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Introduction by Hal Brands, Johns Hopkins University School of Advanced International Studies (SAIS)

It has become accepted wisdom in Washington, D.C., and among many international relations scholars, that East Asia is a region rife with geopolitical rivalry, and that the United States and China are destined for protracted great-power competition and perhaps conflict. In his newest book, David Kang offers a sharply contrarian viewpoint. He argues that East Asia is far more peaceful and stable than it seems, that China's rise is not particularly threatening to most East Asian states, and that the United States will only find itself in an intense competition with China if it makes unwise policy choices that turn that danger into a self-fulfilling prophecy. The United States should, therefore, refrain from the temptation to double-down on its East Asian security commitments and efforts to check China's rise; it should adopt a more restrained posture that minimizes rather than embraces great-power competition.

In multiple ways, Kang's arguments turn conventional thinking about East Asian security and Sino-American relations on its head, and it is not surprising that these arguments invite spirited and diverse rejoinders from the three reviewers in this roundtable. Peter Dombrowski offers the most positive appraisal, broadly accepting Kang's core arguments and noting that his book may provide policymakers with a way of finding off-ramps from escalating competition. Evan Montgomery and Zack Cooper are more critical, arguing that Kang understates the fears being stimulated by China's rise, misses the diplomatic and economic steps East Asian countries are taking to hedge against Chinese assertiveness, underplays the more troubling aspects of Chinese behavior (such as island-building), and obscures the potentially destabilizing consequences of American retrenchment. Cooper also offers a methodological critique, arguing that Kang never establishes clear observable implications of his argument, which makes that argument difficult to test empirically.

In a highly constructive response, Kang seeks to move the debate forward by identifying areas in which political science as a discipline has so far failed to provide much guidance in understanding what is happening in East Asia. He notes that the discipline has not developed useful theories of threat perception, that it is guilty of a "looseness" regarding the much-used but little-defined concept of "free-riding," and that it remains difficult to apply rationalist theories—such as the idea of "costly signaling"—to specific empirical cases. He suggests the need for further research on all of these issues. In advancing provocative and creative arguments, and in pushing scholars to dig deeper on these important issues, Kang's book performs a valuable service in energizing the debate on what is happening in East Asia and what the American response should be.

Participants:

David Kang is Maria Crutcher Professor of International Relations at the University of Southern California, where he also directs the USC Korean Studies Institute.

Hal Brands is Henry Kissinger Distinguished Professor of Global Affairs at Johns Hopkins-SAIS, senior fellow at the Center for Strategic and Budgetary Assessments, and a Bloomberg Opinion columnist. His newest book is *The Lessons of Tragedy: Statecraft and World Order* (Yale University Press, 2019), with Charles Edel.

Zack Cooper is a research fellow at the American Enterprise Institute, where he focuses on Asian security. He previously worked at the Center for Strategic and International Studies and the Center for Strategic and

Budgetary Assessments. He also served as a staffer at the White House and the Pentagon. His completed his Ph.D. at Princeton University's Woodrow Wilson School.

Dr. Peter Dombrowski is Professor of Strategy, Strategic and Operational Research Department, U.S. Naval War College, Newport, Rhode Island. His most recent book *The End of Grand Strategy: US Maritime Operations in the Twenty-first Century* (Cornell University Press, 2018) was co-authored with Simon Reich.

Evan Braden Montgomery is a Senior Fellow and the Director of Research and Studies at the Center for Strategic and Budgetary Assessments. He is the author of *In the Hegemon's Shadow: Leading States and the Rise of Regional Powers* (Cornell University Press, 2016), as well as articles in *International Security*, *Security Studies*, and the *Journal of Strategic Studies*.

Review by Zack Cooper, American Enterprise Institute

Aiming without Arming: Explaining Free Riding Among East Asian Allies

David Kang's *American Grand Strategy and East Asian Security* is centered around a confounding puzzle: Why aren't East Asian states spending more on their milibtaries? Kang's explanation, in short, is that "few countries fear for their survival" (5). This argument is original and counterintuitive. If correct, it would overturn the conventional wisdom that the security situation in East Asia is deteriorating.

American Grand Strategy and East Asian Security will be required reading for many students of contemporary politics in Asia, but in this reviewer's opinion, Kang's main argument is unpersuasive. Kang makes a convincing case that few East Asian states are building up their militaries, but this does not imply that regional states are comfortable with the emerging security situation. Instead, East Asian states are aiming without arming (to paraphrase Stephen Cohen and Sunil Dasgupta).[1] In other words, most East Asian leaders are worried about China's rise, but avoiding substantial increases in military spending.

