

# H-Diplo | ISSF Roundtable XII-2

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INTRODUCTION BY JONATHAN M. DICICCO, MIDDLE TENNESSEE STATE  
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Are China and the United States on a dangerous collision course, and if so, is there any hope of avoiding a Sino-American conflagration over the future of the international order? As important as such questions may be, their ubiquity threatens to render them banal. Steve Chan's new book elevates the discourse around these common questions by compelling readers to see them in a new and distinctive light. With *Thucydides's Trap? Historical Interpretation, Logic of Inquiry, and the Future of Sino-American Relations*, Chan interrogates frameworks commonly used to address such questions without losing sight of their practical significance or the practical consequences of asking and answering the questions in conventional ways.

Essentially, Chan's timely book asks: *how ought we to ascertain* whether China and the United States are headed for a violent collision, and *what do we need to know* in order to avert or minimize such a collision, if indeed one is in the offing? The book may be read as policy-relevant scholarship, with key features rooted in international relations (IR) history and concepts like power, relative decline, status, revisionist intentions, and accommodation. The book is not Chan's first in this vein.<sup>1</sup> But this book goes deeper; with apologies to Bostonians who endured the notoriously ambitious Central Artery and Tunnel Project, this is Chan's 'Big Dig.' In particular, Chan excavates the foundations of two frameworks that are popularly applied to U.S.-China relations: Graham Allison's "Thucydides's Trap," from which Chan draws the title of his book, and power transition theory, which is associated with the late A.F.K. Organski and his students.<sup>2</sup>

Like Boston's Big Dig, Chan's project invites colorful critiques, as readers of this roundtable will find. But the reviewers clearly agree on the value of undertaking the project. Notably, Chan's book unearths prior assumptions, conceptual ambiguities, theoretical baggage, and selective readings of history that inform, and *misinform*, popular viewpoints on power transitions. Such scrutiny is warranted, for at least two reasons. First, no framework is perfect; as Chan's book and the reviews below make plain, scholarly studies that link power shifts and war suffer from flaws in logic and the use of evidence, historical and otherwise. To demonstrate these claims, Chan unpacks historical cases that ostensibly expose weaknesses in the frameworks.

Second, Chan fears that a phenomenon akin to the Heisenberg effect will occur: namely, that our attempts to assess power transitions and associated risks of war might alter these phenomena as part of our lived experience, and not necessarily for the better. Chan questions whether analysts, by talking about transitions as being dangerous, influence how decisionmakers approach apparent power transitions as policy problems—and thus create self-fulfilling prophecies about the increased hazard of war. Ayşe Zarakol pushes Chan, and readers, to dig even more deeply into the notion that shared belief systems create social facts with political consequences. The fault, it seems, is not in our stars, but in ourselves, and how we think about IR. Zarakol's illuminating complement to Chan's book contributes historical perspective on IR scholars' misuse of the work of Thucydides, and highlights the disjunction between "seemingly historical schematics" and the analysis of present-day politics.

Central to the book is Chan's own firmly held belief that human agency influences outcomes, and that misguided beliefs in structural determinism limit leaders who otherwise might be open-minded about how to safely navigate power shifts. (In a

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<sup>1</sup> See, *inter alia*, Steve Chan, *China, the U.S., and the Power-Transition Theory: A Critique* (London: Routledge, 2008).

<sup>2</sup> The original statement of power transition theory appeared in A.F.K. Organski, *World Politics* (New York: Alfred A. Knopf, 1958). Early empirical tests of some of the theory's implications appeared in A.F.K. Organski and Jacek Kugler, *The War Ledger* (Chicago: University of Chicago Press, 1980). The fullest statement of Thucydides's Trap is Graham Allison, *Destined for War: Can America and China Escape Thucydides's Trap?* (Boston: Houghton-Mifflin, 2017). Please see the reviews below for more extensive documentation.

strange twist of fate, Chan and Allison probably agree on this point, as Ja Ian Chong notes in his review.) Consider too the assumption that rising powers are necessarily revisionist, and that established powers are necessarily in favor of the status quo. Such an assumption is, in Chan's estimation, a common misapprehension that biases our understanding of power transitions in general, and Sino-American relations in particular. Chan is among the world's leading scholars on the concept of revisionism in IR, and the book offers a significant contribution in this regard.

But the book's chief purpose is not conceptual; rather, it is to take aim at Allison's popular project, as well as power transition theory, to which Allison arguably owes a sizable (and perhaps underacknowledged) intellectual debt. Allison's 2017 book *Destined for War: Can America and China Escape Thucydides' Trap?* advances a sort of folk wisdom that is loosely supported by historical renderings of power transitions freighted with the risk of war. Chan's demolition work centers in part on the foundations of Allison's project, calling into question Allison's approach, his interpretation of Thucydides's *History of the Peloponnesian War*, and his empirical cases. In his review below, Jack S. Levy characterizes Chan's book as providing "the most thorough critique to date of Allison's argument and the reasoning and evidence underlying it."

Allison's framework is not identical to the more sophisticated power transition theory. Levy and Yuan-kang Wang note as much in their surgically precise reviews. However, some trace the intellectual roots of the "Thucydides Trap" project to the earlier work by A.F.K. Organski, his collaborators, and their scholarly progeny. Indeed, reviewers Tadeusz Kugler and J. Patrick Rhamey come close to accusing Allison of repackaging power transition theory as the Thucydides Trap. Rhamey and Kugler justifiably criticize Chan's book for overlooking recent scholarship in the broader power transition research program, a weakness that Chan thoughtfully addresses in his generous author's response. But what seems even more objectionable to Kugler (and Wang, for different reasons) is Chan's fusion of Allison's Thucydides Trap with Organski's power transition theory. To Kugler, linking the two risks legitimizing Allison's "rebranding of near seventy years of research," while inadvertently delegitimizing the research itself. To be fair, Chan's analysis often parses the two frameworks—but in this moment of historically short attention spans, guilt by association is likely, so Kugler's concerns are scarcely misplaced.

Despite power transition theory's longevity and its expansion into a robust research program, it too is hollowed by Chan's unsparing critique. Rhamey and Kugler rightly question whether Chan's book is sufficiently thoroughgoing to warrant such a broad indictment; they point to a number of undiscussed contributions, including the conceptualization and measurement of states' political capacity. And yet, even Rhamey's spirited defense concedes that power transition researchers have not yet fully resolved the issue of how to measure states' dissatisfaction with the status quo, which, in principle, would help analysts anticipate particular states' adoption of revisionist agendas and strategies. Levy's nuanced discussion helps readers to differentiate between dissatisfaction and revisionism, and to pinpoint some problems with power transition theory that remain unresolved. Indeed, the book and this forum deserve a close read by power transition theory's proponents, because Chan's excavation exposes serious structural issues.

The assembled reviewers disagree as to which edifice remains more intact after Chan has finished his "big dig" into their foundations. Wang, like Levy and others, cautions against equating the Thucydides' Trap with power transition theory—but unlike Kugler and Rhamey, Wang finds Chan's critique of power transition theory the more compelling. But neither is Wang fully satisfied by Allison's approach; he seems prepared to move on, perhaps by building on Robert Gilpin's hegemonic war theory (which, like Thucydides's Trap, also arrived after Organski's power transition theory, and with surprisingly little acknowledgement of it).<sup>3</sup> In keeping with this line of thinking, the question, "where do we go from here?" looms over the book and this roundtable. No consensus emerges from the assembled participants. But the reviewers, and Chan's thoroughgoing response to them, offer reasons for cautious optimism.

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<sup>3</sup> Robert Gilpin, *War and Change in World Politics* (Cambridge: Cambridge University Press, 1981). Organski is cited once, in a footnote, on page 94.

In one sense, what Chan has accomplished in with his excavation in *Thucydides's Trap?* is archaeological. Excavations are necessarily messy, and Chan is to be commended for doing the hard and unpleasant work. As with any archaeological dig, some of the work requires heavy lifting, and some of it requires precise, careful handling of delicate minutiae. Particular artifacts may be missed or mishandled, but the excavation, when completed, illuminates the past and gives us greater perspective on where we are now and where we might be headed. Chan's book provides just such perspective.

With *Thucydides's Trap?*, Chan has provided a valuable contribution to discussions among political scientists, historians, and policy analysts who are engaged with questions about U.S.-China relations, as well as scholars of all stripes who are inclined to ignore the ever-present question, "how do we know what we *think* we know?"<sup>4</sup> By unpacking Thucydides's Trap and power transition theory and some related cases and concepts, Chan moves scholars and analysts to take a step back and reevaluate our shared beliefs about phenomena we think we understand. Digging up the roots of some of our theories and heuristic devices reminds us that looking forward without looking backward—or, looking backward merely to reconfirm what we think we already know—can be fraught with peril.

Happily, excavations also create spaces that may be filled with new construction. Even fierce critics of Boston's Big Dig will allow that the project made possible freer movement and new development in its wake. Chan's act of creative destruction clears the way for progressive revisions of IR theory and analysis. To that end, Levy's review offers valuable advice for theorizing about the preventive motivation for war and other phenomena related to power transitions. Chong encourages greater attention to third parties and regional systems, which are emphasized too by Rhamey and Levy. Kugler and Rhamey, like Wang, highlight efforts to acknowledge complexity, utilize technology, and build durable, sustainable analytical edifices.

As this roundtable demonstrates, Chan's book is already generating debates about the concepts, arguments, and evidence that ground our analyses, which suggests its immense value in the marketplace of ideas about power transitions and Sino-American relations. At the same time, Zarakol and Chong remind us that the ideas themselves, and even the marketplace, are rooted in temporal and cultural contexts that are historically contingent—and that these artifacts require knowledgeable, conscientious scrutiny and continuing (re)evaluation. That Chan's book nudges us to consider such higher-order questions is a credit to its author, and reminds us that (re)building frameworks for understanding IR demands an architectural vision that transcends the confines of familiar structures.

#### Participants:

**Steve Chan** is College Professor of Distinction at the University of Colorado, Boulder, where he teaches political science.

**Jonathan M. DiCicco** is Associate Professor of Political Science and International Relations at Middle Tennessee State University. His research investigates power transitions and revisionism, rivalries and rapprochement, and foreign policy elites' attitudes toward the use of force abroad. His work may be found in journals including the *Journal of Conflict Resolution*, *International Studies Quarterly*, *Political Research Quarterly*, and *Foreign Policy Analysis*.

**Ja Ian Chong** is associate professor of political science at the National University of Singapore and a Harvard-Yenching Institute Visiting Scholar for 2019-2020. Dr. Chong's work crosses international relations, comparative politics, and political sociology, and focuses on security issues in the Asia-Pacific. He follows the interplay of social movements, politics, and foreign policy in East Asia closely. His work appears in a number of journals, edited volumes, and newspapers, including *Asian Security*, *China Quarterly*, *European Journal of International Relations*, *International Security*, and *Security Studies*. He

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<sup>4</sup> Roy E. Licklider, "How Do We Know What We Know?" In Edward Rhodes, Jonathan M. DiCicco, and Dalia F. Fahmy, eds., *International Relations: Introductory Readings*, revised printing (Dubuque: Kendall Hunt, 2017), 331–342.

is author of *External Intervention and the Politics of State Formation: China, Indonesia, Thailand—1893-1952* (Cambridge, 2012).

**Tadeusz Kugler** is an Associate Professor of Political Science at Roger Williams University and his first textbook, with J Patrick Rhamey Jr, *Power, Space, and Time: An Empirical Introduction to International Relations* was published by Rowman & Littlefield in 2020.

**Jack S. Levy** is Board of Governors' Professor of Political Science at Rutgers University and Senior Research Scholar at the Saltzman Institute of War and Peace Studies at Columbia University. He is past-president of the International Studies Association and of the Peace Science Society. Levy's primary teaching and research interests are the causes of interstate war, foreign policy decision-making, political psychology, and qualitative methodology. His recent books include *Causes of War* (2010, with William R. Thompson), *The Arc of War: Origins, Escalation, and Transformation* (2011, with William R. Thompson), *The Outbreak of the First World War: Structure, Politics, and Decision-Making* (2014, co-edited with John A. Vasquez), and *The Oxford Handbook of Political Psychology*, 2<sup>nd</sup> ed., (co-edited with Leonie Huddy and David O. Sears).

**J. Patrick Rhamey, Jr.** is Associate Professor in the Department of International Studies and Political Science at the Virginia Military Institute. His research focuses primarily on global and regional hierarchies, international status, and comparative regionalism. Recently published work includes findings on the consequences of power and status in regional spaces and an undergraduate textbook on empirical approaches to international politics that emphasize hierarchy.

**Yuan-kang Wang** is Professor of Political Science at Western Michigan University. He holds a Ph.D. in political science from the University of Chicago. His research examines the nexus between international relations theory and historical China. He is author of *Harmony and War: Confucian Culture and Chinese Power Politics* (Columbia University Press, 2011). He has published articles and book chapters on the Chinese world order, Taiwan security, and U.S.-China relations. His recent publication "The Durability of a Unipolar System: Lessons from East Asian History" will appear in the October 2020 issue of *Security Studies*.

**Ayşe Zarakol** is Reader in International Relations at the University of Cambridge. She is the author of *After Defeat: How the East Learned to Live with the West* (Cambridge University Press, 2011) and the editor of *Hierarchies in World Politics* (Cambridge University Press, 2017). Her next book *Before Defeat: Rethinking the Decline of the East and the Future of the West* (forthcoming from Cambridge University Press) reconstructs a global history of Eurasian international relations and reinterrogates the concept of decline in IR.

The timing of the publication of Steve Chan's *Thucydides's Trap? Historical Interpretation, Logic of Inquiry, and the Future of Sino-American Relations* is impeccable. From major power friction to institutional fractures, alliance tensions, crises of democracy, overreach, and even skirmishes at sea, international politics today seem ripped from the pages of Thucydides's *The History of the Peloponnesian War*. There is even a plague and politicians who can go toe-to-toe with the ambitious Athenian demagogue, Alcibiades, to boot. Despite the multiple parallels between today's twenty-first century world and the Mediterranean of the fifth century BCE, Chan is at his strongest in warning readers about the analytical pitfalls of pushing imperfect analogies too far. Chan reminds readers of the importance of considering contingency and agency, elements that contemporary academic political science sometimes overlooks in its eagerness to create elegant, universally applicable theories.

*Trouble with the Trap, Problems with Power Transition*

Taking as its foil the idea that present-day international politics centers on a fundamental and intractable tension between an emergent People's Republic of China (PRC) and an established United States, Chan argues that such logic is at best simplistic and mostly inaccurate. He reaches this conclusion from an examination of evidence surrounding several claims about the relationship between major powers that are in relative rise and decline. The major focus of Chan's investigation is the so-called Thucydides's Trap, a concept popularized by Harvard University's Graham Allison and from which this volume draws inspiration.<sup>1</sup> Allison argues that the relative rise and decline of major powers creates substantial pressure for confrontation and conflict, which is avertible only with the use of creative statecraft. Chan contends that Allison's account is overly structural, overlooking agency, contingency, and precise measures of power while mischaracterizing *The History of the Peloponnesian War* (Ch. 2).

Chan further explores the conceptual premises of power transition theory that provide the intellectual underpinnings for Allison's argument. For Chan, various strands of power transition converge in suggesting that conflict is likely as an emergent power approaches parity in capability with the dominant actor and seeks to revise the prevailing status quo (Ch. 3). The trigger for war comes either as the dominant actor tries to pre-empt and put down such change, or when the emergent actor seeks to forcibly push transformation through. Chan sees this class of explanations as doing somewhat better in capturing interactions among major powers in relative rise and decline, allowing more room for socialization, perceptions, and status considerations that lead to outcomes other than war. However, Chan argues that power transition still overly prioritizes structural dynamics over agency and contingency, a problem compounded by arbitrariness in distinguishing between "status quo" and "revisionist" states, along with between parity and dominance (Chs. 4-5).

Given Chan's discomfort with the Thucydides's Trap and power transition theory frameworks, he tries to develop alternative conceptions for the future of U.S.-China relations and how to avoid pitfalls in this key relationship (Chs. 6-9). Like Allison, a key motivation for Chan is averting greater friction, even conflict, between Washington and Beijing. Here, he finds that while indications of a currently rising PRC are clear, evidence for its desire to change the status quo is at best inconclusive (Ch. 7). Going forward, the PRC could continue its growth, or its momentum could peter out as it runs into greater resistance from other states that grow alarmed by the situation. Moreover, Chan submits that Beijing demonstrates willingness to accommodate the status quo despite its unfairness toward PRC interests and that United States under the Donald Trump administration is by far more revisionist, aggressive, and, therefore, dangerous (Chs. 8).

The solution to avoiding major power conflict, according to Chan is for the United States to acknowledge the PRC's growing might and to accommodate its rising status (Ch. 9). Since Beijing is not predisposed towards aggression and is likely

<sup>1</sup> Graham Allison, *Destined for War: Can America and China Avoid the Thucydides's Trap?* (Boston: Houghton Mifflin Harcourt, 2017).

to lash out only when pushed, Washington should cede ground to maintain peace. This includes having the United States back off from currently contested areas, such as diminishing support for Taiwan and accepting PRC claims in the South and East China Seas. The United States should make use of the PRC's pacific inclinations, which are grounded in the ever-elusive, seemingly culturally-informed practices of Chinese "statecraft" (193-204, Chs. 9-10). This contrasts with Washington's strident behavior, informed by the values and ideas of a similarly amorphous "West." (193) That said, Chan does not address how Washington can eschew these proclivities if they are indeed deeply ingrained, even hardwired.

*Is Accommodation Enough?*

A main thrust in the volume is the argument that American acceptance of status adjustment is key to avoiding conflict since the PRC does not have revisionist intentions. Rather, it is the United States, especially under the Trump administration, that is unsettling the existing international order and stability with withdrawal from international arms control treaties and voting against the UN General Assembly majority (134-141). I am left wondering why a natural status adjustment will not occur as the United States limits its own institutional participation, allowing the PRC's relative prominence to grow by default. Additionally, whatever the problems of the Trump administration's policies, its actions say little about the Xi leadership's intentions given the opacity of the current Chinese political system. The PRC may have made declarations in support of the international system and joined various international organizations, but Chan does not elaborate on why he believes such steps equate to China committing to the sorts of strategic restraint that are needed to maintain order (122, 134-8).<sup>2</sup> Further explanation can make Chan's case more persuasive.

Apart from outright lying, actors can have a range of motivations and possibilities for participating in organizations, not all of which conform to the overall interests of the grouping or all its members. Among other things, actors may use participation to frame issues, change agendas, and block decisions. Such effects are particularly pronounced when it comes to powerful actors and applies to the PRC as much as it does to the United States. Indeed, the ongoing U.S.-China tussles over the World Trade Organization, World Health Organization, and World Intellectual Property Organization may indicate the presence of such machinations. Even if there is acceptance of existing arrangements, rules, and international law on Beijing's part today, Chan admits that there is nothing to stop a change of heart and renegeing on China's part.<sup>3</sup>

Indications exist that the Xi leadership may be less satisfied with the status quo than Chan claims given that current PRC actions push up against not only the United States and Taiwan. South Korea reported economic punishment from China as a result of the deployment of a missile defense system to guard against possible North Korean attacks and repeated dangerous behavior by Chinese fishing vessels.<sup>4</sup> Japan expressed concern over increased naval and aerial activity by PRC assets in and over contested areas.<sup>5</sup> Indonesia, Malaysia, and Vietnam indicate growing PRC harassment of their fishing and other civilian vessels in the South China Sea, which is notable given that Indonesia and the PRC do not have overlapping

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<sup>2</sup> G. John Ikenberry, *After Victory: Institutions, Strategic Restraint, and the Rebuilding of Order After Major Wars* (Princeton: Princeton University Press, 2000).

<sup>3</sup> Mira Rapp-Hooper, "China, America, and the World Order After the Pandemic," *War on the Rocks*, 24 March 2020, <https://warontherocks.com/2020/03/china-america-and-the-international-order-after-the-pandemic/>.

<sup>4</sup> Yew Lin Tian and Se Young Lee, "China Says Opposes U.S. THAAD System in South Korea," *Reuters*, 29 May 2020, <https://www.reuters.com/article/us-china-southkorea-usa-thaad/china-says-opposes-u-s-thaad-defence-system-in-south-korea-idUSKBN2350XA>; "Chinese Boat Seized for Illegal Fishing," *Yonhap*, 4 January 2020, <https://en.yna.co.kr/view/AEN20200104002600320>.

<sup>5</sup> Tsukasa Hadano and Alex Fang, "China Steps Up Maritime Activity with Eye on Post-Pandemic Order," *Nikkei Asian Review*, 13 May 2020, <https://asia.nikkei.com/Business/Aerospace-Defense/China-steps-up-maritime-activity-with-eye-on-post-pandemic-order>.

territorial claims.<sup>6</sup> Then there are tensions along the Sino-Indian border that recently resulted in deadly clashes between Indian and Chinese troops.<sup>7</sup>

Current PRC behavior is worrisome for regional actors in other ways as well. Australia reports economic pressure for not conforming to Beijing's preferences on pushing for an open investigation into the origins of the COVID-19 pandemic and complaining about PRC efforts to influence its internal politics.<sup>8</sup> Along with Canada, Australia saw citizens detained and charged for illegal activities under suspicious circumstances.<sup>9</sup> Singapore too faced Chinese state pressure over its insistence on adherence the rule of law over the arbitration over the South China Sea brought by the Philippines against the PRC.<sup>10</sup> That such issues do not receive more treatment by Chan is curious since they raise questions about PRC's commitment to self-restraint and can potentially trigger the chain-ganging effects on U.S.-China ties that Chan warns readers about (21-22, 211-215). Such friction can potentially harden positions and raise the stakes over an issue such that prevailing becomes more tied to status and other concerns, driving more aggressive and even revisionist behavior.<sup>11</sup>

Chan's finding that misplaced worries about the PRC and its intentions stem in part from misunderstandings of perspectives on international politics that are informed by theories from "the West" rather than China deserves elaboration and debate. So-called "Western" international relations theories often have parallels in the Chinese tradition, broadly construed. Work analyzing Spring and Autumn, Warring States, Song, and Ming documents indicate that the strategic thought that is prominent in these periods closely resembles statecraft familiar to those in the contemporary "West."<sup>12</sup> Texts as varied as the Han-era annals *Records of the Grand Historian* and the Ming-era fiction *Romance of the Three Kingdoms* will suggest the same.<sup>13</sup> Parallels between "Western" and "Chinese" approaches to politics are unsurprising. Several millennia of

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<sup>6</sup> Hong Thao Nguyen, "South China Sea: US Joins the Battle of Diplomatic Notes," *The Diplomat*, 10 June 2020, <https://thediplomat.com/2020/06/south-china-sea-us-joins-the-battle-of-diplomatic-notes/>.

