H-Diplo | Robert Jervis International Security Studies Forum

Roundtable Review 15-26


2 February 2024 | PDF: [https://issforum.org/to/jrt15-26](https://issforum.org/to/jrt15-26) | Website: [rjissf.org](http://rjissf.org) | Twitter: [@H Diplo](https://twitter.com/@H Diplo)

Editor: Diane Labrosse
Commissioning Editor: Andrew Szarejko
Production Editor: Christopher Ball
Pre-Production Copy Editor: Bethany Keenan

Contents

Introduction by Peter S. Henne, University of Vermont ............................................................... 2


Review by Asfandyar Mir, United States Institute of Peace ................................................................. 12

Review by Douglas Porch, Naval Postgraduate School ..................................................................... 15

Introduction by Peter S. Henne, University of Vermont

When I began my professional career in the mid-aughts, counterinsurgency, or “COIN,” was beginning to supplant counterterrorism as the most important buzzword in Washington, DC. The steadily expanding civil war in Iraq and insurgency in Afghanistan—not to mention conflicts in other countries like Somalia—convinced foreign policy elites (and wannabe elites like me) that the United States needed to do more than gain intelligence on terrorist networks, disrupt their activities, and enhance homeland security. The United States needed to build (or rebuild) functioning societies, which would ideally be ruled by liberal democratic governments, to ensure long-term security. Effective counterinsurgency strategies were the way to do this.

There was an added bonus to COIN: it promised to align American interests and values. A common wisdom soon developed that bombing protesters from the air, as Great Britain did when attempting to control the insurgency it faced in Iraq in the 1920s, would not work. Instead, the United States needed to win “hearts and minds” by building trust with local populations and turning them against the insurgents. During the last years of the George W. Bush Administration and the beginning of the Barack Obama Administration, the DC foreign policy community focused on organizing events and churning out reports on how best to implement a “hearts-and-minds” strategy. This corresponded with a shift in counterterrorism strategy towards countering violent extremism; this also promised to hew closer to American values, as I discuss in my new book.¹

But “hearts and minds” advocates ended up disappointed. The war in Afghanistan dragged on for years, until Biden’s ignominious withdrawal in 2021. The United States withdrew from Iraq, which soon slipped into misrule and a new civil war with the rise of the Islamic State. President Obama, hoping to shift US focus towards “nation-building at home,” pursued low-effort COIN strategies like deadly drone strikes in Pakistan, Somalia, and Yemen.

This is all just from the US perspective. Other Western states also struggled to implement COIN strategies, like France in the Sahel. Turkey dealt with its long-running war against the Kurdish insurgency led by the (PKK). Pakistan had its own insurgency, partly a spill-over from the Afghanistan conflict. Somalia’s central government strove mightily to stabilize the country in the face of constant attacks by al-Shabaab. Meanwhile, the United Nations’ peacekeeping efforts continued.

As it became clear that “hearts and minds” was not working out the way its proponents hoped, foreign policy wonks came to debate whether these efforts were flawed from the start or whether states just did not try hard enough. A growing group of foreign policy thinkers, whom I call the “restraint crowd” (and their critics call “isolationists”) argued the international community needed to stop trying to fix the world’s problems.² Examples include established scholars and public intellectuals, like John Mearsheimer and Stephen Walt’s calls for “offshore balancing” and Peter Beinart’s worries of US hubris.³ They also include newer voices, such

---

as Emma Ashford, who critiqued critiquing the US impulse to turn to military force and Annelle Sheline arguing for a decreased US role in the Middle East.4

Meanwhile, previously dominant liberal internationalist voices were becoming less influential. Critics characterized DC foreign policy circles as “the Blob,” a bipartisan group of voices who essentially agree on US primacy achieved through military action, and claim this has “captured” Democratic foreign policy thinking.5 Others attacked Democratic-aligned think tanks—the incubators of liberal internationalism—as overly timid and centrist.6 The growing dissatisfaction with the liberal internationalist orthodoxy came to a head when Anne-Marie Slaughter, a prominent advocate of Democratic foreign policy, voiced support for President Donald Trump’s airstrikes on Syria and was mocked as part of the “centrist and liberal wings of the blob [turning] out for Trump.”7

The timing of the publication of Bullets Not Ballots: Success in Counterinsurgency Warfare by Jacqueline Hazelton, was thus impeccable. The foreign policy community seemed to have soured on counterinsurgency, even as it recognized the need to do something about persistent civil conflicts.

In this book, Hazelton breaks with both sides of the above debates. Counterinsurgency can work, but it is not going to be a pleasant experience for anyone involved. “Hearts and minds strategies” are ineffective; she even refers to the “hearts and minds fallacy” in an earlier article.8 She demonstrates this by surveying cases that others point to as examples of “hearts-and-minds” COIN successes: the Malayan Emergency (1948–1960), the Greek Civil War (1946–1949), the Huk Rebellion in the Philippines (1946–1954), the Dhofar rebellion in Oman (1963–1976), and the Salvadoran Civil War (1979–1992). She argues that success came about not through good governance but by gaining elite support—sometimes through bribery—and using compellent, at times brutal, force against the population.

It is a thought-provoking argument and has predictably generated debate, as seen in this roundtable. Jahara Matisek’s review is generally positive. He praises Hazleton for “going boldly against the grain” and accepts that this will force many scholars—himself included—to rethink their works on counterinsurgency. He also helpfully notes the broader implications of Hazleton’s book, such as for literature on statebuilding. Asfandyar Mir’s assessment is more measured. He notes its “rich empirical analysis” but finds a few “gaps.” He calls for greater specificity in the theorizing about elite engagement, questions the definition of counterinsurgency success, and raises concerns about “selecting on the dependent variable.” The review of Douglas Porch is the most critical. He questions the novelty of the book’s focus on compellence, expresses frustration at Hazelton’s dismissal of military history, echoes Mir’s concerns about the concept of elite engagement, and

---


criticizes her empirical analysis. In her response, Hazleton acknowledges Matisek’s insights on the book’s broader applicability, and defends the work against the critiques of Mir and Porch.

I would just add two points. First, I hope this leads to greater acceptance of empirical findings that go against liberal values: what Matisek calls “uncomfortable truths.” It is often easy to get published when you find results in line with your reviewers’ and editors’ consciences. As Ron Hassner said in a discussion on his new book, *Anatomy of Torture*: “you’re not going to like this book.”9 He found that torture is effective in certain circumstances, something that elicited strong pushback from peer reviewers. I discovered this as well when trying to publish an article on the effectiveness of coercive counterterrorism tactics, it is more difficult when you are making a morally questionable claim (although I did eventually find success).10 Pointing out the often unpleasant reality of the world is not fun, but it is important and I am glad Hazleton was able to do so.

The second, and related point, is that we need more debate on what to do when reality conflicts with our ideals: Hazleton’s book can contribute to that.

Liberal internationalists have (hopefully) come to accept that humanitarian interventions and “hearts and minds” COIN strategies will not easily lead to peace and prosperity. There may, however, be times when the international community needs to act to prevent a massive humanitarian crisis or stop a powerful threat; the post-9/11 invasion of Afghanistan and the failure to do anything about the Syrian civil war come to mind.

The restraint crowd, in turn, fashions themselves modern day realists.11 Their basic argument is that the United States needs to be more cautious and humble in its military engagements.12 Many, however, have their own naïveté. By arguing that the United States should only become military involved when its interests are threatened, while calling for a narrow definition of US interests, they imply that the threats arising from civil conflicts and terrorism will never affect their own country, or that the suffering caused by these conflicts is not the United States’ problem to fix.13 Some argue that international tensions are the result of American primacy, thus suggesting that if America stays out of world affairs many of these conflicts will go away.14

Hazleton’s work can inform both groups. The book can better prepare advocates of intervention for what they need to do to succeed. Meanwhile, it can prod the restraint crowd away from their simplistic view of international relations and force them to be clear about the trade-offs their policies require: i.e. they could argue that intervention is never worth it, considering what COIN success would require.

---


© 2024 The Authors | CC BY-NC-ND 3.0 US
This also extends to academic debates. Constructivists have confidently told us that norms and identities drive international relations, and that transnational networks can affect state behavior. But they have suspiciously focused on “good” ideas and actors. The typical response from conventional realist-inspired security studies is that these values and ideas do not matter that much. As I argue in my new book, however, value-laden appeals—in my case appeals to religion—do have significant effects on power politics, but they tend to be disruptive and unpredictable. One could extend Hazleton’s work to argue similarly about COIN. Well-meaning values lead us to commit to counterinsurgency operations, the success of which require actions contrary to those values.