Why might states aim without arming? One reason is that arming will not change the fundamental balance of power. Indeed, none of China's neighbors can hope to match its military modernization. Given China's massive military edge, additional defense spending by regional states has limited value. Instead, these states' best hope is that the United States will intervene if a crisis or conflict occurs. This explains why Kang finds "no evidence that a balancing coalition is forming to counter China's rise" (191).

Indeed, most regional states will maintain political relations with China as a hedge, in case the United States forgoes its traditional role as security provider. Kang himself notes, "The greatest risk for East Asian countries would be that they embrace a new Trump doctrine only to see Trump change his mind" (175). Thus, as Indonesian Foreign Minister Retno Marsudi says, "We will wait until President Trump releases his real policies… We will determine our stance once he's made up his mind" (160). Until then, East Asian states will rely on U.S. security guarantees and avoid military spending that could draw the ire of leaders in Beijing.

Freeriding Flourishing

Kang sets out to explain why states in East Asia spend so little on their militaries. Given the familiar refrain about China's rapid rise, military modernization, and increasing assertiveness, one might expect high levels of defense spending. After all, Kang argues, if regional states are severely threatened by China, they should send costly signals of their intention to defend themselves. Instead, he finds that "almost all countries in the region see their security environment as relatively benign" (4-5).

The argument that regional states see their security situation as benign will surprise many regional experts. That his book goes against the conventional wisdom is to Kang's credit, but it raises the evidentiary requirement to prove his hypothesis. In this reviewer's opinion, *American Grand Strategy and East Asian*

[1] Stephen P. Cohen and Sunil Dasgupta, *Arming without Aiming: India's Military Modernization* (Washington, D.C.: Brookings Institution Press, 2010).

Security provides a convincing argument that regional military spending is surprisingly low, but is unconvincing about the cause.

As noted above, Kang's explanation for low military spending in the region is straightforward: most East Asian states are not particularly worried about China. Yet, this argument conflates low military spending with an absence of concern. An alternative explanation is that spending by regional militaries would be unlikely to change the outcome of a conflict, which China would likely win unless the United States intervenes. Kang explicitly acknowledges this possibility when he writes that "while some countries may be modernizing and expanding their navies, the rate and scale at which this occurring is so small as to make no realistic difference in a maritime dispute with China" (41).

If more military spending by China's smaller neighbors would not change the military balance, why should they bother? Kang argues that weaker powers can win asymmetric conflicts, so military spending should still have value. But the literature on asymmetric conflicts seldom assesses situations in which a state can buckpass by relying on a larger ally like the United States.[2] Would it not be smarter for regional states to attempt to convince the United States to intervene, or to bandwagon with Beijing if Washington forgoes intervention? This is classic buck passing behavior, often practiced by smaller states in bipolar systems.

In short, facing China's growing might, East Asian states have an incentive to free ride on the United States. Kang acknowledges low levels of military spending by U.S. allies and partners, but he finds "little evidence of free-riding by American allies in the region" (187). This is a surprising verdict. After all, what would constitute evidence of free riding other than low defense spending by U.S. allies?

Kang's answer is that regional states do not want more U.S. commitment. He argues, "the hypothesis that East Asian states are mainly or solely relying on external balancing relies heavily on a counterfactual: if the United States weren't forward deployed in East Asia, then East Asian states would feel more insecure and thus would spend more on their militaries" (165). However, this need not be the case. Regional states might simply bandwagon with China if the United States were not present. After all, most regional states are so weak compared to China that their only realistic hope for military success is U.S. military involvement.

Kang himself acknowledges this possibility when he notes, "If a smaller power fears abandonment, it may be very cautious about making any firm *ex ante* commitment to a larger power. If the larger power can abrogate its responsibilities at any time, the smaller power can very likely be left *ex post* having annoyed a third party, but with no alliance support from its larger ally… As a result, Asian countries are likely to be highly cautious about joining an American-led coalition" (117). The implication of this statement is that free riding is logical for East Asian states. Kang notes that, "If East Asian states were going to compete with China and attempt to keep up, they would have started long ago" (32). East Asian states are so far behind now that there is little incentive to try to catch up. But that does not mean that they are comfortable with China's rise.

[2] See, for example, Patricia L. Sullivan, "War Aims and War Outcomes: Why Powerful States Lose Limited Wars," *Journal of Conflict Resolution* 51:3 (2007): 496-524; Ivan Arreguin-Toft, "How the Weak Win Wars: A Theory of Asymmetric Conflict." *International Security* 26:1 (2001): 93-128; Andrew Mack, "Why Big Nations Lose Small Wars: The Politics of Asymmetric Conflict," *World Politics* 27:2 (1975): 175-200.

