization’. The concept ‘democracy’ appears only in Appendix B as part of the name given to ‘Operation Uphold Democracy’ in Haiti in 1994 (2007, p. 314). In a related vein, they miss the field manual’s broader Weberian discussion of the basic concept of ‘legitimacy’ (2007, p. 37, para. 1-114). The manual discusses not only the western liberal tradition (which is associated with democracy) but also other forms of governance that are sometimes considered legitimate by the bulk of a society. Thus the field manual leaves room for other forms of legitimacy that might better work in a particular local context.

Smith and Jones also discuss the concept of ‘center of gravity’. ‘COIN assumes that the population is the “center of gravity”. Counterinsurgent forces must win the population’s loyalty, which is the key to defeating insurgents, and this imperative must drive all operations. As the field manual makes clear, “At its core, COIN is a struggle for the population’s support. The protection, welfare, and support of the people are vital to success”’ (2007 cited in Smith and Jones 2015, p. 86). However, a close reading of FM3-24 reveals only one direct and relevant discussion of ‘center of gravity’: ‘...[T]he ability to generate and sustain popular support, or at least acquiescence and tolerance, often has the greatest impact on the insurgency’s long-term effectiveness. This ability is usually the insurgency’s center of gravity’ (2007, p. 101, para. 3-76). Notice that the field manual does not say the population is *the* center of gravity (as Smith and Jones assert); the manual states only that the population is *usually* the center. Even more significant is the revised field manual, published the year prior to Smith and Jones’ book. Although the new manual continues to explore subcategories of war, it conveys an image that from Smith and Jones’ perspective is surprising: ‘...[T]he population is not necessarily the center of gravity for an insurgent. A center of gravity could be external support from another country, it could be a group of core leadership or believers, or it could be a host of other factors or vital functions. Center of gravity analysis begins with the understanding that every environment is unique, and a center of gravity analysis must not begin with a preconceived center of gravity’ (2014, p. 7-6, para. 7-21).

Chapter 4 demonstrates that the authors are widely read in differing views and attitudes toward both classical insurgency and what David Kilcullen terms post-classical insurgency and counterinsurgency (2015, p. 97). As in other chapters, their purpose is to persuade the reader of the wisdom of their views and assessments without raising hypotheses to be tested, evaluated, and perhaps refined. Their effort in this chapter is largely informed but is also provocative, with the weakest section toward the end where the authors posit policy recommendations. Their solution ‘...requires governments operating multinational coalitions to deal with jihadism both through external interventions (such as the removal of the Taliban regime in Afghanistan in late 2001) and through domestic, police-level interventions’. They continue: ‘...In the case of a number of European states such as the United Kingdom, which has possessed acute problems in integrating second-generation Muslims into its multicultural society, this challenge requires reasserting political sovereignty, securing state borders, and elaborating an inclusive national identity as part of a shared public morality and counterideology’ (2015, p. 113).

Smith and Jones may be right, but they present no significant case study evidence showing that such policies would work or even stand a chance of being adopted by a state like the UK. Instead, they forward prescriptions for what should be done: ‘Democratic governments must persuade the majority of the validity of the struggle. This cannot be achieved by conceding important points of principle in foreign policy or compromising political values at home to appease vocal but intolerant minorities under the aegis of grievance settling’ (2015, p. 116). These policy recommendations are provocative because they are controversial, and unsatisfying because the authors provide insufficient historical evidence to demonstrate they are both desirable and feasible. In effect, the authors don’t clearly answer their own series of questions about a country’s core national interests; see the first offset quotation below (2015, pp. 33-34).

Grand Strategy and Political Will

Two themes dominate Chapter 5: the claim of a lack of British traditions, and the centrality of ‘grand strategy’ to a state’s engagement in expeditionary counterinsurgency operations. We give priority to the second major theme of this chapter: how ‘success’ in a military campaign depends less on operational progress with a set of COIN techniques and more on larger calculations of the state’s core national interests.

The authors complain that discussions of counterinsurgency tend to focus on a series of techniques and related measures of progress in the field. ‘Nowhere in these works on COIN do they expound why forces should be confronting insurgencies. ... These questions are not raised or answered, it would seem, because they involve or require political judgment’ (p. 34). They mean, of course, political judgment about strategy, the link between a state’s core national interests and the means to achieve them. Thus the authors wisely posit that

...strategy requires answers to the following existential questions: What principles, values, and outcomes are being sought? How can the means available help attain desired outcomes? How will it be known when those outcomes have been achieved? How can those outcomes be achieved or maximized, at what proportionate cost and without causing further problems later? Ultimately, strategy in war can be distilled into two simple questions: For what is one fighting, and is that object likely to be worth the fight? (2015, pp. 33-34, notes omitted here and elsewhere in this discussion).

Returning to the chapter’s discussion of British experiences, Smith and Jones connect strategy to the importance of a state’s political will to persevere in a COIN campaign.