Hazleton’s book may not settle debates about COIN, as some of the respondents argue. But it does ensure these debates will continue and be more productive.

Contributors:

Jacqueline L. Hazelton is the Executive Editor of the journal International Security. She was previously an Associate Professor at the US Naval War College. Bullets not Ballots won the American Political Science Association Foreign Policy section’s best book award for 2020-2021. Her current book manuscript examines the role of liberalism in the military intervention policies of the United States, France, and Britain.

Peter S. Henne is an Associate Professor of Political Science and Director of Middle East Studies at the University of Vermont. During the spring of 2023 he was a Research Fellow at the University of Edinburgh’s Institute for the Advanced Studies of the Humanities. Henne is the author of Religious appeals in power politics (Cornell University Press, 2023) and Islamic Politics, Muslim States and Counterterrorism Tensions (Cambridge University Press, 2017). He previously directed the Pew Research Center’s work on international religious freedom and the National Consortium for the Study of Terrorism and Responses to Terrorism’s project on domestic radicalization. Henne was also a Senior Consultant with Booz Allen Hamilton, working with the US government on counterterrorism.

Lieutenant Colonel Jahara “Franky” Matisek, PhD is a Military Professor in the National Security Affairs department at the US Naval War College, Research Fellow at the European Resilience Initiative Center, and US Department of Defense Minerva co-principal investigator for improving United States security assistance. Lt. Col. Matisek has published over 100 articles and essays in peer-reviewed journals and policy-relevant outlets on strategy, warfare, and security assistance. He is a command pilot who previously served as a senior fellow for the Homeland Defense Institute and associate professor in the Military and Strategic Studies Department at the United States Air Force Academy.

Asfandyar Mir, PhD, is a Senior Expert in the South Asia program at the United States Institute of Peace. Dr. Mir’s research interests include the international relations of South Asia, US counterterrorism policy and political violence—with a regional focus on Afghanistan and Pakistan. Previously, Dr. Mir taught in the

18 Henne, Religious Appeals in Power Politics.
political science department and held various fellowships at the Center for International Security and Cooperation of Stanford University.

It is rare for a scholar to go boldly against the grain. However, Jacqueline Hazelton’s *Bullets Not Ballots: Success in Counterinsurgency Warfare* bravely excels in demonstrating that many of us, particularly in the field of conflict studies, missed the bigger picture of domestic political processes and foreign assistance in civil wars. Most scholars have overlooked the critical role of partner governments that receive foreign aid (e.g., economic, military, etc.) and create coalitions of elites (formal and informal) in effectively using violence to deny, degrade, and destroy an insurgency.1 Many liberal interventionist scholars downplayed the role of partner violence against the population and insurgency, perhaps due to biases in believing that a Western power, like the United States, could provide assistance and advisors to a partner government to help them to beat an insurgency without violating human rights and international laws.2 It is easy to buy into the myths of how some intra-state conflicts were won; there is too much confirmation bias at stake. Many scholars endorsed the positive-sounding counterinsurgency (COIN) narratives of protecting the population and winning them over. For instance, while the British government tried extremely hard to portray its minimalist military approach to the Mau Mau Uprising in Kenya (1952–1960) as abiding by international law, Huw Bennett recently showed how brutal the campaign was against the Mau Mau and how the British eventually co-opted elites in other Mau Mau tribes in order to eliminate the dissenting tribes.3 The spectacular collapse of the Afghan government in August of 2021 further illustrates how the Western approach to winning hearts and minds failed due to the successful Taliban co-optation, coercion, and infiltration of the Afghan government and security forces.4

In many ways, *Bullets Not Ballots* confirms descriptions of the often intimate, internecine conflict that defines any internal war, as has best been outlined by Stathis Kalyvas in *The Logic of Violence in Civil War* and David Armitage in *Civil Wars: A History in Ideas*.5 Moreover, Hazelton’s work on the value and importance of elites in any internal war echoes the work of Amartya Sen’s *Identity and Violence: The Illusion of Destiny*, as usually violent political entrepreneurs pursue “the imposition of singular and belligerent identities on gullible people,” requiring “proficient artisans of terror.”6 Hazelton’s work illustrates the importance of getting enough elites into a winning coalition by showing that these elites do better at controlling and motivating their followers by virtue of different power levers, whether they are acting based upon greed, grievance, or sometimes just safety and protection from the ill-effects of a civil war.7 For scholars who are caught in the argument about greed versus grievance, Hazelton’s book indirectly reveals that both of these processes are intertwined with how the

government and an insurgency interact, and how they capture formal and informal elites in pursuit of defeating the other. The population in any civil war is an afterthought.

Since the Vietnam War, scholars, practitioners, and policymakers have sought the “perfect recipe” (and equation) for a successful counterinsurgency campaign. Several camps have emerged, from the firepower-intensive believers (i.e., members of the military), to the “hearts and minds” practitioners (i.e., those who believe in good governance), and even the sociologists and anthropologists who advocated that governments learn the social context and human terrain to win. However, Hazelton’s compellence theory demonstrates the importance of co-opting enough elites to generate the necessary political and military power to defeat an insurgency, with little concern for the average citizen. Her theory does a better job of explaining civil war outcomes by demonstrating that a government can engage in various atrocities against its own citizens to defeat an insurgency. Hazelton’s findings, which present reforms as either symbolic and/or not being completed, challenge those of the school of good governance that advocate for policies that generate “popular support.” At the same time, Hazelton’s analysis takes agency away from average citizens by showing how much the government and elites can structure the way in which a conflict unfolds—and the need to attack the population, which is the center of gravity of an insurgency. This translates into starving, moving, and/or destroying population centers in order to prevent the insurgencies from acquiring resources.

Hazelton’s work is most convincing in indirectly describing larger processes, such as nation- and state-building, tracing policy decisions and outcomes, and what future Western military interventions might look like. The book falls in line with the literature on war-making, state formation, and development as illustrated by those of Charles Tilly, Brian Downing, Miguel Centeno, and Victoria Hui. Hazelton provides the structural context of coalition building and the narratives and myth making that are needed to achieve victory, thus creating a more capable state. Additionally, the book demonstrates the brutality and co-optation approach taken by governments to generate a nation-state identity, and the numerous collective action

---

problems for a rebellion that is trying to get more than five percent of the population to risk their lives to actively fight against the government.17

Second, Hazelton’s in-depth work on the Malayan Emergency (1948–1960), the Greek Civil War (1946–1949), the Huk Rebellion in the Philippines (1946–1954), Dhofar rebellion in Oman (1963–1976), the Salvadoran Civil War (1980–1992), and the Kurdish-Turkish conflict (1978–present), demonstrates the value in digging deeper into the policy decisions made by political leaders. All too often, scholars miss the “black box” in which a certain political reform and/or elite accommodation was made, without tracing that decision back to its origins, and the way it evolved during the policy formation process. In many instances, Hazelton deftly reveals that the “supposed evidence” for gentle COIN campaigns were implemented after the insurgency had been militarily defeated. She further argues that when many of these policies came to fruition, they were generally symbolic and/or were performed for propaganda purposes—either to please foreign aid observers or to make the government look less repressive to the average citizen.

Simply put, many scholars (myself included), will need to reassess their previous work on insurgencies. For example, in my research on the Casamance Conflict (1982–present) in Senegal, there is a possibility that I may have overlooked some of the firepower-intensive campaigns (for example, torture, landmines, cross-border raids, etc.) of the Forces armées du Sénégal against the Mouvement des forces démocratiques de Casamance (MFDC) and accept that the MFDC was likely militarily defeated by 2000, with a formal end of hostilities in 2012.18 To this day, the government in Dakar continues to engage in symbolic reforms and other co-optation campaigns against elites in the Casamance region.19 In essence, conflict scholars will need to do better in parsing official government narratives—and fact-check them with archives, newspaper accounts, and fieldwork interviews in order to ensure a conflict is properly perceived with due regard to backroom policy discussions, facts, realities on the ground, and narratives.20

Finally, if we accept Hazelton’s analysis and conclusions, these uncomfortable truths likely mean that the famed Field Manual (FM) 3-24 Counterinsurgency will need a major re-write. This core text on American COIN should be updated with the hard truths that Hazelton provides instead of its utopian desires of a liberal, democratic COIN fighting force.21 Idealistic military interventionist policies, such as those suggested by

---


18 If we accept the way Hazelton redefines the military defeat of an insurgency, rather than the formal ending, then the battle deaths associated with the Casamance Conflict ceased around 2000. Consider UCDP data on the Casamance Conflict: https://ucdp.uu.se/country/433.