<sup>7</sup> Ajai Shukla, How China and India Came to Lethal Blows, *New York Times*, 19 June 2020, <https://www.nytimes.com/2020/06/19/opinion/China-India-conflict.html>.

<sup>8</sup> Natasha Kassam and Darren Lim, "Australia can Expect China to Lash Out More Often. We Must Foster Resilience and Sangfroid," *The Guardian*, 10 June 2020, <https://www.theguardian.com/commentisfree/2020/jun/10/australia-can-expect-china-to-lash-out-more-often-we-must-foster-resilience-and-sangfroid>.

<sup>9</sup> Nathan Vanderklippe, "China's Australia Pressure Tactics Echo Its Treatment of Canada After Meng Arrest," *The Globe and Mail*, 11 June 2020, <https://www.theglobeandmail.com/world/article-chinas-australia-pressure-tactics-echo-its-treatment-of-canada-after/>.

<sup>10</sup> Greg Torode, "China Leaning on Singapore to Keep ASEAN Calm over South China Sea: Sources," *Reuters* 8 August 2020, <https://www.reuters.com/article/us-asean-china-singapore-analysis/china-leaning-on-singapore-to-keep-asean-calm-over-south-china-sea-sources-idUSKBN1AO17D>.

<sup>11</sup> Todd H. Hall, "More Significance than Value: Explaining Developments in the Sino-Japanese Contest Over the Senkaku/Diaoyu Islands," *Texas National Security Review* 2:4 (September 2019): 11-37.

<sup>12</sup> Victoria Tin-Bor Hui, *War and State Formation in Ancient China and Early Modern Europe* (Cambridge: Cambridge University Press, 2005); Alastair I. Johnston, *Cultural Realism: Strategic Culture and Grand Strategy in Chinese History* (Princeton: Princeton University Press, 1994); Yuan-kang Wang, *Harmony and War: Confucian Culture and Chinese Power Politics* (New York: Columbia University Press, 2011).

<sup>13</sup> Luo Guanzhong [羅貫中 (明)]. *Sanguo Yanyi* [《三國演義》] (Jilin: Changchun Publishing, 1995); Sima Qian [司馬遷 (漢)], *Shiji* [《史記》] (Urbana: Project Gutenberg, 2008), <http://www.gutenberg.org/cache/epub/24226/pg24226-images.html>.

collective human experience, thought, and debate over statecraft, conflict, as well as governance are almost certainly bound to produce similarities in responses.

Dividing the world into “Western” and “Chinese” views of the world ignores the fact the PRC has disagreements with ostensibly “non-Western” polities such as India, Indonesia, Japan, Korea, and Vietnam, each with their own distinct philosophical traditions.<sup>14</sup> Also, despite sharing cultural origins, people in the PRC and on Taiwan disagree fundamentally issues of political values and rights, not the relatively simple issues of who should rule China or what a Chinese state should entail geographically.<sup>15</sup> Moreover, the PRC’s ruling Chinese Communist Party draws at least some of its inspiration from European thinkers in the form of Karl Marx and Vladimir Lenin. Successive dynasties from historical China also proved themselves very adept at conquest—that is how regimes and empires get built.<sup>16</sup> Attributing tensions between the United States and PRC to culture suggests an overly monolithic view of the rich and varied philosophical and political traditions both major powers draw from, giving them less credit than is due.<sup>17</sup>

To claim that contemporary international scholarship and U.S. policy are unable to adequately understand China because they are “Western” may oversimplify the nature and seriousness of problems dogging U.S.-China relations and their consequences for the world. Relegating difference to culture is not only Orientalizing, it can encourage a misplaced expectation that understanding can bring some sort of happy, mutually acceptable outcome. Perhaps Beijing and Washington understand each other well. They simply disagree fundamentally over values and interests in ways that make finding mutually acceptable accommodation increasingly difficult. This does not have to imply that either side is morally superior or normatively “better” than the other, just that understanding provides little promise for improving relations and avoiding confrontation. Better accounting for such possibilities invites fuller consideration of the roles that agency and contingency play in major power relations, two features that Chan clearly identifies as critical in the volume.

*Thucydides’s Trap?* deserves much credit for grappling with important, pressing, and difficult questions about the drivers behind the downturn in U.S.-China relations and possible ways to address this slide. Yet, Chan’s outlook is more similar to Graham Allison’s than he initially lets on. Allison’s call for creative statecraft is possible only if the United States and China are not locked in a structural situation which neither can escape or beset by contingent circumstances that prevents Washington and Beijing from effectively exercising the agency Chan believes is central. Chan offers some insight when he points to divergences in perspectives between Washington and Beijing but may be overly limiting the ways he conceives of effects of culture and socialization. Likewise, the volume can go further in conceptualizing the various ways third parties such as regional actors and international organizations can affect U.S.-China ties, given that world politics is not just major powers going at each other—a fact both Chan and Allison recognize. Major power interactions simply do not occur in a vacuum. Such dynamics may reinforce competition as much as ameliorate them, but their effects await further clarification and explanation.

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<sup>14</sup> Amitav Acharya, *Whose Ideas Matter? Agency and Power in Asian Regionalism* (Ithaca: Cornell University Press, 2009).

<sup>15</sup> Eleanor Albert, “China-Taiwan Relations,” *Council on Foreign Relations*, 22 January 2020, <https://www.cfr.org/backgroundunder/china-taiwan-relations>.

<sup>16</sup> Tonio Andrade, *Lost Colony: The Untold Story of China’s First Great Victory Over the West* (Princeton: Princeton University Press, 2011); Peter Perdue, *China Marches West: The Qing Conquest of Central Eurasia* (Cambridge: Harvard University Press, 2005).

<sup>17</sup> Kung-chuan Hsiao [蕭公權], *History of Chinese Political Thought* [《中國政治思想史》] (Taipei: Linking Publishing, 1982).

## REVIEW BY TADEUSZ KUGLER, ROGER WILLIAMS UNIVERSITY

In Greek mythology, Atlas, one of the Titans, rebelled against the Olympian gods who usurped them. Upon losing this war of divinities, he was sentenced to hold up the skies, which were imagined at the time as a sphere, and later re-imagined as the terrestrial world. From this narrative, we likely gained the term “atlas” to denote a collection of maps serving collectively as a comprehensive guide to the world, human anatomy, and other discourses. It is a method of producing comprehensive knowledge by collecting multiple facets of a broad topic. This is what I believe Steve Chan attempts to do with his work, *Thucydides’s Trap? Historical Interpretation, Logic of Inquiry, and the Future of Sino-American Relations*. Yet, more than merely assembling nearly seventy years of research on how nations use corrective force, this book examines how the theories underpinning that research are marketed to the public, policymakers, and rival groups of researchers.

The book concisely outlines many of the significant inconsistencies of various approaches in terms of strategy, across a sweeping historical range: from the Peloponnesian War on which the fifth-century BCE historian Thucydides wrote, to a comparison of the attempted invasion by the Axis Powers of the USSR (Operation Barbarossa) with Japan’s attack on the U.S. naval base at Pearl Harbor, and finally to the book’s main concern: the current questions of the rise of China and whether this will follow the pattern hypothesized by Thucydides that when a rising power threatens an established one, conflict becomes inevitable. In doing so, the book opens with analyses of the concept of Thucydides’ Trap and Power-Transition Theory. Two counterexamples to both theories follow. The first involves the fact that war did not break out when the United States overtook the United Kingdom as a global hegemon, and the second discussed the invasion by a stronger Germany of a weaker USSR alongside the example of a weaker Japan attacking a stronger United States. This is presented as evidence that the “bilateral balance of power does not appear to be the critical determinant for choosing war” (82). The first half of the book lays the groundwork for its main question of whether present-day China’s increased global power will result in conflict with the current dominant power, the United States

It is in this examination of Sino-American relations that the book attempts to expand upon traditional dyadic studies by adding discussions of research agendas which link domestic factors to foreign policy considerations, use measurements of national status, and even consider the idea of what countries ‘want’ to the future evaluations of what China would or also could do as a rising power. His view is that China has not and will not take more aggressive actions on the international front as it has no need or interest in them, and he suggests that this is the most critical missing piece of traditional theory. Why should countries play a significant part in global politics, and why is it as important as measurements of power, GDP growth, or population?

In general, debates within the field of international relations have moved from grandiose generalizations to quantitative studies founded in theory with names such as power transitions (PT), hegemonic stability theory, long cycle theory, neorealism, and of course, the balance of power.<sup>1</sup> Each theory attempts to link various components to policy-relevant proposals and over the last seventy years proponents of each have engaged in spirited debate, reevaluation, and expansion.<sup>2</sup> This led to reconsiderations of hierarchy at both global and regional levels as a means of evaluation of power transition. In particular, the use of political economy concepts dealing with economic growth or the effectiveness of policy to development allowed for estimations of what could be the outcomes of future rivals. The continuous need to measure is a driving force within the attempts to quantify status as well as domestic considerations of the policy actors themselves towards foreign

<sup>1</sup> For an extensive overview of modern political science please see J. Patrick Rhamey, Jr., and Tadeusz Kugler, *Power, Space, and Time: An empirical introduction to international relations* (London: Rowman and Littlefield, 2020).

<sup>2</sup> Jack Levy and William Thompson, *Causes of War* (Oxford: Wiley-Blackwell, 2010) as well as Ronald Tammen, “The Organski Legacy: A Fifty-Year Research Program,” *International Interactions* 23:4 (2008): 314-332 are two good foundational readings on the core of power transition and its alternatives.

policy being of particular popularity often called rational political ambition theory, and the all-important creation of new measurements of state capacity and power.

*Thucydides's Trap* illustrates how much of the core issues of this debate have been missed by the debaters themselves. Chan's counter-arguments to previous theories and prominent case studies have frequently featured in the works of other scholars and has been the genesis of continued research agendas. The continued criticism of measurements from projects in the infancy of research agendas, is not as applicable to those ongoing.<sup>3</sup> Much of this criticism has long been acknowledged and rectified. With some modification theories such as power transition or balance of power by incorporating new thoughts on global governance with measurements of status, domestic politics, or hierarchy, adding to the complexity of the theory.

Chan frames much of the evaluation of policy itself as emerging out of a desire for an alternative to traditional theories with a focus on power transitions (PT). This later, as he points out, has seemed to need large scale conflict between a dominant power and a rising rival when entering a period of transition, and should this not happen, the basic premise of the construct is somehow flawed. The claim that better measurements of policy, domestic interest, status, or the extent of the number of rivals of each is as an alternative to PT, and balance of power, I believe misses the fact that early PT did not claim a causal relationship between transition and war instead argued that this would be the situation most likely to have a conflict.<sup>4</sup> World Wars proved to be the genesis of PT research and in them we see major conflicts coming from dissatisfied rising powers against dominant powers. PT worried primarily about the concept of 'satisfaction' in that the 'why' of what a country wants is as important as its relative power. Peaceful transition is a goal of policy recommendations.<sup>5</sup>

The question of what China wants to do and is capable of doing is a driving question within international relations and later chapters deal with this vexing question. Chan asserts that China is not as powerful as many of the researchers believe, that they are not interested in becoming a global power, and are primarily concerned with their locality and region. He also claims that should China have interests in changing the global system or a rise to power it would be peaceful. These claims are the research extensions beyond the theory overviews seen earlier in the book, and all, I would suggest, need to be more quantitatively comprehensively studied. There is a disconnect with the discussion of the individual case study flaws, outliers, and the various data issues within theories, and the later claims on the interests of China which relied upon believe but not much direct support.

What evidence suggests that China is peaceful now and will be so in the future? The lengthy list of China's hostile actions against other global powers stretches from fistfights with India along a disputed border, the construction of the second-largest navy in the world with a focus on force projection, the expansion of its nuclear arsenal, the installation of permanent overseas bases, land and sea grabs within the South China Sea, the production of only non-U.S. stealth fighter at full output, extensive modernization of missile systems, and even an operational moon program.

The global implications of investing in the capacity of a power to coerce outside its region could simply be a domestic propaganda campaign, but it does seem to be quite an expensive one. Military mobilization, even if unarmed, with a fellow nuclear-armed power on a disputed border and the direct military antagonization of a whole list of major trading partners in

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<sup>3</sup> Ronald Tammen, Jacek Kugler, Douglas Lemke, "Foundations of Power Transition Theory," in William R. Thompson, ed., *The Oxford Encyclopedia of Empirical International Relations Theory*, (Oxford: Oxford University Press, 2018): 19-63, is as comprehensive a guide to the usages, changes, and investigations over nearly seventy years of those who use some version of power transition.

<sup>4</sup> Lemke, *Regions of War and Peace* (Cambridge: Cambridge University Press, 2002) shows the importance of regions as a primary component of national evaluation and is a direct example of how power transitions has been extended with Ronald Tammen, *Power Transitions: Strategies for the 21<sup>st</sup> Century* (New York: Chatham House, 2000).

<sup>5</sup> Glenn Palmer and T.C Morgan, "Power Transitions, the Two-Good Theory, and Neorealism: A Comparison with Comments on Recent U.S. Foreign Policy," *International Interactions* 33:3 (2007): 329-346.

South Asia can only be a method for causing a rallying around the flag effect or is a sign of an expanding and more aggressive foreign policy. The weight of those two concepts are an essential addition to the field, and it need some greater form of measurement to support.

The discussion of how powerful China is or will become is an important one. The use of merely GDP, be it nominal or PPP, is of continued problematic practice, and Chan discusses some of the criticisms well but his arguments could be a bit more nuanced. Again, I would suggest that in terms of power, this is not the only measurement used by most scholars of global politics, and discussions on how to measure it are far beyond the space limits of this review. From the scale of industrialization, to complex multi-variable indexes, power is and continues to be an important and nebulous measurement.

My criticism is that the book extends some of this argument to its discussions of how scholars overstate the importance of population. If that factor is so critical, why wasn't China constituted as a global power in previous decades? My argument would be that China had a domestic policy designed directly to undercut the economy until the government enacted significant reforms in the late 70s. The fact that China suffered the most massive famine in human history and had the largest number of people on earth classified by global poverty measurements until 1980 was due the domestic economic policies of the government. That was a policy choice issue, not a problem with power measurements. The size of the country's population and geography is the source of its potential as people and resources create economies but potential itself is not sufficient for success, which is based on organization choice and political capacity. China could have been a major global power decades before given differing economic policies. If the economic reforms had been undertaken in 1950, the world would be a different place.

Last, the book both overlooks and (in its very title) links traditional scholarship. As mentioned earlier recent researchers using this power transition theory have already incorporated much of this work's suggestions for a more comprehensive framework with domestic politics, status, dominance vacuums, regions vs. global power structure, alliances, decision models, and even demographic structures all as attempts to measure that most famous of variables, 'want' or in PT terms 'satisfaction.'<sup>6</sup> The limited nature of how scholars knew to measure political attitudes in the early days of a research agenda does not preclude the idea that in the decades after, they would have had more success.<sup>7</sup>

Yet simultaneously, the volume positions itself in conversation with the popular example of the day: Allison's writing on the concept of Thucydides's Trap in terms of U.S.-China relations.<sup>8</sup> The public popularity of Allison's work leads to the question as to whether academia should continue to use the term power transitions.<sup>9</sup> Has this rebranding of near seventy years of research both created a greater popular belief in the policy suggestions but also put pressure on a generation of scholars to consider changing their terminology?<sup>10</sup> Does a book, in this case, Allison's, need to be written with the same level of citations or concern for fellow academics, or is it more relevant to sell the outcome of the theory to the accessible public, in which case those considerations are not suitable? On one level Allison's book can appear to be the popular rewriting of

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<sup>6</sup> Please see Tammen, Kugler, and Lemke 2018 for an exhaustive list of research and citations.

<sup>7</sup> See, for example, Lemke, "Investigating the Preventive Motive for War," *International Interactions* 29:4 (2003): 273-292.

<sup>8</sup> The work of Graham T. Allison seen his 2015 article for *The Atlantic*, "Thucydides Trap: Are the U.S. and China Heading for War" 24 September 2015, <https://www.theatlantic.com/international/archive/2015/09/united-states-china-war-thucydides-trap/406756/> and *Destined for War: Can America and China escape Thucydides's Trap?* (Boston: Houghton Mifflin Harcourt, 2017) are influential to the text from the title of the book itself to the ending arguments on whether the U.S. is an inevitable opponent in any struggle of power.

<sup>9</sup> Allison, "Thucydides Trap: Are the U.S. and China Heading for War?" *The Atlantic*.

<sup>10</sup> A.F.K. Organski, *World Politics* (New York: Knopf, 1958) as an origin point.

A.F.K Organski and Jacek Kugler's *The War Ledger*,<sup>11</sup> using the near exact national examples but yet without including citations of this in terms of the data of a long list of later scholars and without the expected rigor given to definitions and measurements, as is seen in the work of Jonathan M. DiCicco.<sup>12</sup> This is complicated by the *War Ledger*'s citations of Allison's work as well his latter's direct knowledge of and personal interactions with Organski. The "does it matter" aspect is part of *Thucydides's Trap*, as we as academics are told so often to take what we know and put it in the hands of policymakers and the public. When someone has been successful with this, as Allison so clearly has, do we then follow that new lead in scholarship and change citations, names, and theoretical paradigms to the follow this popular reception of the concept?<sup>13</sup>

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<sup>11</sup> Organski and Kugler, *The War Ledger* (Chicago: University of Chicago Press, 1980).

<sup>12</sup> Jonathan M. DiCicco, "Power Transition Theory and the Essence of Revisionism," in Thompson, ed., *The Oxford Encyclopedia of Empirical International Relations Theory*, 188-214, is an accurate clear investigation into modern power transition theory and it's extremely high degree of similarity with alternatives such as Allison's new work and the degree of differing usages such as the limited incorporation of extensions into measurements within the *Thucydides's Trap?* framework.

<sup>13</sup> Please see 200-201 for specifics.

## REVIEW BY JACK S. LEVY, RUTGERS UNIVERSITY

The idea that power shifts between great powers are one of the major causes of interstate war goes back to Thucydides's argument that Sparta's fear of the rising power of Athens made the Peloponnesian War inevitable.<sup>1</sup> A.F.K. Organski systematized the argument in "power transition theory" in the 1950s, and Organski and his followers have engaged in an ongoing research program to further develop and test the theory.<sup>2</sup> More recently, Graham Allison captured the argument in the concept of the "Thucydides Trap," and argued that the rise of China has created the structural conditions that might be conducive to a Sino-American war.<sup>3</sup> Allison has been particularly vocal in warning the U.S. policy community and public about the increased risk of war as China approaches or surpasses the U.S. in power. His argument, and the Thucydides's Trap Project upon which it is based, have been less influential among scholars. One can better understand why after reading Chan's *Thucydides's Trap*, which provides the most thorough critique to date of Allison's argument and the reasoning and evidence underlying it. Chan also provides an extensive critique of power transition theory, which has been more influential among international relations scholars.<sup>4</sup>

Those familiar with Chan's earlier writings will recognize in this book Chan's longstanding insistence on theoretically coherent arguments, systematic research design and empirical evidence, and interest in policy implications.<sup>5</sup> The book makes many important contributions to our understanding of power shifts and international conflict and of the possible implications of the rise of China. Chan introduces some conceptual distinctions that are central to the analysis of power transitions but that are often neglected by scholars—for example, between regional and global ambitions and between the hierarchy of power and the nature of international order. Chan makes a strong argument that the dominant power does not always have status quo motivations and that the rising power does not always have revisionist motivations, and that these are empirical questions to be investigated. He notes the importance of the status concerns of rising and falling powers, which power transition theorists generally neglect. In his important critique of the monocausal nature of Allison's Thucydides's Trap, Chan reminds us of the role of human agency in Thucydides's *History*. Chan also makes the important argument that the use of power-transition concepts by policy makers and others are sometimes best evaluated as strategic rhetoric driven by identifications of friends and foes and by policy preferences. In addition, Chan supports his theoretical arguments with an impressive range of historical evidence. He uses those arguments and evidence to highlight the limitations of Allison's Thucydides's Trap arguments about China's rise, its power and intentions, and the likely trajectories of each.

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<sup>1</sup> Thucydides, *History of the Peloponnesian War*. In Robert B. Strassler (ed.), *The Landmark Thucydides* (New York: Free Press, 1996), 1.23.

<sup>2</sup> A. F. K. Organski, *World Politics* (New York: Alfred A. Knopf, 1958), chap. 14; A. F. K. Organski and Jacek Kugler, *The War Ledger* (Chicago: The University of Chicago Press, 1980), chap. 1; Jacek Kugler and Douglas Lemke (eds.), *Parity and War* (Ann Arbor: University of Michigan Press, 1996); Ronald L. Tammen et al., *Power Transitions: Strategies for the 21st Century* (New York: Chatham House Publishers, 2000).

<sup>3</sup> Graham Allison, *Destined for War: Can America and China Escape Thucydides's Trap?* (Boston: Houghton-Mifflin, 2017).

<sup>4</sup> On similarities and differences between power transition theory and the Thucydides' Trap Project and on the more solid scientific grounding of the former, see Jonathan M. DiCicco, "Power Transition Theory and the Essence of Revisionism," in William R. Thompson (ed.), *The Oxford Encyclopedia of Empirical International Relations Theory*, vol. 3 (New York: Oxford University Press, 2018), 188-214. Given the overlap between the power transition theory research program and the Thucydides Trap Project, it is surprising that the latter does not acknowledge the former.

<sup>5</sup> Steve Chan, *China, the U.S., and the Power-Transition Theory: A Critique* (London: Routledge, 2008); Steve Chan, *Trust and Distrust in Sino-American Relations: Promises and Challenges* (New York: Cambria Press, 2017).