Quibbling over Case Studies

In seeking to prove his argument, Kang reviews the behavior of North Korea, South Korea, the Philippines, Vietnam, Japan, Australia, Indonesia, and Singapore. These sections are good reads, particularly those that address the Korean Peninsula. Kang provides a convincing account of the increase in North Korean spending and the continuing focus of South Korea on North Korea, rather than China. The other cases, however, are open to nitpicking. In particular, Kang never establishes a specific set of observable implications that flow from his argument, choosing instead to search for different types of evidence in different cases. This opens the case studies to criticism of cherry picking.

Take, for example, the Philippines. Kang argues that "There is little evidence that [the Philippines] is free-riding on the U.S.-[Philippine] Mutual Defense Treaty" (112). In the next sentence, however, he notes that "The Philippines has no military to speak of, and has spent a generation reducing that funding further" (112). Kang attempts to square this circle by arguing that "the Philippines desires a reduced military relationship with the United States" (95). He therefore concludes that "the Philippines is not engaged in chain-ganging, free-riding, or external balancing" (100).

A simpler explanation exists, one which in this author's view is more convincing: the Philippines desires a stronger U.S. commitment but does not believe it will get one. Under the Benigno Aquino administration, the Philippines tried repeatedly to clarify U.S. security guarantees, failing most dramatically in 2012 during the Scarborough Shoal standoff. This, in itself, is evidence that the Philippines was seriously worried about China. Yet, Kang does not address this incident in detail. Although he says "this chapter pays particular attention to the administration of Benigno Aquino" (94), most of his evidence comes from Rodrigo Duterte. By the time Duterte came to power, U.S. support appeared limited, so Philippine leaders felt they had to bandwagon.

Japan is another questionable case. Kang argues that Tokyo is not spending more on its military because it is not particularly worried about China. He insists that "there is little evidence, other than [Prime Minister Shinzō] Abe's pronouncements, that Japan is in any position to return to a position of leadership in East Asia" (143). Yet discounting the statements of the Abe administration ignores the clearest signs of regional concern about Chinese behavior. There is in fact ample evidence of growing Japanese concern about China. It is not clear why one would pay such close attention to Rodrigo Duterte's erratic statements but ignore the consistent messages voiced by Shinzo Abe and other Japanese leaders.

Vietnam is yet another surprising case. Kang uses the number of visits by senior American and Vietnamese leaders to the opposite capital as a proxy for concern about China. He notes that U.S. leaders visit Asia less frequently than Europe or the Middle East, suggesting "This demonstrates that despite the intensified speculation about the instability in the South China Sea, counterterrorism and coalition with NATO have continued to be the top priority of national and defense leaders of the United States" (129). Kang suggests that the imbalance in visit numbers is evidence that Vietnamese leaders are not worried about China. But are visits truly costly signals? Vietnam's strategic documents and public statements expressing concern about Chinese activity in the South China Sea surely are better sources. Recent U.S. ship visits and arms sales to Vietnam do not receive the attention they deserve.

In short, Kang's case studies are uneven. Decisively proving the thesis of the book would clearly identifying and consistently measuring observable implications. Instead, Kang presents different data from different cases, leaving unexamined much evidence that would counter his central argument.

Policy Implications: More Calls for Burdensharing?

Where does this leave U.S. policymakers? Many leading scholars on Asia have suggested that the United States should engage more deeply in Asia. Take, for example, recent books such as Kurt Campbell's *The Pivot*, Victor Cha's *Powerplay*, Michael Green's *By More than Providence*, or Thomas Christensen's *The China Challenge*, all of which advocate for a continued (and often increased) U.S. regional economic and security commitment to Asia.[3] Each of these books argues that regional challenges are worsening, requiring more U.S. attention and resources.

Kang, on the other hand, suggests that Washington should be minimalist in its approach to Asia. He believes that a more hardline strategy of "containment and confrontation of Chi[n]a is unlikely to find many supporters in East Asia" (189). Instead, Kang suggests that the United States should essentially accept that a power transition is underway. Advocacy for restraint is frequent in U.S. policy circles, but infrequent among U.S. experts on Asia, so Kang's book will stand out from the crowd.[4]

The 2017 National Security Strategy calls for allies and partners to "contribute the capabilities, and demonstrate the will, to confront shared threats."[5] Although Kang's recommendations stand in direct opposition to the Trump administration's recommitment to "great power competition," this book's longest lasting contribution may be to jumpstart academic debate about the limits of burdensharing by U.S. allies and partners.

[3] Kurt M. Campbell, *Pivot: The Future of American Statecraft in Asia* (New York: Twelve, 2016); Michael J. Green, *By More Than Providence: Grand Strategy and American Power in the Asia Pacific Since 1783* (New York: Columbia University Press, 2017); Thomas J. Christensen, *The China Challenge: Shaping the Choices of a Rising Power* (New York: W.W. Norton, 2015).