When examined in historical perspective, the British ‘COIN legacy’ reveals that what ultimately determined military success or failure in any theater was *political will* – that is to say, the political investment in the cause that determined whether the British state stayed the course. Political will provided the time for the learning/adaptation process to occur in each contingent setting. It is this variable that holds the key to understanding the British military experience in so-called small wars (pp. 140-141, italics in original). ...Further, if there is any wisdom contained in the British experience of contending with small wars, perhaps knowing when not to fight – in other words, when to withdraw – is as important as committing to stay over the long term (p. 145).

These are important ideas too often absent from discussions of counterinsurgency. They direct our attention to two questions. First, what is the most useful way to think about the level of success of a military campaign? Standard approaches seek to determine the success of accomplishing operational tasks in the process of achieving certain military and local political outcomes. Smith and Jones point to another measure of success, the extent that a campaign (or its absence) serves the state's national interests. We sense that an either/or choice between these two approaches would be unsatisfying. It is also not obvious whether the optimal approach would be to ask about both types of success or try to develop a hybrid measure that somehow considers both. This question would benefit from further deliberation among scholars and practitioners. Second, if a state's political will is as important as Smith and Jones claim, is it not only a necessary condition for success at the local level but also a sufficient condition?

COIN DOCTRINE VS. COIN OPERATIONS

At Columbia University, Austin Long is an assistant professor at the School of International and Public Affairs. In the preface to his book, *The Soul of Armies*, he states his purpose of analysis: '...this is a book about why military organizations are the way they are, and how that affects the way in which they conduct counterinsurgency (2016, p. viii). The book's subtitle is *Counterinsurgency Doctrine and Military Culture in the US and UK*. Accordingly, his work is a continuation of efforts to understand sources and consequences of organizational behavior.

Hypotheses and Theory

Since the 1980s several hypotheses have been forwarded on the origins of military doctrine, patterns of operations, and sources of change to doctrine and operations. Long discusses these ideas in a literature survey, leading to four hypotheses.

Hypothesis One assumes organizational rationality in response to the demands of COIN, especially if a current approach is not working. Operations are expected to conform to new doctrine as it is formulated. This view coincides with a claim that Smith and Jones make about the true nature of counterinsurgency: '...COIN is a statement of the obvious. It is self-evident that any military organization should be subtle, learn from mistakes, and jettison old ways of doing things as the unique circumstances of war demand' (2015, p. 52). Hypothesis Two originates from Barry Posen's understanding of doctrinal change, which he posits comes from civilian leaders who are more attuned to larger questions of balance of power and grand strategy (1984). Civilian intervention should impact all military organizations in the same state, thus producing national convergence in doctrines and operations. Hypothesis Three reflects a generic analysis of military institutions: all organizations seek to maximize their resources, status, and autonomy or to minimize uncertainties relevant in planning. Agreement on how to secure these benefits should produce convergence in doctrine and operations both within and between states engaged in COIN campaigns. Hypothesis Four focuses on the cultures of military organizations and the condition under which culture is especially important for explaining doctrine and operations. Organizations are assumed to develop cultures based on their foundational experience in combat and in their professionalization. These experiences give the organization

certain beliefs about the nature of war and military organizations, shaping how members of the organization interpret a contemporary environment and decide how to respond to threats and opportunities. The fourth hypothesis is Long's contribution to the discussion, and he devotes chapter 2 to stating his theory. He distinguishes what organizations implicitly believe (culture) from what they are supposed to do (doctrine) and actually do (operations). Doctrine changes over time in every military organization; culture does not.

The 'first war' is a critical formative experience for military organizations, although it may not always be a single war. This is a major conflict that the organization takes as its template for developing professional education. In a profession, knowledge is gained through formal education in a body of abstract theory and specialized skills; a code of conduct is also part of professionalization. These features contribute to an integrated organizational culture. Long proposes that military organizations have different cultures as the result of different formative experiences (pp. 24-25).

Based on a state's (and therefore its army's) geostrategic position and domestic society, there are only a few archetypical first war experiences (pp. 25-27). The two most prominent 'ways of war' are reflected in a continental army and a maritime army; each has distinct preferences for the size of units conducting operations, the level and targeting of firepower, and the level of integration with other organizations or groups (typically other militaries or civilians).

The ideal type of a continental army arose over the nineteenth century when a new form of warfare emerged from the combination of the industrial revolution and intense nationalism. This type of war has various names, such as 'total war' or 'national-industrial war'. It was so influential that it became the military format for most professional armies that had neighbors that could possibly invade and conquer them, or that they might wish to conquer. The war could be interstate (as in Europe) or intrastate (the Russian and American civil wars). This archetype tends toward large-unit sweeps, heavy use of firepower with relatively indiscriminate targeting, and little cooperation with civilians.