Given the China expertise among other contributors to this roundtable, I will leave to them assessments of Chan's arguments about the likely trajectory of Chinese power, its current and future intentions in Asia and in the world, and the implications for Sino-American relations. I will focus on Chan's theoretical and historical critique of Allison's Thucydides's Trap Project and of power transition theory in particular. Although there are important points of overlap, there are many differences as well, and some critiques of the Thucydides Trap do not necessarily apply to power transition theory. Although I agree with many of Chan's criticisms of power transition theory, I think that others are misplaced, though in many cases our disagreements are due to ambiguities in power transition theory itself.<sup>6</sup>

I begin with Chan's basically correct statement that "Power-transition theorists are ... explicit in arguing that great-power wars are started by rising upstarts" (31). This raises several issues, in part because the question of who initiates war is closely connected to the question of when the war begins. I limit my focus to wars that occur during power transitions.<sup>7</sup> True, Organski was quite clear in his original statement of power transition theory that it is the rising challenger who initiates the war and does so before overtaking the dominant state.<sup>8</sup> Recognizing that Organski based his argument on a small handful of cases from the 1930s, Organski and Kugler conducted a more thorough empirical investigation. They concluded that the rising challenger initiated the war but only after it had surpassed the dominant state.<sup>9</sup> Power transition theorists have yet to resolve the 'when' issue, other than that it must be after "parity," which is operationally defined as the point at which the rising challenger attains 80% of the power of the dominant state.<sup>10</sup> Assessing the power transition research program from the inside, Ronald L Tammen et al. argued in 2000 that "overtakings provide the preconditions for conflict because the challenger anticipates a fair chance of winning." A few pages later, however, they argue that "Power Transition scholars continue to debate this question. The most recent research indicates that the probability of war increases prior to the overtaking .... After the overtaking, the probability of war decreases...."<sup>11</sup> Thus the "when" question remains unresolved and needs more attention.

Questions about when during power transitions wars occur lead directly to the 'who initiates' question. One problem with Organski's challenger-initiates-before-overtaking argument is that the challenger is likely to lose. It has incentives to "bide its time and hide its brilliance," to quote Chinese leader Deng Xiao-ping, and wait until it is stronger and in a better

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<sup>6</sup> There are several variants of power transition theory. I focus on the theory developed by Organski and further revised and tested by his students and colleagues (see fn. 2), which is the most influential one. An important alternative is Robert Gilpin's theory of hegemonic transitions and war: Gilpin, *War and Change in World Politics* (Cambridge: Cambridge University Press, 1981). This is a theoretically rich and historically well-grounded book, but it has not generated a sustained research program involving empirical tests and subsequent theoretical emendations.

<sup>7</sup> Organski and Kugler (*The War Ledger*, 39) and many other power transition theorists argue that power transitions are a necessary condition for great power war, or at least for war between "contenders" in the "central system." This is incorrect, as Chan documents by identifying numerous great power wars that have occurred in the absence of power transitions. Chan argues effectively that power transitions are neither necessary nor sufficient for war (44, 194). Note, however, that to say that a particular factor is neither necessary nor sufficient for war does not settle the question of its causal importance, because it does not preclude very strong probabilistic relationships. To say that something is either necessary or sufficient is a much more informative statement. On necessary and sufficient conditions see Gary Goertz and Jack S. Levy, "Causal Explanation, Necessary Conditions, and Case Studies," in Goertz and Levy (eds.), *Explaining War and Peace: Case Studies and Necessary Condition Counterfactuals* (New York: Routledge, 2007), 9-45.

<sup>8</sup> Organski, *World Politics*, 333. The challenger's motivations are to accelerate the power transition and then use its power to restructure the international order and to reap the benefits it deserves from the system based on its increased power.

<sup>9</sup> Organski and Kugler, *The War Ledger*, chap. 1

<sup>10</sup> Tammen et al., *Power Transitions*, 21.

<sup>11</sup> Tammen et al., 22, 28.

bargaining position.<sup>12</sup> The dominant state, in relative decline, anticipates this, and has possible incentives to initiate a preventive war before the point of transition, with the aim of blocking the challenger's rise before the window of opportunity closes.<sup>13</sup> Organski briefly acknowledged this possibility in his original formulation of the theory, adding that it would be "foolish" for the rising state to attack while it was still weaker.<sup>14</sup> However, neither Organski nor his followers devote adequate attention to the theoretical problems associated with the hypothesis that the challenger initiates war before overtaking the declining power. This is probably due to the fact that most power transition research has been focused on the empirical question of whether power transitions lead to war, and under what conditions.

One attempt to engage the issue theoretically is the work of Tammen et al., who note that the international order created by the dominant state includes "standard rules and norms" that provide benefits for itself and for other satisfied states. Tammen and his colleagues then argue that the dominant state recognizes that a strategy of preventive war would be "counterproductive" because it would "abrogate the rules and cause uncertainty in the alliance of satisfied states, even possibly tearing it apart."<sup>15</sup> This is not a persuasive argument. There are many reasons why a declining dominant power facing a dissatisfied challenger might not resort to a preventive war and instead might allow a power transition, despite the fact that the transition could lead to increasing demands from a more powerful adversary. I doubt, however, that concerns about violating norms and alienating allies are at the top of the list.<sup>16</sup> Moreover, why would the allies of the dominant state, who are satisfied with the existing system, be more concerned with the breaking of norms than with the dominance of a new power and its imposition of a new international order? This is, of course, an empirical question, and it would be worth exploring the deliberations of dominant states and their allies when faced with rising challengers. Even more basic, however, is the descriptive empirical question of who initiates wars and when during power transitions. It has received far too little attention from both power transition theorists and their critics.

I regard initiation by the rising power and preventive war by the declining power as two alternative causal mechanisms through which war might occur during a power transition.<sup>17</sup> There may be other mechanisms as well. Chan himself suggests an alternative mechanism through which power transitions can lead to war. He argues that "human emotions—such as envy, anger, resentment, arrogance, and even desperation in addition to fear—can possibly provide the missing link connecting power shifts to war outbreak because these feelings can incline leaders to undertake more belligerent actions," and traces this argument to Thucydides (149). Chan also brings in prospect theory to help explain why leaders of declining states are often risk-acceptant in their choices and behavior.<sup>18</sup>

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<sup>12</sup> Cited in Chan, 6.

<sup>13</sup> Jack S. Levy, "Declining Power and the Preventive Motivation for War," *World Politics* 40:1 (October 1987): 82-107 at 83-84. See also Chan, *China, the U.S., and the Power-Transition Theory*.

<sup>14</sup> Organski, *World Politics*, 333.

<sup>15</sup> Tammen et al., *Power Transitions*, 27.

<sup>16</sup> Things get more complicated in the nuclear age, but I suspect that the diplomatic costs of violating norms and rules by launching a preventive war against an established nuclear power pale in comparison with the material costs.

<sup>17</sup> I interpret Chan as seeing things a little differently, with preventive war as being a distinct path to war, outside of power transition theory. He may be right about the Organski et al. power transition theory, but I am speaking more broadly about power transitions.

<sup>18</sup> On prospect theory see Daniel Kahneman and Amos Tversky, "Prospect Theory: An Analysis of Decision under Risk," *Econometrica* 47:2 (March 1979), 263-291. Note that while Chan emphasizes human emotions (149), prospect theory is primarily

I leave it to those with more expertise on Thucydides and on the Peloponnesian War to comment on the validity of Chan's argument about the impact of emotions and other non-rationalist factors in Thucydides, but it strikes me as plausible and reasonably well supported by some of the secondary literature.<sup>19</sup> It is certainly a plausible mechanism leading to war in other power transitions, most likely in combination with other mechanisms. It would help explain, for example, the decisions of desperate and fearful leaders of declining states to take extraordinary risks in adopting preventive war strategies. It would also help explain why leaders of rising states, instead of being 'wise' and waiting until they are stronger, are instead 'foolish' and lash out at the dominant state prematurely.<sup>20</sup>

Although I fully agree with Chan on the importance of non-rationalist elements in contributing to the slide to war during power transitions, I think it is misleading to suggest that power transition theory in the Organski tradition gives much emphasis to non-rationalist arguments.<sup>21</sup> One can certainly find a few elements of non-rationality in Organski's original 1958 formulation. His argument that it is foolish to start a war before the point of overtaking is one example. Organski's mention of the status concerns of rising states is another.<sup>22</sup> As I noted above, however, most scholars in the power transition research program are more interested in constructing and implementing tests to determine the empirical association between power shifts and war than in explaining the intervening causal linkages. As Chan argues, power transition theorists are not particularly interested in mechanisms, and leave unexplored how one gets from power shifts to war/peace outcomes (149). Another way to say this is that power transition theory lacks a theory of bargaining.<sup>23</sup>

One central power transition theory variable that is rarely incorporated into empirical tests of the theory is the rising power's satisfaction or dissatisfaction with the status quo. This factor was critical for Organski, who made it clear that rising challengers went to war only if they were dissatisfied with the status quo. As Chan notes, Organski and Kugler argue that "satisfied powers do not fight" (39). Power transition theorists have made some efforts to operationalize the satisfaction/dissatisfaction concept, but each attempt has generated substantial objections, and no consensus has emerged. Perhaps this explains the neglect of this critical variable in empirical tests.<sup>24</sup>

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cognitive, and arguably includes rationalist as well as non-rationalist elements. Jack S. Levy, "Prospect Theory, Rational Choice, and International Relations," *International Studies Quarterly* 41:1 (March 1997): 87–112.

<sup>19</sup> Richard Ned Lebow, *The Tragic Vision of Politics: Ethics, Interests and Orders* (Cambridge: Cambridge University Press, 2003), chap. 3; Jonathan Kirshner, "Handle Him with Care: The Importance of Getting Thucydides Right," *Security Studies* 28:1 (2018), 1–24.

<sup>20</sup> Under some conditions reputational interests and alliance considerations may lead rising powers to adopt hardline bargaining strategies. For an application to the relatively uncompromising behavior of an increasingly powerful Russia prior to World War I, see Jack S. Levy and William Mulligan, "Shifting Power, Preventive Logic, and the Response of the Target: Germany, Russia, and the First World War," *Journal of Strategic Studies* 40:5 (2017): 731–769.

<sup>21</sup> Sharing this rationalist assessment of power transition theory is DiCicco, "Power Transition Theory," 195–196.

<sup>22</sup> Organski, *World Politics*, chap. 14.

<sup>23</sup> Jonathan M. DiCicco and Jack S. Levy, "Power Shifts and Problem Shifts: The Evolution of the Power Transition Research Program," *Journal of Conflict Resolution* 42:4 (December 1999), 675–704 at 698–699.

<sup>24</sup> Proposed indicators of the degree of dissatisfaction with the status quo include similarity of alliance portfolios and the cost of borrowing money. For a review and critique see Andrew Q. Greve and Jack S. Levy, "Power Transitions, Status Dissatisfaction, and War: The Sino-Japanese War of 1894–1895," *Security Studies* 27:1 (2018), 148–178, at 151–153. A more promising recent effort to operationalize (dis)satisfaction is Susan G. Sample, "Power, Wealth, and Satisfaction: When Do Power Transitions Lead to Conflict?" *Journal of Conflict Resolution* 62:9 (October 2018):1905–1931.

In this conceptual vacuum, Chan makes an important contribution. He correctly observes that power transition theory posits that the dominant power is always satisfied with the status quo because it created the international order to maintain and advance its interests. Chan is doubtful, and correctly argues that this issue should be settled not by definition but instead by observation and measurement (46). Chan also makes the important argument that states' concerns about their status in the system can influence their degree of satisfaction with the status quo.<sup>25</sup> Power transition theorists have neglected this important variable of status satisfaction/dissatisfaction. Chan goes on to make a powerful argument that the United States, during its position as the dominant power, has not been satisfied with the status quo and has in fact been a revisionist power. He also argues that China has for the most part been a non-revisionist power. Scholars will debate this question about China's orientation, but Chan is certainly correct that who is the revisionist and non-revisionist power is a matter for empirical study guided by analytic criteria applied consistently across cases.

Despite Chan's important contributions to the discussion of status quo and revisionist orientations in power transitions, I have a few quarrels. One is the relatively minor point that Chan confounds the concepts of dissatisfaction and revisionism. Whereas Organski and his followers focus on satisfaction/dissatisfaction as a key predictor of peace/war within power transitions, Chan focuses on status quo orientations versus revisionist motivations. Although most revisionist states are presumably dissatisfied with the status quo, not all dissatisfied states are revisionist. First, small and medium powers lack the capabilities to adopt revisionist policies. In addition, some dissatisfied great powers that have the capabilities to pursue revisionist policies may choose to adopt alternative strategies that carry a lower risk of war. Chan may be right that there is a strong link between dissatisfaction and revisionism in power transition theory, but it is best to separate dissatisfaction from the policies that may flow from it, and leave the link between them for theoretical and empirical exploration. As DiCicco asks, when do dissatisfied states act on their discontent and become revisionist states?<sup>26</sup>

This leads us to Chan's generally excellent discussion of nineteenth century Anglo-American relations in chapter 4. One thing that is problematic, however, is his argument that the absence of a war between Britain and the United States at the end of the century runs "contrary to the expectations of Thucydides's Trap and power transition theory" (61). Chan is correct that the absence of war in this case is a problem for Allison's Thucydides's Trap but wrong about the implications for power transition theory. Given power transition theory's central propositions that a power transition and the rising state's dissatisfaction with the status quo are each nearly necessary conditions for a great power war, and given the plausible argument that the United States was satisfied with the existing liberal international order and only wanted a bigger piece of the pie, it is fair to say that the peaceful Anglo-American transition does not violate power transition theory.

This is certainly the view of most power transition theorists. As Tammen et al. argue, despite the strong growth of the United States (surpassing the United Kingdom in GDP between 1875 and 1880), the transition was peaceful because "both nations supported the status quo established under the Pax Britannica." They go on to say that "This satisfaction probably derived from British leadership, a common institutional heritage, American political separation from European affairs, and a profitable market for British capital in America." British understanding of American satisfaction "helped reduce British anxieties and suspicions of American growth."<sup>27</sup> Chan makes a useful contribution, however, by reminding us how tense the relationship was during the U.S. Civil War (and the 1861 Trent Affair in particular), and that by the time of the 1895-1896 Venezuela boundary crisis the rising power of Germany posed a greater threat to Britain.

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<sup>25</sup> This is not limited to rising states demanding more status. It includes aggressive behavior by dominant states in decline who are fearful of losing their status. Chan's emphasis on "intrinsic" as well as "instrumental" dimensions of status reinforce his argument about non-rationalist variables influencing behavior during power transitions. Instrumental aspects of status are generally tied to more material considerations.

<sup>26</sup> DiCicco, "Power Transition Theory," 205-208. DiCicco also questions the utility of treating (dis)satisfaction as a dichotomous variable, and asks what level of dissatisfaction is necessary to trigger revisionist policies.

<sup>27</sup> Tammen et al., *Power Transitions*, 49-50.

In his discussion of the Anglo-American transition Chan argues that it is “the one clear case of power transition in history” (102, 195). It is certainly a clear case, but it is not the only one. In the modern world, Britain’s rise in the seventeenth century and its replacement of the Netherlands as the leading commercial, financial, and naval power in the global system would be an example, one that involved a series of wars in which commercial issues and naval dominance were paramount.<sup>28</sup> On the European continent, the shift from Spanish to French dominance by the end of the Thirty Years War and the Treaty of Westphalia would be another.<sup>29</sup>

These last two examples raise questions relating to possible differences between power transition dynamics in the global system and in continental systems. Few analysts of power transitions are explicit about the nature of the system they are investigating. Organski and his followers, by operationalizing power in terms of GDP and by neglecting land-based military power, implicitly focus on the global system.<sup>30</sup> Some things are different in the global system, including balance of power dynamics. There is strong evidence that while great powers have generally balanced against very high concentrations of power in the European system during the last five centuries, they have rarely balanced against very high concentrations of power in the global system.<sup>31</sup>

Another possible difference in power transitions in global and regional systems relates to the Organski et al argument that the dominant state use its power to create an international order and a set of rules and norms that benefit itself and its allies. Their historical examples are Britain and then the United States, the leading commercial, financial, and naval powers of the time. Do continental land powers, who often lack the economic strength to provide public goods for the system, create the same kind of orders? Chan (chap. 3) is right to question Organski and Kugler’s inclusion of the Franco-Prussian War (1870-1871) and the Russo-Japanese War (1904-1905) along with the two world wars in their empirical analysis of power transitions. None of these states was interested in challenging Britain or the global order.<sup>32</sup>

Chan also makes strong arguments that in the two world wars Germany was driven primarily by the aim of continental hegemony rather than challenging Britain’s position as the dominant power in the world (chap. 4). This gets complicated, because the European and global systems have been interconnected throughout the last five centuries of the modern system. Britain’s grand strategy for centuries was to prevent any state from achieving a dominant position on the continent for fear that state would then have the resources to challenge Britain’s dominant position in the global system. But Chan is basically right that power transition theory gets it wrong by framing the two world wars as a power transition struggle between Britain and Germany for global supremacy. In World War I in particular, the main threat perceived by Germany was the rising power of Russia.<sup>33</sup> Allison also mischaracterizes World War I as an Anglo-German struggle. The Anglo-German naval

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<sup>28</sup> Charles Wilson, *Profit and Power: A Study of England and the Dutch Wars* (The Hague: Martinus Nijhoff, 1978); Jack S. Levy, “The Rise and Decline of the Anglo-Dutch Rivalry, 1609-1689,” in William R. Thompson, ed., *Great Power Rivalries* (Columbia: University of South Carolina Press, 1999), 172-200.

<sup>29</sup> Geoffrey Parker, *The Thirty Years’ War* (London: Routledge & Kegan Paul, 1984).

<sup>30</sup> An important exception is Douglas Lemke, *Regions of War and Peace* (Cambridge, UK: Cambridge University Press, 2000), which is a theory of regional power transitions.

<sup>31</sup> Jack S. Levy and William R. Thompson, “Balancing on Land and at Sea: Do States Ally Against the Leading Global Power?” *International Security* 35:1 (Summer 2010): 7-43.

<sup>32</sup> Chan makes a useful distinction between the dominant state in the international power hierarchy and the international order, and criticizes power transition theory for its conflation of the two (47).

<sup>33</sup> I also agree with Chan’s argument that Germany was driven by preventive logic (68-69). What Chan fails to emphasize, however, is the context of the Franco-Russian alliance. German leaders worried about a two front war once growing Russian strength undermined the assumptions upon which the Schlieffen Plan was based. They did not fear the scenario of a bilateral Russo-German war.

race was basically over by 1911-12, at which point Germany made a “retreat to the European continent” and reached a détente with Britain.<sup>34</sup> The Anglo-German arms race left lingering tensions that were consequential, but they were secondary to the primary competition on the continent.<sup>35</sup> For an analysis of the outbreak of the two world wars, balance of power theory, with its focus on the distribution of military power and alliances in Europe, is a better place to start than is power transition theory.

These examples suggest that power shifts in regional systems differ in important ways from power shifts in global maritime systems, and that some power shifts involve both regional and global dimensions. Different kinds of power shifts involve different dynamics and presumably different probabilities of escalation to war, though this question remains undertheorized and underexplored empirically. Still, we can ask how the Sino-American rivalry fits into these categories of power shifts. Is this a competition between a rising regional power and a global power for regional dominance, or a competition for dominance in the global system?<sup>36</sup> The later, but presumably, not the former, might involve a remaking of the rules and norms of the global system that Organski posited was at the heart of power transitions. As I have argued elsewhere, applications of power transition theory have focused too much on power shifts in the global system and not enough on the distinctive dynamics in regional systems, and consequently have neglected the critical question of how the rise of a regional power might affect its relationship with a global power. The theoretical integration and empirical investigation of power dynamics at the regional and global levels is an urgent task for future research, with important implications for theory, historical interpretation, and policy.<sup>37</sup>

This is just one of the many ideas that Chan’s *Thucydides’ Trap?* stimulated me to consider. Chan has written the most thorough critique I have seen of Allison’s Thucydides’s Trap project and of power transition theory, a critique that is richly informed by history as well as by theory. Reading and engaging with this book has led me to rethink some of my old ideas about power transitions, to consider some new wrinkles and how to formulate and test them, and to think in new ways about the likelihood of a Sino-American transition and its possible implications.

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<sup>34</sup> V.R. Berghahn, *Germany and the Approach of War in 1914* (New York: St. Martin's, 1973), chap. 7: “Retreat to the European Continent”; Sean M. Lynn-Jones, “Detente and Deterrence: Anglo German Relations, 1911-1914,” *International Security* 11:2 (Fall 1986), 121-50.

<sup>35</sup> Allison adds a few pages at the end of his World War I chapter on the German-Russian dyad, but this discussion is not really integrated into the rest of the chapter. It comes across as a last-minute addition, as if it were in response to the suggestions of a reviewer. Allison, *Destined for War*, chap. 4.

<sup>36</sup> William R. Thompson, *American Global Pre-eminence: The Development and Erosion of Systemic Leadership* (New York: Oxford University Press, 2021), chap. 5. Thompson notes that Allison includes both regional and global power shifts in his data base of sixteen power transitions since the late fifteenth century, but that Allison fails to differentiate among them in his analysis of the tendency of power transitions to end up in war.

<sup>37</sup> Jack S. Levy, “Power Transition Theory and the Rise of China,” in Robert S. Ross and Zhu Feng (eds.), *China’s Ascent: Power, Security, and the Future of International Politics* (Ithaca, New York: Cornell University Press, 2008), 11-33, at 32. For a useful analysis of the historical interaction between rising European powers and the leading global power, see Karen A. Rasler and William R. Thompson, *The Great Powers and Global Struggle, 1490-1990* (Lexington: The University Press of Kentucky, 1994).

## REVIEW BY J. PATRICK RHAMEY JR., VIRGINIA MILITARY INSTITUTE

In *Thucydides's Trap?*, Steve Chan offers an important reminder of the complex causal process that underlies policy-making and the dangers of basing policy on overly simplistic theories. Chan's admonition reminds us not to repeat the mistakes of the Cold War, where variables related to domestic politics, ideology, bureaucracy, and economics were all often ignored in favor of systemic concepts like power distribution, which, according to the neorealists of the time, had to be balanced. No matter how far policies departed from common sense, such as balancing thousands of nuclear warheads or building irrelevant numbers of tanks, foreign policy became subservient to an overly simplified, monocausal, systemic theory that both produced a number of foreign policy blunders and, on several occasions, nearly resulted in nuclear war. Adherence to the theory resulted in policymakers being blindsided by the collapse of their major power opponent, as the Soviet Union could not overcome internal economic and political pressures. Meanwhile, continued devotion to the paradigm, despite the reality unfolding about them, lent itself to predictions that described a world directly opposite to that which actually occurred.<sup>1</sup>

As the attention of American policymakers is increasingly oriented toward anxiety over the United States' relationship with China, Chan reminds American leaders and academics alike that they would do well not to forget that variables related to preferences, geography, economics, and domestic politics matter, and these should not be subordinate to systemic variables like the distribution of power. In challenging Graham Allison's *Destined for War: Can America and China Escape Thucydides's Trap?*,<sup>2</sup> Chan artfully demonstrates that international politics is far more complicated than such a monocausal, systemic theory could possibly suggest. Designing policy after such a theory will not only result in a failure to explain outcomes, but may also lead to a conflict that would not necessarily otherwise have occurred. By treating China as an inevitable, hostile challenger through American provocations and punitive measures, we create a self-fulfilling prophecy. Dissatisfaction should not be treated as inevitable, and on this perhaps most important point of Chan's analysis, many researchers operating within the power transition approach would agree.<sup>3</sup> Indeed, some suggest that without a stronger centralized state and demographic transformation in the aging structure of the population, a clear and rapid overtaking of the United States by China may not be inevitable.<sup>4</sup>

While quite thought provoking and offering a number of potentially interesting questions for future research, the text is less compelling when Chan shifts his focus away from the Thucydides's Trap onto the multivariate, multilevel paradigm that is power transition theory. To some extent, there is an irony in Chan's criticism of power transition theory, as A.F.K. Organski and Jacek Kugler's history of research<sup>5</sup> seeks to accomplish similar goals as Chan's. For these founders of power transition theory, international politics also cannot be explained by a monocausal, systemic, and deterministic theory, like balance of power neorealism or the Thucydides's Trap. They agree that other variables matter, and that neorealism failed by refusing to include variables like satisfaction and the sources of power, which are rooted in institutional capacity,

<sup>1</sup> See, for example, John J. Mearsheimer, "Back to the Future: Instability in Europe after the Cold War," *International Security* 15:1 (1990): 5-56.