[4] On restraint, leading voices include Barry R. Posen, *Restraint: A New Foundation for US Grand Strategy* (Ithaca: Cornell University Press, 2014); Paul K. MacDonald and Joseph M. Parent, "Graceful Decline? The Surprising Success of Great Power Retrenchment," *International Security* 35:4 (2011): 7-44.

[5] The White House, "U.S. National Security Strategy," 2017, https://www.whitehouse.gov/wp-content/uploads/2017/12/NSS-Final-12-18-2017-0905.pdf.

Review by Peter Dombrowski, U.S. Naval War College

With the release of *American Grand Strategy and East Asian Security in the Twenty-first Century*, David Kang swims strongly against the current of conventional wisdom in Washington. While many, if not most, of Washington-centric national security specialists view East Asia as a volatile region chock full of conflicts and potential crises requiring American management and even military intervention, Kang views the region as stable and even trending toward peace. He explicitly compares it to South America, a region that most American strategists view as a backwater, aside perhaps from the perpetual rub of illegal narcotics trafficking and simmering insurgencies.

Professor Kang is well armed and well positioned to take on the conventional wisdom. He is the author of five well-received single authored or co-authored books and dozens of scholarly articles, chapters, and other professional writings. While he is a specialist in the Korean peninsula,[1] he has contributed enormously to our thinking about several other East Asian countries—China, Japan, Vietnam, and the Philippines among others—and the entire region as a distinct whole.[2]

Given the policy and political stakes involved in getting East Asia right, Kang will need every ounce of his credibility and scholarly firepower if he is to influence how Washington views the region and constructs its grand strategy. Many experts and many institutions have far too much at stake in the seething cauldron trope of Asia to take a more nuanced analysis sitting down. Besides, in addition to the material, ideological, and institutional interest Kang skewers, he also takes a strong position relative to some of the most contested debates among International Relations scholars and those of a more comparativist bent. Unlike traditional Realists, he clearly believes in the importance of domestic politics, economics, and institutions as drivers of grand strategy, military strategy, and diplomacy. Moreover, as his chapters on the Korean peninsula and Vietnam demonstrate especially convincingly, he is mindful of history, traditions, and culture. His analysis of contemporary strategic choices is informed by his understanding of how the bilateral relations between Korea and China and Vietnam and China temper the contemporary strategic calculus of South Korea, North Korea, and Vietnam.

The foundation of Kang's empirical and contemporary historical analysis is determining whether the region is in the midst of arms race by examining how military expenditures affect, and are affected by, economic growth (31-59). He organizes a large amount of budgetary data by country to demonstrate comparatively (both within the region and vis-a-vis other regions) that despite the hype no such race is occurring by standard measures. Further, he debunks the notion that greater economic growth rates allow and encourage East Asian states to spend more on national defense. Finally, Kang takes the time to examine the types of weapons and naval systems acquired by countries in the region. The figures and tables convincingly

[1] David C. Kang, *Crony Capitalism: Corruption and Development in South Korea and the Philippines* (Cambridge: Cambridge University Press, 2002).

[2] David C. Kang, *China Rising: Peace, Power, and Order in East Asia* (New York: Columbia University Press 2007), and David C. Kang, *East Asia Before the West: Five Centuries of Trade and Tribute* (New York: Columbia University Press 2012).

demonstrate that even the domain-specific fears of international security specialists and American strategists are largely unfounded.

A slightly less successful dimension to Kang's scholarship is his reliance on rationalist theories of war and conflict (especially bargaining models explained in chapter 2) to explore intra-regional relationships and the bilateral diplomacy the United States shares with individual East Asian states. The weakness of the argument is mostly empirical and appear most often in his case studies of North Korea (chapter 4), South Korea (chapter 5, the Philippines (chapter 6), and Vietnam (chapter 7). He makes an excellent case for his interpretation of the signaling and costly (or not) commitments made by the national leaders in each country. In addition, Kang marshals some interesting, even novel, data—for example, a database and timeline of state visits (127-129) made by Vietnamese officials with the United States, China, and Vietnam's neighbors—to demonstrate overly optimistic American interpretations of Vietnam's interest in strengthening bilateral security relations with the United States. Rather than balancing against the Chinese threat, Vietnam is carefully resisting American entreaties and making certain to keep China informed and at the forefront of its foreign relations. This said, for non-specialists in the individual countries, Kang's account might be a bit too one-sided to be entirely persuasive. To truly solidify the argument, Kang would have needed to spend more time exploring and, if possible, discrediting alternative interpretations of Vietnamese (and other states in the region) behaviors. This, of course, is too much to ask in a single monograph dealing with such a large and complex region, so Kang cannot be faulted. But we can look forward to other scholars exploring his interpretations with more empirical detail.

