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Author's Response to H-Diplo/ISSF Article Review by David Ucko and Jason Fritz of Jacqueline L. Hazelton, "The 'Hearts and Minds' Fallacy: Violence, Coercion, and Success in Counterinsurgency Warfare," *International Security* 42:1 (2017): 80-113.
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Response by **Jacqueline L. Hazelton**, U.S. Naval War College

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I welcome the H-Diplo/ISSF editors' selection of my *International Security* article, "The Hearts and Minds Fallacy: Violence, Coercion, and Success in Counterinsurgency Warfare," for review, and thank them for the opportunity to respond to the review by David Ucko and Jason Fritz. I appreciate the reviewers' attention to my work. The debate on counterinsurgency is divisive and sometimes even passionate. The policy implications of the leading prescriptions for successful counterinsurgency reflect this debate in their radical differences. They range from long-term liberal state-building, as the United States and its partners have attempted in Iraq and Afghanistan, to a primarily or purely military focus on killing or capturing insurgents and their enablers. Many people who research counterinsurgency hold strong views on what the correct response to insurgency should be, treating research as a normative question.¹ The lead author of the review of my work, David Ucko, has taken a very different approach to the study of counterinsurgency than I have. This difference is reflected in the review.

It is unfortunate that the reviewers fail to engage with my core argument that good governance as a solution to insurgency fails to explain even cases touted as examples. For those who did not read my article, I briefly summarize its three main elements before addressing the review.

¹ Examples of the debate include ISSF Roundtable 7-20 on *Wrong Turn: America's Deadly Embrace of Counterinsurgency*, 20 July 2015, <http://issforum.org/roundtables/7-20-wrong-turn> and Review Essay 22 on *Counterinsurgency: Exposing the Myths of the New Way of War*, <https://issforum.org/essays/22-new-way-of-war>.

Simply put, “The Hearts and Minds Fallacy: Violence, Coercion, and Success in Counterinsurgency Warfare” tests the explanatory power of the conventional wisdom that counterinsurgency success requires implementing good governance reforms in order to reduce or remove the grievances causing the insurgency. Reforms may include political liberalization, such as widening participation in governance, respect for human rights, or the distribution of public goods. The good governance approach also requires governments and their armed forces and associated fighters to take great care to prevent unnecessary harm to civilians. The strategic logic of this approach is that implementing reforms gains the government popular support and denies it to the insurgency, and avoiding harm to civilians prevents the creation of new insurgents.

In the article, I derived a causal theory from the existing literature since I could identify none that had been previously developed. I compared the predictions of the good governance theory to the processes causing success in cases that proponents of good governance counterinsurgency present as models. Surprisingly, given what one would expect from the literature on COIN, I found few or no systemic reforms that reduced popular grievances.² In no case did counterinsurgents gain popular support. In no case did the counterinsurgent government systematically prevent harm to civilians even at military cost. In all cases the counterinsurgent state succeeded and remained politically stable over the long term, and in all cases the counterinsurgent government was strongly supported by a liberal Western power, either Great Britain or the United States. These findings fundamentally challenge the governance approach to counterinsurgency success: If we are to see good governance defeat insurgency anywhere, we should see it in the cases proponents present as models: The Malayan Emergency; the Philippines campaign against the Huk; the Greek Civil War; the conflict in Dhofar, Oman; and El Salvador.

Also, my article tests a rival theory of counterinsurgency success: my coercion theory. It, and the good governance theory, provide opposing predictions about what we should see in cases of successful counterinsurgency. The coercion theory predicts success when the counterinsurgent government targets the insurgency directly with military force; when it targets the insurgency indirectly by cutting off insurgents from the resources they need to survive, often by using brute force to control civilians supplying the resources; and when it accommodates the interests of rival elites who provide the military power and intelligence necessary to target insurgents.³ This is a major new finding.