The geopolitics of a maritime army is a country surrounded by an ocean or oceans, with no other great powers on the border, and a strong navy that provides the main defense against invasion. The country may have a need for an army to conduct missions abroad, however: imperial policing or contribution to coalition warfare on the continent. Offshore balancing is a strong option for preventing the rise of a hegemon on the continent. Imperial policing is frequent but, with a low threat to national existence, only minimal sacrifice may be demanded. The manpower for this mission tends to be volunteers of relatively high quality, a situation that gives more confidence in the capabilities of small units while putting the emphasis on small-unit leadership. Firepower is used discriminately. Finally, the empire is typically not managed by the military alone, so civilian resources are often available.

For professional organizations, the key mechanism of transmission is the formal professional education system. For military organizations, the professional schools and not the service academies are central to professionalism. For example, a Marine officer commissioned directly from the Naval Academy must still attend the Basic School for new officers; the Basic School is the center of culture (p. 30).

Culture is best used to explain organizational behavior at the aggregate level, with attention to what can be expected of most units of the organization most of the time. How much does culture matter? ‘The effect of culture, a filter for reducing ambiguity, will be strongest when information is most ambiguous. Highly ambiguous environments include peacetime or counterinsurgency. Culture will be weakest when information is least ambiguous, such as four years into a major war’ (p. 34).

Chapters 3-5 focus on the professionalization of the US Army (and US Army Special Forces), the US Marine Corps, and the British Army. Long shows how the US Army adopted the continental army ‘way of war’, while the other services more closely resemble a maritime army.

Case Studies and Principal Findings

Chapters 6 and 7 are case studies of the United States in Vietnam; chapter 8 is the British Army in Kenya; chapter 9 is the more recent case of Iraq, in which both US and UK forces participated; and chapter 10 is post-9/11 Afghanistan that also features US and UK forces. These chapters are theory-informed with sound and sometimes innovative treatment of the historical record.

The book addresses two related puzzles of COIN doctrine. ‘First, why do military organizations, despite agreement on the theoretical outlines of counterinsurgency doctrine, continue to diverge in terms of operations? Second, why does this variation in the conduct of operations not yield any reliable positive results?’ (2016, p. 222).

Starting with the first puzzle, Long finds that sometimes counterinsurgency doctrine does not correspond to actual operations. The previous conventional wisdom was an expected congruence between doctrine and operations, as Posen (1984) indicated for interstate relations and Nagl (2002, 2005) for counterinsurgency. In the four case studies, congruence for counterinsurgency held almost all the time for the US Marine Corps, US Special Forces, and the British Army. With the new US Army FM3-24 (2006) and new top leadership sympathetic to that approach, the expectation was that the US Army would conduct operations accordingly most of the time. In practice, operations shifted substantially only in the area centering on that new leadership. Army operations elsewhere, covering a larger area, retained their old ‘continental army’ approach that emphasized large-unit sweeps, heavy use of firepower, and priority to attacking insurgents and suspected insurgents over protecting the population. The military culture hypothesis most strongly explains these observations. (pp. 197-198, 222). The three competing hypotheses are either indeterminate or unable to explain observed variations in operations.

Military culture also best explains the very different approaches the Marine Corps and Army took toward working with indigenous Sunni forces in Iraq (pp. 198-201). This story is worth reading because in the future the United States is more likely to support a COIN campaign indirectly rather than directly with its own ground forces.

Long then turns to the second puzzle: ‘Why do none of the variations in operations seem to reliably lead to success?’ He adds, ‘This puzzle is one that I did not initially anticipate. In the beginning the [of the] research that resulted in this book, I believed that some organizational

cultures were better than others for counterinsurgency...[and thus] I expected to find the US Army failing while the others succeeded' (p. 223). His acknowledgement of failure in Iraq by the US Marine Corps, US Special Forces, and the British Army shows sound, objective scholarship on the author's part. His four cases demonstrate that military organizations of the maritime army ideal type are more successful than their continental army counterparts, but that acting in the maritime army tradition does not guarantee success.

Long's findings have implications for theory. The case studies demonstrate that organizational culture best explains observations about a military institution's doctrine and operations, especially when the information environment is ambiguous as with counterinsurgency. It does so better than competing hypotheses. Culture therefore has predictive utility during periods of extended peace or limited war including counterinsurgency (p. 223).

Concerning predictions about convergence in doctrine and operations, Long found that with lower intensity operations, the only convergences were similar approaches being implemented by military organizations with similar cultures. Thus in the aggregate, continental armies have very different approaches to unit size, use of firepower, and extent of cooperation with local actors than maritime armies. In an unambiguous environment, including high-intensity conventional warfare, both types of military organizations tended to adopt similar approaches to operations. While the US Army was already acculturated to conventional war, the Marine Corps shifted operations when the conflict turned conventional (although they tended to retain small-unit operations).

A final implication for theory arises from Long's finding that even if a military organization follows organizational 'best practices', success is not guaranteed. In general terms, he notes that 'success has much more to do with local conditions than with military doctrine and operations. The principal policy lesson from both historical and contemporary counterinsurgency is that the agency of outsiders is limited' (pp. 3-4).