In my opinion, Kang makes a valuable contribution to the literature on American grand strategy beyond what he intended with this book. Implicitly, he stands with those who (1) examine the strategic challenges and threats on a region-by-region and state-by-state basis[3] rather than some grand theory of how state operate within the international system—Realism, Liberalism and so forth[4]—and (2) who suggest that American grand strategy is not one-size fits all.[5]

[3] For discussions of U.S. grand strategy toward a single country or region see, for example, Robert D. Blackwill and Ashley J. Tellis, eds., *Revising U.S. Grand Strategy Toward China* (New York: Council on Foreign Relations, 2015) or Michael Green, *More Than Providence: Grand Strategy and American Power in the Asia Pacific Since 1783* (New York: Columbia University Press, 2017).

[4] Recent and influential books that derive U.S. grand strategy, explicitly or implicitly, from the main traditions of International Relation theory include Stephen G. Brooks and William C. Wohlforth, *America Abroad: Why the Sole Superpower Should Not Pull Back from the World* (New York: Oxford University Press 2016); Eliot Cohen, *The Big Stick: The Limits of Soft Power and the Necessity of Military Force* (New York: Basic Books; 2017) and Barry R. Posen, *Restraint: A New Foundation for U.S. Grand Strategy* (Ithaca: Cornell University Press, 2015). Historians studying American grand strategy, of course, are far less prone to putting deductive theory before the evidence. An excellent example is Hal Brands, *What Good Is Grand Strategy?: Power and Purpose in American Statecraft from Harry S. Truman to George W. Bush* (Ithaca: Cornell University Press, 2014).

[5] Simon Reich and Peter Dombrowski, *The End of Grand Strategy: US Maritime Operations in the Twenty-First Century* (Ithaca: Cornell University Press, 2018), and Christopher Hemmer, *American Pendulum: Recurring Debates in U.S. Grand Strategy* (Ithaca: Cornell University Press, 2015).

American Grand Strategy and East Asian Security in the Twenty-first Century should be required reading for American policy makers and military leaders charged with formulating and implementing strategy in East Asia. It will serve as a useful counter-narrative to the near hysterical threat-mongering common in many blogs, journals, and public fora. If Kang's account is accurate—and personally I have little reason to doubt its main thrust—viewing the region as a tinderbox awaiting a spark that might drag the United States into a regional conflict is a serious mistake. East Asia, for all its political, economic, and security challenges, is not the Balkans circa 1914. The main regional players are not poised for war. They are not seeking outside allies in hope of pushing them towards war in order to serve their own particularistic security and national interests.

Kang's analysis will also go a long way tempering American expectations about the behavior of its allies, partners, and even adversaries in the region. If the United States has concerns about the direction of China's economic, military, and political rise, it is from the position of a global superpower responding to a rising great power and maintaining its superior position. The United States is not altruistically defending state sovereignty, the maritime commons, or the long-term economic interest of the individual states, regional economic patterns, or even the overall health of the global economy. By Kang's account, East Asian states have long histories with China and their own means of accommodating China's rise. They have checkered histories not just with China but other regional powers like Japan and even, or most especially, the United States. Their national policies are not dictated by Washington nor are they intended to make Washington comfortable. Lower expectations, in terms of shared threat perceptions, military commitments, and burden sharing, will allow American strategists to adopt a more achievable set of bilateral and regional objectives. It may even allow American political leaders to develop off ramps from confrontation that will serve U.S. national interests and to avoid blundering into a conflict that few really want.

Review by Evan Braden Montgomery, Center for Strategic and Budgetary Assessments

Few issues are more important for contemporary international relations than U.S. grand strategy in East Asia. Over the past decade, Washington has been reevaluating and reorienting its policies toward the region as a whole and the People's Republic of China (PRC) in particular. Not only has it elevated the prominence of East Asia in policymaking after being consumed by events across the Middle East, but it is also beginning to embrace the idea that Beijing is more of a strategic competitor than a strategic partner.

Washington's recent emphasis on East Asia and its concerns about the PRC are not surprising. The region's economic and strategic relevance have steadily increased over time, while Beijing is an emerging great power that poses significant challenges to both its neighbors and the United States. These developments have fueled a long-standing debate over whether U.S. interests are best served by a grand strategy that prescribes close ties with allies, persistent efforts to maintain freedom of the global commons, and an enduring commitment to prevent the domination of key regions, or one that proscribes this type of engagement and intervention until it becomes unavoidable.

David Kang's new book, *American Grand Strategy and East Asian Security in the Twenty-First Century*, offers more ammunition for those who favor the latter approach. According to Kang's assessment, East Asia is extremely stable, local states have little to fear, and Washington should scale back its involvement in the region rather than binding itself more closely to allies and adopting a more competitive posture toward Beijing. In fact, with the notable exceptions of the PRC and North Korea, local states are devoting so few resources to national defense – and appear so reluctant to pursue their interests via military means or to escalate potential crises – that East Asia does not look very different from Latin America in this light.