I identify three causes of success in the cases in the article (Malaya, Dhofar, and El Salvador). All of the cases align with the predictions of the coercion theory and contradict the predictions of the governance theory. The counterinsurgents in these cases succeeded through the use of brute force against civilians, without popular support, and without making reforms. Indeed, two key empirical findings are that the liberalizing reforms claimed in the secondary counterinsurgency literature were rarely implemented, and never fully implemented, and that the counterinsurgent governments did not gain popular support. My analysis draws on

² According to the predictions of the governance model, all three cases I cite in my article and all five cases I examined should be counterinsurgent failures.

³ Thomas C. Schelling, *Arms and Influence* (New Haven: Yale University Press, 1966), defines brute force as the ability to take what one wants, to keep what one has, “by sheer strength, skill, and ingenuity,” to “repel and expel, penetrate and occupy, seize, exterminate, disarm and disable, confine, deny access, and directly frustrate intrusion or attack.” See 1-2.

contemporaneous campaign documents, interviews with participants, the massive secondary literature on counterinsurgency, and research by scholars, including comparativists and historians.

Despite our differences, the reviewers and I agree on several basic points relating to the scholarship on COIN. Ucko and Fritz properly identify the core debate in the counterinsurgency literature as disagreement over the causes of success – political change versus uses of military force. We agree that the conventional wisdom on counterinsurgency success holds that the embattled government must gain popular support through reforms. We also agree on the need for more rigorous research on counterinsurgency.

What is lost in the review, however, is the fact that my argument moves beyond the political-versus-military dichotomy by identifying the political effects that uses of force supply to successful counterinsurgency campaigns, and that it moves beyond the reforms element of the debate by finding that in all of the cases that I studied, counterinsurgents promised reforms but rarely implemented them. It also moves beyond the focus on popular support by identifying a key variable as counterinsurgent elite coalition building.

The reviewers present three main criticisms of my article, none of which accurately identifies or discusses the findings of the article

The first set of criticisms involves theorizing about counterinsurgency. The reviewers complain that I create a “strawman” version of the good governance approach and unfairly compare it to the coercion theory in a false “dichotomy” (1, 2, 3, 4, 5, 6, 10).⁴ They argue that counterinsurgency in practice requires both coercion and good governance and thus it is difficult for a scholar to isolate either to show that it had a stronger effect in any given case (10). The reviewers rather oddly call it a flaw (10) that I identify the scope conditions for the theories I examine, even though doing so is normal scholarly practice. As I write, “neither the good governance theory nor the coercion theory explains all cases of counterinsurgency success” (95). In the article I specify that I examine only cases in which insurgencies rely on civilians for support (95) because these are the cases that governance proponents present as models. The reviewers also identify as a flaw (10) my distinguishing between the work of scholarship and that of policy prescription when I write, “I do not claim that governments must choose one theory or the other” (95). In fact my article specifically focuses on theories, declining to prescribe a strategy for any particular counterinsurgent or guidelines for counterinsurgents generally. A strategy is a logical linking of ends, ways, and means to address a specific problem. I offer no strategy in my article. What my piece actually does is lay out and test two theories of counterinsurgency success. Understanding this is crucial to understanding and evaluating the article itself.

The reviewers also claim that I do not provide a causal explanation for counterinsurgency success. In fact, I present a causal explanation for both theories (83-85, 89-93) and analyze the cases according to their predictions by tracing cause and effect (99-112). Further, although the reviewers argue that I do not examine

⁴ These assertions include: presenting my work as “unconvincing dichotomy” of two theories (3); claiming I ignore the variety of ways in which counterinsurgents win and lose (4); and that counterinsurgency campaigns must be “tailored” to the specifics of the situation (5), all of which imply that it is impossible to theorize about counterinsurgency success, making their concern about my theoretical choices puzzling. If it is impossible to theorize about counterinsurgency success then the alleged problems with my theoretical approach should be irrelevant. The reviewers also object to my analytical separation of the phenomena of government accommodation of elites and government efforts to gain popular support (4-5). But scholars must clearly identify and define variables to accurately evaluate them.

variation in levels of causal factors within each case, my goal of identifying the explanatory power of the two theories in fact required exactly what the article does: focus on identifying the presence or absence of the variables in question in the cases I examine.⁵

This set of criticisms severely marginalizes the role of theory in social scientific investigation. The purpose of building and testing theories is to understand what factors are most important in explaining the phenomenon of interest. The processes of social science, including attention to definitions and scope conditions, help scholars build knowledge upon knowledge to improve our understanding of the world. Theories help us see through messy and complex reality to identify, rank, and understand the interactions of causal factors. Simplification is a necessary element of building explanatory and predictive theories. No theory is going to explain any single case perfectly and there is, of course, no reason to expect it to do so.