20 For a great example of this sort of fieldwork and a need to properly assess competing narratives over a violent event, see: Paul R. Brass, Theft of an Idol: Text and Context in the Representation of Collective Violence (Princeton: Princeton University Press, 1997).

Samantha Powers, Anne Marie Slaughter, and other Responsibility to Protect (R2P) advocates do not account for many of Hazelton’s findings concerning the use of brute force to coerce an insurgency and population into compliance, especially since the book’s analysis and findings ably dispel the myths of what it really takes to defeat an insurgency. It is less about winning hearts and minds, and more about crushing enough hearts and minds to win an insurgency. Because intervention appears to require the same sort of violence and political maneuvering of insurgencies, those policymakers—and pro-interventionist scholars—who support wielding any Western military force to intervene in a future civil war will be forced to re-examine their assumptions.

Fence-sitters, free-riders, and spoilers can alter conflict dynamics at the lowest levels, leading to major strategic outcomes. Few leaders in Western capitals have an appetite for the violence needed to coerce and dissuade actors in a foreign civil war. Perhaps it also means reconsidering what building a Western-looking army in a failed state to fight an insurgency requires; the political context may make that host-nation military weaker—as recently seen with the Afghan Army—than the insurgency because it is an ephemeral condition. Moreover, we need to ask tough questions about what it takes to maintain a stable regime five years after the defeat of an insurgency—and whether all the armed elites (i.e., warlords) can become reliable “peacelords” so that the country can escape the civil-war trap.

It should be no surprise that Russia’s brutal campaign (with Iranian help) in the Syrian Civil War that began in 2011 was so effective in supporting pro-regime forces and keeping President Bashar al-Assad in power. If anything, we should be considering the implications of Russian COIN, especially in context of how successful the Russians were in the Second Chechen War (1999–2009), razing population centers and co-opting enough Chechen elites to take a dirty fight to Chechen insurgents. However, this also leads us to consider why the Russians were unable to achieve enough elite co-optation to properly defeat the Mujahideen during the Soviet-Afghan War (1979–1989).

Simply put, Hazelton’s work shows that in order for COIN to be effective governments must capture elites, use maximal violence against insurgents and population, and implement symbolic reforms. However, the success of her book leads to larger questions about the critical junctures in Western COIN campaigns that faltered in Iraq, Afghanistan, Vietnam, and elsewhere. Fortunately, Hazelton provides the sort of theory and

---

research template needed to further study failed COIN campaigns and to identify where policies failed to capture and accommodate enough elites in pursuit of destroying an insurgency.
Review by Asfandyar Mir, United States Institute of Peace

What does it take to succeed at counterinsurgency? This was one of the major policy questions for US foreign policy in the post-9/11 era. In the early part of the US intervention in Iraq, the debate on counterinsurgency converged on a set of softer population-centric approaches—including measures to improve population security and governance reforms to gain popular support—as central to security stabilization and to the consolidation of state control. Influential doctrinal perspectives, such as the US Army’s counterinsurgency field manual FM 3-24, did so in part by relying on revisionist accounts of a handful of counterinsurgency cases. A large body of scholarly work emerged formalizing the descriptive doctrinal accounts, taking some of the baseline assumptions in the doctrine as a given. Building on that, micro-approaches evaluating hearts-and-minds counterinsurgency interventions elevated technocratic governance reforms and under-theorized assumptions about pathways to popular legitimacy; they also downplayed, perhaps inadvertently, high politics to the outcomes of counterinsurgency.

Jacqueline Hazelton’s *Bullets Not Ballots* challenges this conventional wisdom of the post-9/11 years for explaining counterinsurgency success. With a rich empirical analysis of an important set of counterinsurgency cases, she demonstrates that the actual conduct of successful great power counterinsurgency has been fairly light on governance reform and, instead, dominated by brutal violence and political deal-making. Specifically, contrary to the pervasive wisdom on the importance of popular support and military-doctrinal choices of the great power’s counterinsurgents, she emphasizes two counterinsurgent choices: politics of elite accommodation in counterinsurgent state structures and—very troubling—brute-force violence against civilians who serve as a base for an insurgency. She argues that a strategy centered around these choices has the potential to compel insurgents to back down and allow great powers and their clients to consolidate state control. These claims are supported by case studies of British colonial counterinsurgency in Malaya and Oman, and the US government’s support for beleaguered allied governments in El Salvador, Greece, and the Philippines. There is also an out-of-sample test of Turkish counterinsurgency against the Kurdistan Workers’ Party (PKK).

This is an important theoretical intervention. Based on my reading of the literature, the efficacy of large-scale violence at subduing anti-government civilian mobilization and crippling the insurgency in an important set of cases is generally recognized. Policy practitioners have emphasized this point under the rubric of population-control counterinsurgency (while contrasting it with softer population-centric counterinsurgency). In recent literature, scholars of political violence have examined this strategy through the concept of indiscriminate violence. But the focus on political inclusion of and deal-making with key elites is novel—especially the idea that the synergy of elite accommodation and violence against civilians can break the will of the insurgency to

---

3 For a review, see David Kilcullen, *Counterinsurgency* (Oxford: Oxford University Press, 2010).
fight on. Taken together, this argument offers a troubling account of the deeply violent and politically insidious nature of great power counterinsurgency.

Despite the importance of the arguments and the empirical contributions of the case studies in the book, the analysis leaves some theoretical and empirical gaps. I want to highlight three. For one, the theoretical discussion on elite accommodation dynamics appears incomplete, leaving important questions unanswered and making the theory difficult to falsify (17). As I see it, there are three gaps. First, it is unclear who the “elites” are that need to be accommodated to ultimately compel an insurgency to back down. It is also unclear if the problem of elite accommodation is one of incorporation of rivals on the outside, maintaining cohesion of a pre-existing coalition of power brokers, or both. The theoretical discussion is unclear on this point. The case studies do not illuminate this point, as they cover a wide range of political actors at various levels, including Malayan and Chinese communal leaders and Indian business leaders in Malaya, centrist and right-wing politicians and militias in Greece, the political coalition of former president Ramon Magsaysay in the Philippines, tribal firqat leaders in Oman, civilian militia leaders in El Salvador, and local landowners and tribal chiefs (Aghas) in Turkey.

This raises a related concern: How many elites need to be accommodated for compellence dynamics to kick in? And how open might the elites be to an accommodation? It is possible that in highly violent contexts elites take any deal that is offered to them. But if not, what do counterinsurgents need to offer to convince the elites to be accommodated? Without having firm answers to these questions, it is difficult to evaluate the politics of elite accommodation and the value add of the variable of elite accommodation for explaining great power counterinsurgency cases.

I considered the applicability of the elite accommodation concept on the US intervention in Afghanistan, which is salient due to the recent return of the Taliban to power and the overall failure of the US government’s counterinsurgency efforts there over the last two decades. One question I have about the case is who were the elites that the US government needed to accommodate for the counterinsurgency campaign to be successful? One can argue that many elites were accommodated. After the Bonn conference of 2001, the US government accommodated almost all major non-Taliban elites in the resulting political order. Some efforts were made to cut deals with a few Taliban leaders on multiple occasions both before and after the invasion. And, for much of the US intervention, a range of local elites were incorporated, even if through local strongmen. One suggestion in the literature is that the US government needed to have cut a deal with the defeated Taliban leadership soon after the invasion. Or, perhaps, it should not have pursued a policy of “de-Talibanization.” Another possibility is that the US government needed to have divided the nascent Taliban, or wean away some of its major local backers to support the US-backed political order. Would one or the other approach be the kind of elite accommodation that is necessary under the compellence theory? The answer to these questions cannot be gleaned from the arguments in the book.