The first implication for policy is that if a government aims to support or engage in a counterinsurgency operation, its leaders should understand the limitations of deploying continental armies in environments that do not match their cultures. Most importantly, the ambiguous environment of counterinsurgency is a huge mismatch. Continental armies tend to interpret the environment in ways that are often problematic, focusing on large units and purely military operations to the exclusion of other approaches such as cooperation with locals and civilian agencies. Maritime armies are much better suited for most COIN operations. Their interpretation of an ambiguous environment is more likely to highlight the importance of small-unit operations, relatively limited firepower, and cooperation with locals and civilians. Concretely for the United States, it would do better to rely on the Marine Corps or Army Special Forces than the regular Army—both for direct expeditionary COIN and for indirect support for others engaged in COIN; Long does not forward this implication explicitly, but it does follow.

A second policy implication is understanding that success is not guaranteed even for maritime armies. When the British Army did well in Kenya, it had many effective local and civilian partners. The British faced the opposite situation in Iraq; despite their Army's 'best practices', it had no significant local allies. The US Marine Corps had good local allies in Iraq, but the scale was too small for such a large country. A corollary of this policy implication is that

success would be more likely with a confluence of important conditions: a military organization implementing COIN that had a maritime army culture, and the presence of effective local allies along with civilians with extensive local knowledge. Long's Afghanistan case study suggests a precondition for getting good local allies: unlike in Afghanistan, the local government cannot be unpopular and intransigent.

Judging the book based on its own purpose of analysis, to learn why organizations are the way they are, we find no significant weaknesses. There are some minor editorial problems, but they do not detract from the work's contributions to the literature. Of course, any research project will have limitations. An obvious one is that Long had only four case studies. Yes, his cases tell us a lot about sources of organizational culture and how and why the cultures of continental and maritime archetypes are so different. They tell us little, however, about other factors that may be important for explaining the outcomes of counterinsurgency campaigns.

COIN AS DIVERSE SET OF PRACTICES AND CIRCUMSTANCES

Paths to Victory: Lessons from Modern Insurgencies is written by a team of scholars at the RAND Corporation: Christopher Paul, Colin P. Clarke, Beth Grill, and Molly Dunigan. These authors, unlike others writing about COIN over the past dozen years, do not conflate a specific operational method or approach to governance with the concept of counterinsurgency. Paul et al. define it 'as efforts undertaken by a government and its security forces (or the security forces of supporting partners or allies) to oppose an insurgency' (2013, p. 2). The plural nature of 'paths' in *Paths to Victory* conveys that successful approaches to countering an insurgency exist on a spectrum from enemy-centric to population-centric. The subtitle, *Lessons from Modern Insurgencies*, alludes to the two companion volumes that contain case studies of all 71 insurgencies begun and completed between 1944 and 2010; these cases are summarized in the main book. The authors use a diverse set of 24 hypotheses on correlates of successful COIN, what they call 'concepts', that are drawn from scholars and practitioners who prefer a variety of military and political approaches. The concepts – which involve agency by COIN forces, by a government beset by an insurgency, or both – are tested against the case studies. The research leads to findings about a small number of concepts that, if achieved, can explain all of the successful historical case studies.

This book is definitely an improvement over the first iteration, *Victory Has a Thousand Fathers: Sources of Success in Counterinsurgency* (Paul et al., 2010a). The changes and added value are summarized in *Paths to Victory* (2013a, pp. xviii, 2, and 235-249),

To ease the reader into the deep analysis in the body of the new book, a summary introduces ideas that will be developed, key findings, and policy recommendations (2013, pp. xvii-xxxv). Chapter 1 discusses the purpose of the study, defines key terms, and summarizes research methods. Chapter 2 focuses on the case studies, explaining how they were selected and how case outcomes are coded as COIN wins or losses. It also offers short summaries of the 71 cases. The full case studies are available in two other books (Paul et al., 2010b and 2013b).

Chapter 3 identifies subcategories of case studies. Because Paul et al. sought policy-relevant findings, they culled the collection of 71 case studies down to the 59 most likely to resemble present and future insurgencies. Removed, but still available for comparative analysis, are cases that unfolded in a particular anti-colonial historical context. Case studies are also distinguished by the primary COIN path used, iron fist vs. motive-focused (which parallel enemy-centric vs. population-centric approaches).

Chapter 4 is a long and complex discussion of the 24 hypotheses, how they were tested, and findings from the tests. We first summarize the findings, particularly the discovery of three ‘prime implicants’ – the three out of 24 concepts that are all necessary and together are sufficient for explaining successful COIN. Attention also goes to a broader collection of factors that receives evidentiary support; it leads to their ‘counterinsurgency scorecard’. Second, we compare iron fist vs. motive-focused approaches to COIN. This discussion leads to a major theoretical contribution of *Paths to Victory*, a reconceptualization of approaches to counterinsurgency that goes well beyond the enemy- and population-centric dichotomy.