<sup>2</sup> Graham T. Allison, *Destined for War: Can America and China Escape Thucydides's Trap?* (New York: Houghton Mifflin, 2017).

<sup>3</sup> This is stated particularly clearly in Douglas Lemke and William Reed, "Power is Not Satisfaction," *Journal of Conflict Resolution* 42:4 (1998): 511-516. See also the final chapter of J. Patrick Rhamey Jr. and Tadeusz Kugler, *Power, Space, and Time: An Empirical Introduction to International Relations* (Lanham: Rowman and Littlefield, 2020).

<sup>4</sup> Kugler and Siddharth Swaminathan. "The Politics of Population," *International Studies Review* 8:4 (2006): 581-596.

<sup>5</sup> See, for example, A. F. K. Organski, *World Politics* (Berkeley: University of California Press, 1968) and Organski and Jacek Kugler, *The War Ledger* (Chicago: University of Chicago Press, 1980).

demography, and development. There are, in fact, many parallels between Chan's criticisms of power transition and Organski's own criticisms of neorealism.<sup>6</sup>

*The Power Transition Literature*

Throughout the text, Chan's criticisms are based upon a few older works from power transition researchers,<sup>7</sup> unfortunately overlooking a great deal of more recent research. Perhaps most importantly for the fate of power transition as a valuable research paradigm, and therefore the most relevant point for policymakers, Chan's most noteworthy criticisms, such as the timing of conflict onset relative to transition and measurement issues, are not part of what Johnathan M. DiCicco and Jack S. Levy label the "core" of the paradigm in their own previous evaluation.<sup>8</sup> In short, these challenges are empirical questions that may require empirical answers, but the nature of that answer does not negate the core tenants of power transition theory. For example, while power transition theorists appear to maintain that war is most likely after a challenger surpasses a dominant power, domestic, regional, and geographic variables thoroughly moderate the causal process. Furthermore, while demonstrated empirically that pre-emption by a dominant power is unlikely compared to initiation by a challenger after a transition,<sup>9</sup> whether conflict is more likely before, after, or during a transition by either challenger or dominant state, the core tenant is merely that parity increases the probability of war: an empirical proposition that is consistently confirmed.<sup>10</sup>

Therein lies a key misunderstanding behind Chan's critique. Chan seems to portray power transition theory as a simple and deterministic theory like the Thucydides's Trap. But, this portrayal is somewhat unfair given the progression of the power transition research program. First, power transition theory is not deterministic, but probabilistic in its causal process, which is rooted in its core theoretical assumptions that a hierarchy among actors governs the system and power is a dynamic quality rooted in the domestic economic and political realm.<sup>11</sup> War is more likely if two states are at parity. War is more likely if a state is dissatisfied. War is particularly more likely if two states are at parity and one is dissatisfied. It is never a certainty, and satisfaction is not a causal consequence of parity.<sup>12</sup>

Second, as Chan points out in his illustrative case studies, evaluation of these probabilistic hypotheses is challenging because of the rare number of transitions at the pinnacle of the international system. With such a small pool of cases where both parity and satisfaction are present, a statistical analysis producing generalizable conclusions is problematic. The bulk of the text then hammers this point, with repeated rhetorical questions followed by a series of case studies that appear to dismantle power transition theory, at least in the form in which it is presented by Chan. These case studies often effectively dismantle monocausal, systemic theories like Allison's Thucydides's Trap, but fail to fully engage the more complex power transition approach. In some cases, they are directly refuted by extant power transition research, such as Chan's criticism of the power

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<sup>6</sup> See the discussion in Organski, *World Politics*, 287.

<sup>7</sup> See, for example, Organski and Kugler, *The War Ledger*.

<sup>8</sup> Jonathan M. DiCicco and Jack S. Levy, "Power Shifts and Problem Shifts: The Evolution of the Power Transition Research Program," *Journal of Conflict Resolution* 43:6 (1999): 675-704.

<sup>9</sup> Lemke, "Investigating the Preventative Motive for War," *International Interactions* 29:4 (2003): 273-292.

<sup>10</sup> Alex Braithwaite and Lemke, "Unpacking Escalation," *Conflict Management and Peace Science* 28:2 (2011): 111-123.

<sup>11</sup> DiCicco and Levy, "Power Shifts," 684.

<sup>12</sup> Lemke and Reed, "Power is not Satisfaction."

balances in World War I contradicting power transition's expectations.<sup>13</sup> However, in some cases, Chan raises some important points, and events like the Crimean War deserve more attention regardless of large-n results in the power transition literature.

Chan's criticisms on the small-n problem are not new, however, as power transition theory has long recognized the rare frequency of major power war as an empirical problem. Yet, this problem brought about the expansion of empirical inquiry beyond the most powerful states to all actors and a variety of topics while not losing sight of the theoretical core. Analyses include<sup>14</sup> research on domestic political capacity,<sup>15</sup> the consequences of parity across geographic space,<sup>16</sup> the sources of satisfaction in domestic values,<sup>17</sup> the development of regional orders,<sup>18</sup> onset of civil wars,<sup>19</sup> and post-war recovery.<sup>20</sup> Indeed, insights into demography and political capacity may be power transition's greatest contributions to contemporary international relations research. Given his effective dismantling of monocausal, systemic approaches, I would have been very interested to read Chan's evaluation of this more complete body of literature within the power transition tradition. In many cases, answers to Chan's questions and criticisms lie in this more recently developed research.

Focusing on power transition theory but overlooking much of extensive research that has occurred since the publication of *The War Ledger* in 1980<sup>21</sup> muddles the effectiveness of Chan's *Thucydides's Trap* criticism, with which scholars who have analyzed power transition would certainly agree. For example, in a comparison with power transition, DiCicco finds the

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<sup>13</sup> Setting aside that it is not reasonable to include American power in evaluating the German decision to initiate the conflict, as the United States did not enter the war until 1917, see Kugler, Ronald L. Tammen, and John Thomas, "How Political Performance Impacts Conflict and Growth," in Kugler and Ronald L. Tammen, eds., *Performance of Nations* (Lanham: Rowman and Littlefield, 2012). A debate on all the cases could fill volumes, but some cases employed, such as Japan and the US in World War II, the War of 1812, the Korean War, and the Spanish-American War, are not directly relevant to rising challenger overtaking a dominant power hypothesis of power transition theory.

<sup>14</sup> For a more complete accounting of the wide array of research with the power transition paradigm, see Tammen, Kugler, and Lemke, "Foundations of Power Transition Theory," in William R. Thompson, ed., *Oxford Encyclopedia of Empirical International Relations Theory* (Oxford: Oxford University Press, 2018).

<sup>15</sup> Kugler and Mariana Arbetman, "Choosing among Measures of Power," in Richard J. Stoll and Mark Ward, eds, *Power in World Politics* (Boulder, CO: Lynne Rienner, 1989).

<sup>16</sup> Lemke, *Regions of War and Peace* (Cambridge: Cambridge University Press, 2002); Rhamey Jr., Michael O. Slobodchikoff, and Thomas J. Volgy, "Order and Disorder across Geopolitical Space: the Effect of Declining Dominance on Interstate Conflict," *Journal of International Relations and Development* 18:3 (2015): 383-406,

<sup>17</sup> Birol Yesilada, *EU-Turkey Relations in the 21<sup>st</sup> Century* (London: Routledge, 2013).

<sup>18</sup> Tammen and Kugler, *The Rise of Regions: Conflict and Cooperation* (Lanham: Rowman and Littlefield, 2020).

<sup>19</sup> Michelle Benson and Kugler, "Power Parity, Democracy, and the Severity of Internal Violence," *Journal of Conflict Resolution* 42:2 (1998): 169-209.

<sup>20</sup> T. Kugler, Kyung Kook Kang, J. Kugler, Marina Arbetman-Rabinowitz, and John Thomas, "Demographic and Economic Consequences of Conflict," *International Studies Quarterly* 57:1 (2013): 1-12.

<sup>21</sup> Organski and Kugler, *The War Ledger* (Chicago: University of Chicago Press, 1980).

failures of the Thucydides's Trap approach are partly in its simplicity and omitted key variables.<sup>22</sup> As researchers of international politics, we should not ignore the systemic level, but should also recognize that it is not the sole domain of political behavior, forcing states regardless of their characteristics into a single course of policy action. Instead, the system provides the canvas on which international politics take place. As Organski and power transition researchers emphasize, the characteristics of the states on that canvas determine capabilities and behavior.

### *Measurement and Satisfaction*

The most compelling of Chan's criticisms of power transition lie in problems of measurement that help contribute to emerging views of China as a hostile challenger. Power is but one example, and as mentioned, treating China as currently overtaking the United States misses a variety of indicators to the contrary. Indeed, predictions of China's inevitable rise and dissatisfaction with the status quo by some power transition theorists have not always aged well,<sup>23</sup> but those are primarily due to measurement problems of causal variables rooted in domestic politics, like population and economic growth. The extent to which China appears to possess 'parity' is heavily dependent on the measure employed. In the case of the popularly used Correlates of War CINC score,<sup>24</sup> there is no proper accounting for either capacity (which is essential to power transition theory) or Chinese ability to project its military power beyond its borders. Chan appears to be in agreement with power transition on this issue, as power transition theory has developed measures that amend raw capability indicators, like GDP, to better capture the ability of states to employ their resources (political capacity). Unfortunately, though suggested by Organski and now empirically measured,<sup>25</sup> this research program on political capacity is overlooked in Chan's text, though its existence is in direct contradiction to the claim that power transition researchers "often overlook the more important but less tangible determinants of national power such as a country's institutional capacity (112)."

On the question of measuring satisfaction, however, Chan provides power transition theory a possible way forward for future research with his discussion of status. Satisfaction is more complex than any single measure, whether participation in institutions, UN voting behavior, or increases in military spending. Satisfaction is, instead, a commitment to the broader suite of norms and behaviors that the dominant power has used to shape the system, in this case liberal normative values of democracy, commerce, and human rights, however imperfectly those values may be protected by the United States at home or abroad. This order exists in contrast to the prior Westphalian system of sovereignty, to which China still often makes reference in defending its conduct as it engages in internally draconian measures, such as the genocide of the Uyghur people. Indeed, it is one thing to suggest that the United States imperfectly embodies the status quo, it is another entirely to imply that China is more satisfied with a liberal status quo than the United States.

On measuring satisfaction, as with the advancements and adaptations made by power transition to the concept of power, the likely answer may not be any one variable, but perhaps more likely a range of variables that capture different facets of the

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<sup>22</sup>Jonathan DiCicco, "Power Transition Theory and the Essence of Revisionism," in William R. Thompson, ed., *Oxford Encyclopedia of Empirical International Relations Theory* (Oxford: Oxford University Press).

<sup>23</sup>For example, there was a tendency in some power transition research to use PPP adjusted GDP. See Tammen, Kugler, Lemke, Allan C Stam III, Mark Abdollahian, Carole Alsharabati, Brian Efirid, and Organski, *Power Transitions: Strategies for the 21<sup>st</sup> Century* (Washington, D.C.: CQ Press, 2000). However, those projections suggested that China would already have surpassed the United States by 2020, a claim most contemporary observers would find dubious, as Chan notes on page 108. Chan makes a similar warranted criticism about measurement using the Correlates of War data, used in earlier power transition research such as *The War Ledger*, on page 109-110. That dataset now shows China surpassing the United States even earlier in the late 1990s (see [www.correlatesofwar.org](http://www.correlatesofwar.org)).

<sup>24</sup>J. David Singer, Stuart Bremer, and John Stuckey, "Capability Distribution, Uncertainty, and Major Power War, 1820-1965," in Bruce Russett, ed., *Peace, War, and Numbers* (Beverly Hills: Sage, 1972).

<sup>25</sup>Tammen and Kugler, *Performance of Nations* (Lanham: Rowman and Littlefield, 2012). Also see the data available at <https://transresearchconsortium.com/>.

concept. Status, as discussed by Chan, may be one, but we might also include related concepts like authority,<sup>26</sup> values,<sup>27</sup> or ideology.<sup>28</sup> Status is about a state's perceived standing in the international system and importance granted to it by the community of states. To this end, it closely mirrors the concept of satisfaction being an evaluation of the status quo used by Organski. One aspect of the international system which states may be dissatisfied with is their status within the international hierarchy at the core of the power transition paradigm. Findings show that this status inconsistency affects state behavior, with those deprived of status empirically engaging in more conflict, thereby mirroring the expectations of power transition theory. Furthermore, this finding is applicable to both major<sup>29</sup> and regional powers.<sup>30</sup> One avenue of future research for power transition researchers prompted by Chan's analysis may be a broader, robust integration of measures like status to better capture the concept of satisfaction in both theorizing and empirical models.

However we might define satisfaction, it is necessary to disentangle it from the dependent variable of study. We cannot claim that due to an absence of violent behavior, an actor is therefore satisfied, as Chan appears to argue regarding China. We might equally suggest that the reason for peaceful Chinese behavior is incapability rather than satisfaction. For example, in the case of Taiwan, it seems more likely that China's failure to take aggressive action is due to the problem of power projection and logistics for the Chinese military to accomplish an amphibious invasion across the Taiwanese straits rather than satisfaction with the status quo. Instead, with a power transition view toward China's current political capacity, development, and aging demography leading it toward the middle-income trap, it is more plausible that China's quiet behavior represents its lack of opportunity due to weakness rather than a lack of willingness.

### *Moving Forward*

Unlike the previously referenced evaluation of power transition theory by DiCicco and Levy, Chan's work focuses explicitly on the impact of research on the present and the dangers of an oversimplified version in the hands of policymakers. The policy consequences of research are an issue that we should all take more seriously, regardless of the theoretical perspective. What the Thucydides's Trap illustrates is how a small portion of a theoretical paradigm, in this case, power transition theory, can be borrowed, distorted, and then repackaged in a way that is persuasive to policy makers and the public. However, in that repackaging, particularly when no reference is given to the original, the clarity of the causal process and the volumes of research exploring related empirical hypotheses are lost. Parity is not a necessary or sufficient cause for war, and it is certainly not the only relevant variable to the investigation of international politics.

Power transition researchers, therefore, should work diligently to combat oversimplified, monocausal systemic approaches, like the Thucydides's Trap. In particular, Chan's text highlights that attention should continue on domestic or regional factors and how those interact with systemic variables to affect state behaviors. Reflecting reality, international politics is a multilevel conceptual world, where variables from domestic and systemic levels interact in complex ways to affect the probability of outcomes like war. The prime independent variables, power and satisfaction, are themselves rooted across levels. As added complexity continues to build in power transition research, so it might develop more complete tests that account for many of Chan's criticisms. Further, power transition researchers must continue to recognize that concepts like

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<sup>26</sup> David A. Lake, *Hierarchy in International Relations* (Ithaca: Cornell University Press, 2009).

<sup>27</sup> Yesilada, *EU-Turkey*.

<sup>28</sup> Mark L. Haas, *The Ideological Origins of Great Power Politics, 1789-1989* (Ithaca: Cornell University Press, 2007).

<sup>29</sup> Renato Corbetta, Volgy, and Rhamey Jr., "Major Powers Status In(Consistency) and Political Relevance in International Relations," *Peace Economics, Peace Science and Public Policy* 19:3 (2013): 291-307.

<sup>30</sup> Volgy, Paul Bezerra, Jacob Cramer, and Rhamey Jr., "The Case for Comparative Regional Analysis in International Politics," *International Studies Review* 19:3 (2017): 452-480.

parity, hierarchy (contested or otherwise), and satisfaction are relevant to more than just challengers and a dominant power, but to all the actors in the international system.

Finally, one area of possible future research that does potentially create significant challenges for power transition theory is the idea of a dissatisfied dominant power. As Chan repeats often, power transition theory, alongside most other systemic theories, assumes the dominant state is satisfied with the status quo that it created. If this is not the case, it must be addressed. However, we should exercise caution in not overweighting contemporary observations when evaluating theoretical claims. The Donald Trump administration may represent a departure from the American status quo and thus dissatisfaction, reflected by some actions such as withdrawal from the World Health Organization. But, a broader, non-anecdotal evaluation of Trump's national security policy may suggest that this is more rhetoric than substance.<sup>31</sup> Even if Trump does represent a dissatisfaction with the status quo, is it a trend or an outlier? At the end of Trump's tenure as president, does it seem more likely that the new president will continue Trump's departure from traditional American post-Cold War status quo, or will they instead revert back to the norm? Do we believe no matter how "dissatisfied" Trump's foreign policy may be that it is relatively more dissatisfied than a Communist China that is engaged in building concentration camps and islands? Chan's careful evaluation of power transition theory in the context of its possible impact on policymakers underscores that these questions and many more require clearly articulated empirical answers so that policymakers do not instead adhere to simplistic theories like the Thucydides's Trap that misconstrue international relations theory and lead to the committing of foreign policy blunders.

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<sup>31</sup> See the roundtable discussion in the *Texas National Security Review*: <https://tnsr.org/roundtable/policy-roundtable-make-trumps-national-security-strategy/>.

Does China's rise increase the likelihood of a systemic war? Graham Allison coined the term "Thucydides's Trap" to capture the idea that war tends to break out when a rising power is overtaking the dominant state in the international system. The concept is not new; international relations scholars have been studying the impact of power shifts on the likelihood of war for a long time. What is new is that the repackaged Thucydides's Trap has become so popularized that it has entered the lexicon of leaders, including Chinese President Xi Jinping, who cautioned in 2014 that "We all need to work together to avoid the Thucydides's Trap."<sup>1</sup>

In *Thucydides's Trap? Historical Interpretation, Logic of Inquiry, and the Future of Sino-American Relations*, Steve Chan offers an absorbing critique of the scope conditions, logic, and empirical evidence of this view. Despite having "Thucydides's Trap" in the title, the book's main focus is power transition theory. Chan persuasively reveals the weaknesses of the quantitative approach adopted by power transition theorists, showing how different coding criteria could drastically change the results, how key cases are miscoded or omitted, and how local wars and systemic wars are mixed up in the dataset. As Chan points out, power transition theory posits two independent variables that work together to cause war: power shift and revisionism, but subsequent analyses have only studied the impact of power shifts while neglecting revisionism. The theory implicitly assumes that rising states must also harbor revisionist intentions, thus conflating power shift with revisionism. Chan forcefully criticizes power transition theory's portrayal of the rising state as "a cocky and impatient upstart itching for a fight" (8, 190), arguing instead that the rising state can have a status-quo orientation because it has benefitted from the existing international order that assists its rise. He argues that the declining hegemon can be revisionist and has powerful incentives to launch a preventive war while time is still on its side. Chan concludes that a power shift is neither a necessary nor a sufficient condition for war and advocates for a strategy of accommodation, granting China the status and sphere of influence it seeks.

Chan's book is impressive in the breadth and depth of its critique of power transition theory. It is highly readable and offers valuable insights into studying power shifts. Like all scholarly works, it leaves room for debates and disagreements. Putting aside a few quibbles, I will focus on three main critiques. First, I question whether it is fruitful to equate the Thucydides's Trap idea with power transition theory while glossing over important differences. Second, Chan rules out territorial expansion as an indicator of revisionism, leading him to make the odd assertion that changing the existing territorial arrangement is not revisionist behavior. Third, Chan warns that the danger of the discourse of Thucydides's Trap is the creation of a self-fulfilling prophecy, but he does not consider the possibility that the same discourse could also induce caution from leaders.

### *Apples and Oranges*

The study of power shifts is a research program. As such, it is bound to contain disparate theories with incongruent arguments. Although Chan acknowledges that "Thucydides's Trap...does not engage power-transition theory as its predecessor" (194), he nonetheless considers them to be virtually identical. According to him, both make the same monocausal explanation of war: "if a rising state reaches parity or overtakes a ruling state, war between them becomes more likely" (19); both ignore human emotions and human agency; and both put the onus of war initiation on the rising latecomers. Throughout the book, Thucydides's Trap is almost indistinguishable from power transition theory.

Despite hailing from similar backgrounds, Thucydides's Trap is not the same as power transition theory. Unlike power transition theory, it does not have an explicit theoretical framework specifying causal variables and testable hypotheses, nor

<sup>1</sup> Nicolas Berggruen and Nathan Gardels, "How the World's Most Powerful Leader Thinks," *Huffington Post*, 21 January 2014, [http://www.huffingtonpost.com/2014/01/21/xi-jinping-davos\\_n\\_4639929.html](http://www.huffingtonpost.com/2014/01/21/xi-jinping-davos_n_4639929.html). Xi mentioned Thucydides's Trap again on his visit to the United States the following year.

does it attempt to predict the initiation, timing, and severity of war. Rather than being a “theory,” Thucydides’s Trap is at best a heuristic device to study U.S.-China relations.