Specifically, Kang notes that despite China's growing power (as well as North Korea's enduring antagonism), most states in the region are not augmenting their military forces in ways that suggest they perceive significant threats to their territory, interests, or autonomy. Rather, they are pursuing "comprehensive security," which consists of "a wide range of diplomatic, institutional, and economic strategies—as well as military strategies—in their dealings with one another" (4). The pursuit of comprehensive security, Kang argues, cannot be explained by the considerable relative power gap that exists between the PRC and its neighbors or by the desire of smaller states to free-ride on a stronger security provider like the United States (24-29). Instead, it reflects the absence of any existential dangers. Moreover, because states do not seriously fear for their safety, Washington is unlikely to find support for a more competitive approach toward Beijing, especially since local states want to avoid choosing sides between the two (189).

American Grand Strategy and East Asian Security in the Twenty-First Century represents a rare blend of area studies, international relations theory, and policy analysis. Not only does it shed light on defense spending trends across the region, it also focuses more narrowly on the complex set of factors that can inhibit local states from standing up to the PRC on their own and that make serious external balancing outside the bilateral, hub-and-spoke U.S. alliance network a difficult proposition – observations that proponents of more forward-leaning grand strategy options need to grapple with. Nevertheless, the book does have several limitations which cast some doubt on its conclusions.

First, Kang's notion of comprehensive security is underdeveloped. According to his account, East Asian states have clearly opted for this approach since they do not rely on military tools alone in pursuit of their interests. But all actors on the international stage employs a combination of diplomatic, economic, and institutional measures to manage the threats they face. In essence, comprehensive security appears to be synonymous with grand strategy: using all the tools of national power to enhance security in the context of resource constraints and adaptive competitors. Drawing such a sharp distinction between full-blown military balancing and almost any other approach risks creating a strawman argument and makes comprehensive security difficult to debunk.

Second, Kang's approach to assessing security strategies obscures genuine fears on the part of local states and overlooks examples of balancing behavior that are taking place. To determine whether East Asian states are competing militarily or adopting comprehensive security instead, Kang looks first and foremost to defense spending trends. Applying the logic of costly signaling, he argues that defense budgets across the region, which remain relatively low as a percentage of gross domestic product, indicate that states are not resolved to fight over existing disputes and therefore must not think their security situations are precarious (32-46). In general, the costly signaling framework offers a useful lens for understanding when and how states communicate their motives to external audiences, as well as the escalatory implications of attempting to demonstrate resolve. Because overall defense spending trends do not capture what states actually do with their resources, however, they are a problematic indicator of state preferences. After all, states could increase spending in ways that are not directed at external threats or, as we see in East Asia, they can apply the available resources in different ways to better manage those threats. To take one concrete example, Japan's defense spending is not increasing enough to keep place with the PRC's military buildup, but Tokyo has still updated its defense strategy and posture to better match the types of challenges that China poses, divested of legacy capabilities that are not well-suited for those challenges, and invested in new capabilities that should enhance its ability to deter or respond to threats – in addition to pursuing closer ties with Washington and loosening legal restrictions on its ability to use force.[1] A state that did not fear its larger neighbor would not take these steps, which makes them costly signals, just not the costly signals that are captured by the metrics that Kang uses.

Third, for a book on the future of East Asia and the United States' role in the region, Kang's work is noticeably silent about the most important actor located there: the PRC. Yet Beijing is unambiguously sending costly signals via an ongoing arms buildup and overtly taking steps to steps to alter the territorial status quo via island-building. Developing predictions about East Asia's future and recommendations for American strategy without a deeper consideration of China's willingness and ability to exploit its growing power—as well as the extent to which it can calibrate threat perceptions and leverage positive inducements to mitigate resistance on the part of its neighbors—is a risky proposition. After all, if Kang is correct and the PRC is signaling its resolve while its neighbors are standing pat, then the very trends he identifies could set the stage for broader changes to the status quo because Beijing can engage in revisionism without experiencing significant pushback. And if the United States adopts the minimalist strategy he advocates under these conditions for fear of alienating allies and partners, the result might be stability without security—that is, the

[1] For recent discussions of Japan's changing security policy, see Andrew L. Oros, *Japan's Security Renaissance: New Policies and Politics for the Twenty-First Century* (New York: Columbia University Press, 2017); Eric Heginbotham and Richard J. Samuels, "Active Denial: Redesigning Japan's Response to China's Security Challenge," *International Security* 42:4 (Spring 2018); and Adam P. Liff, "Japan's Security Policy in the 'Abe Era': Radical Transformation or Evolutionary Shift?" *Texas National Security Review* 1:3 (May 2018).

absence of arms racing and crises, but the erosion of norms and institutions that have preserved the peace for more than half a century.