My second concern is about the dependent variable of “counterinsurgency success.” As defined, counterinsurgency success implies broad-reaching state consolidation/control and a diminished insurgency. But for both domestic and great powers counterinsurgency, this may not be the goal. Based on my reading,

---

6 From an empirical standpoint, how can elites be observed and coded ex-ante?
there is often no objective end-state of success. Instead, at times, states might seek to lower levels of violence (as in the case of the US campaign in Iraq in 2007–2008), stabilize regions around government capitals (as with the Soviets in Afghanistan in 1980s), or apply just enough military pressure that brings the insurgency to the negotiating table (as occurred in the later years of the US campaign in Afghanistan). One implication of this is that the choice of offering political deals to elites or the level of violence against civilians might be a function of how states define their end-state. Another is that certain cases that might appear to be failures might actually be successes from the perspective of the counterinsurgent. The fact that the strategies of counterinsurgent states are dynamic in the end-state they pursue has implications for the inferences we can draw about their causes.

Finally, I am not persuaded by the case study selection. It is clear that the cases evaluated are most likely cases for the good governance approach, and they demonstrate the limited explanatory power of the strategy to explaining counterinsurgency success. But I do not see the value of six such cases and selecting on the dependent variable across all of them. Assessing the theory with some negative cases (of counterinsurgency failure) or cases with more variation in the independent variables (like a case where counterinsurgent used the good governance approach, but the variables of compellence theory were absent) would have improved confidence in the theoretical claims. A minor concern I have is about the class of counterinsurgency the cases evaluated in the book represent. The claim that the examined sample are cases of limited intervention, which is the dominant form of great-power counterinsurgency, is not convincing. Situating case selection in a more detailed discussion of the universe of cases along key theoretical dimensions is important to supporting claims on the generalizability of the findings.

Notwithstanding these criticisms, this is an important book that will remain essential reading for scholars and practitioners of counterinsurgency. Given that it is also a major challenge to the doctrinal focus on good governance and population-centric counterinsurgency approaches, the book offers another important reason for re-thinking US counterinsurgency doctrine and the overall US policy approach to stabilization after intervention.

---

Counterinsurgency, acronymized as COIN, has a contentious history, mostly due to the inherent contradictions of the nature of war. It is the result of waves of boosterism by its most fervent proponents dating from the era of nineteenth-century imperial expansion, the promotion by Lowell Thomas of “Lawrence of Arabia” as the alternative to Great War butchery, through the General David Petraeus-impresarioed “Anbar Awakening,” a marketing campaign to rescue America’s bumbling intervention in Iraq. At first, military strategist Carl von Clausewitz categorized the rubric of rebellion, partisan warfare, or small wars as disputes between or among civilians that remained below the threshold of actual war. But the popular insurrection against the Napoleonic invasion of Spain in 1807–1812 caused Clausewitz to realize that a small war could coalesce into national war. In the era of the French Revolution and Napoleonic wars, he concluded that popular insurgencies contributed to the transition of warfare from a dynastic endeavor into the “business of the people…The people become a participant in war; instead of governments and armies as heretofore…”

As Hew Strachan notes with an eye to the past and present, COIN conflates the tactical, operational, strategic, and political levels of war, so that low-level operational requirements drive policy and strategy, a rare event in major wars. For this reason, counterinsurgency operations acquire euphemisms like emergencies, stabilization ops, unconventional or “low-intensity” conflict, counter-terrorism, and so on, which suggest that they constitute a category of violent disputes between or among civilian actors below the threshold of war. In this way, such limited conflict might stay off the public radar, or be kept secret altogether—in short, war without war.

Because insurgencies are habitually waged separate from, or perhaps in tandem with, conventional campaigns, successful counterinsurgents are thought to require social skills, an anthropological awareness of the “human terrain,” and a firm grasp of the political issues, rather than mere technical and tactical skills of battle associated with conventional conflict, prosecuted in a policy vacuum. A second problem for command in counterinsurgency operations in Clausewitz’s view was how to maintain the combat power and “military virtues” in the lower ranks of armies that “disperse” to fight “a people in arms,” even if they were “little more than a band of partisans.” This is especially so if the “natural qualities of a people mobilized for war” included “bravery, adaptability, stamina, and enthusiasm” (italics in original).

In a new critique of six post-World War II counterinsurgency operations, Jacqueline Hazelton claims that, by focusing on “the needs of the population” (8) who must be lured away from insurgency through “good governance” (8) and other endeavors calculated to “attract civilian cooperation against the insurgency, and marginalize the insurgents” (8), counterinsurgents have become masters of strategic misdirection. From the perspective of the year 2021, this critique is justified, even if the method of this study leaves much to be

1 For a history of COIN, see Douglas Porch, Counterinsurgency: Exposing the Myths of the New Way of War (Cambridge: Cambridge University Press, 2013).
4 The British government went to extraordinary lengths in the 1970s to make certain that London’s intervention in Dhofar was never reveal to a British public that allegedly would have been shocked by the scorched earth tactics deployed by the SAS against the population. Ian Cobain, “Britain’s Secret Wars,” The Guardian, 8 September 2016, https://www.theguardian.com/uk-news/2016/sep/08/britains-secret-wars-oman.
5 Howard and Paret, eds., Carl von Clausewitz: On War, 188, 281.
desired on both a theoretical and historical level. In the book’s cases, Hazelton sets out to ascertain “the role of brute force and compellence in internal conflict involving a domestic ruler, a domestic armed challenger, and an intervener” (3). Hazelton bundles the conclusions into what she labels “compellence theory,” which is defined as “a violent process of state building in which elites contest for power, popular interests matter little, and the government benefits politically from use of force against civilians as well as insurgents” (4). Success in the cases she studies requires the counterinsurgent to repress the insurgent base rather than “providing public goods to the people to gain their support for the government” (2)—i.e., “bullets not ballots.” Rather than defining “successful counterinsurgency… (as) a process of building a centralized, modern, liberal, democratic state; providing political, economic, and social reforms intended to support such an effort; and providing public goods to people to gain their support for the government” (2) does, for instance, Andrew Gawthorpe, Hazelton argues that counterinsurgent success is contingent upon the cooptation of “armed and unarmed elites” (4–5). These include “warlords and other armed actors, regional or cultural leaders, and traditional rulers,” drafted so as “to gain fighting power and information about the insurgency” (5). Because these figures are the very people who invariably view a realignment of political power or more equitable redistribution of economic resources as a challenge to their positions, “brute force” (5) including incarceration, starvation, and even slaughter feature as daily specials on the menu of successful counterinsurgencies. In this way, starved of manpower and resources, insurgencies in action wither into a manageable threat (2–5).

Hazelton is quick to point out that she is not advocating “compellence theory” as a COIN formula. On the contrary, her purpose is simply to raise awareness of the political risks and high “human and moral costs” of embarking on irregular campaigns usually grouped under the deceptively anodyne title of “state-building,” which she defines as “the centralization and regularization of power” (101), minus Gawthorpe’s “legitimacy.” Although anchored in “modernization theory” (9) that suggests a promise of democratic reform and economic rejuvenation, “low intensity conflict” (as it was often called more-or-less prior to the attacks of 11 September 2001, and has been termed COIN especially after 2004 or so), invariably aims to crush opposition and consolidate the power of anti-democratic elites, not create a constituency for liberal democracy. Hence, if there is a “lesson” the author wishes to convey, it is simply that the intervening power must decide if the reasons for intervention—humanitarian, the requirement for reasons of regional stability to keep the incumbent government in power, and so on—are in the national interest (5).

Hazelton’s approach promises several benefits: first, by focusing on the brutal nature and authoritarian outcomes of “low intensity warfare,” she blows the cover on the myth-laden explanation for COIN success through “hearts and minds” and other allegedly benign humanitarian endeavors, political reforms, and economic redistribution as targeted propaganda calculated to reduce opposition to COIN operations. Second, Hazelton’s decision to eschew what she calls as “secondary literature” (19) in her case studies, results in archival research for several of her cases or, when possible, interviews with some of the participants. This is never a bad thing, as research at the source holds the promise of new perspectives that might challenge a generational consensus on issues in scholarship and policy. A third advantage that emerges from Hazelton’s archival and memorial plunge is that her study focuses on in-country actors rather than viewing the conflict from the perspective of the outside intervener. Hence, not only does she emphasize the moral hazards of COIN, but the book’s reminder that a conflict is not won or lost in Washington, London, or Paris, but on the battlefield, hopefully will, in view of the 2021 Kabul air-field debacle, short-circuit a reprise of the post-1975 debates over “who lost Vietnam.” Unfortunately, the polemic over the United States’ hurried—many argue “botched”—evacuation of Afghanistan already shows that this goal is probably out of reach.