The marriage of Thucydides’s Trap with power transition theory leads to three unsubstantiated charges. First and foremost, Chan argues that, like power transition theory, “Thucydides’s Trap is biased in the sense that it is only concerned with wars started by the supposed rising latecomers and not those started by the existing dominant powers” (27). But Thucydides’s Trap does not specify whether the war will be started by the rising power or the existing hegemon. In fact, it is agnostic. Notably, the Peloponnesian War, from which Thucydides’s Trap is derived, was started not by the rising latecomer but by the existing dominant power (Sparta). The sixteen cases of power shifts in Allison’s study, albeit problematic in case selection, include examples of wars initiated by *both* the rising latecomer and the dominant state.<sup>2</sup>

Second, Chan asserts that Thucydides’s Trap neglects emotional factors other than fear (17). But Allison does discuss how human emotions—overconfidence, anxiety, arrogance, and fear—drove both Spartan and Athenian leaders into war. Allison notes, “Objective conditions have to be perceived by human beings — and the lenses through which we see them are influenced by emotions.”<sup>3</sup>

Third, Chan alleges that Thucydides’s Trap ignores human agency (17). But again, Allison does talk about how leaders make choices that make war more likely or less possible: “Destiny dealt the hands, but men played the cards.... Different choices would have produced different results.”<sup>4</sup> Allison dismisses the idea that war is “inevitable” and instead suggests taking actions to avoid it, including Chan’s recommended strategy of accommodation.<sup>5</sup>

Instead of treating it synonymously with power transition theory, a better intellectual cousin of Thucydides’s Trap is Robert Gilpin’s theory of hegemonic war.<sup>6</sup> Both study the Peloponnesian War, both are agnostic about war initiation, and both rely on qualitative case studies that capture the causal mechanisms. Although Gilpin’s theory did not spawn as much subsequent study, it is “theoretically richer than [A.F.K.]Organski’s,” observes Jack Levy, and provides more clues to study revisionism.<sup>7</sup> In Gilpin’s theory, uneven growth in power creates discrepancies between the distribution of power and key elements of the system (hierarchy of prestige, division of territory, and international order), causing the status of the rising state to become incommensurate with its power. As the rising state and the ruling power compete for allies, two camps of entangling alliances emerge, making the system increasingly bipolarized. As tensions build in the system, a crisis or an accident can trigger a hegemonic war. The postwar system will reflect the new distribution of power. Gilpin’s theory parallels Allison’s account of Thucydides’s Trap much better than power transition theory does. Gilpin also does not rule out peaceful change.

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<sup>2</sup> Chan incorrectly asserts that “the Crimean War (1853-1856) was also omitted from Allison’s inventory” (26). In fact, the Crimean War is Allison’s Case #8, a preventive war in which the “ruling” Britain and France decided to initiate against the rising Russia. Graham T. Allison, *Destined for War: Can America and China Escape Thucydides’s Trap?* (Boston: Houghton Mifflin Harcourt, 2017), 263-266.

<sup>3</sup> Allison, *Destined for War*, 34-39. The quote is at 39.

<sup>4</sup>Allison, *Destined for War*, 233.

<sup>5</sup> Allison, *Destined for War*, 221-223.

<sup>6</sup> Robert Gilpin, “The Theory of Hegemonic War,” *Journal of Interdisciplinary History* 18:4 (Spring 1988): 591-613; Robert Gilpin, *War and Change in World Politics* (New York: Cambridge University Press, 1981).

<sup>7</sup> Jack S. Levy, “Power Transition Theory and the Rise of China,” in *China’s Ascent: Power, Security, and the Future of International Politics*, ed. Robert S. Ross and Zhu Feng (Cornell: Cornell University Press, 2008), 12. Iain Johnston expands on Gilpin’s work to study Chinese revisionism. Alastair Iain Johnston, “Is China a Status Quo Power?,” *International Security* 27:4 (2003): 5-56.

Far from offering a “monocausal explanation” (194) as Chan asserts, Thucydides’s Trap provides a more complicated pathway linking power shifts and war.

### *Revisionism*

One of the analytically ambiguous terms in international relations is revisionism. It is used widely but seldom clearly defined. When a state is said to be revising the status quo, what exactly is being revised? Without a shared definition, scholars often talk past each other and sow confusion. Allison does not use the term “revisionism” in his book, but he refers to “status quo” at least 18 times without providing a definition. To his credit, Chan has a definition: “Revisionism refers to whether a state objects to the existing international order.” The status quo is the prevailing international order, which consists of the widely shared rules about appropriate interstate behaviors (123). If a state attempts to change the rules of the system, it is revisionist.

Oddly, Chan insists that territorial expansion should not be used as “a yardstick to judge revisionism” (47). If a state expands its territorial reach but does not attempt to change the rules of the system, it is not revisionist. He distinguishes territorial expansion from challenges to the international order and cautions that we should avoid “the problem of conflating the measurement of revisionism with that of power” (50). Wilhelmine Germany and imperial Japan were aggressive and expansionist, but “they were not revisionist” because they did not seek to alter the rules of the international order (48, 144). Napoleonic France was both expansionist and revisionist because it sought to dominate Europe *and* to overturn the rules of the prevailing monarchical order. The United States during its rise was both expansionist and revisionist because it engaged in a series of territorial expansion *and* espoused the Monroe Doctrine to redraft the rules of the system. Viewed in this light, Chan surprisingly does not consider China’s territorial expansion in the South China Sea and obstruction of US freedom of navigation operations as revisionist because they do not produce new rules (118). Changing the existing territorial arrangement is often viewed as a type of revisionist behavior.<sup>8</sup> Chan’s restrictive definition of revisionism effectively rules out this important aspect of rising state behaviors.

Of course, scholars are free to adopt any definition as long as they are consistent in its usage. It is thus puzzling to see Chan arguing that Wilhelmine Germany and imperial Japan “were not revisionist” (144) on the one hand, while citing approvingly Steven Ward’s description of imperial Germany and interwar Japan as “radical revisionists” (142) on the other. There is an apparent contradiction here. Another example is whether rule breaking constitutes revisionism or not. At one place, Chan seems to suggest that rule breaking is not revisionist. Imperial Japan in the late nineteenth and early twentieth centuries routinely violated China’s sovereignty and colonized Korea, but “it is not at all clear why it should be considered a revisionist state for alleged violations of the norms, customs, and practices commonly shared by the other great powers of that time” (48). Japan was simply imitating Western colonial and imperialist powers and sought to be accepted into the ranks of “civilized” countries. Imperial Japan was conformist, not revisionist.<sup>9</sup> Yet Chan labels U.S. rule-breaking behavior, such as the post-Cold War policy of regime change or the George W. Bush doctrine of preventive war in the 2003 Iraq War, as revisionist (131-132). Is violation of the prevailing rules of the system an indicator of revisionism? Or is it only changing or creating new rules qualified as such?

### *Throwing the Baby Out with the Bathwater?*

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<sup>8</sup> Gilpin considers a rising state’s attitude toward “the international distribution of territory” as the most important indicator of revisionism. Gilpin, *War and Change in World Politics*, 187.

<sup>9</sup> This type of behavior is “organized hypocrisy” in Stephen D. Krasner, “Organized Hypocrisy in Nineteenth-Century East Asia,” *International Relations of the Asia-Pacific* 1 (2001): 173-197; Stephen D. Krasner, *Sovereignty: Organized Hypocrisy* (Princeton: Princeton University Press, 1999).

Chan warns that the discourse on Thucydides's Trap and power transition can create a self-fulfilling prophecy. If leaders believe in Thucydides's Trap and act accordingly, it may create the anticipated conditions that make war more likely. Talking and thinking in terms of Thucydides's Trap will influence the state's construction of its identity as well as its definition of interests and preferences. The discourse is harmful because it encourages 'othering' the opponent and contributes to confrontation.

Should we, then, throw out the proposition that war is more likely when the system is undergoing a power transition?

It might be worthwhile to go back to what Thucydides's Trap refers to: "the severe structural stress caused when a rising power threatens to upend a ruling one. In such conditions, not just extraordinary, unexpected events, but even ordinary flashpoints of foreign affairs, can trigger large-scale conflict."<sup>10</sup> Instead of creating a self-fulfilling prophecy, this statement should induce caution from leaders in Beijing and Washington. Understanding the danger of war is the first step to avoid being trapped in it. Like Chan, Allison seeks to offer "a set of principles and strategic options for those seeking to escape Thucydides's Trap and avoid World War III."<sup>11</sup>

Obviously, historical analogies cannot completely capture an ongoing event. Allison himself cautions against "facile analogizing" and emphasizes that "the differences matter at least as much as the similarities."<sup>12</sup> The purpose of analogizing Thucydides's Trap is not to shoehorn China and the United States into the roles of Athens and Sparta respectively, as Chan suggests (17-18), but to underscore the enduring feature of international politics throughout the ages. The dynamics of conflict highlighted by Thucydides remain as relevant today as it was two thousand years ago.

Many scholars accuse structural theory of determinism, as Chan does, (14, 34), even though structuralists do not adopt it. States can go to war for a variety of reasons. Attempting to isolate a single cause for all wars is impossible. The proposition that war tends to break out during a power transition is better understood as a probabilistic—not deterministic—statement. The structural tensions cause by power shifts can substantially increase the probabilities of war, much like dry leaves waiting for a spark, but it does not mean that war will inevitably break out. Properly understood, Thucydides's Trap cautions us to be prepared for the danger of war during a power transition.

Overall, Chan's book provides a stronger critique of power transition theory than of Thucydides's Trap. Students of power shifts should take his argument seriously and avoid the pitfalls he identifies. We should not, however, hastily dismiss the warnings of Thucydides's Trap.

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<sup>10</sup> Allison, *Destined for War*, 29.

<sup>11</sup> Allison, 215.

<sup>12</sup> Allison, 218.

*Use of Historical Analogies in IR Theory*

**T**hucydides's *Trap? Historical Interpretation, Logic of Inquiry, and the Future of Sino-American Relations* by Steve Chan aims to offer a definitive critique of the notion of 'Thucydides's Trap' as advanced by Graham Allison in *Destined for War: Can America and China Escape Thucydides's Trap?* and elsewhere.<sup>1</sup> As is clear from the subtitles of the aforementioned books, what is at stake in this debate is no less than our prognosis for the future of the U.S.-China relations. Allison's contention, as attributed to Thucydides and described by Chan, is that "the danger of a great-power war increases when a rising power overtakes an incumbent hegemon" (1). This maxim is supposed to be corroborated by the historical record: in twelve of the sixteen past episodes Allison includes, he argues that this prediction held true. Chan develops his critique of Allison and his counterargument in ten chapters. After the Introduction, Chapter 2 specifically critiques the so-called 'Thucydides's Trap' hypothesis and the use of this historical analogy to make sense of the present. Chapters 3 to 5 broaden the critique from 'Thucydides's Trap' to the assumptions of power transition theory in general, and its treatment of the historical record. The second half of the book, Chapters 6-9, is focused on setting the record straight on China and developing a better explanation of China's rise, which is portrayed as being more peaceful than assumed, and that takes into account status considerations. In Chapter 10, Chan concludes by arguing that "Thucydides's Trap and power-transition theory tend to be sensational...States have gone to war in the absence of a power shift" (220) and that "revisionist or status-quo orientations are clearly not an inherent character of rising or established states" (221). In other words, according to Chan, the United States and China do not have to go to war; whether they do or not depends on statecraft and the choices of policymakers on either side.

As someone who is interested in questions of rise and decline in world politics<sup>2</sup> but not particularly invested in paradigmatic debates on power transition, I would pronounce Chan the clear winner of this round. It is hard to disagree with Chan's contention that Allison's treatment of the historical record is rather problematic, and that power transition theory is too deterministic in its predictions when it comes to U.S.-China relations. Given my general agreement with Chan on the substantive argument, in this review I will focus on two other issues that the book raises: the use of historical figures such as Thucydides and historical texts such as *The Peloponnesian War* in IR theory and the use of seemingly historically corroborated schematics such as 'Thucydides's Trap' in predicting future behaviour in world politics.

On the first point, let us start by noting that though Chan argues that Thucydides's own account gives reasons to doubt Allison's predictions because it allowed for alternative explanations for the outbreak of the Peloponnesian War (20-21), he still yields too much ground to the idea that Thucydides must be on Allison's side just because he is claimed by Allison (and others in Allison's camp).<sup>3</sup> Thucydides is even called a "structuralist" in this chapter: "Allison and Thucydides are 'structuralists' in the sense that they both emphasize the interpersonal forces of the 'international' system (consisting of the Greek polities even though Persia was a significant 'extra-systemic' power) to be the primary determinant of war and peace" (17). This mischaracterisation arises from reading Thucydides only through the eyes of IR theorists: by contrast, any classicist or historian of the period will tell you that none of these terms — structure, international system, great power etc.

<sup>1</sup> Graham Allison, "The Thucydides Trap: Are the U.S. and China Headed for War?" *The Atlantic*, 24 September 2015, [www.theatlantic.com/international/archive/2015/09/united-states-china-war-thucydides-trap/406756/](http://www.theatlantic.com/international/archive/2015/09/united-states-china-war-thucydides-trap/406756/); Graham Allison, *Destined for War: Can America and China Escape Thucydides's Trap?* (Boston: Houghton Mifflin Harcourt, 2017).

<sup>2</sup> See Ayşe Zarakol, *Before Defeat: Rethinking the Decline of the East and the Future of the West* (Cambridge University Press, under contract, forthcoming in 2021).

<sup>3</sup> See also David A. Welch, "Why International Relations Theorists Should Stop Reading Thucydides," *Review of International Studies* 29.3 (2003): 301-319. (Welch features in chapter 2 prominently, but his argument about the misuse of Thucydides still remains applicable.)

— would have meant anything to Thucydides (or any of his contemporaries). These concepts are part of *our* ecumene and *our* vocabulary for making sense of the world, not those of Thucydides. While Thucydides thought he was observing something deep and likely to be repeated about the human condition in the war between Athens and Sparta (as many historians do about the episodes they are writing on), he was not at all thinking with our post-nineteenth century IR concepts.

This point is simultaneously banal and important. We tend to read IR concepts back into classical texts and periods not because they are brilliantly trans-historical, but because presenting them as such is a legitimation strategy for our presentist arguments. By pointing this out, I do not mean to argue that there are no trans-historical dynamics — to the contrary— but rather that such dynamics are difficult to locate and even harder to theorise without bringing in biases and connotations of the present. A genuine attempt at theorising about trans-historical dynamics would therefore at least aim to maintain a similar distance to present-day thinking as it does to the thinking of the past; it would not privilege present-day (and often also culturally bound) schematics and concepts as universally applicable across time and space. But this is very difficult to do, and it requires a considered commitment to both a type of historicist thinking *and* generalist theorising, traits which are rarely found in the same researcher.<sup>4</sup> In contrast, it is much easier to take our beliefs about dynamics from the present and read them into various historical episodes, historical texts and the thinking of historical figures, such as Thucydides. Confirmation bias, if you will, passing as *argumentum ad verecundiam*. Allison has not learned what he thinks from Thucydides, nor do we have any reason to believe that Thucydides would have agreed with Allison; Allison is simply calling his own argument ‘Thucydides’s Trap’ and thus giving it the appearance of being timeless (when it is not). There is no reason to concede Thucydides to Allison.

This brings me to my second point about the effects of the use seemingly historical schematics such as the ‘Thucydides’s Trap’ to make sense of present day world politics. What Chan does not acknowledge is that there is a way Allison in which can be ‘right’ about the future despite being wrong in his theoretical, historical and contemporary analyses in all the ways that Chan details, that is: if Allison’s model makes sense to enough people as a sound explanation of our world. I want to sidestep tired paradigmatic debates here, and so will offer an example from another discipline, as well as another time, to illustrate my point. I happened to be reading Azfar Moin’s excellent *The Millennial Sovereign* at the same time as *Thucydides’s Trap*.<sup>5</sup> *The Millennial Sovereign* is a historical deep-dive into the notions of sovereignty that animated the Persianate world in the post-Timurid era, focusing on the Safavid and Mughal dynasties in the sixteenth century especially. Though we tend to reductively think about these dynasties as just Islamic (or Sunni/Shia), Moin shows that their practices of sovereign legitimation were drawing from many different wells: for example, Mongolian dynastic lineages, pre-Islamic practices, world history etc. But most importantly for my purposes here is the fact that astrology played a very important role in sovereign legitimation in this period and in the creation of sacred authority.

An oft-invoked and very powerful title of sovereignty in this period was ‘Lord of Conjunction’ (*sahib kiran*). The ‘Lord of Conjunction’ was a messianic figure of universal sovereignty, connected somehow with a Jupiter-Saturn conjunction. At the time, such conjunctions were believed to be connected with great political events (ends and beginning of epochs, dynastic change etc.) and conjunction astrologers were in great demand to help make sense of world events, as sort of the IR theorists of their time. I do not mean this as an insult to IR theorists, but rather as a suggestion of the kind of predictive space such thinking occupied in the politics of the day. (In any case the court astrologers of this time were taken much more seriously by the sovereigns of their day than we as IR theorists are by ours.) And by all accounts, these astrologers were also very meticulous about their tasks: at a time when astrology and astronomy, science and religion, history and myth had much more porous boundaries, those who studied astrology were among the intelligentsia of their day, and their predictions about politics relied on rigorous study of star charts and historical events. They were quite ‘scientific’ in their approach. And they seemed to have good empirical basis for their conjunction theories: Alexander the Great was a ‘Lord of Conjunction,’ so was

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<sup>4</sup> William McNeill comes to mind as an exception.

<sup>5</sup> Azfar Moin, *The Millennial Sovereign: Sacred Kingship and Sainthood in Islam* (New York: Columbia University Press, 2012).

Chinggis Khan and Timur (Tamerlane), and a conjunction had also signified the birth of Islam. (I note here with some bemusement that according to my google search, 2020 is also a 'great conjunction' year, the likes of which have not been witnessed in centuries. Finally an explanation!)

Obviously, nowadays we do not subscribe to this particular explanation for great events and the rise of great sovereigns (or 'great powers') in world history. If we cared to we could show the aforementioned correlations to be spurious or the star charts to be inaccurate or the data to be cherry-picked, but we do not even bother because this type of causal reasoning is just not part of our political ecumene.<sup>6</sup> But Moin shows that in the period he explores these notions were not simply beliefs or superstitions; they were *social facts* accepted by sovereigns and the people alike. Different astrologer-thinkers may have disagreed about particular interpretations or on who deserved the title of 'Lord of Conjunction,' but hardly anybody questioned the underlying premise that this was a significant label and with predictive power. And as a social fact, the belief in 'Lord of Conjunction' had a materiality: Shah Isma'il, the founder of the Safavid dynasty was a 'Lord of Conjunction,' and so was Akbar of the Mughal dynasty, and so was Shah Jahan, and so on. One could say these rulers were cynically exploiting the 'irrational' beliefs of their subjects, but that would be reading something into the situation that was not there. Of course the rulers of this period preferred astrological interpretations that favoured them as 'lords of conjunction' over other candidates, but they also wanted to *believe* that they were celestially marked in this unique manner, and were always looking for confirmatory signs from the heavens. And when they believed that they were 'Lords of Conjunction,' which was a prerequisite of sovereignty claims in this period, they *were*.

All of this is to say that we have other social facts in our ecumene that *we* take for granted: that the world is arranged as an 'international system' of 'states,' that there are 'great powers,' that our 'system' is characterised by 'anarchy,' etc. And to the extent that a particular trait is rendered as part of the definition of a particular identity category that is understood as social fact, it actually does become generally predictive, either because those who have that trait are now expected to enact that identity or because those who have that identity are now driven to develop that trait. If association with a Jupiter-Saturn conjunction predicts sovereignty in world of millennial thinking, then sovereignty predicts association with that conjunction in the same world; if conflict is associated with great power transition in a world of great powers, then great power transition predicts conflict. The way to cleanly break that association for good is not to quibble about the prediction but to change our understanding of the underlying premise about how the world works. Or to put it another way, no matter how diligently we prove it to be based on faulty analyses, a prediction will continue to have some power as long as people take its starting point and unstated assumptions about as social facts. In that sense, whether U.S.-China fall into the so-called 'Thucydides's Trap' depend less on skilled statecraft and more on how much decision-makers (especially on the Chinese side) think about world politics in this post-nineteenth century theoretical idiom of great power competition.

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<sup>6</sup> I am of course aware that present day world leaders sometimes consult fortune tellers or horoscopes, but they do so in secret. Their authority is hurt when this information becomes public.

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 RESPONSE BY STEVE CHAN, UNIVERSITY OF COLORADO AT BOULDER
 

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I thank all the participants of this online forum. I am indebted to them for taking time from their busy schedules to provide me with constructive feedback on my book, *Thucydides's Trap?*<sup>1</sup> I have learned much from these colleagues, who have made me to reflect deeply even when I disagree with some of their comments. Naturally, I am also grateful when they call my attention to where I have erred and where I could have been clearer or more forceful in staking out my position. This roundtable has given me the opportunity to think more thoroughly about different issues and to address them in this essay.

Ayşe Zarakol makes two important points. They pertain to the appropriation of historical figures and texts (such as Thucydides and *The History of the Peloponnesian War*) in contemporary theorizing about international relations, and the “use of seemingly historically corroborated schematics such as ‘Thucydides’s Trap’ in predicting future behaviour in world politics.” She notes that I have yielded too much ground to Graham Allison and others in his camp in not contesting more vigorously the impression that Thucydides must be on their side simply because they have invoked his name. She is right in pointing out that “Allison has not learned what he thinks from Thucydides, nor do we have any reason to believe that Thucydides would have agreed with Allison; Allison is simply calling his own argument ‘Thucydides’s Trap’ and thus giving it the appearance of being timeless (when it is not). There is no reason to concede Thucydides to Allison.”<sup>2</sup> In my book I questioned the extent to which an analogy from ancient Greece is valid for understanding today’s world after many important changes have occurred in the intervening 2,500 years (such as the advent of modern states, democratic institutions, nuclear weapons, and nationalism). We should indeed be more careful in imposing our own contemporary ideas and concepts on people from a different era (or for that matter, from a different culture), attributing to them ideas and concepts that are alien to them.

Zarakol is also right in pointing out that Thucydides’s account cannot be characterized as ‘structural’<sup>3</sup> unless one relies only on his statement that “the truest cause [of the Peloponnesian War], but the least spoken of, was the growth of Athenian power, which presented an object of fear to the Spartans and forced them to go to war.”<sup>4</sup> Indeed, Thucydides’s own narrative is far more nuanced and richer in detail than can be captured by this maxim. I have tried to show in my book that we can discern from his account multiple and not mutually exclusive pathways to war.<sup>5</sup> Power shifts among the Greek polities were not the only plausible cause and hence my objection to monocausal explanations that fail to consider competing hypotheses.

I also take seriously Zarakol’s observation that “We tend to read IR concepts back into classical texts and periods not because they are brilliantly trans-historical, but because presenting them as such is a legitimization strategy for our presentist

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<sup>1</sup> The title of my book, referring to Thucydides’s Trap (with a question mark), is a response to Graham Allison’s book, *Destined for War: Can America and China Escape Thucydides’s Trap?* (New York: Houghton Mifflin, 2017).

<sup>2</sup> For a trenchant critique of Allison’s interpretation of Thucydides, see Jonathan Kirshner, “Handle Him with Care: The Importance of Getting Thucydides Right,” *Security Studies* 28:1 (2018): 1-24.