Despite these critiques, *American Grand Strategy and East Asian Security in the Twenty-First Century* is a valuable contribution to the literatures on regional security, balancing behavior, and alliance management. It does an admirable job of using theory and evidence to inform policy recommendations. And it presents arguments that are sure to be debated as the United States updates its approach to the region given the enormous geopolitical changes taking place there.

Author's Response by David Kang, University of Southern California

My sincere thanks to H-Diplo, Hal Brands for organizing the roundtable and writing the introduction, and Zack Cooper, Peter Dombrowski, and Evan Braden Montgomery for their insightful and careful reviews of my book and the opportunity to discuss its ideas in this public forum. This outstanding group of reviewers are eminently well-qualified to assess the theory and empirics of my book. They have devoted considerable time and energy to engaging the book, and it is a genuine honor to discuss it with them. It is the height of collegiality and professionalism and their comments are all important and thoughtful.

Perhaps the best way forward in my response is not simply to go point by point, nor is it to simply repeat the arguments of the book. What might be a more productive way to proceed would be to point out three issues from the book that these reviews highlight—three issues that the field as whole does not deal with particularly well. For political science as a discipline to provide any real contributions to understanding the empirical reality of East Asia and the possible consequences of various policies, scholars should deal directly with these three issues. They are, first, the absence of any good theories of threat perception; second, a looseness with the conceptual term "free riding"; and third, the surprising difficulty in applying rationalist theories to actual cases. In each of these instances, the reviewers raise important points that highlight the need for further, focused research.

Poor theories and measures of threat

Evan Braden Montgomery focuses on a key issue: are countries truly afraid or not? Threat perception is notoriously difficult to define and measure, and it is relatively easy to find circumstantial evidence that could support virtually any assessment of any country. Whether or not countries feel a threat is central to most of our theories about why wars do or do not begin, and why countries behave as they do. Yet many of these theories are almost completely useless in helping us define, conceptualize, and measure perceptions of external threat. Theory should provide clear guidance for what a concept is and how to measure it in the real world—but we have almost no theories of threat other than 'big countries are more threatening than small countries.' But even this seemingly straightforward and intuitive causal statement is clearly at best a guess, and more often wrong than right: in recent years North Korea has caused more fear and reaction in the U.S. than almost any other country, despite being one of the smallest and poorest countries in all of East Asia. Some scholars maintain that intentions are more important than size, but assessing a country's intentions, and another country's perceptions of those intentions, has proven notoriously difficult. Other than *ad hoc* conjectures, there is little guidance from the literature. This is a problem for our scholarly discipline, and it is a problem for assessing the empirical reality of any particular region, as well.

Because theory is so inconclusive, we are left with a number of superficial ways to assess which countries are afraid and what their level of fear is. One reason I spent so much time in the book assessing defense spending over twenty-five years is that this is directly related to how countries prepare for conflict, and reflects national priorities made over a long time span. Government spending shows choices made, and for that reason I find it more insightful than rhetoric, which is not forced into those types of choices. The quarter century that I chose as my time frame is sufficiently long that it goes beyond short term swings of the political pendulum. It also shows where countries—despite their rhetoric—actually put their money. For many reasons, I find budgetary allocations to be powerful evidence of what a country fears or values.

Yet I accept that arguments like mine come with caveats and that qualitative evidence and opinion polls and many other ways of measuring priorities and fears exist. Scholarship that takes threat as its main focus and provides clear guidance for defining and measuring threat would be a genuine and important contribution.

Poor theories and measures of free-riding

Zack Cooper makes the reasonable argument that East Asian countries are not arming themselves because they can rely on the United States to protect them. Although I tried to deal with this argument in the book, it was not a central theme. In this response I want to address the question more directly. As with measuring threat, conceptualizing and measuring a "free ride" by an ally is notoriously difficult, in this case because it rests solely on a counterfactual: if a client did *not* have a powerful patron, then it would invest more in its own defense. But we only have one reality, so it is difficult to assess what each East Asian country that has a close relationship with the U.S. would do if the U.S. were not a reliable ally.

Yet, despite the absence of a counterfactual, it is possible to be more precise about what a free ride is. Adjudicating whether or not a country is free riding requires clear answers to three questions. *First, is it worth it?* The client must value the disputed issue sufficiently to be willing to fight with any challenger, even if it must fight alone. If the client does not care about the issue sufficiently to fight for it, then it is no surprise that the client does not invest in arms whether or not it has a powerful patron. Second, c*ould we win?* A country that has a very low probability of winning a war that it fights without a patron is unlikely to try, no matter how much it values the issue in question. This then leads to the third question: *will the patron support us?* From America's withdrawal from South Vietnam to the steady drip of departures from the coalition of the willing in Afghanistan and Iraq, countries repeatedly withdrew soldiers from war against the wishes of coalition partners. Consequently, at some threshold of uncertainty regarding the patron's commitment (or, the likelihood of abandonment), we would expect that even a client that initially attempts free-riding would begin to invest in its own defense. Put differently, there must be some relatively high probability that the patron will come to the client's aid during times of war—otherwise the client is taking an enormous risk.