---

What might be some of the hazards of Hazelton’s approach? To begin with, if “compellence theory” sounds eerily familiar, it is probably because Clausewitz defined war as “an act of violence intended to compel our opponent to fulfill our will.” So, even if the copyright of On War has reverted to public domain, the early nineteenth century strategist might still claim prior dibs on this concept about primordial violence, anger, hatred which lurks at the core of the more bureaucratic term “compellence.” Second, Hazelton’s observation that state-building is often a “nasty business,” (6) and that by playing backup, the intervening power is handing its client the whip hand (5-6), is not altogether original.8 “The reliance on proxies on the ground forfeits political control of the outcomes,” write Hew Strachan and Ruth Harris, especially when proxies lack capacity or fail to respect international law.9 Potentially, this muddles the Clausewitzian trinity of government (reason), armies (chance and probability), and people (passion) when the government supplies the passion for the war, while the public may oppose intervention for reasons of politics and morality. This forces the government to hide behind secrecy, subterfuge, and semantics that suggest an intervention below the threshold of war. To reduce “the play of probability and chance” inherent in a confrontation, the government may limit its military commitment to stand-off weapons, advisors or special forces. The problem with “the failure to be open about the use of force” is that it undermines national resilience, which weakens deterrence. It breeds distrust of the government among a public that may even “come to doubt the utility of force as a general principle,” which may pose an existential threat to the military.10

Hazelton’s eschewing of the “secondary literature on successful counterinsurgency campaigns,” (13) which in her view fails to ask the right questions, is too “data driven,” or takes counterinsurgent claims at face values, leads to a third problem. Military history in particular she dismisses as too “didactic, determined to deliver lessons rather than analytical or explanatory arguments, and much of the counterinsurgency literature falls within this bailiwick” (13). The source she cites for this statement, Frank Tallet’s 1992 book, War and Society in Early Modern Europe: 1495–1715, fails however to backstop her sweeping indictment. The work of military historians, as well as other “secondary sources” written by knowledgeable professional scholars, would have been valuable.11 Even more, a dismissal of the discipline of military history as “didactic” (13) is curious, in that it subverts the intellectual approach to the teaching of strategy in the author's own department of the Naval War College, whose Clausewitz-centered curriculum approaches military and strategic history within the broader context of foreign policy aims and allied strategic relations, national mobilization, and the war’s domestic social, political, and economic effects.

---

7 Howard and Paret, eds., Carl von Clausewitz: On War, 75.
8 Indeed, this reviewer’s memory of my time as a faculty member at the Naval War College in the 1990s is that the dependency of the intervening power on its in-country client was one of the central concepts emphasized in a Strategy and Policy curriculum.
11 In fact, Tallet specifically exempts the nineteenth-century founder of modern military history Hans Delbrück from this general assessment, before discussing the evolution of military history post-World War II. In his memoirs, the late Michael Howard offers an interesting view of the state of the profession when he was asked to take over the Department of War Studies at the University of London in 1950, and the “eclectic” nature of military studies’ cross disciplinary appeal. He was particularly influenced by the “War and Society” approach pioneered by the French annales school in the 1920s-1950s. Michael Howard, Captain Professor: A Life in War and Peace (London, New York: Continuum, 2006), especially chapter 7. Hazelton does not discuss or refer to the significant “military history” debate over COIN in Vietnam, and that served as a basis for criticism of US COIN endeavors in Iraq and Afghanistan. See, for instance, Gian Gentile, Wrong Turn: America’s Deadly Embrace of Counterinsurgency (New York: The New Press, 2013) as well as Porch, Counterinsurgency.
And far from “delivering lessons,” military and colonial historians have for some decades now pointed out that the West’s “civilizing mission”—the nineteenth century harbinger of “modernization theory”—was little more than a sales pitch that targeted Western vanity and sense of cultural superiority to provide cover for often brutal regimes of economic exploitation, cultural imperialism, and religious proselytization. After all, “population resettlement” and “reconcentration camps” became a controversial flash point for popular opposition to colonial warfare from Cuba to South Africa at the turn of the twentieth century. Such distinguished authors as Hannah Arendt and Elizabeth Hull have also pointed out that the tactics and the civil-military fusion of policy and ethos that were worked out in these racist and even genocidal “low intensity war” endeavors inevitably returned home with often catastrophic consequences. Military historians have written about how COIN compellence politicized British and French armies in the nineteenth and twentieth centuries. And United States Marine Corps (USMC) Major General Smedley Butler might be viewed as the harbinger of a trend that, in the wake of intervention of Iraq and Afghanistan, has tested the limits of Samuel Huntington’s “objective control” foundations of US civil-military relations. This century has seen soldiers and veterans become propagandists, or, openly attempt to sabotage policy decisions like International Security Assistance Force in Afghanistan commander General Stanley McChrystal, turn politically partisan like General Michael Flynn, be mobilized to face down demonstrators in Lafayette Park, or mustered to make up a significant component of the mob that attacked the Capitol Building on 6 January 2021. In the process, Senior Airman Ashli Babbitt became the Horst Wessel martyr of “compellence” in COIN.

Hazelton’s decision mostly to opt out of “secondary studies” and “literature on counterinsurgency” which must encompass a significant volume of recent military history, whose alleged shortcomings are to be found in its “administrative focus” and discounting of the “political realities of the campaigns,” (163 note 15) comes with costs to validity of the book’s generalizations about this form of conflict. Excellent histories of the Huk rebellion and Malayan emergency in the 1950s, not to mention the Greek Civil War in the 1940s, that place them in the context of civil wars precipitated by tensions exacerbated by World War II Axis occupation or Soviet interference, might have provided a broader perspective to Hazelton's work. Far from ignoring “political realities,” many of these studies adopt a more global history approach which argues that “…so-called Global and transnational ‘turns’ have long foregrounded flows of people, goods, and ideas across globalized space while downplaying traditional nation-centric modes of historiographical discourse,” as Anglo-American historian Andrew Buchanan writes. This view would argue that, beneath a stouthearted façade of imperialism or nationalism, most of these states were fragile social compositions of antagonistic classes and ethnicities which failed to withstand the traumas of the Second World War.

For instance, Great Britain’s failure to defend Southeast Asia in 1942 meant that at the Japanese surrender, Malaya had been cured of British imperialism. Indeed, had the Communists launched their revolution there in 1946, there is little that the British could have done to oppose it. But the Communists delayed because they lacked the resources and arms to do so.17 Likewise, although it eventually propped up partisans of the collaborationist Hellenic ancien régime, precipitous British intervention in Athens in 1944 rescued a very chaotic Greece from sharing the fate of Yugoslavia. Yet, as Buchanan writes, Washington’s belated attempt to send troops to bolster the Kuomintang in 1945–1946 was met “by a transnational wave of GI protests[….].] Largely airbrushed out of history, the largest strike-mutiny in American history was a rare historical moment when rank-and-file soldiers stepped forward as conscious actors. And while many simply wanted to go home, their desire for repatriation was infused with anti-imperialist sentiment.” This left US Secretary of State George Marshall to attempt unsuccessfully to broker a coalition government.18

A consideration of the contingent nature of all wars in general, and the period-specific character of these three cases in particular, might situate them as closing chapters of the long Second World War, which began arguably with the 1931 Japanese invasion of Manchuria and ended with the July 1953 armistice in Korea. In this expanded timeframe, the triumph of “resistance” in China and Yugoslavia had little to do with the enlistment of “elites,” but occurred largely as a result of Soviet intervention, facilitated by overly generous lend-lease aide, according to Sean McMeekin, and President Franklin D. Roosevelt’s decision to invite Moscow to join a war against a Japan that was already on the ropes.19 Fears lingered into 1947 of resistance aftershocks in France precipitated by communists in the “French Resistance” armed and mobilized largely by the wartime “interface services of the SOE and OSS.” Indeed, Buchanan argues that Roosevelt’s grand strategy in World War II, “required defeating the Axis powers while simultaneously preparing for a new world order of capitalist nations and free markets structured under the hegemony of the United States.” The alternative, in his view, was a return to inter-war autarky and/or revolution.20 In his excellent 2018 monograph, Andrew Gawthorpe explains that “state-building” held great appeal in post-WWII Washington, precisely because US policymakers likened it to expanding Roosevelt’s New Deal to the developing world.21 (In this context, the absence of a bibliography in this upper-echelon university press book is inexcusable).