<sup>3</sup> ‘Structural’ in the sense that macro factors at the level of interstate relations (specifically in this case, shifts in the power balance between Athens and Sparta) were the primary and original cause precipitating war.

<sup>4</sup> Donald Kagan, *The Outbreak of the Peloponnesian War* (Ithaca: Cornell University Press, 1969), 2-3. Sparta’s fear of Athens’s rise is sometimes translated as the ‘inevitable’ cause of the Peloponnesian War.

<sup>5</sup> David Welch, “Can the United States and China Avoid a Thucydides Trap?” *E-International Relations*, 2015, <https://www.e-ir.info/2015/04/06/can-the-united-states-and-china-avoid-a-thucydides-trap/>, accessed 18 May 2018. Welch also gives a compelling analysis in “China, the United States, and ‘Thucydides’s Trap’” in Huiyun Feng and Kai He, eds., *China’s Challenges and International Order Transition: Beyond “Thucydides’s Trap”* (Ann Arbor: University of Michigan Press, 2020), 47-70.

arguments.” I note in my book that the discourse on Thucydides’s Trap reflects social and political construction, intended partly to frame issues, set agendas, mobilize elite and public opinion, and legitimate certain views and policies. Zarakol has articulated this concern much more eloquently and persuasively than I did. She has also helped me to see more clearly that popular and elite beliefs matter as social facts regardless of their factual accuracy. The example about the “Lord of Conjunction” is fascinating and illuminating. People’s beliefs about the world and about how the world expects them to behave have consequences. These beliefs have a life of their own and can become generally predictive. I of course concur with this proposition when I warned about the danger of self-fulfilling prophecy and discussed Antonio Gramsci’s hegemony of ideas.<sup>6</sup>

Zarakol’s caution about undertaking trans-historical work calls attention to the challenges of capturing international dynamics across epochs and avoiding the biases and connotations of the present. Yuan-kang Wang’s review however, reveals a much more sanguine view about such undertaking.<sup>7</sup> He avers that “The purpose of analogizing Thucydides’s Trap is not to shoehorn China and the United States into the roles of Athens and Sparta respectively, as Chan suggests (17-18), but to underscore the enduring feature of international politics throughout the ages. The dynamics of conflict highlighted by Thucydides remain as relevant today as it was two thousand years ago.” For the reasons stated above, I question how informative an analogy from ancient Greece can be for understanding contemporary Sino-American relations. This stance should not be taken to dismiss the danger of a conflict between these two countries. A conflict could happen for reasons other than a power transition between them (or in addition to this power shift as suggested by my earlier reference to multiple and not mutually exclusive pathways to war), and it may happen simply because leaders believe that there is an ongoing or impeding power transition even though this process may not in fact be unfolding (as mentioned earlier with respect to the proposition that beliefs as social facts can be predictive even without a basis in fact).

Wang is of course right to remind us that historical analogies can serve as powerful warnings to mobilize efforts to head off looming dangers. And he is right that Allison warns about ‘facile analogizing.’ But the packaging of Allison’s work in a simplistic (monocausal) and sensational fashion, perhaps in order to communicate to a lay public that cannot be bothered with technical details of concern to professional researchers of international relations, tends to drown out such cautionary remarks. I acknowledge the possibility that references to salient past episodes can energize people to undertake collective action to avert disasters. But as I have also argued in my book, we should not overlook that misapplied analogies can have the reversed effect of producing misguided policies.<sup>8</sup>

Wang suggests that I see power-transition theory as a monocausal explanation. I do not. On the contrary, this theory considers not just power shifts between two leading states but also a rising power’s revisionist agenda to be jointly determinative of the danger of a war occurring between them. Even though the latter idea (revisionism or dissatisfaction)

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<sup>6</sup> Antonio Gramsci, *Selections from the Prison Notebooks of Antonio Gramsci* (New York: International Publishers, 1971).

<sup>7</sup> He has studied the foreign policies of imperial China, specifically the Song Dynasty (960-1279) and Ming Dynasty (1368-1644), concluding that past Chinese rulers had practiced realpolitik and a grand strategy of expansion that generally conformed to the propositions of offensive realism. See Yuan-kang Wang, *Harmony and War: Confucian Culture and Chinese Power Politics* (New York: Columbia University Press, 2011). For offensive realism, see John Mearsheimer, *The Tragedy of Great Power Politics* (New York: Norton, 2001). This theory expects a country to expand its power until it is stopped by resistance from other states, and it expects ‘balancing’ behavior from countries facing a dominant power rather than ‘buckpassing’ or ‘bandwagoning.’ This terminology reflects of course our contemporary discourse among researchers of international relations, as Zarakol reminds us. Mearsheimer does not quite explain why, given his expectation of ‘balancing’ by other states, the U.S. managed to establish regional hegemony in the Western Hemisphere. For such an attempt, see Colin Elmer, “Extending Offensive Realism: The Louisiana Purchase and America’s Rise to Regional Hegemony,” *American Political Science Review* 98:4 (2004): 563-576.

<sup>8</sup> I cited Yuen Foong Khong’s work to make this point. See his *Analogies at War: Korea, Munich, Dien Bien Phu, and the Vietnam Decisions of 1965* (Princeton: Princeton University Press, 1992).

has been undertheorized and has not been incorporated in many quantitative studies of power-transition theory,<sup>9</sup> this theory nevertheless features two independent variables and not just one. Wang is aware of and acknowledges all of this, and he would be right that in practice many proponents of power-transition theory have carried out their research as if the revisionism variable would not matter for the results of testing this theory if it is omitted from their study. Significantly (and I think this point is worth emphasizing), the original formulation of power-transition theory stipulates that it is the compound effect of a newcomer's increased relative power *and* its revisionist intentions that leads to war. That is, it is the interaction between these two variables and not their separate, independent influence that should be considered in testing this theory.<sup>10</sup> This view in turn suggests that much of the evidence produced by examining *only* the impact of power shifts on war occurrence (while excluding consideration of the newcomer's revisionism) is suspect so far as its relevance for power-transition theory is concerned.

For reasons such as systematic data collection, transparent empirical criteria, and several generations of researchers engaging in replicating and extending a growing research program, power-transition theory is favored by more members of the international relations community than Allison's project. Wang expresses instead a preference for the latter but his reasons for this preference are not obvious. On this question he disagrees not only with me but also other participants of this roundtable. In their reviews,<sup>11</sup> Tadeusz Kugler, and Patrick Rhamey have offered reasons that are similar to mine, and they and Jack Levy (in his contribution to this roundtable) have also pointed to the odd phenomenon that Allison's work has failed to acknowledge its intellectual predecessors,<sup>12</sup> a significant departure from customary academic practice. This remark in turn suggests that although Wang views Allison's formulation to be distinct from power-transition theory, other colleagues see a much greater affinity between the two.

Having said all of this, I very much agree with Wang that "States can go to war for a variety of reasons. Attempting to isolate a single cause for all wars is impossible. The proposition that war tends to break out during a power transition is better understood as a probabilistic—not deterministic—statement." In order to assess the relevant probability of war, it is imperative that researchers develop a valid and comprehensive data base to enable such assessment. Sensible, consistent, and theoretically informed rules for the inclusion and exclusion of cases would be necessary to implement this data project. Otherwise, an analysis can be seen to engage in cherry-picking, ransacking history for evidence supporting its proposition(s).

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<sup>9</sup> See, for example, Susan G. Sample, "Power, Wealth, and Satisfaction: When Do Power Transitions Lead to Conflicts?" *Journal of Conflict Resolution* 62:9 (2018): 1905-1931; and Jonathan M. DiCicco, "Power Transition Theory and the Essence of Revisionism," in William R. Thompson, ed., *Oxford Encyclopedia of Empirical International Relations*, vol. 3 (Oxford: Oxford University Press, 2018), 188-214, <http://www.acsu.buffalo.edu/~fczagare/PSC%20504/DiCicco%20PT%20and%20Revisionism.pdf>, accessed 15 August 2020. Both DiCicco and Jack Levy make the important point that dissatisfaction and revisionism are two distinct concepts, a topic I will address (albeit only briefly) later. One may further add that power-transition theory is not unique in considering 'dissatisfaction' in its analyses. For example, status-discrepancy theory and the theory of relative deprivation also feature this concept prominently in their analyses of international relations, a point that Patrick Rhamey also recognizes. On whether China is a 'status-quo' power and on comparing its 'revisionist' tendencies with that of the U.S., see, respectively, Alastair I. Johnston, "Is China a Status Quo Power?" *International Security* 7:4 (2003): 5-56; and Steve Chan, Weixing Hu, and Kai He, "Discerning States' Revisionist and Status-Quo Orientations: Comparing China and the U.S.," *European Journal of International Relations* 27:2 (2019): 613-640.

<sup>10</sup> This position receives some empirical backing such as in Sample, "Power, Wealth, and Satisfaction."

<sup>11</sup> See also DiCicco, "Power Transition Theory and the Essence of Revisionism."

<sup>12</sup> The writings of A.F.K. Organski and Jacek Kugler on power-transition theory predate Allison's project by a considerable amount of time. Robert Gilpin has also written about similar general ideas. See A.F.K. Organski, *World Politics* (New York: Knopf, 1958); Organski and Kugler, *The War Ledger* (Chicago: University of Chicago Press, 1980); and Robert Gilpin, *War and Change in World Politics* (Cambridge: Cambridge University Press, 1981).

Wang is indeed also right in pointing out that Allison's formulation of Thucydides's Trap is not the same as power-transition theory. To be sure, there are some important similarities between them. To me, the former theory consists of the following key ideas (some as its 'hard core' assumptions and others as its testable hypotheses): states are the proper units of analysis; states are unitary, rational actors; they operate in a hierarchical (rather than anarchical) world; systemic wars are led and fought by the two most powerful countries in the world; these wars are more likely to happen when the power gap between them becomes smaller and when a rising power has revisionist intentions; they fight over who should determine the rules of international order; these fights are usually started by a rising power to challenge the dominant power; and conditions internal to a country are the main determinants of its industrialization and hence its relative position in the interstate balance of power.<sup>13</sup>

Allison's formulation also views the world as state-centric and dominated by great powers, and it also agrees that power parity elevates the danger of war—its most important and prominent similarity with power-transition theory. Allison, however, only considers power dynamics that narrows the gap between two polities to be a putative cause of war (hence it is monocausal) and as Wang points out, he appears to be agnostic about whether a ruling state or a rising state is more likely to start war.<sup>14</sup> This said, if one is to take Thucydides's maxim seriously (that Sparta's fear of Athens's rise was the basic, even inevitable, cause of the Peloponnesian War),<sup>15</sup> then the 'fault' of war lies more with the sense of anxiety, insecurity, and alarm felt by a declining ruling power than a rising power's ambition or greed.<sup>16</sup> Whereas at least the original formulation of

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<sup>13</sup> For an excellent review of power-transition theory as an evolving research program and an appraisal of this program in Lakatosian terms, see DiCicco and Levy, "Power Shifts and Problem Shifts: The Evolution of the Power Transition Research Program," *Journal of Conflict Resolution* 43:6 (1999): 675-704, and "The Power Transition Research Program: A Lakatosian Analysis," in Colin Elman and Miriam F. Elman, eds., *Progress in International Relations Theory: Appraising the Field* (Cambridge: The MIT Press, 2003), 109-157. For a summary of this research program's extensions, see Kugler and Douglas Lemke, eds., *Parity and Power: Evaluations and Extensions of the War Ledger* (Ann Arbor: University of Michigan Press, 1996); and Ronald L. Tammen, Kugler, and Lemke, "Foundations of Power Transition Theory," in William R. Thompson, ed., *Oxford Encyclopedia of Empirical International Relations* (Oxford: Oxford University Press, 2017), <https://oxfordre.com/politics/view/10.1093/acrefore/9780190228637.001.0001/acrefore-9780190228637-e-296>, accessed 15 August 2020.

<sup>14</sup> Allison's sixteen case files consist of wars that were initiated by both ruling and rising states, although he did not take up explicitly and systematically the determination of 'who started it.' Note that Organski's original analysis has mentioned the dominant power's adjustment of its role and expectations to accommodate the rising power, suggesting this to be one of the variables affecting whether a power transition would be peaceful or violent. Subsequent studies by power-transition analysts tend to overlook this variable, either implying or asserting that the rising power is solely responsible for starting wars. They overlook, for example, the possibility that psychological rigidity or bureaucratic inertia can cause a declining hegemon to fail to adjust its self-designated role as the world's policeman or to downsize its military footprint abroad, policies that may make a collision with a rising power more probable. On Organski's views on the dominant power's adjustment and other variables that can possibly affect the probability of a peaceful transition, see DiCicco and Levy, "Power Transition Research Program." I agree with Levy that in contrast to the tendency just reported about assuming or asserting a rising power's responsibility for starting wars, it is more promising and valid to treat the occurrence of war as a *bargaining* failure between the two sides (that is, *both* the rising and ruling states) to reach a settlement to avoid conflict.

<sup>15</sup> Although, as Wang observes, Allison does refer to human emotions affecting officials, he does not undertake a systematic analysis or give a sustained discussion on this topic (such as building on the relevant literature from psychology or social psychology on decision making), thus inclining me to argue that the causal connection between putative power shifts at the interstate level of analysis and their effects at the individual or group level is missing from his work.

<sup>16</sup> This is of course another irony of applying an analogy from ancient Greece to contemporary Sino-American relations, one that is overlooked by Allison and others. In my book, I also point out other ironies that make this analogy problematic when applied to Sino-American relations today. Sparta, the supposed ruling power, was an agrarian society, political oligarchy, and land power (its infantry of hoplites was the source of its military strength), and a polity that was in constant fear of rebellion by its slaves. In contrast, Athens, the rising power, drew its strength from its maritime capabilities and overseas commerce. It was also a democracy judged by the standards of its time. Do people seriously believe that these traits correspond to the U.S. and China, respectively? Can they afford to disregard these other determinants of war and peace as demonstrated by voluminous research on international relations? Historians disagree about

power-transition theory attended specifically to the two most powerful countries at the apex of interstate hierarchy, Allison has included in his analysis other ‘great powers’ although his decision rule for such designation is not clear. As I argue in my book, Allison’s criteria for inclusion or exclusion of specific wars and for the designation of ruling and rising states are also not clear. For example, China’s Opium War with Britain was omitted from his list,<sup>17</sup> and whether China should be considered a ruling state (albeit a declining one) is an interesting question because it still had the world’s largest economy in the early decades of the nineteenth century.<sup>18</sup>

Additionally, whereas power-transition theory offers a reason why great powers fight, Allison’s work does not. According to the former theory, the world’s two strongest countries fight over who should determine the international order or the ‘rules of the game.’ It is not clear why in Allison’s view China and the U.S. would want to fight. What stake would be so important to motivate them to risk mutual nuclear destruction? Here lies another difference between the two formulations: whereas power-transition theory sees states as rational actors guided by their cost-benefit calculations, it is not so clear whether Allison would admit non-rational factors as a cause of war.

Thucydides’s remark about Sparta’s *fear* of Athens’s rise (which may or may not be justified) naturally opens the door for the consideration of non-rational, including psychological and bureaucratic, factors in explaining the occurrence of war. Whether a rising power is revisionist or for that matter, whether a ruling state can also be revisionist is not an issue of concern to Allison. As another difference between the two perspectives, it is difficult to discern any specific scope conditions for Allison’s project, although A.F.K. Organski was quite explicit in stating one such scope condition for power-transition theory, namely, the dynamics described by this theory only obtain during the industrial age.<sup>19</sup>

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whether Athens and its coalition were in fact gaining power on Sparta and its allies on the eve of the Peloponnesian War, although naturally evidence on power shifts so long ago is sketchy and often based on speculation.

<sup>17</sup> I thank Wang for pointing out my mistake that the Crimean War was in fact included in Allison’s inventory of cases.

<sup>18</sup> This is a relevant issue because Organski and others have argued that gross domestic product provides the most direct and relevant indicator of a country’s power. See, for example, Organski and Kugler, *The War Ledger*.

<sup>19</sup> DiCicco reminds us of this important qualification, see his “Power Transition Theory and the Essence of Revisionism.” In contrast, Allison’s case files clearly include preindustrial societies and of course, the analogy from Thucydides refers to premodern polities (Sparta and Athens). By the way, I did not cite or stress some of the studies that are supposed to test power-transition theory because they do not appear to me to meet this theory’s hard-core assumptions or scope conditions, or address its main empirical claims. For example, Woosang Kim has employed alliances to study power transition, a move that in my view departs from one of power-transition theory’s core tenets, namely, states should be the basic units of analysis. See his “Power, Alliance, and Major Wars, 1816–1975,” *Journal of Conflict Resolution*, 33:2 (1989): 255–273, “Alliance Transitions and Great Power War,” *American Journal of Political Science* 35:4 (1991): 833–850, and “Power Transitions and Great Power War from Westphalia to Waterloo,” *World Politics*, 45:1 (1992): 153–172. By including years as far back as 1648, Kim also departs from power-transition theory’s scope condition, limiting it to the industrial era, as DiCicco and Levy point out in “The Power Transition Research Program,” 136. Not to put too fine a point on it, the original formulation of power-transition theory is concerned explicitly with the struggle at the very pinnacle of interstate hierarchy for global control. It therefore seems to me that later extensions of this theory to regional dynamics also deviate from its hard core. Others, such as DiCicco and Levy, who consider this extension to represent a progressive problem shift within the power-transition research program, may disagree. See their “The Power Transition Research Program.” On extending this program to regional politics, see Douglas Lemke, *Regions of War and Peace* (Cambridge: Cambridge University Press, 2002). Additionally, aggregate studies that encompass many ‘politically relevant’ pairs of states do not in my view meet the theory’s focus on the two most powerful states or at least the ‘great powers’ (the dynamics of rivalry among regional rivals or minor states can obviously be influenced and distorted by extra-regional actors whereas almost by definition, great powers are supposed to be able to ‘stand on their own’). Nor, it seems to me, does the theory’s purview cover armed contests that were clearly not fought over the nature of ‘international order,’ such as the Franco-Prussian War and the Russo-Japanese War. Conflicts falling short of the criterion of a systemic war, such as militarized interstate disputes, would also not qualify for testing it. As a final example, although some may see inclusion of states’ domestic politics as an extension of power-transition theory, I consider such studies to violate this theory’s core tenet that states are unitary, rational actors. This remark also suggests that factors such as human emotions are outside

While Allison discusses possible steps to avoid Thucydides's Trap (even though his work offers primarily a structural proposition), Organski appears to be more skeptical about the efficacy of human agency to overcome structural pressures as DiCicco has noted.<sup>20</sup> It is, moreover, important to point out that because both Allison and Organski give analytic primacy to power shifts between pairs of great powers, states' domestic factors are secondary in their importance in deciding outcomes of war or peace. Even though Allison (as Wang points out) does refer to human agency and policies to mitigate the structural strains created by power shifts, one is left wondering how effective these factors can be in his view (the structural condition caused by power shifts is described as a trap for a reason). On this point, I think Organski's position (that is, his more skeptical view on how and indeed whether human intervention can overcome structural pressures) is more consistent with his (and Jacek Kugler's) theoretical formulation. Note, however, that power-transition theory as well as Robert Gilpin's work point to domestic factors as the primary drivers of differential national economic growth.<sup>21</sup> Thus, both imply that attempts by foreigners to influence another country's growth trajectory are likely to have at best only a limited effect.<sup>22</sup>

Finally, it seems obvious but the point still needs to be made that both Thucydides's Trap and power-transition theory are formulations that privilege material conditions and for this reason do not focus on human agency, including people's emotions, the effective mobilization of their effort, and the skills with which they put the resources at their disposal to work. Rhamey is right to emphasize political capacity, an important factor determining a country's economic and military performance.<sup>23</sup> It appears to me that this concept or variable has not received as much attention as it deserves in the power-transition research program.<sup>24</sup>

Wang's comments seem to suggest a preference for qualitative, historical studies rather than large-N quantitative studies that have dominated research on power-transition theory. I will not address this difference in epistemology and methodology that evidently separates him from Rhamey and T. Kugler. I will, however, respond to his understandable question why I do not include territorial expansion in my consideration of a state's revisionism.

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its purview. For reasons given here, many recent works claiming an affinity to power-transition theory were not included in my book's discussion as Rhamey and Kugler have both noticed in their reviews.

<sup>20</sup> "Power Transition Theory and the Essence of Revisionism."

<sup>21</sup> Thus, proponents of power-transition theory point to the importance of population size and political capacity as determinants of a country's potential to become a competitive great power. See, for example, Ronald Tammen, Kugler, Lemke, Allan Stam III, Mark Abdollahian, Carole Alsharabati, Brian Efirid, and Organski, *Power Transitions: Strategies for the 21<sup>st</sup> Century* (New York: Chatham House, 2000).

<sup>22</sup> This point is driven home most vividly by the so-called Phoenix phenomenon, whereby even a disastrous defeat in foreign war would not permanently alter a country's development course. A country vanquished in war would rebound to its prewar growth trajectory in about two decades. See Organski and Kugler, "The Costs of Major Wars: The Phoenix Factor," *American Political Science Review* 71:4 (1977): 1347-1366.

<sup>23</sup> Organski and Kugler devoted an entire chapter in *The War Ledger* to explain how political capacity has enabled the Davids of international relations to prevail over the Goliaths. See also Jacek Kugler and Maria Arbetman, eds., *Political Capacity and Economic Behavior* (Boulder: Westview, 1997). Note, however, that the very emphasis on population size by power-transition theory suggests that there is a limit to how much political capacity can do to enhance a small country's power or international standing.

<sup>24</sup> As pointed out by DiCicco and Levy, this variable was not factored into Organski and Kugler's assessment of the belligerents' relative power in the wars that they examined in *The War Ledger*. Organski and Kugler's rationale that these were roughly comparable states in political development in turn raises the question whether political capacity matters for "a theory that is explicitly limited to great power behavior" ("The Power Transition Research Program," 129).