Individual cases will have different answers to these questions. But conceptualizing free riding this way is an improvement over simple assertions of an unshakeable faith in the U.S. military commitment. Some may be free riding, but others may not be; and it depends on the issue and the country. For example, does the Philippines value the Spratly islands sufficiently in order to fight over them even if they had no allies? Could the Philippines conceivably develop any type of navy that could contest those islands by itself without allies? And finally, does the Philippines have such a high confidence in the willingness of the U.S. to fight for them over the Spratlys that they undertake none of those measures? Only if all three of those questions are answered positively can we conclude that the Philippines is free riding on a U.S. commitment.

The answers to these questions will vary case by case, but at a minimum I think we should more closely interrogate claims of free riding, and be more cautious about claiming that there is a free ride simply because a country is not arming itself at a certain level or pace.

The problem with rationalist theories

Peter Dombrowski raises a third point—the difficulty in applying rationalist theories to actual cases. Dombrowski is absolutely right: the theory does not quite fit. But I think this is more than simply a problem with the application to this particular case. Rather, it raises questions about the theory as much as it does about East Asia. Bargaining theory is widely viewed as the default theory about war and conflict in the international relations literature. It has assumed an almost hegemonic status as the go-to theory about how wars begin, and end. If the theory has any utility, its application to the East Asia case should be clear and straightforward. That this is not the case tells us as much about the theory as it does about East Asia.

Over two decades ago, James Fearon's seminal article laid out a clear theory of bargaining, war, and costly signals. The model was straightforward—resolved countries send costly signals that are different from cheap talk, and these costly signals communicate information about a country's willingness to fight over a particular issue. Interestingly, the bulk of theoretical attention and empirical testing focused on what I consider to be a minor element of the model: one particular type of costly signal—audience costs—and their impact on credibility and bargaining. The main model is more straightforward: *by definition,* costly signals are actions an unresolved leader would not take. The typical measures that Fearon identifies as costly signals include:

…building weapons, mobilizing troops, signing alliance treaties, supporting troops in a foreign land, and creating domestic political costs that would be paid if the announcement proves false….To be genuinely informative about a state's actual willingness or ability to fight, a signal must be costly in such a way that a state with lesser resolve or capability might not wish to send it.[1]

It seems straightforward that a resolved state would send costly signals, and an unresolved state would not. After all, the entire theory is based on the intuition that states are bargaining directly with each other over consequential issues. However, if resolved states might or might not send costly signals, then the model has less utility when applied prospectively to actual cases. In fact, the model remains largely silent on the issue of resolved state leaders and the absence of observed costly signals. Even if leaders are willing to fight over an issue they value deeply, would they choose not to send costly signals to make their threats credible? In this sense, the absence of costly signals in international crises points to some theoretically interesting questions about the nature of resolve and adjudicating between observationally equivalent outcomes derived from the model. Yet the present model does not provide sufficient theoretical guidance for determining why we might observe an absence of costly signals when they could have been sent.

Despite the widespread application of costly signaling in models of international bargaining, there are only limited attempts to apply the theory of costly signals *ex ante* to contemporary empirical cases. However, in real life, actual leaders must make judgments about how much another country's citizens and leaders care about an issue, and whether they are willing to fight over it. In international bargaining, actual leaders continually decide what they think their opponents will do in the future, attempt to signal clear resolve themselves, and anticipate reaction to their own actions. These are all forward looking, prospective assessments of other state leaders' valuations, capabilities, and costs from fighting. This is all *ex ante* prospective signaling. If the model is truly inapplicable to cases such as contemporary East Asia, then this raises more questions about the model and the way researchers test it than it does about East Asia itself.

The three excellent reviews highlight these three areas that the field as a whole needs to research more carefully—and these three conceptual issues are important for any assessment of East Asian security today, and for crafting U.S. policies to the region. These three issues are at the heart of any application of IR theories

[1] James Fearon, "Rationalist Explanations for War," *International Organization* 49:3 (1995), 396-397.

to actual cases. All three of these issues reveal how difficult it is to make compelling claims about cases. But the possibility of progress both in the scholarly and policy discourse is real, and we should encourage it.

Thanks again to *H-Diplo* and the reviewers for engaging in a stimulating roundtable. I hope that this discussion and my responses will help guide future research more carefully and systematically to think about how to measure threats, understand interactions among allies such as free riding, and indeed how to apply some of our most central theories to actual empirical cases.