Conditions Favoring Success

The major finding of the study is that three of the 24 core counterinsurgency concepts are central to all COIN wins: tangible support reduction, commitment and motivation, and flexibility and adaptability. These are the ‘prime implicants’ that are most strongly supported by case study evidence. For a COIN win, all three concepts are necessary and together they are sufficient. These generalizations apply to both the iron fist and motive-focused approaches (2013, pp. 149-150).

Tangible Support Reduction is the ability of a COIN campaign to decrease the insurgent’s access to resources that allow it to function (e.g., recruits, weapons and material, funding, intelligence, and sanctuary). While in many cases this condition is correlated with backing from the population, sometimes insurgents gain support instead through trade in illegal commodities (e.g., narcotics, ‘blood diamonds’, or rare animal poaching), ransom from kidnappings, or external funding and volunteers. *Commitment and Motivation* refers to the extent to which the local government and COIN forces demonstrated that they were actually committed to defeating the insurgency. *Flexibility and Adaptability* refers to how well, or poorly, the counterinsurgency is able to acknowledge and change tactics or strategy based on shifts by the insurgency.

These findings are offered at a high level of abstraction. They reflect accomplishments, not attempts. The research program highlights the factors that, when present, have made successful COIN operations more likely. It is not intended to be a tool for offering specific policy prescriptions. Local context – the distinctive features of individual insurgencies – has a great deal of impact on actual efforts to carry out any best practice and to sustain or create favorable circumstances. It does provide, however, historical context for policy makers to consider before or during a COIN campaign.

For higher resolution Paul et al. develop a counterinsurgency scorecard, a broader analytic framework. To the three prime implicants, the authors add concepts whose importance is strongly (but not most strongly) supported by research. They also consider factors that reduce prospects for COIN victory. Many of the concepts are at too high a level of abstraction, however,

to determine their presence or absence directly. Therefore each concept was divided into basic tenets, and from the tenets the authors developed factors that could easily be measured as present or absent. Each concept was therefore represented by 1-10 discrete factors. The factors were analyzed and coded in the 59 core case studies. The authors determined how many of a set of factors for a concept needed to be met for the concept to differentiate outcomes among the 59 cases. Those concepts, framed as good or bad practices, were included in the scorecard if they obtained significant empirical support. The scorecard can explain all completed case study outcomes. It can also be used as a diagnostic tool to explore ongoing case studies. In another manuscript, the authors apply it to the continuing conflict in Afghanistan (Paul et al., 2013c).

Subpopulation of Core Cases: Iron Fist vs. Motive-Focused Approaches

The authors compare cases by the primary path taken. The iron fist approach seeks to eliminate the insurgent threat mainly through kinetic action against active insurgents and their supporters. The motive-focused approach, in contrast, primarily seeks to reduce motives to join or support an insurgency. Their research shows that completed COIN campaigns following an iron fist path won 38 per cent of the time, while campaigns that were motive-focused won 73 per cent of the time (p. 172).

An iron fist approach is represented in the study by several concepts, the most extreme of which is ‘crush them’. It is solely focused on the kinetic elimination of both active insurgents and their supporters (pp. 107-109). At worst it involves two factors, escalating repression and group punishment of those known or suspected of supporting active insurgents. When both factors are used, success is possible but not probable. Two additional significant concepts are associated with an iron fist approach: initiative and intelligence. When both were present, which occurred in ten of the 44 iron fist cases, the counterinsurgents won nine and lost one. So why haven’t iron fist campaigns won more times than they lost? The authors answer, ‘...COIN forces in iron fist cases are much more likely to seize the initiative without the necessary foundation of intelligence than are more motive-focused COIN forces, and they are thus much more likely to suffer the consequences of such indiscriminate application of force’ (pp. 171-172).

Turning to motive-focused cases, not surprisingly popular support is strongly correlated with success (pp. 172-173). All motive-focused COIN successes also did many things right; the lowest scorecard tally, good practices minus bad practices, was positive six – well above the success threshold of positive one (p. 173). Finally, successful cases, ‘...though primarily on the motive-focused side of the equation, were often balanced with kinetic efforts to kill, capture, or constrain active insurgents...’ (p. 7).

A key insight of the book is that both iron fist and motive-focused approaches can succeed. The authors challenge this generalization, however, and ask if there is more than one distinct path to victory. They answer in the negative; the same factors associated with both types of wins are common to wins in general (pp. 169-170). Most obvious are indicators for the three prime implicants – commitment and motivation, tangible support reduction, and flexibility and adaptability – but there are additional factors (p. 170).