I agree with Wang whole heartedly that revisionism is an important topic but one that has been largely neglected by power-transition researchers.<sup>25</sup> “It is [a concept] used widely but seldom clearly defined.” I offer a few factual observations here on territorial expansion, postponing a theoretical explanation until later. How did the so-called great powers become great powers in the first place? Of course, territorial expansion was a big part of the story of their rise, whether in the case of Portugal, Spain, the Netherlands, Britain, France, or Russia before 1945. Except for Napoleonic France and Czarist Russia (especially in regards to St. Petersburg’s ambitions in the Balkans), I do not recall any characterization of these countries as ‘revisionist’ in the power-transition literature (of course, this consideration of revisionism is irrelevant to Allison’s project). Certainly, conventional historiography and power-transition discourse have not considered the U.S. to be a revisionist power during its period of ascendance, even though in U.S. Senator Henry Cabot Lodge’s words it had compiled “a record of conquest, colonization, and territorial expansion unequalled by any people in the nineteenth century.”<sup>26</sup> John Mearsheimer also observes, “Indeed, the United States was bent on establishing regional hegemony, and it was an expansionist power of the first order in the Americas.”<sup>27</sup> It acquired a massive amount of territory by defeating Mexico, taking over all or parts of what are today Arizona, California, Colorado, Nevada, New Mexico, Utah, and Wyoming. It went on later to annex Hawaii and waged a bloody campaign to colonize the Philippines. Imperial Germany’s territorial acquisitions would appear puny by comparison. Territorial expansion has not been a consideration in the power-transition literature’s treatment of Britain as a ‘satisfied’ ruling state (after all, what process had produced the phenomenon that the sun never set on the Union Jack?), or the U.S. as a ‘satisfied’ rising state.<sup>28</sup> The latter country’s supposed ‘satisfaction’ has been commonly invoked to explain why the Anglo-American power transition was peaceful.<sup>29</sup>

I do not deny that imperial Japan and Wilhelmine Germany were aggressive states and that their policies were reprehensible, but I do not consider them to be revisionist because as I will explain further below, they did not promote new ‘rules of the game’—in fact, they were imitating and following precedents set by the British and French in acquiring overseas colonies and advancing imperial claims. For example, in its aggrandizement at China’s expense, Japan did not do anything different in imposing its corporate interests, demanding war reparations, asserting a sphere of influence and extraterritorial concessions, sending expeditionary forces, practicing gunboat diplomacy, and waging war from what the British, French, Germans, and Americans had also done. Though deplorable, why should Japan’s colonization of Korea and Taiwan be treated differently from the conduct of other imperialist powers? I am not sure which of the then-prevailing rules Wang has in mind that Japan had broken but the others had not.

Finally, Wang mentions my reference to Steven Ward’s book on great powers’ revisionism and their quest for status recognition.<sup>30</sup> This reference is naturally pertinent to the important question of defining and studying revisionism. How do

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<sup>25</sup> Many other colleagues, including Sample and DiCicco cited in footnote 9, would agree with Wang on this point. I am also working on a co-authored book on this topic. Jonathan M. DiCicco and Victor A. Sanchez have written a review on this concept. See their “Revisionism in International Relations,” manuscript (2020).

<sup>26</sup> Quoted in Mearsheimer, *The Tragedy of Great Power Politics*, 238.

<sup>27</sup> Mearsheimer, *The Tragedy of Great Power Politics*, 238.

<sup>28</sup> Here is an example of bias and hypocrisy often hidden in our supposedly objective, empirical studies. There are other examples to be found in my discussion here.

<sup>29</sup> Fast forward to the more recent past, Iraqi President Saddam Hussein’s invasion of Kuwait and Russia’s annexation of Crimea have been interpreted and condemned as ‘revisionist’ moves, but not Israel’s acquisition of the Golan Heights and the West Bank (at least not by many Americans, including the Trump administration). Naturally, Iraq and Israel are not typically thought of as great powers. Nevertheless, they have been heavyweights in regional politics, and power-transition theory has been extended from its original focus on global struggles to regional competition.

<sup>30</sup> Steven Ward, *Status and the Challenge of Rising Powers* (Cambridge: Cambridge University Press, 2017).

we recognize a revisionist state? To clarify, although Ward considers imperial Japan and Wilhelmine Germany to be revisionist, I do not.<sup>31</sup> Ward calls attention to a state's rejection of international institutions and conferences and its refusal to enter into international agreements and accords, especially those pertaining to arms control and limitation, to indicate 'radical revisionism.' He considers imperial Germany, interwar Japan, and Nazi Germany to be such states. According to him, we can recognize radical revisionists when they refuse to join (or remain in) international organizations and decline to participate in arms control conferences or agreements, and when they make concerted efforts to improve their military capabilities and thereby to upset the interstate balance of power. I will not go into details here describing decisions by Germany and Japan to renounce treaties or reject agreements (e.g., the Versailles Peace Treaty, the Washington naval accord), to decline restraints on arms competition (e.g., the Anglo-German naval race), or to withdraw from the League of Nations (which, of course, the U.S. did not join). I would just offer as a factual observation that the U.S. has in recent years spent as much on its military as the *combined* defense expenditures of the next seven or eight highest countries and at one time shortly after the USSR's collapse, nearly as much as the rest of the world *combined*. Rather than being the proverbial 800-pound gorilla in the room, the U.S. has been in this regard the 330,000-pound blue whale! It is in a class by itself.<sup>32</sup>

I would also call attention to the fact that in 1999 the U.S. Senate refused to ratify the Comprehensive Test Ban Treaty despite President Bill Clinton's support for it. Along with China, the U.S. has remained outside the Treaty to Ban Landmines and the International Criminal Court. It is among the very few countries that have never joined other international accords such as the Basel Convention to regulate cross-border shipment of hazardous material, the Kyoto Protocol to limit greenhouse gas emission, and the United Nations Convention on the Law of the Sea (UNCLOS). Presidents George W. Bush and Donald Trump moreover withdrew the U.S. from the Anti-Ballistic Missile Treaty and the Intermediate-Range Nuclear Forces Treaty in 2002 and 2018, respectively. Trump has also pulled the U.S. from multilateral accords or institutions such as the Paris Climate agreement, the Trans-Pacific Partnership, the United Nations Human Rights Council, the United Nations Education, Scientific, and Cultural Organization (UNESCO), the United Nations Global Compact for Migration, the multilateral agreement to curb Iran's nuclear program (the Joint Comprehensive Plan of Action or JCOPA), and most recently, the Arms Trade Treaty, the Open Skies Treaty, and the World Health Organization. He denounced the North American Free Trade Agreement as the worst deal that the U.S. has ever signed,<sup>33</sup> insisting that it be renegotiated with Mexico and Canada to gain more favorable terms for the United States. He also announced that the U.S. will withdraw thousands of military personnel from Germany and threatened to do so to South Korea and Japan to show his displeasure that U.S. allies have failed to share adequately the burden of collective defense. As I have asked elsewhere, based on his criteria, how would Ward judge U.S. revisionism given many of these developments since his book's publication?

I am indebted to Wang for encouraging me to think more thoroughly about Ward's conclusions and my own views. To avoid confusion, my own personal view is that the more decisive indications of a country's revisionist agenda come from its propagation and pursuit of novel doctrines that depart significantly from existing international norms, such as republican France's challenge to monarchical rule, the USSR's ideology of the dictatorship of international proletariat, Nazi Germany's views on rule by a master race, Maoist China's support for armed insurrections to overthrow bourgeois governments, and U.S. doctrines of preventive war and regime change. Revisionism requires not only a wish to implement an agenda but also a

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<sup>31</sup> Thus, I do not believe that there is a contradiction here as Wang suggests. Nazi Germany, however, is a different matter. Both Ward and I consider it to be a revisionist state.

<sup>32</sup> See, for example, Stephen G. Brooks and William C. Wohlforth, "The Rise and Fall of the Great Powers in the Twenty-first Century: China's Rise and the Fate of America's Global Position," *International Security* 40:3 (2016): 7-53; Keir A. Lieber and Daryl G. Press, "The End of MAD: The Nuclear Dimension of U.S. Primacy," *International Security* 30:4 (2006): 7-44; and Barry R. Posen, "Command of the Commons: The Military Foundation of U.S. Hegemony," *International Security* 28:1 (2003): 5-46.

<sup>33</sup> <https://www.cnbc.com/video/2018/10/01/trump-nafta-was-worst-trade-deal-ever-made.html>.

capability to do so.<sup>34</sup> Maoist China, for example, lacked the necessary wherewithal to support insurgency movements abroad except for a few countries in its immediate neighborhood and even then, its efforts had only limited success.

This view has an important implication. Countries with limited power are less consequential, even if they harbor revisionist intentions. It is a different matter, however, when a large, powerful country decides to pursue a revisionist agenda. If this is a preponderant power without peers, other countries are less able or unable to stop it. Being overwhelmingly powerful, its policies are likely to reflect its domestic politics, natural instincts, and its leaders' personalities rather than its concerns about adverse reactions from foreign quarters.

These remarks bring me to Ja Ian Chong's comments, enumerating Beijing's acts of bellicosity or assertiveness. A comparative and historical perspective would be helpful to introduce some balance. For example, how have the U.S. and Chinese authorities reacted to their respective recent protest movements (in the case of China, I am referring to Hong Kong), and how have their responses in words and deeds differed from those of other countries that have also recently experienced mass protests in the Middle East (Iran, Iraq, and Lebanon) and Latin America (Bolivia, Chile, Colombia, and Ecuador)? Which countries have had more casualties due to police action? Presumably, governments that abuse their own citizens are unlikely to treat foreigners any better (hence the logic that one can infer a country's aggressive intentions in foreign policy by examining its history of treating its own people, especially its political dissidents, its ethnic and racial minorities, and its most disadvantaged socioeconomic groups).

Yes, China has sovereignty disputes with the other claimant countries in the South and East China Sea. And indeed, China and India's military personnel had a recent border clash resulting in fatalities on both sides. But again, some comparative and historical context is warranted. Except for its border with India, Beijing has settled all its land boundaries with its neighbors and often on terms more favorable to the latter.<sup>35</sup> It is true that Beijing has rejected the Permanent Court of Arbitration's jurisdiction in its sovereignty dispute with Manila in the South China Sea,<sup>36</sup> invoking a provision for this position afforded by UNCLOS just as other signatories have done. As already noted, Washington declined to join UNCLOS in the first place, and it also refused to accept the International Court of Justice's ruling that it had violated international law when it mined Nicaragua's ports. Other episodes relating to freedom of navigation come to mind, such as U.S. naval harassment of the Chinese freighter *Yinhe*<sup>37</sup> and of course, its "quarantine" of Cuba in 1962. What would one

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<sup>34</sup> Kai He, Huiyun Feng, and Steve Chan, "Rethinking Revisionism and Soft Revisionist Strategies in World Politics," manuscript (2020).

<sup>35</sup> M. Taylor Fravel, *Strong Borders, Secure Nation: Cooperation and Conflict in China's Territorial Disputes* (Princeton: Princeton University Press, 2008).

<sup>36</sup> China's economy and foreign trade are vulnerable to a U.S. blockade, and its navy and air force have lacked the ability to launch an invasion of Taiwan (as Rhamey has noted). Its maritime claims are not unrelated to its fear of U.S. trade embargo and interdiction of sea lanes, described sometimes as Beijing's 'Malacca Dilemma' (in reference to the Strait of Malacca presenting a chokepoint to its seaborne commerce). According to Aaron Friedberg, "In the event of a crisis or war, the United States and its partners could seize or sink Chinese commercial vessels at critical chokepoints or on the high seas, and there would be very little that Beijing could do about it. Because of its rapidly growing need for imported oil and other raw materials, the great bulk of which reach it by water, China is already vulnerable to the effects of a naval blockade, and it will become even more so as its economy grows." Aaron Friedberg, *A Contest for Supremacy: China, America, and the Struggle for Mastery in Asia* (New York: Norton, 2011), 228. Therefore, Beijing's moves in the South and East China Sea can also be motivated by defensive considerations. Space does not allow me to discuss that economic and naval blockade had figured prominently in Germany's conclusions about why it lost World War I, and that concerns about this vulnerability had motivated Japan to undertake the course of action that led eventually to Pearl Harbor.

<sup>37</sup> Washington claimed that this ship was carrying chemical weapons, a claim that turned out to be false. See [https://en.wikipedia.org/wiki/Yinhe\\_incident](https://en.wikipedia.org/wiki/Yinhe_incident).

say if China were to “quarantine” Taiwan? Allison is right in saying that all great powers, not just China, have ignored international law when they see their vital interests imperiled.<sup>38</sup>

Rather than focusing on specific incidents, we should examine a country’s behavioral pattern and contrast it with those of its counterparts. The last war fought by China was against Vietnam in 1979 around their border. One may ask how many wars the U.S. has fought and how often it has been involved in militarized interstate disputes (MIDs) in the last four decades?<sup>39</sup> Having “fistfights” (T. Kugler’s words) with another country’s border guards or deploying coastguards and civilian fishing vessels in maritime confrontations are not quite the same thing as applying massive and overt force to invade and occupy another country (Iraq, Afghanistan) or to impose a naval blockade against another country (Cuba) or for that matter, to seek regime change (Nicaragua, Grenada, Libya, Serbia, Syria, Venezuela in recent years and earlier in Salvador Allende’s Chile, Mohammad Mosaddegh’s Iran, Jacobo Arbenz’s Guatemala, and Fidel Castro’s Cuba), and to literally seize another country’s leader (Panama’s Manuel Noriega) or to assassinate its official in a third country (Iran’s General Qasem Soleimani in Baghdad).

Obviously, people tend to recall some events more readily than others given their varying familiarity or sympathy. For example, Americans are probably more likely to remember the downing of Korean and Malaysian civilian airliners by the Russians than the downing of a similar Iranian aircraft by the U.S. navy. Although Russia’s meddling in the 2016 U.S. election has been in the news headlines, I have not encountered any references in this context by the U.S. mass media to the numerous and flagrant instances of U.S. interference in other countries’ elections, dating as far back as the Central Intelligence Agency’s attempt to prevent the Italian Communist Party from winning the 1948 election.<sup>40</sup>

Rhamey criticizes Beijing for building islands and (concentration) camps.<sup>41</sup> Similarly, T. Kugler refers to China’s naval buildup, its installation of an overseas base (it would seem that he has Djibouti in mind), and its expansion of its nuclear program. But we need again to put these activities in a comparative context. These activities pale in comparison to the size of U.S. nuclear program, the number of its overseas bases, and the amount of its armament spending. If this behavior by China can possibly be interpreted as indicating its revisionist intentions (now or in the future), what are we to conclude about U.S. revisionism relying on these same indicators? China, after all, has not taken war to Britain, France, Japan, or the U.S.; these latter countries took war to China in their various military encounters fought on Chinese soil (except, of course, the Korean War fought at China’s doorstep).<sup>42</sup> I assume that Chong, Rhamey, and T. Kugler would not consider the U.S. to be a revisionist power despite its acquisition of extraterritorial jurisdictions such as in Cuba (Guantanamo) and Panama

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<sup>38</sup> Allison, Graham, “Of Course China, Like All Great Powers, Will Ignore an International Legal Verdict,” *The Diplomat*, 11 July 2016, <http://thediplomat.com/2016/07/of-course-china-like-all-great-powers-will-ignore-an-international-legal-verdict/>, accessed 12 June 2017.

<sup>39</sup> We know that disputed territory is the most common reason for states to get into wars and militarized interstate disputes. China has the largest number of neighbors in the world, and the U.S. has land borders with just two other countries. Therefore, it makes sense to adjust a country’s war/dispute proneness by the number of its neighbors. This adjustment will make it even clearer whether China or the U.S. has a greater propensity for war and MIDs. On contested territory as a source of war and MIDs, see John A. Vasquez, *The War Puzzle Revisited* (Cambridge: Cambridge University Press, 2009).

<sup>40</sup> On U.S. election interference abroad, see Dov H. Levin, “A Vote for Freedom? The Effects of Partisan Electoral Interventions on Regime Type,” *Journal of Conflict Resolution* 63:4 (2018): 839-868, and “When the Great Power Gets a Vote: The Effects of Great Power Electoral Interventions on Election Returns,” *International Studies Quarterly* 60:2 (2017): 189-202.

<sup>41</sup> China was not the first or only country to build artificial islets in the South China Sea, but it has undertaken the most massive land-reclamation projects.

<sup>42</sup> As a counterfactual, imagine what Donald Trump or for that matter, any U.S. president would do if an invading Chinese army is reaching the Mexican side of Rio Grande.

(the Canal Zone, also see the quote from Allison below), its annexation of Hawaii and colonization of the Philippines, its policies of confining Native Americans to reservations, its interning of citizens of Japanese ancestry during World War II, or its failure to address persistent socioeconomic inequalities and police brutality against racial minorities. As college professors, we do not give a test to just one student while exempting the rest of class from taking the same test when we assign grades. A sense of scale and proportion would also be helpful.<sup>43</sup>

Allison writes that during the late 1800s and early 1900s:

The US [had] declared war on Spain, expelling it from the Western Hemisphere and acquiring Puerto Rico, Guam, and the Philippines; threatened Germany and Britain with war unless they agreed to settle disputes on American terms; supported an insurrection in Colombia to create a new country, Panama, in order to build a canal; and declared itself the policeman of the Western Hemisphere, asserting the right to intervene whenever and wherever it judged necessary—a right it exercised nine times in the seven years of TR’s [Theodore Roosevelt’s] presidency alone.<sup>44</sup>

In a similar vein, Martin Jacques states:

As the world once more enters dangerous waters, in my view our concern should not so much be China but the United States. One of the remarkable things about China is how relatively peaceful it has been during its rise: contrast that with the US in its equivalent period, notably between 1860 and 1914, with the wars of westward expansion against Spain, Mexico, the annexation of Hawaii and the conquest of the Philippines. The same can be said, by the way, of the UK, France and also Japan, all of which fought many wars of expansion during their rise. In contrast, China’s rise has been characterised by an extraordinary restraint, a fact that is largely, if not overwhelmingly, ignored.<sup>45</sup>

Given the United States’ own historical record when it was a rising power, Allison warns his American readers to be more careful in wishing China to be “more like us.”<sup>46</sup> Significantly, to my knowledge no one working in the tradition of power-transition theory has argued that the U.S. was a revisionist power during its rise or is one today. Many Western scholars, whether they are proponents of the power-transition theory or not, have called China a revisionist state. These statements do not mean that I expect China’s rise to be peaceful. I am agnostic but believe China’s intentions will depend on many factors, including how Beijing feels it has been treated by the established powers. Moreover, I would not dismiss the idea, as prospect theory would suggest, that China may become more bellicose, assertive, or revisionist when it senses that its growth has stalled and that it faces imminent decline.<sup>47</sup> That is, a declining rather than growing China may be more a threat to international stability. This hypothesis of course goes against the grain of power-transition theory, and it also suggests that

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<sup>43</sup> A traditional Chinese saying refers to “officials outlawing ordinary people from lighting candles while they themselves commit arson.”

<sup>44</sup> *Destined for War*, 90.

<sup>45</sup> Martin Jacques, “When China and the U.S. Collide: The End of Stability and the Birth of a New Cold War” Jeju Peace Institute Peace Net, 2019, [http://jpi.or.kr/eng/regular/policy\\_view.sky?code=EnOther&id=5360](http://jpi.or.kr/eng/regular/policy_view.sky?code=EnOther&id=5360), accessed 10 June 2020.

<sup>46</sup> *Destined for War*, 89.

<sup>47</sup> On applications of prospect theory to international relations and foreign policy analyses, see William A. Boettcher III, *Presidential Risk Behavior in Foreign Policy: Prudence or Peril?* (New York: Palgrave, 2005); Jack Levy, “Declining Power and the Preventive Motivation for War,” *World Politics* 60:1 (1987): 82-107, and “Preventive War and Democratic Politics,” *International Studies Quarterly* 52:1 (2008): 1-24; and Rose McDermott, *Risk-Taking in International Relations: Prospect Theory in American Foreign Policy* (Ann Arbor: University of Michigan Press, 1998).

even an incumbent hegemon may become more revisionist as it experiences relative decline, a possibility that Rhomey acknowledges.

As he, T. Kugler, and Chong have all asked, how can we be sure about Beijing's peaceful intentions in the future? It is true that "states can always have a change of heart." This question has been raised by James D. Fearon as the commitment problem.<sup>48</sup> He argues that costly undertakings can give more credence to a country's promises.<sup>49</sup> A country's embeddedness in its deep and wide involvements with external institutions (such as Germany with respect to the European Union and the North Atlantic Treaty Organization, and China's connections with cross-border production chains and multilateral financial arrangements) can also have a reassuring effect because these ties are difficult and costly to reverse (given especially vested domestic interests that would oppose such reversal). Recent work by Stacie Goddard suggests that even "cheap talk" can enhance a country's policy resonance with foreign audiences and therefore their confidence in it.<sup>50</sup> Reneging on one's promises can have domestic and international repercussions, such as incurring a domestic audience cost and an international reputation cost.<sup>51</sup> Thus, it is not quite true that "there is nothing to stop [a state from] a change of heart," a position that Chong attributes to me. Parenthetically, whereas socialization might have altered China's outlook after it took on a more extensive and active role in international organizations and multilateral diplomacy, could this process work in reverse? Can we also talk about un-socializing? What can explain recent U.S. moves to disengage from international institutions and to abandon multilateralism?

My final reaction to Chong's review concerns national and cultural biases in our theories just as Zarakol has cautioned about the tendency to interject our understanding of the present into our reading of the past. Political science, including international relations, is as Stanley Hoffmann has put it succinctly, "an American social science."<sup>52</sup> U.S. scholars and those based in or educated by U.S. institutions dominate the pages of the most visible publications and they also train the largest number of graduate students who go on to propagate and sustain U.S. perspectives on international relations. This is a factual statement and not intended as a criticism of either Allison or participants in the power-transition research agenda. It

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<sup>48</sup> James D. Fearon, "Rationalist Explanations for War," *International Organization* 49:3 (1995): 379-414.

<sup>49</sup> James D. Fearon, "Signaling Foreign Policy Interests: Tying Hands versus Sinking Costs," *Journal of Conflict Resolution* 41:1 (1997): 68-90.

<sup>50</sup> Stacie Goddard, *When Right Makes Might: Rising Powers and World Order* (Ithaca: Cornell University Press, 2018).

<sup>51</sup> Kurt T. Gaubatz, "Democratic States and Commitment in International Relations," *International Organization* 50:1 (1996): 109-139; Anne E. Sartori, *Deterrence by Diplomacy* (Princeton: Princeton University Press, 2005); Jessica L. Weeks, "Autocratic Audience Costs: Regime Type and Signaling Resolve," *International Organization* 62:1 (2008): 35-64; and Jessica Chen Weiss, "Authoritarian Signaling, Mass Audiences, and Nationalist Protest in China," *International Organization* 67:1 (2012): 1-35. The extent of domestic audience costs, however, may not be very severe. See Jack Snyder and Erica D. Borghard, "The Cost of Empty Threats: A Penny, Not a Pound," *American Political Science Review* 105:3 (2011): 437-456; and Marc Trachtenberg, "Audience Costs: An Historical Analysis," *Security Studies* 21:1 (2012): 3-42.