The review is silent on the theoretical strengths of my research, including its utility as a strong test of the conventional wisdom on what defeats insurgencies.⁶ The article offers solid evidence that good governance theory fails the test, meaning that reforms and popular support have little or no explanatory power even in the very cases supporters present as models – where it should be most evident. While the review mentions “counterinsurgency theory” in criticizing the governance theory I present, there is no discussion of specifically what this theory is or where it may be found in the literature. The term is used repeatedly as a vague statement about a vague concept that deserves examination with greater rigor.

The reviewers’ second set of unsupported claims involves my case selection criteria and methods, which, they assert, cast doubt on my findings (5, 6). In fact my article is well grounded in standard social science methodology, as is clearly outlined in the text. This set of assertions stems from a lack of attention to that methodology. The reviewers do not mention that I lay out my universe, domain, and case selection criteria (94) or that the article also outlines the predictions I derive from each of the two theories and identifies the conditions under which each theory fails to explain campaign outcomes (89, 92-93).⁷ The article examines evidence on implementation of reforms, uses of force against civilians, accommodation of elites, and popular support for the government (82), tracing cause and effect through the cases. As I write, “Asking what governments actually did in their successful campaigns identifies choices overlooked or downplayed in most work on counterinsurgency” (94). The counterinsurgency literature consistently downplays government uses of force, particularly against civilians. It typically assumes that any reforms demanded by a counterinsurgent government’s great power sponsor are in fact implemented. It also routinely claims that the government thus gained popular support and thus defeated the insurgency. Evidence supporting these assertions is, also typically, thin to nonexistent. One of the strengths of my research is the volume of evidence that I present on

⁵ Scholars should identify what they expect to see in their research based on their theoretical choices, including any sequencing of events. I encourage researchers to further refine theories of good governance success to determine how much reform and popular support is enough and how much force is too much.

⁶ I created my strong test of the governance theory by making sure that its predictions are unique; that is, they are extremely unlikely to appear in cases if the theory is not in operation. I also made sure that the theory’s predictions are certain—if the theory holds, then its predictions must be evident.

⁷ It is important to identify the predictions that flow from theories. If one is not clear on what one expects to occur, then it is more difficult to ascertain the strength of the theory when examining reality.

counterinsurgent uses of force, targeting of civilians, and political effects, as well as my examination of promised reforms and any changes in popular support.

The reviewers further argue that I do not support my argument. Ignoring the 172 footnotes in the article, and the entire scholarly apparatus of the research, the reviewers rather self-defeatingly insist that the conventional wisdom must be correct ... because it is the conventional wisdom.⁸ The results of my research may well unsettle the conventional wisdom, but we cannot reject findings simply because they make us uncomfortable by demanding that we re-think the results of policies that we presume are not only successful but normatively correct. The goal of scholarship is to break new ground, after all.

In fact, my analysis of the evidence on reforms, uses of force, and popular support supports the predictions of the coercion model and cast doubt on the explanatory power of the governance theory. In all three cases, reports from the counterinsurgents themselves contradict the assertions of the reviewers. The counterinsurgents, including military and political leaders, explained that in each case the government neither made systemic reforms nor gained popular support. They also acknowledged that their uses of force against civilians helped weaken the insurgency (99-112). In the Malaya case, for example, authors of official, internal, contemporaneous British documents made reports from the theater of operations that cut against their interest in showing progress to London and cut against their beliefs about what successful counterinsurgency required (99-111).