An unsurprising difference between the two approaches involves popular support: while this factor was common in motive-focused COIN campaigns, it was not required for most iron

fist COIN wins. Eleven of the 17 successful iron fist cases did so without the support of the majority of the population in the area of conflict (p. 172). Yet popular support is just one way that an insurgency can gain the material resources it needs. If COIN forces could reduce tangible support for insurgents, one of the three prime implicants, then prospects for victory were enhanced but not guaranteed. Those leading motive-focused campaigns also need to be attentive to the full range of ways that insurgents gain tangible support. Achieving tangible support reduction, the first good practice in the scorecard, requires meeting at least three of 10 factors; yet only one of them explicitly refers to popular support (p. 133).

An iron fist approach can succeed more often when its actions against insurgents take into account those who support the insurgency. At the very least, kinetic actions need to extend beyond just killing or capturing insurgents. They also need to interdict tangible means of support that the insurgency requires, whether that support comes from the population, across porous borders, or from criminal actions such as taking hostages or selling narcotics. Even better would be an approach that finds ‘a balance between types of targets (insurgent support and the insurgents themselves) and types of actions (efforts to kinetically eliminate insurgents / support and efforts to diminish the motives for insurgency / support)’ (p. 180).

Here the authors present a major implication for theory – that the traditional dichotomy of enemy-centric or population-centric approaches to COIN should be replaced with two dichotomies of types of targets and types of actions. This reconceptualization of approaches to COIN is much richer and explains more observations, and it offers a more informed perspective for practitioners. Paul et al. develop it at length in their book, with an accompanying diagram that visually aids understanding it (pp. xxix-xxx, 5-8).

Policy relevance: Influence On May 2014 Field Manual

The two *Paths to Victory* books have already influenced policy, most obviously in the May 2014 version of the US Army and Marine Corps *Field Manual FM 3-24 MCWP 3-33.5 Insurgencies and Countering Insurgencies*. In the preface to this edition, nine sources are acknowledged for having influenced its contents; six are by members of this team of RAND scholars! Four of the six are selections from the *Detailed Case Studies* volumes and two are pre-publication selections from *Paths to Victory*. Unlike the previous edition, Galula is not even mentioned. Several examples illustrate how *Paths to Victory* and the two sets of case studies have enhanced the quality of the new field manual.

First, in Chapter 1 of the new field manual, ‘Understanding the Strategic Context’, we read, ‘Defeating an insurgency requires a blend of both civilian and military efforts that address both assisting the host-nation government in defeating the insurgents on the battlefield and enabling the host nation in addressing the root causes of the insurgency’ (2014, p. 1-1). This certainly sounds like the ‘balance’ that Paul et al. call upon for successful COIN. Compared to the December 2006 FM3-24, moreover, the new one is less optimistic that COIN forces following ‘best practices’ will lead to success. The new manual cautions: ‘...US civilian and military participants in counterinsurgency cannot compensate for lack of will, acceptance of corruption, or counterproductive behavior on the part of the supported government or the population’ (2014, p. 1-1). Second, to make ideas in Chapter 1 more concrete historically, the new field manual adds verbatim a section from *Detailed Case Studies* that the manual titles ‘The

Laos Insurgency’ (2014, pp. 1-3 to 1-4). Third, another insight from *Paths to Victory* is that reducing insurgents’ tangible means of support is one of three prime implicants strongly associated with successful COIN. This insight is reflected in the new field manual, discussed above in our review of Smith and Jones’ book (2014, p. 7-6, para. 7-21). In contrast, the December 2006 field manual (which did not have the benefit of either the 2010 or 2013 research by Paul et al.) more strongly associated ‘center of gravity’ with the population (2006, p. 3-23).

Assessment

Paths to Victory is an important book, but it is not without limitations. A minor problem is that there is no index. Sometimes when reading the book we found ourselves trying to determine what certain important concepts meant. For example, the concept ‘prime implicants’ is obviously important and it arises a number of times in the book. Still, it is not easy to figure out exactly how many prime implicants can be inferred from the data. Sometimes it seemed like two, three, or even four (2013a, pp. xxiii, 78-79, 182, 217-221). Ideally the narrative would have better clarified the issue. Short of that, an index with all page citations for the use of this concept would have helped to settle it. Yet apparently research manuscripts like this one don’t usually come with indexes. Given that the book can be downloaded free by anyone, problems like this are not serious.

Even though the book presents the best set of ideas and data out there at its level of abstraction, one should not settle for the status quo. Analysts, faculty, scholars, and students who wish to replicate the research and findings could readily do so. The authors and the RAND Corporation are totally transparent on such issues. All 71 detailed case studies are easily available on the RAND website, so that it is not difficult to revisit case studies or more complex research methods and findings. Speaking of case studies, the reader learns that English-language secondary sources were used (2013a, p. 203). That makes sense for a project with many case studies. Yet sometimes primary sources reveal nuances not available in secondary sources, or more recent research and publications may lead to a different history and set of conclusions. Secondary and primary sources written in non-English languages could also offer new insights, and area studies specialists could make new contributions to the case studies.