A second challenge lies in Hazelton’s concept of “elites.” Indeed, the book’s definition of elites appears so broad and imprecise, that it forfeits explanatory power, a fact exacerbated by the refusal to employ good historical monographs on these cases or others where sound historians explore such elites in rich detail. Elites is a fluid concept, and Hazelton does not sharpen her argument by defining client states as well as insurgencies attempting to overthrow them as a “conglomeration of elites pursuing their own interests in tandem” (24). In fact, intervening counterinsurgent forces manufacture their own elites—the British appointed headmen in the Malayan Kampongs,22 while French and US intermediaries in South Vietnam were in the main minorities, such as Catholic carpetbaggers from Tonkin who rallied to the intervening power for protection and profit. Insurgent leaders may be aspiring elites who flame out because they fail to assemble a following or are knocked off by rivals. Military officers may fall into this category of elite collaborators, as the army is often the best organized entity in a developing country, one prioritized by the intervening power as the foundation of regime security. But indigenous militaries may be drawn from a narrow segment of society,

21 Gawthorpe, To Build as Well as Destroy.
22 Bayly and Harper, Forgotten Wars, 361.

© 2024 The Authors | CC BY-NC-ND 3.0 US
live in their own social or geographical space, and commit excesses which alienate the population. For instance, the 1933 revolution that overthrew the Cuban government was sparked by complaints of common soldiers over pay and promotion, and so acquired the title of the “Sergeants’ Revolt.” When political power is thrust upon military folk, the result may exacerbate political, institutional, and personal divisions, which dilute their security mission. Nor would relying on elites invariably provide security, as they are often made up of minorities or opportunists whose support is contingent so long as they continue to profit personally, as Hazelton acknowledges. Or they may be denationalized cosmopolitans who speak the language of the intervening power precisely because they have been educated or have spent many years abroad. A member of the elite with whom the intervening power is comfortable, like for instance Afghanistan’s Hamid Karsai, may have shallow roots in the polity and no popular following precisely because the conferral by the intervening power of elite status delegitimizes them. The intervenor’s elite may be viewed locally as a Quisling—a figure who had more traction in World War II Norway than had many of the elite figures analyzed in the study here. From this perspective, the Diem regime in South Vietnam appeared to replicate the “deal” struck in November 1942 with Admiral François Darlan, calculated to win the French High Seas Fleet as well as French North Africa to the Allies through elite recruitment. Roosevelt’s attempt to entice the Vichy French back into the war through a strategy of elite recruitment, which was labeled “Vichy engagement,” proved a total failure, in the face of General Charles de Gaulle’s crusade to build a base of support through popular resistance.

Hazelton argues that “compellence theory explains more than just the cases I analyze” (7). This may be correct, because “compellence” does not promote social “resilience.” Elites may resist “accommodation.” Like “insurgency,” “collaboration” may translate into an elastic concept, whose pliability may be dictated by ideology as much as self-interest. Collaboration might transition from involuntary “adaptation” to force majeur, through “cooperation” in the national interest, to outright treason and betrayal of one’s own population. While collaboration d’état plays out on an official level, “collaborationism” is a choice of individuals for reasons of politics, religion, clan, or family loyalties, a lesser of two evils, self-interest—even survival. Turning to more recent events, “the need to accommodate the few rather than provide benefits for all” (5) which characterized US policy in the Cuba of General/President Fulgencio Batista in the 1940s and 1950s and in South Vietnam in the 1950s and 1960s, failed to assure the survival of those regimes. Afghanistan in this century offers an example where that country’s president blamed the United States for having created the Taliban, and which he suspected was plotting to oust him from office in the manner similar to that of Ngo Dinh Diem in Vietnam in 1963. As a consequence, he banished those whom he considered too close to Washington from his government. Not surprisingly, the Afghan government with its rigged elections and extended networks of shady supporters, and frequently unpaid and desertion-prone national army and police folded like a house of cards in the face of a Taliban surge. This in turn explains why rather than become dependable collaborators, elites may often take the precaution of opening bank accounts in Switzerland, the Emirates, or the Cayman Islands.

Indeed, in his important study of World War II, Buchanan rightly questions the value of elite support in the absence of a mobilization of popular will. In so doing he echoes Clausewitz rather directly, and also in his emphasis on the shared responsibility of national resilience, which coalesces when society as a whole takes ownership of national defense.

Fighting quality is intertwined with an even more abstract quality, perhaps best described as “will,” writes Buchanan:

---

This intangible element combines top-level political and military leadership with broader moral factors, including popular political commitment, nationalism, resilience, and fear of defeat. The determination of the Soviet people to fight on in the face of the stunning defeats suffered during Barbarossa demonstrated collective will, as did the willingness of the British people to face large-scale bombing after the disaster at Dunkirk. Will is easily mythologized and packaged as national exceptionalism, but that does not mean that it should be discounted. The effects of its absence—in France in 1940, British-ruled Southeast Asia in 1942, and Italy in 1943—all underscore its significance. Nor, of course, was will unique to the allies. In Germany, popular determination to continue fighting was stoked by anti-Semitic propaganda after the intensification of Allied “terror bombing” in summer 1943, and in both Japan and Germany the war was sustained long after defeat had become inevitable. The outcome of World War II was determined by the complex interplay between economic determinism and contingent factors that included fighting quality and will.  

Hazelton’s case studies consist of a catalogue of repressive counterinsurgency measures directed against the population, followed by a failure to implement reforms, “which would severely constrain elite power” (62). In this way, she does not explore other contributory explanations for failures of these insurgencies beyond bringing together the elements of “compellence theory”—for instance, these might include overly ambitious political goals or unfortunate strategic decisions, like that of the Greek Communists to execute hostages from “reactionary” families as they evacuated Athens in 1945 which, according to the historian of Europe and especially of the Axis occupation of southeastern Europe, Mark Mazower, “destroyed the moral credibility of EAM/ELAS” and hastened the exit of non-Communists from the resistance; to boycott the March 1946 elections which catapulted the royalists to power; or the insurgent Democratic Army of Greece’s decision to shift to conventional warfare in 1947; to make the Peloponnese—a notoriously royalist area—a priority, and so on.  

The Tito/Stalin split in these years delivered the coup de grâce to the Greek insurgency, and serve to underline the globalized determinants of outcome. Insurgents in Malaya cited the 1947 defection of Communist Party Secretary Lai Tek and the failure of a “united front” strategy, as well as repression, and a lack of cash and weapons as a major reason for their failure. Both revolutionaries and counterinsurgents are often basically romantics whose strategies are anchored in flawed analysis compounded by incompetent tactics. But none of that figures in Hazelton’s binary repression/elite stonewalling of reform account, because the imponderable factor of contingency makes a poor fit with the predictive nature of “compellence theory,” in a way that is reminiscent of some studies of counterinsurgency. The military historian Hew Strachan argues that armies which are engaged in counterinsurgency operations become ensnared in the web of their own theoretical expectations, and so seek to apply an off-the-shelf solution rather than ask theoretical and policy questions about the type of war that confronts them.  

While Hazelton is certainly correct to argue that by intervening in these local civil wars, outsiders proffer the whip hand to their in-country clients, it is equally true that, without outside intervention, these indigenous regimes most probably would have quickly succumbed. The active engagement of the United States and its Western allies made these truly global wars subject to global influences and trends. As mentioned above, had the Communists in Burma and Malaya seized power in 1946, there was little the British, needing to demobilize their army to kick-start the economy, and facing turmoil in India, could have done about it. By

28 Strachan, The Direction of War, 208-209.
1950, more British troops were stationed in Malaya to fight a ragtag uprising of Chinese immigrant “squatters” than London had committed to defend the colony from the Imperial Japanese Army in 1942.\textsuperscript{29} Prime Minister Winston Churchill’s October 1944 decision to divert divisions from an already overstretched British Eighth Army in Italy to thwart a Communist power grab in Athens was heavily criticized at a time when fronts had congealed in Northern Europe and the Apennines, the Warsaw uprising had been crushed, and the Red Army had been temporarily driven out of East Prussia.\textsuperscript{30} Even so, had Washington not supplanted an exhausted Britain in the Greek civil war in 1947, thus offsetting support from newly communized Balkan governments for the Democratic Army of Greece, events there may well have taken a different turn. And while even at the time Anglo-American support of former Nazi collaborators in Greece was awkward, in the context of Cold War zero-sum Communist/“Free World” competition, failure to support “democracy” could have significant political repercussions which, in the views of the intervening power, made the “moral risks” a small price to pay—especially after Soviet dictator Joseph Stalin had demoralized the Communist Party of Greece (KKE) leadership by condemning the Greek uprising as contrary to Soviet interests.