In line with the predictions of the coercion theory, the British recognized that they were not making reforms. They had been planning for Malaya's independence since 1942, intending to create a pluralistic liberal democracy (105). Britain was forced by ethnic Malay elite recalcitrance, however, to give up its goal of equal rights for all residents (105). Ethnic Malay, ethnic Chinese, and ethnic Indian elites ultimately accepted formation of a state with special rights for the Malay sultans and ethnic Malay residents and limited rights for the ethnic Chinese and ethnic Indian residents who made up about half of the population (106), a ratification of the status quo ante. British authorities also believed that they needed popular support to defeat the insurgency and throughout the conflict they continued reporting that they were not gaining it. Yet they succeeded without it, as the coercion theory predicts.

When it comes to evidence regarding uses of force, reports from British officials in Malaya also confirm the predictions of the coercion theory. The officials reported that the use of aerial bombing and area weapons and forceful control of civilians broke down insurgent groups and drove them into the jungle where they could barely feed themselves, much less organize and plan attacks. There is little reason to disbelieve these officials' reports. Authorities were concerned about political damage from media reports on uses of force within Malaya. It is thus unlikely that they would overstate their uses of force or the outcomes they observed. These outcomes are as predicted by the coercion theory. Furthermore, insurgent leader Chin Peng himself confirmed the British analysis. He reported that the decline of the insurgency began in 1949, and attributed

⁸ The reviewers write, "following extensive research, there is a loose consensus on the tide having turned [in Malaya] by 1951 or 1952 ... Hazelton's counterclaim of 1949 being the turning-point therefore seems questionable" (7); "This achievement [in Malaya], the literature is quite clear, was the direct result of the Briggs Plan" (7); of Dhofar, they say, "other analyses of the case detail the efforts of Sultan Qaboos bin Said Al Said" to make reforms" (9); and on El Salvador, they say, "This interpretation fails to account for the reforms that did take place" (9).

insurgent defeat largely to the British cutting off insurgents from the populace by forcing civilians into prison camps (100).

Analyzing the success (or failure) of a counterinsurgency campaign requires looking at which reforms were actually implemented, as opposed to simply having been promised. The article examines government promises and implementation in all three cases (105-109) and finds that democratizing, liberalizing reforms were more often promised than implemented. If reforms are unnecessary to defeating an insurgency, as the coercion theory predicts, or if promises of reform suffice, then the conventional wisdom and policy choices based upon it must be considerably modified. The question of how much political liberalization, or promise thereof, suffices to defeat an insurgency is worth further investigation.

The reviewers' claims that reforms did indeed occur in Malaya, Dhofar, and El Salvador are undermined, since they cite as evidence certain "reforms" that occurred after the insurgency was defeated. Attention to one phrasing element of my argument – break the insurgency first, pay off elements of the populace later if necessary -- might have provided a more accurate account of my article, as would consideration of my definition of reforms (89) and how this definition may differ from theirs. Indeed, the reviewers fail to present their own definition of reforms, making it impossible to assess their claims.

There are several important corrections to be made. Significantly, the article does not in fact make the claims that Ucko and Fritz attribute to it regarding success in Malaya.⁹ Indeed, my theoretical and empirical arguments are the opposite of what they claim. I argue that uses of force against civilians and insurgents plus the political accommodation of rival elites together defeated the insurgency, as predicted by the coercion theory.

Also, nowhere do I say that elite bargains are "universally" more successful than gaining popular support (4). Nowhere do I provide policymakers with the choice of accepting savagery or not conducting counterinsurgency campaigns (2).¹⁰ The reviewers' moral condemnation ignores the evidence that liberal Western states have already conducted counterinsurgency campaigns using force against civilians without gaining the reforms they desired, as seen in Malaya, Dhofar, and El Salvador. What I do point out in my article, on the basis of my findings, is that attempting to use good governance to defeat an insurgency is likely to raise human, moral, financial, and economic costs without making success more probable (82), and that "Western policymakers should consider just how high the moral and human costs of a successful counterinsurgency campaign are likely to reach before choosing the path of intervention" (113).

⁹ The reviewers write, "Hazelton advances a thesis on the Malayan Emergency that builds on three claims: 1) that the Malayan National Liberation Army (MNLA) was defeated by 1949 through the indiscriminate use of force; 2) that no reforms were undertaken to win popular support; and 3) that success came instead through elite accommodation" (6-7) and, "This analysis proves that force alone did not break the insurgency: that is a mistaken lesson" (7).