To illustrate this point, a co-author of this review completed an honors thesis on Peru’s counterinsurgency against the Shining Path (*Sendero Luminoso*). The final sentence in the thesis abstract raises a question regarding Paul et al.’s treatment of the Peru case: ‘In sum, there was a shift toward population-centric policy in 1989, but it was never implemented’ (Strayer 2014, p. iv). In *Paths to Victory*, Paul et al. identify the Peru case as an example of a motive-focused (population-centric) COIN win; in fact, they name it among the top four cases of successful COIN. When this co-author explored the Peru case study in the Paul et al. collection (2010b) and its summary in the current book (2013a), several major sources used in the 2010 case study were familiar. The conclusions by several authors were credible for when they were written, but more recent primary research (in Spanish) has added nuance to the arguments. Overall, the Peru case demonstrates a limitation of Paul et al.’s exclusive use of English language secondary sources. These problems with the Peru case study are not just academic, because the 2014 version of the US Army FM 3-24 quotes verbatim from and about the Peru case study (pp. 8.1-8.2).

CONCLUSIONS

We recommend reading all three books, because together they provide insights and suggest hypotheses that are greater than the sum of their parts. This final section begins with a comparative analysis of the books, with special attention to insights that each offers as well as what each book says is the most important factor or factors needed for successful counterinsurgency. We approach them in reverse order, starting with the book by Paul et al. that was just discussed. These various ideas are interrelated in important ways and point toward productive research paths. We end with the largely ignored relationship between counterinsurgency and grand strategy.

Comparative Analysis and Connections among the Books

Paths to Victory is the most general analysis of counterinsurgency. Paul et al. test 24 diverse hypotheses against 71 case studies of completed insurgencies. It is also at a fairly high level of abstraction. Successful COIN campaigns require that in the decisive phase of a conflict the three prime implicants code as accomplishments and not just efforts: insurgents' tangible means of support was reduced significantly, COIN forces were flexible and adaptable, and the local government and the COIN forces were indeed motivated and committed to prevail. These generalizations hold regardless of local context, although local context greatly influences whether or not an implicant or other best practice or favorable condition can be achieved. It was not the authors' purpose, however, to learn 'causes of causes' (i.e., the conditions under which an important condition supporting success can be achieved).

Fortunately, Long's book offers insights into preconditions for accomplishing the prime implicant of flexibility and adaptability. *The Soul of Armies* does an excellent job testing four competing hypotheses that might explain variations in military doctrine and operations, the fourth one being his own hypothesis about the prominence of military culture when the information environment is ambiguous. His four case studies show that his hypothesis best explains observations. There needs to be some translation between the two books' conception of flexibility and adaptability, however. For Long, his ideal type of a maritime army serves as a proxy for characteristics of a military organization that would exhibit appropriate flexibility and adaptability during a COIN campaign: small-unit operations, discriminate use of firepower, and cooperation with local security forces and civilian agencies.

To his surprise, Long also found that having a competent maritime military service is not sufficient for success. The agency of an expeditionary COIN force is limited, and additional causal factors are at play in counterinsurgency. These observations are consistent with findings of Paul et al., including their three prime implicants, each of which is necessary but is not sufficient for success.

Long's four case studies led him to be attentive to local context, especially the importance of having good local allies. He suggests further research into the causes and consequences of local allies.

What about the other two prime implicants from Paul et al.? Local context matters for them too. Their case studies could be mined for clues about when and how it matters. For example, their research offers contrasting case studies where a prime implicant of interest was present in some instances but absent in others. In developing this research design, an analyst could propose hypotheses that might account for whether or not a prime implicant is achieved; they could be tested by exploring relevant case studies to learn which hypotheses are most strongly supported. Such research might also lead to new findings discovered inductively. This effort should help to determine the conditions under which prime implicants are achieved and help to explain why those conditions are important.

Scholars who seek to conduct new case studies have a ready-made template to follow. They could add fresh concepts / hypotheses to test, and develop appropriate tenets and factors that could be measured. Paul et al. also make available through a URL link spreadsheets with all factors for all case studies; they could be modified or added to as desired (2013a, p. iv). Their basic findings concern hypotheses and factors that receive empirical support. Many of the hypotheses were derived from particular theories or principles of operation, but Paul et al. make no claim that some theory can explain all of their findings. Thus there is room for budding theorists to try to construct and refine theories that can explain observations of interest better than competing accounts.

The Political Impossibility of Modern Counterinsurgency is distinct from the other two books because Smith and Jones do not forward hypotheses to test but rather seek to ‘deconstruct’ the concept of counterinsurgency and prove that it is misconceived and otherwise flawed. Instead, they assert that issues of tactics and operations are much less important in explaining COIN outcomes than understanding the political will of the country engaged in COIN. The implication is that the state’s political will, when present, is a necessary condition for success. BUT is it also sufficient? If not, what other conditions might combine with it to provide a sufficient set of circumstances? These questions invite a research design intended to provide answers.