The ethnic composition of insurgencies also limited their appeal—for instance, the Greek civil war was both an ideological and an ethnic conflict. According to some estimates, together with Communists and Greek refugees from Anatolia, Slavic Macedonians made up to forty per cent of the Greek insurgency, which allowed Athens to brand the insurgents not only as “Communists,” but also as reviled “Bulgarians.”\textsuperscript{31} The fact that the beating heart of the Malayan insurgency was immigrant Chinese wage laborers limited sympathy for their plight among Malays and Indians, not to mention Chinese diaspora communities, some of which dated to the fifteenth-century, who had become increasingly Creolized, identified with their emerging post-colonial nation-states, and found nothing particularly seductive about Beijing’s latest ideological export.\textsuperscript{32} The Kurdistan Worker’s Party (PKK) was a minority within a minority. In fact, Christopher Bayly and Tim Harper (an important source that Hazelton does not cite) argue that, rather than apply “compellence theory,” British counterinsurgency in Malaya boiled down to a series of ad hoc measures precisely because, beyond the recognition that the enemy was “communism,” the British never could decide, exactly, what they were fighting in Malaya—banditry, “communist terrorists,” an ethno-religious conflict as in Palestine (a popular analogy at the time), or a full-blown insurgency.\textsuperscript{33} Therefore, the frontier between death-squad and torture eliminationists and conversion via civic action “state-building” approaches was ill defined and simultaneous. As Clausewitz cautioned, there is nothing particularly theoretical or ideal about compellence in small wars in which tactics drive strategy, the “military virtues” of the army become attenuated by dispersion, the political objectives of a conflict vanish in random violence, and the morale of counterinsurgent troops invariably weakens over time.

Hazelton’s El Salvador case fits poorly in the book’s compellance framework. Arguably, the conflict in that Central American country that peaked in the 1980s seems to flip Hazelton’s script, in that Washington’s intervention bolstered the blowback from an attempt by a liberal political and ecclesiastical elite clearly intent on a profound political and economic restructuring of their country, and in the process kept the Salvadorian civil war churning for more than a decade. So, maybe El Salvador’s “troubles” can be seen as a clash of elites, although to categorize a claque of army officers, which in Latin America usually means men of modest, often

\textsuperscript{29} Bayly and Harper, Forgotten Wars, 239-240, 470.
\textsuperscript{31} Richard Clogg, A Concise History of Greece (Cambridge: Cambridge University Press, 2002), 139.
\textsuperscript{32} Wen-Quing Ngoei, Arc of Containment: Britain, the United States and Anticommmunism in Southeast Asia (Ithaca: Cornell University Press, 2019).
\textsuperscript{33} For the miscalculations made by the communists in Malaya, see Bayly and Harper, Forgotten Wars, 418-421, 436.
indigenous social origins, and their paramilitary thug allies as “elites” serves to empty the concept of its social dimensions at least, with implications for power dynamics.

El Salvador might demonstrate that attempts to modernize or radically transform historic relationships and the “moral economy” of a community by either side is likely to spark opposition. For instance, the post-1968 radical land reforms of General Juan Alvarado’s government in Peru aimed to shatter the hacienda system which allegedly locked a largely indigenous population into a form of social and economic immobility, and create an independent class of famers. In fact, by breaking up the hacienda system that was both an economic organization and a social safety net, Peru’s reformist government induced “social dislocation” by atomizing a part of Peruvian society that actually made it vulnerable to insurgent recruitment. In similar fashion, Gawthorpe notes how the “Land to the Tiller” program in Vietnam from 1970, which aimed to create a class of peasant proprietors, instead induced social dislocation and political tensions in the Mekong Delta and failed in its goal of creating a political constituency for the government of Vietnam. Modernization through land reform thus has at best a checkered record at creating social, economic, and political stability, and not just because it may alienate elites, however defined.

The ensuing civil war in El Salvador was certainly long on “compellence,” which featured serial military-sponsored and US enabled massacres and assassination squads, which only prolonged that brutal conflict, rather than resolve it. But the counterinsurgent strategy in El Salvador also contained a robust “civic action” component that included periodic elections, “self-defense groups” whose purpose is not so much to “defend” as to deny insurgents a potential recruitment pool, infrastructure improvements, and so on. Hazelton labels these efforts as largely ineffective because elites resisted them. The fact that violence dropped and that war termination culminated a comprehensive peace ensued that saw insurgents integrated into a political process, Hazelton abridges as “accommodations of rival elites,” especially the military. But “elites” in the military and the right-wing ARENA party were not so much “accommodated” as forced to accept a “humiliating” compromise peace that stripped of the army of its internal security role, allowed the insurgent Farabundo Martí National Liberation Front (FMLN) to transition into a political party that captured the Salvadorian presidency in 2009, and that tightened civilian control of the military precisely because popular support to continue the war had cratered.

The implication is that the war ended because of popular pressure, as well as war fatigue in Washington. Hazelton supports the book’s “elite accommodation” verdict with the conclusion that high-level cronyism and judicial corruption as well as political violence continue to characterize life in El Salvador. A weak state whose institutions are hobbled by the accelerated war-induced atomization of Salvadorian society manifest in increasing urbanization and emigration offers a partial explanation, as does a metastasizing drug trade since the 1990s, which itself has created a panoply of new “elites.” For sure, El Salvador isn’t Switzerland. But America’s “legal corruption” takes “elite accommodation” to stratospheric levels that Salvadorians could only dream of.

In short, Hazelton’s effort seems conceptually dubious and, given the book’s boycott of “secondary literature,” not à la page with trends historical research, at least. I and others have argued that war among the people invariably translates into war against the people. That outside security assistance turned Latin American militaries in particular into killing machines that menace their own populations is hardly an original observation. On the theoretical level, Hazelton’s book mostly endorses what Clausewitz wrote two centuries ago—that violence, hatred, anger and will lie at the core of conflict. It offers little of the historical context

35 Gawthorpe, To Build as Well as Destroy, 180-184.
which shaped the decisions of the outside power to intervene, or to calculate the fortunes of “compellence” in conflict that Clausewitz insisted, “is so continuously or universally bound up with chance.” Instead, Hazelton delivers a shallow, twenty-first century moral condemnation sheathed in a straightjacket of “alliance building among elites” (1).

36 Howard and Paret, eds., *Carl von Clausewitz On War*, 85.

I am honored to have scholars of this caliber review my book. I thank all of them for their time and the thought they put into their reviews. I also thank Andrew Szarejko for organizing this roundtable and the invaluable H-Diplo/RJISSF for publishing it.

Rather than addressing each point in each review in turn, I will focus on a few key issues, including one shared by two reviewers.

First, Jahara Matisek’s review provides a refreshingly forward-looking element to the roundtable. Matisek adds several cases that are worth exploring on their own merits and also in light of my findings. Huw Bennett, as Matisek writes, has already underscored how the successful British campaign in Kenya involved brutal British treatment of insurgents and civilians and the successful co-optation of elites.¹ Matisek notes the importance of future study of Russia’s campaign in the current Syrian civil war; its two recent campaigns in Chechnya, one failed and the next a success; and the failed Soviet intervention in Afghanistan. The list also includes Matisek’s own study of the Casamance conflict (1982-present) in Senegal.²

Matisek also provides compelling subjects for further research. He identifies, for example, the extreme degree to which average citizens play no role in my argument. As Matisek indicates, my focus is about “showing how much the government and elites can structure the way in which a conflict unfolds.”

I must add here that I was surprised by my finding that the views and interests of ordinary people played little or no role in the cases I examined. Matisek argues that the populace is the center of gravity in an insurgency. This is a question that itself demands further research. There is also work to do in identifying the factors and mechanisms through which elites persuade or compel civilians to accept brutal military treatment.

In terms of future research, Matisek points out the need for scholars to look at not only official government statements but also archives, newspaper accounts, and fieldwork interviews. He quite rightly argues that much work on counterinsurgency lacks due regard for “backroom policy discussions, facts, realities on the ground, and narratives.” The types of sources he mentions as examples enabled me to see that any reforms implemented in my cases took place after the military defeat of the insurgents. Government talk of reforms in these cases was for show.