¹⁰ "We believe that while the work is described as "explanatory" (92), it nonetheless posits a false choice: that policymakers either accept savagery or do not engage in counterinsurgency at all" (2). Charles Tilly, "Mechanisms in Political Processes," *Annual Review of Political Science* 4 (2001): 21-41, which is cited in the article, distinguishes between explanatory theories and prescriptive or programmatic theories.

Finally, in attempting to challenge my empirical finding that counterinsurgent uses of force against civilians contributed to success, the reviewers provide the unfortunate suggestion that militias help governments control civilians without harming them. They use Northern Ireland and Colombia as examples. This ignores significant evidence that government-sponsored and aligned militias contribute to political instability and increased violence.¹¹ Indeed, use of militias raises practical and ethical questions for governments because it “can have unexpected and unwelcome consequences including violations of laws of armed conflict, the undermining of governmental authority and the prospects of endemic internal strife and state collapse.”¹² Research on Northern Ireland finds that state-affiliated terrorist groups engaged in brutal human rights abuses with government assistance.¹³ In Colombia, “ordinary Colombians were often victimized—not protected by—paramilitaries. The armed groups displaced indigenous communities from their land, massacred civilians, and kidnapped political figures.”¹⁴

Throughout their review, Ucko and Fritz fail to engage with my core finding that good governance fails to explain counterinsurgent success in cases claimed as leading examples of the governance approach. They do not address fundamental questions about what constitutes reforms and what effective and ineffective government uses of force look like. They present familiar concepts from the counterinsurgency literature as critiques of my analysis without defining them or considering under what conditions each might become more or less salient. Nonetheless, the reviewers do make good suggestions for further research. They write, for example, of the need to mobilize networks to support the government and also co-opt elements of the populace (3-4). This series of ideas deserves clarification, definition, operationalization, and testing. Similarly, they urge authors to consider the need for both uses of force and good governance in successful counterinsurgency (10). I urge scholars to drill deeper into these familiar words and concepts to develop rigorous theories testing beliefs, assumptions, and empirical observations about counterinsurgency success.

¹¹ Kimberly Marten, *Warlords: Strong-Arm Brokers in Weak States* (Ithaca: Cornell University Press, 2015); Geraint Hughes, “Militias In Internal Warfare: From The Colonial Era To The Contemporary Middle East,” *Small Wars and Insurgencies* 27:2 (2016): 196-225; Mujib Mashal, “U.S. Plan for New Afghan Force Revives Fears of Militia Abuses,” *The New York Times*, 15 September 2017; Human Rights Watch, “Iraq: 2016/2017,” <https://www.amnesty.org/en/countries/middle-east-and-north-africa/iraq/report-iraq/>; Sabina C. Carey and Neil J. Mitchell, “Pro Government Militias and Conflict,” in *Oxford Research Encyclopedia of Politics*, published online October 2016, <http://politics.oxfordre.com/view/10.1093/acrefore/9780190228637.001.0001/acrefore-9780190228637-e-33>.

¹² Geraint Hughes and Christian Tripodi, “Anatomy of A Surrogate: Historical Precedents and Implications for Contemporary Counter-Insurgency and Counter-Terrorism,” *Small Wars and Insurgencies* 20:1 (2009): 1-35.

¹³ Bill Rolston, “‘An Effective Mask for Terror’: Democracy, Death Squads and Northern Ireland,” *Crime, Law, and Social Change* 44:2 (2005): 181-203.

¹⁴ Stephanie Hanson, “Colombia’s Right-Wing Militaries and Splinter Groups,” Council on Foreign Relations Backgrounder, 10 January 2008, <https://www.cfr.org/backgrounder/colombias-right-wing-paramilitaries-and-splinter-groups>.

Jacqueline L. Hazelton is an assistant professor of strategy and policy at the U.S. Naval War College. Her research interests include grand strategy, great power military intervention, asymmetric conflict, counterinsurgency, terrorism and counterterrorism, and U.S. foreign and military policy.

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