Their assertion about political will overlaps Paul et al.’s prime implicant about commitment and motivation, yet the concepts are not identical. Smith and Jones focus political will on the state sponsoring a COIN campaign. Paul et al., in contrast, have a compound concept that includes the commitment and motivation to win by the COIN forces and the commitment and motivation to prevail by the local or host government. Paul et al., like Long, found that the agency of an expeditionary COIN force is limited and therefore they would disagree with Smith and Jones about the importance of just the political will of the state supporting COIN. Since Paul et al. do not disaggregate their commitment and motivation prime implicant, a task for further research would be to do just that – develop an hypothesis for each factor and test them in the same case studies.

This discussion connects to Smith and Jones’ assertion that only ‘strategic theory’ is valid. Their assertion seems to be based on belief that a state’s political will is by far the most important factor determining outcomes of COIN. However, if political will is not a sufficient condition for success, and other factors (with agency from COIN and locals) are important much of the time, then strategic theory is not sufficient and other ideas and theories are plausibly important.

Paul et al. found that flexibility and adaptability was another of their three prime implicants. Smith and Jones do not suggest that flexibility and adaptability are unimportant, but they assert it is secondary to political will. They say it ‘...provided the time for the learning/adaptation process to occur in each contingent setting’ (2015, p. 140). Paul et al. did not find one prime implicant more important than another. Their findings about the sequence of supportive factors, moreover, also shed no light on whether or not commitment and motivation are prerequisites for flexibility (2013a, p. 156). Further analysis and research is needed to settle this issue.

There is additional overlap among the books, with several points of agreement. One concerns Paul et al.’s prime implicant about the need to reduce insurgents’ tangible means of support. We learned that the counterinsurgents’ ability to gain popular support is only one of several ways to accomplish this objective. Smith and Jones would not be surprised by this finding, which shows that in practice the ‘center of gravity’ of a conflict does not always involve popular support (although it often does). Another finding from Paul et al. is that successful motive-focused approaches to COIN need balance, with at least some attention to the use of force (2013a, p. 7). Smith and Jones would agree, and so would Nagl and Long.

This comparative analysis helps put to rest the image that successful COIN typically involves a purely ‘hearts and minds’ approach, as well as the belief that success will always accompany a COIN campaign that is conducted with appropriate doctrine and operations. The historical record creates a more nuanced picture, with success more likely under certain conditions. We also learned that achieving success depends on local contexts, only some of which can be influenced by outside agency.

Grand Strategy

The final issue involves Smith and Jones’s insight about the centrality of a state’s core national interests to its level of commitment to a COIN campaign. The discourse about grand strategy focuses on military, political, and economic remedies for threats to a state’s security (Posen, 1984, p. 13). These ideas are worth exploring because they highlight factors that cause variations in how a state’s leaders view counterinsurgency.

During the Cold War, the US grand strategy was containment. In the post-cold war period a debate over alternatives arose. A simplified view of this debate appears with two competing recommendations for US grand strategy in the Jan./Feb. 2013 issue of *Foreign Affairs*. The titles of the articles reflect the approaches. Barry Posen wrote ‘Pull Back: The Case for a Less Activist Foreign Policy’. In the realist tradition, he advises that the United States stay out of the counterinsurgency business because it needlessly leads to involvement in conflicts where American vital interests are not at stake. Stephen Brooks et al. wrote ‘Lean Forward: In Defense of American Engagement’; they support the liberal internationalist theoretical perspective of deep engagement. Brooks et al. don’t explicitly discuss counterinsurgency, but their expansive perspective of US interests and values that are at stake in the world suggest that they would be more favorable to it than Posen.

Whichever perspective or combination of views dominates US foreign policy, it would likely have its own particular assessment of how important a distant insurgency or hybrid war is for US national interests. That calculation would probably influence whether or not the United States might see value in an expeditionary COIN campaign, in a more advisory and support role for locals to do the major tasks, or in neither. Knowing which grand strategy prevailed or might prevail would likely have utility for predicting the level and durability of country's potential political will to engage in or support a COIN campaign.

The other two books are agnostic on whether or not the United States should engage in counterinsurgency as part of its grand strategy. However, they offer analytic tools for making informed predictions on the prospects for success in particular contexts. Those favoring a forward grand strategy or a contingent approach would find such predictions useful. If the likelihood of success is not high, then perhaps COIN should not be conducted. If some of the conditions might be made more favorable through various policies, making success likely, then a cost-benefit analysis would be needed to decide if the effort would be worthwhile. For others who want a restrained grand strategy, such predictions would be less useful because COIN is seen as an undesirable approach most likely to be used in a region of the world where US vital interests are not at stake. The scholarship is also attentive to variations of implementing COIN, including indirectly by supporting others. Such indirect efforts are likely to be undertaken at least occasionally, and knowledge and understandings from the study would be valuable.

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