Finally, in his review Matisek provides a useful list of intellectual forebears of my work. These include Stathis Kalyvas, David Armitage, Amartya Sen, Charles Tilly, Brian Downing, Miguel Centeno, and Victoria Hui. Matisek notes that my findings are strengthened by my drawing on a variety of intellectual traditions in the book.

Second in my list of replies to these thought-provoking reviews comes one striking similarity in the reviews by Asfandyar Mir and Douglas Porch. Mir criticizes a lack of specificity in my theory and an insufficiency of specificity in my empirical research. Mir argues that a theory without a specific list of which elites are going to be involved in any conflict, what specific accommodations these specific elites will accept, which specific

elites might be open to accommodation, and whether the government must bring in rivals or keep a coalition together is theoretically unclear and indeed insufficient.

Porch’s exhaustive and entertaining discussion of counterinsurgency sometimes touches down to add reflections on my book. Like Mir, he claims that my theoretical concepts are too vague. He also argues that they are too narrow. Porch singles out my definition of “elites,” which he argues does not include a variety of actors important in counterinsurgency wars. His review criticizes my inclusion of members of the Salvadoran military in the category of elites, while at the same time arguing that I do not include members of the government, insurgent leaders, members of the military, and Malayan community leaders as elites.

These arguments of both Mir and Porch are wrong. Mir’s is wrong because a typology is not intended to be a detailed map of reality.3 It is a tool. Typologies can help us identify and understand concepts relevant to the question at hand. They provide examples of types, as the word suggests. It would be impossible to collate a list of all individual roles that will be present in all counterinsurgency campaigns. One case may involve tribal leaders, another urban businesspeople, a third popular civic action groups, a fourth may involve all of them.

Kenneth Waltz’s call for elegant theories comes to mind here. An elegant theory is not intended to replicate reality, which is impossible and would provide no benefit anyway. An elegant theory is not a reflection of reality but an attempt to “concentrate on central tendencies, and to single out the strongest propelling forces.”4

Mir’s criticism is incorrect on methodological grounds and on factual grounds. Porch’s is wrong on factual grounds. I do in fact include the actors he claims are missing in my typology of counterinsurgent elites (16-17, 20-21). A brief list includes “those within the government, including the military; those outside the government but with power or influence, such as religious, social, political, and militia leaders; and elites within or aligned with the insurgency” (16). More specifically, this category includes religious, cultural, and community leaders, as well as those whose primary role is organizing and supporting violence, namely


4 Kenneth Waltz, Theory of International Politics (Reading, MA: Addison-Wesley Publishing Co., 1979), writes that “theory explains some part of reality and is therefore distinct from the reality it explains” (7). Waltz also argues that “Explanatory power … is gained by moving away from ‘reality,’ not by staying close to it. A full description would be of least explanatory power; an elegant theory, of most” (7). He adds, “No matter what the subject, we have to bound the domain of our concern, to organize it, to simplify the materials we deal with, to concentrate on central tendencies, and to single out the strongest propelling forces” (68).
entrepreneurs of violence. Elites include defectors, other sources within the insurgency, and social,
technological, and business leaders (16). Also, police, media, criminals, warlords, and political entrepreneurs.

In terms of accommodation, there are three types of elites and three types of accommodation. There are elites
in government, elites outside government, and elites in the insurgency. The likelihood of accommodation
depends on five factors: 1) whether elites see the insurgency as an existential threat, 2) whether they want to
stay unified to ensure continued great power support, 3) when they identify a need for specific
counterinsurgent capabilities, 4) when the insurgency is more interested in gaining a bigger slice of the pie
than in overturning the system, and 5) when neither counterinsurgent nor insurgent is primarily driven by
ideology. The likelihood of accommodation also depends on the characteristics of the actors involved. Four
characteristics enable accommodation: when elites see room for gain through cooperation; when both sides
are making relatively limited demands; when accommodation is relatively less costly than reforms; when
counterinsurgent elites do not see the insurgency as an existential threat. I hope readers turn to the book itself
for the logic of these choices.

Mir identifies his second concern as my dependent variable, counterinsurgency success. I use the US
Department of State’s definition in the *U.S. Government Counterinsurgency Guide* that was reportedly developed
by famed counterinsurgent David Kilcullen. On page 1, “I define success according to the *U.S. Government
Counterinsurgency Guide* as the ‘marginalization of the insurgents to the point at which they are destroyed, co-
opted, or reduced to irrelevance in numbers and capability.’”

My definition provides the lowest common denominator of success. It encompasses Mir’s suggestion that
some governments might only want to lower levels of violence, stabilize regions around a capital, or bring the
insurgency to the bargaining table. None of Mir’s examples represents a successful end to a counter-
insurgency conflict, which is my subject of study. The US Surge in Iraq was neither a success nor the end of
the war there that was waged by the US and its partners. The Soviet effort to stabilize the area around Kabul
was not the end of the Soviet Union’s war. The US effort to bring the Taliban to the table was not an end
state. Each of these examples feature the counterinsurgent’s effort to shift to a new stage in the war, one
which would play to its own advantage. None of these efforts were intended to end the war. None did so.

My use of the definition of success advanced by the US government and a prominent counterinsurgent puts
my findings on the same ground as the conventional wisdom that good governance defeats insurgencies. Had
I used a different definition, I might be accused of making an unfair or biased comparison between the two
theories of counterinsurgent success. The fact that my theory, which is based on the same definition of
success, provides more explanatory value than the conventional wisdom is a confirmation of the strength of
my findings.

Finally, Mir finds my work unconvincing in calling the conflicts I examine cases of limited intervention.
Unfortunately, Mir does not provide details on this matter, so it is difficult to know what to make of this

---

criticism. He does indicate that he would have liked to see a more detailed discussion of the universe of cases to strengthen the generalizability of my findings.

Porch’s review ranges across a remarkable, learned collection of wars without regard for type, time, international context, internal dynamics, or relationship to my argument. This historical hodgepodge is of great value to those interested in counterinsurgency because of its sweep. Its relevance to my theory and cases is less clear.

The criticism Porch’s review shares with Mir’s involves my definition of “elites.” Porch writes that the definition

appears so broad and imprecise, that it forfeits explanatory power, a fact exacerbated by the refusal to employ good historical monographs on these cases or others where sound historians explore such elites in rich detail. “Elites” is a fluid concept, and Hazelton does not sharpen her argument by defining client states as well as insurgencies attempting to overthrow them as a “conglomeration of elites pursuing their own interests in tandem.”

Porch concludes that my work replicates the work of Carl von Clausewitz. My book, he says, “mostly endorses what Clausewitz wrote two centuries ago—that violence, hatred, anger, and will lie at the core of conflict.” Porch adds that “the early nineteenth-century strategist might still claim prior dibs on this concept about primordial violence, anger, hatred which lurks at the core of the more bureaucratic term compellence.”

I am flattered by the comparison. However, my theory and analysis do not include the role of hatred and anger, or even primordial violence. My focus is on the threat and use of organized violence, compellence, to attain a government’s political objectives. There undoubtedly are books that analyze primordial violence, anger, and hatred, but Bullets Not Ballots is not one of them.

Rather than the great Clausewitz, my intellectual forbear is Thomas Schelling, the Nobel laureate economist and trenchant scholar of national security and foreign policy. His foundational analytical concept of compellence runs through my work. Compellence, Schelling says, is not about brute force, “the power to take and to hold.” It is about using or threatening to use force to achieve political ends. My own argument is that “the use of compellence (the use or threat of force to change an actor’s behavior) and brute force (the power to take and to hold) together breaks the challenger’s ability and will to fight” (2).

---

6 My definition of limited intervention is this: “In all of my cases, the great power believed that partner survival was an important security interest and was also unwilling to commit combat forces to preserve its partner’s rule. I made this choice for policy relevance. Cases of limited intervention are far more common than those involving occupation and thus more useful for informing policy choices” (23-24). I also explain that I examine the type of intervention when great power backs a client government facing an insurgency, an armed, organized, persistent, internal political challenge (3).

7 See pages 23-25 for my universe of cases and case selection criteria.


9 Porch insists that I “eschewed” and “boycotted” secondary sources. A glance at the endnotes will dispel this misunderstanding.


These reviewers provide perceptive and enjoyable comments on my book. I cannot fully express how much I appreciate their time and attention. They present what I hope is a compelling case for reading *Bullets Not Ballots*. 