<sup>52</sup> Stanley Hoffmann "An American Social Science," *Daedalus* 106:3 (1977): 41-60.

does caution us to beware of hidden biases and prejudices in the practice of our scholarship<sup>53</sup> which is disproportionately influenced by U.S. and European experiences.<sup>54</sup>

Chong is of course right that both Western and Chinese philosophical traditions (as well as those of other countries with a long and rich cultural heritage) are not monolithic and both feature heterogeneity and diversity. So it is hard to disagree with his observation that “Attributing tensions between the United States and PRC [People’s Republic of China] to culture suggests an overly monolithic view of the rich and varied philosophical and political traditions both major powers draw from, giving them less credit than is due.”<sup>55</sup> This remark may refer to my ‘stylized’ comments on Chinese and Western traditions, but it was not my intention to engage in sweeping generalizations that ‘essentialize’ China or the U.S., nor to suggest that their disagreements can be relegated to culture.<sup>56</sup> I also take seriously Chong’s excellent point that Chinese and U.S. officials may understand each other all too well and that their disagreements may be mainly due to reasons other than misunderstanding.

I of course concur with Rhamey that dissatisfaction with the international order (or revisionism) is not an inevitable trait of a rising power. He is also right that my basic outlook on international relations shares much with the founders of power-transition theory (thus, for example, I am far from being a ‘culturalist’ who adopts an idiographic approach to studying international relations). It is true that my references to power-transition theory tend to focus on those older studies that in my view are most directly relevant to this theory’s hard core. I did not mention many of the more recent works because they

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<sup>53</sup> A disturbing feature of power-transition theory is that much of its discourse allows for ‘U.S. exceptionalism.’ That is, it exempts the U.S. one way or another from its theoretical, empirical, or policy claims—such as this country was not revisionist when it was a rising power, thereby making its overtaking of Britain a peaceful transition; it was not a ‘contender’ in the “central system” of international relations (or it was an isolationist country as late as 1939), thereby rendering it possible to represent the two world wars as cases of Germany overtaking Britain; it was a ‘status quo’ country even though it had made vast territorial conquests, waged wars, and practiced hardball diplomacy during its ascent; it has been committed to the ‘international order’ even though it has propagated and pursued doctrines (including the Monroe Doctrine) in defiance of the then-prevailing norms and opposition from other countries. Taking this U.S. exceptionalism away makes this theory’s claims much more doubtful.

<sup>54</sup> Among others, David C. Kang has commented persuasively on the distortions and blind spots stemming from this phenomenon. See David C. Kang, *East Asia before the West: Five Centuries of Trade and Tribute* (New York: Columbia University Press, 2012), and “Getting Asia Wrong: The Need for New Analytical Frameworks,” *International Security* 27:4 (2003): 57-85. Remarkably, a survey of syllabi for seminars introducing international relations to Ph.D. students found plenty of European examples, but none (zero!) from Asian history. See David Kang and Alex Yu Ting Lin, “US Bias in the Study of Asian Security: Using Europe to Study Asia,” *Journal of Global Security Studies* 4:3 (2019): 393-401. For a fuller discussion of this topic, see the recent special issue of the aforementioned journal, with an introduction by Jeff D. Colgan, “American Perspectives and Blind Spots on World Politics,” *Journal of Global Security Studies* 4:3 (2019): 300-309.

<sup>55</sup> Chong cites approvingly several trans-historical books that study Chinese culture and practices to reflect on Western theorizing on international relations. In addition to Yuan-kang Wang’s *Harmony and War*, he refers to Victoria Tin-bor Hui, *War and State Formation in Ancient China and Early Modern Europe* (Cambridge: Cambridge University Press, 2005); and Alastair I. Johnston, *Cultural Realism: Strategic Culture and Grand Strategy in Chinese History* (Princeton: Princeton University Press, 1994). Precisely because as Chong suggests, Chinese civilization contains considerable heterogeneity featuring discourses on diverse strands of statecraft, it can accommodate a variety of behavior and competing interpretations of Beijing’s strategic culture, thus raising the question to what extent one may confidently conclude that it is more supportive of or congenial to, say, the dicta of realpolitik than Confucian thoughts on rule by the virtuous.

<sup>56</sup> This said, culture is of course also not unimportant. I cannot imagine that many people in China, Taiwan, Korea, or Singapore would protest their government’s mandate on wearing face masks during a pandemic, claiming that it violates their constitutional right. This example of course suggests convergence within a cultural tradition and divergence between cultural traditions.

seem to me to have drifted from this theory's basic tenets and concerns for reasons that I have given in footnote 19.<sup>57</sup> What should be considered this theory's hard core is something that, as Rhamey reports, DiCicco and Levy have addressed.<sup>58</sup>

I would contend that the timing of war is very much at this theory's heart—indeed, Levy brings up precisely this question of 'when' in his essay for this forum.<sup>59</sup> Does war happen when a latecomer reaches parity with an established power and if so, how do we define and specify parity? How long can this period of parity, or transition, last? Or does war happen after a latecomer has already overtaken an erstwhile leading state? Naturally, these questions depend on what measures of national power and what units of analysis we use. For instance, although Germany had a stronger army and a larger population than Britain before World War I, it did not reach Britain's level of per capita income and its population certainly did not exceed that of the British Empire.

Fast forward to today, has China reached parity with or even overtaken the U.S.? Some estimates of purchasing power parity would indicate that China's economy has already surpassed that of the U.S., just as the Composite Index of National Capability (CINC) of the Correlates of War Project had shown that the USSR had overtaken the U.S. before its demise.<sup>60</sup> I submit that such important issues remain unresolved. I do not view these issues as merely technical ones but consider them to pertain directly to the theory's core claims. They address the question whether these claims can be falsified. Rhamey suggests that "domestic, regional, and geographic variables thoroughly moderate the causal process [producing war after a latecomer has overtaken an established power]." Unless these mediating effects are specified more clearly, the theory's main proposition that parity (or overtaking) leads to war is difficult to verify.

Let me expand on this point. Rhamey states that "the [theory's] core is merely that parity increases the probability of war: an empirical proposition that is consistently confirmed."<sup>61</sup> Tammen et al, however, state that "Yet mere parity, even accompanied by an overtaking, is not the direct cause of conflict. Parity and overtaking must be accompanied by a challenging state's determination to change the status quo and a willingness by its elite to incur significant risks in order to

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<sup>57</sup> In "Foundations of Power Transition Theory," Tammen, Kugler, and Lemke provide a long list of references on research in the power-transition tradition, including many recent studies.

<sup>58</sup> "Power Shifts or Problem Shifts."

<sup>59</sup> See also John Vasquez, "When Are Power Transitions Dangerous? An Appraisal and Reformulation of Power Transition Theory," in Kugler and Lemke, eds., *Parity and War: Evaluations and Extensions of the War Ledger* (Ann Arbor: University of Michigan Press, 1996), 35-56.

<sup>60</sup> On this point, Rhamey and I agree (see footnote 23 in his commentary). Quantitative studies of power transitions tend to rely on rather crude measures of national power. For three studies that argue China is still far from catching up to the U.S., see Michael Beckley, "China's Century? Why America's Edge Will Endure," *International Security* 36:3 (2011/2012): 41-78; Brooks and Wohlforth, "The Rise and Fall of the Great Powers in the Twenty-first Century;" and Sean Starrs, "American Economic Power Hasn't Declined—It Globalized! Summoning the Data and Taking Globalization Seriously," *International Studies Quarterly* 57:4 (2013): 817-830.

<sup>61</sup> That X increases the chances of Y happening does not necessarily establish that X causes Y. Rhamey later qualifies his remark, stating that parity is neither a necessary nor a sufficient condition of war. Kugler also remarks that "early PT did not claim a causal relationship between transition and war [but] instead argued that this would be the situation most likely to have a conflict." Did early researchers of power-transition theory eschew attribution of causality? The authors of *The War Ledger* did after all use the language of necessary and sufficient conditions when discussing the relationship between power parity or overtaking on the one hand and war onset on the other. If we strip the idea that power shifts cause or at least presage war from power-transition theory, what is left of it? Having said all of this, it is also true that as Levy mentions in his essay here, power-transition analysts have been heretofore generally interested in the statistical association between power shifts and war/peace outcomes, leaving unexplored the causal mechanisms that gets one from the former to the latter. Finally, I agree with Levy on the matter of necessary and sufficient conditions. See Gary Goertz and Jack Levy, "Causal Explanation, Necessary Conditions, and Case Studies," in Goertz and Levy, eds., *Explaining War and Peace: Case Studies and Necessary Condition Counterfactuals* (New York: Routledge, 2007), 9-45.

alter the rules of the existing hierarchy.”<sup>62</sup> Rhamey states that power-transition theory offers a probabilistic and not deterministic prediction of war occurrence. But to what extent do parity, overtaking, or “domestic, regional, and geographic variables” affect this probability? I assume that from Rhamey’s perspective, parity should at the very least have a greater effect on this probability than the other variables. Otherwise, the theory’s core claim would be invalidated. But should we consider all the variables mentioned by him separately for their individual, independent impact on this probability, or should we attend to *only* their *interactive* effects such as when parity is *combined* with a dissatisfied latecomer? And under what circumstances should we expect the probability of, say, parity on war occurrence to be affected and even overridden or nullified by the other pertinent variables such as when the relationship in question pertains to two democracies?<sup>63</sup> I submit that these are not unfair questions and that a clear specification of expectations is necessary to judge whether the theory’s core claim that parity leads to war is confirmed or disconfirmed and if so, in what specific sense and context. Perhaps I have overlooked recent scholarship that has settled questions such as these which according to T. Kugler, have been “acknowledged and rectified.” But I remain unclear about the theory’s expectation on *when* war should occur, and Levy in his commentary here also thinks that this important issue remains unresolved.

As another example, Rhamey seems to suggest that China may not be catching up to the U.S. and may never be able to do so. Do other colleagues working in the tradition of power transition agree with this assessment? If there is not a consensus about this basic (descriptive and measurement) question,<sup>64</sup> it would be difficult to move on to debating about whether a power transition augurs war. Moreover, if China lacks the requisite capability (that is, if it fails to catch up to the U.S.), it would not matter whether it is ‘dissatisfied’ because according to power-transition theory, war would not occur in the absence of an ability to carry out a revisionist agenda.<sup>65</sup> I would of course never suggest that power-transition theory or any other social science theory asserts certainty but at the same time, some a priori criteria would be helpful for evaluating whether its core claim is supported or not.

I agree with Rhamey that this is not the time and place to have an in-depth debate on the selection of war cases pertinent to power-transition theory. However, I am confused by his statement that “some cases employed [in my book’s discussion], such as Japan and the US in World War II, the War of 1812, the Korean War, and the Spanish-American War, are not directly relevant to rising challenger overtaking a dominant power hypothesis of power transition theory.” On its face, this statement is certainly correct as it reflects accurately the original formulation of power-transition theory and in fact, captures well my complaint that much of the research undertaken in the name of this theory has not followed its original focus on the two leading powers at the apex of interstate hierarchy. This said, can we infer that Rhamey would also argue that the Franco-Prussian War and the Russo-Japanese War should be excluded from testing this theory (a point also raised by Levy in his contribution to this discussion)? These two wars were of course included in Organski and Kugler’s original study in *The War Ledger*. Given his statement just quoted, would Rhamey insist that we consider the U.S. overtaking Britain as a pivotal case to test this theory? This overtaking of course did not produce war even though Washington did certainly challenge London, as I discuss in my book, and Germany of course never overtook the U.S. before World War I or at any time since (by 1914, the U.S. had already overtaken Britain to become the world’s dominant power).

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<sup>62</sup> *Power Transitions*, 22.

<sup>63</sup> See, for example, Randall L. Schweller, “Domestic Structure and Preventive War: Are Democracies More Pacific?” *World Politics* 44:2 (1992): 235-269.

<sup>64</sup> I very much agree with T. Kugler that population and territorial size only point to a country’s potential power. Its political capacity is one of the keys determining whether it can realize this potential.

<sup>65</sup> As thus formulated, this statement stipulates that power parity (or overtaking) and a revisionist latecomer are each a necessary condition and are, moreover, jointly sufficient to produce war.

These remarks suggest considerable confusion about which countries and wars should or should not be in power-transition theory's purview, and this is important because their inclusion or exclusion can have a decisive influence on an analysis's conclusion. In part to make this point, I referred to the Korean War and some other conflicts to argue that parity or overtaking is not a necessary condition for war to happen. To avoid the problem of selecting on the dependent variable (that is, to only ask whether parity or overtaking has happened around the time when a war has been observed), I also point to cases of power transition involving other than *the* dominant or most powerful country, such as the Franco-Prussian and Russo-Japanese dyads before World War I, but ones that were not followed by war in their wake (such as China's overtaking of Japan and Germany's overtaking of Russia in recent decades). These latter cases show that parity or overtaking is also not a sufficient condition for war to occur.<sup>66</sup>

Rhamey mentions recent studies on power-transition theory, including research "on domestic political capacity, the consequences of parity across geographic space, the sources of satisfaction in domestic values, the development of regional orders, onset of civil wars, and post-war recovery." His observation is echoed by T. Kugler, who argues that researchers on power-transition theory having made significant progress since its original formulation, suggesting that recent scholarship has "already incorporated much of [my book's] suggestions for a more comprehensive framework with domestic politics, status, dominance vacuums, regions vs. global power structure, alliances, decision models, and even demographic structures all as attempts to measure that most famous of variables, 'want' or in PT terms 'satisfaction.'" Although I have criticized monocausal explanations and favor multivariate analyses, I do not see adding more and more variables to the power-transition theory necessarily to be a virtue to the extent that these new additions violate this theory's core tenets as I have described them earlier. Some such additions (or modifications) alter or do not pertain to the essence, or the hard core, of this theory, and they sometimes detract from it. We can certainly debate about whether these studies reveal progressive advances in this research program, or whether they are peripheral and perhaps even contradictory to this theory's basic tenets as originally stated. Even if we come down on one or the other side of this debate, I think we can all agree that there is a tradeoff between, on the one hand, a theory's parsimony, clarity, and even elegance, and on the other hand, its complexity and descriptive accuracy but at the possible expense of its falsifiability.

It is certainly true that an absence of violent behavior does not mean that a state is satisfied. As Rhamey correctly points out, both opportunity (capability) and willingness (dissatisfaction as a motivation) are required for this behavior to occur.<sup>67</sup> Therefore, an absence of, say, Chinese violent behavior may simply mean incapability rather than unwillingness. The converse of Rhamey's observation, however, is equally important. Surely, when a state exhibits violent behavior such as when it goes to war, attacks another country, or engages in MIDs, it must mean that it is dissatisfied in some way. This observation in turn returns us to my earlier question asking about China's incidence of being involved in wars and MIDs compared to the U.S.

I now turn to the question of '(dis)satisfaction,' a topic that has also been raised by Chong, T. Kugler, Levy, and DiCicco and in my view, one that points to the weakest part of power-transition's research agenda. What is that which a state can be satisfied or dissatisfied about? Sample, DiCicco, Levy, and others have called attention to the fact that a country can be dissatisfied with another country without being dissatisfied with the rules, norms, and institutions of the international order.<sup>68</sup> I have also argued that a failure to recognize this distinction has often led analysts to confuse a country's quarrel with the incumbent hegemon with its challenge of the international order or the entire international community (in other words, I submit that the dominant power and international order are not synonymous). I also wish to emphasize that 'the status

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<sup>66</sup> A moment's reflection suggests that whether the overtaking country or the country being overtaken is a democracy or ostensibly revisionist did not affect the peaceful outcome of these transitions.

<sup>67</sup> The concepts of opportunity and willingness come from Benjamin A. Most and Harvey Starr, *Inquiry, Logic and International Politics* (Columbia: University of South Carolina Press, 1989).

<sup>68</sup> Sample, "Power, Wealth, and Satisfaction;" and DiCicco and Levy, "The Power Transition Research Program," 132.

quo' and 'international order' are not the same thing.<sup>69</sup> But what is this distinction all about? I turn to former U.S. National Security Advisor and Secretary of State Henry Kissinger.

He characterizes world politics as “a set of commonly accepted rules that define the limits of permissible action and a balance of power that enforces restraints where rules break down, preventing one political unit from subjugating all others.”<sup>70</sup> When people conflate these two distinct (though of course interrelated) analytic concepts and empirical domains (the international order and the interstate system's distribution of power), they tend to naturally confuse changes in the relative capabilities of states (or attempts to promote such changes) with an assault on the international order. States may be dissatisfied with their relative position in the interstate hierarchy or distribution of power ('the status quo' or the current balance of power) without, however, being prepared to challenge and overthrow the existing international order (that is, being revisionist). Moreover, a revisionist state does *not* necessarily have to resort to armed aggression to alter the existing international order as suggested by recent scholarship<sup>71</sup> (there is a distinct post hoc tendency in the prevailing discourse on revisionism, attributing this motivation to a state after knowing that it has fought a war against 'our' side). That a state can be dissatisfied with its power position but still not be considered a revisionist state bent on overhauling the international order should be evident to all power-transition proponents who see the U.S. as a non-revisionist power during its years of ascent even though it engaged in extensive territorial expansion. Therefore, as DiCicco and Levy have reminded us, dissatisfaction and revisionism are not the same thing. Moreover, and as it should have become evident by now, by international order—the key idea in Organski and Kugler's original formulation of why states at the very top of interstate hierarchy get themselves into a fight—I have in mind the norms, rules, and institutions of international *society* as discussed by the English School<sup>72</sup> and as hinted at by Kissinger, if you will, states' common expectations and shared understandings about the 'rules of the game.'

All this has led me to argue that states can engage in territorial expansion, seek colonial conquest, undertake coercive diplomacy, and compete for spheres of influence without being revisionist because these actions were permitted by the then-prevailing rules of the game. These countries were engaging in aggressive policies of aggrandizement, thus seeking to alter the 'status quo' as defined by the existing balance of power. But this behavior does not necessarily mean revisionism as defined above. I do not believe Organski would deny that states are motivated to pursue their interests (including power) but he clearly did not expect them all to be revisionist. By conflating 'the status quo' (again defined as the existing distribution of power) with the 'international order,' many analysts end up treating a late-arriving state's attempt to improve its position in the interstate hierarchy as ipso facto evidence of its intention to challenge the international order (except of course in the

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<sup>69</sup> I do not quite understand what Rhomey means when he says that “the United States ... embodies the status quo.” I assume that he is saying that the U.S. had played a dominant, even decisive, role in shaping and sustaining the rules, norms, and institutions of the post-1945 liberal international order. Ironically, as G. John Ikenberry has written, the main threat to this order is coming from none other than the U.S. See “The Plot against American Foreign Policy: Can the Liberal Order Survive?” *Foreign Affairs* 96:3 (2017): 2–9, and “The End of Liberal International Order?” *International Affairs* 94:1 (2018): 7–23. Schweller has also commented that the U.S. has not hesitated to abandon this order when its important interests are threatened. See Schweller, “The Problem of International Order Revisited: A Review Essay,” *International Security* 26:1 (2001): 161–186. Of course, this order has not been always truly international (that is, it has been ideologically and geographically bounded) or for that matter, truly liberal (such as its inclusion of illiberal members and its acceptance of some forms of protectionism such as in agriculture).

<sup>70</sup> Henry Kissinger, *World Order* (New York: Penguin Books, 2014), 9.

<sup>71</sup> Goddard, “Embedded Revisionism: Networks, Institutions, and Challenges to World Order,” *International Organization* 72:4 (2018): 763–797; and Scott L. Kastner, Margaret M. Pearson, and Chad Rector, “Invest, Hold Up, or Accept? China in Multilateral Governance,” *Security Studies* 25:1 (2016): 142–179.

<sup>72</sup> Hedley Bull, *The Anarchical Society: A Study of Order in World Politics* (New York: Macmillan, 1977); and Barry Buzan, *From International to World Society? English School Theory and the Social Structure of Globalisation* (Cambridge University Press, 2004).

case of the U.S. when it was a rising power). At the same time, an already dominant power's attempt to preserve or enhance even further its preponderance is rarely seen as disturbing 'the status quo' or intended to revise the international order.

There is a palpable status-quo bias in many studies on power transition which treat 'international order' as somehow sacrosanct, even though it may not serve the cause of justice, fairness, human dignity, or even peace.<sup>73</sup> Revisionism is often used as a pejorative term or codeword to indicate one's disapproval of another country's policy without considering that as Evelyn Goh has pointed out,<sup>74</sup> international order is always in flux and being contested. All countries agree with and support some norms, rules, and institutions of this order while disagreeing with and subverting some others. They play offense and defense at the same time. Contrary to common insinuations in power-transition discourse, the dominant power is not the same thing as international order, nor is it always its chief defender. Moreover, international order is not a settled or fixed matter, as this discourse implies. It is instead being constantly negotiated and renegotiated. Yet, 'international order' is often invoked euphemistically to endorse the maintenance of the 'status quo' which, even power-transition theorists recognize, reflects a hierarchical world favoring those that are its current top-dogs.

It is also evident that one may agree with and support some new norms, rules, and institutions introduced by a dominant state (that is, some of its revisionist moves), such as prohibitions against crimes against humanity and peace after World War II and the ban on nuclear proliferation more recently, while objecting to other doctrines such as 'preventive war,' 'regime change,' and 'responsibility to protect.' Is it not the case that these are all attempts to revise or alter the principles that are (were?) enshrined in the Westphalian order (especially its bedrock principles of states' sovereignty and their territorial integrity)?<sup>75</sup> This view argues that a dominant power *can* be revisionist (at least on some occasions and about some matters, which in turn means that a country's revisionism can vary across issues and over time).<sup>76</sup> It also goes without saying that lesser powers can also promote revisionist ideas to alter existing norms, such as the developing countries' campaigns for a 'new international economic order' and against apartheid and other forms of racial discrimination. 'Norm entrepreneurs' have also sought to ban land mines and weapons of mass destruction, among other causes.

Clearly, this is not a binary matter of assigning states to dichotomies of being either revisionist or not. Although space does not permit a more detailed explanation, I argue that the dominant power cannot be assumed to be satisfied<sup>77</sup> or for that matter, disinclined to be revisionist. Finally, although it seems obvious, the point should still be made that international order is not something that can be simply imposed unilaterally by a dominant power—the consent and cooperation of subordinate states are also necessary. Yes, the dominant power can and has played a leading and even decisive role such as in

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<sup>73</sup> This should have been clear from my discussion on territorial expansion, colonial conquest, imperial wars, gunboat diplomacy, spheres of influence, and racial discrimination—all of which were once condoned and indeed legitimated by the rules of 'international order.' Scholars such as Organski and Gilpin have acknowledged forthrightly that these rules were designed to serve and advance the interests of strong, established powers.

<sup>74</sup> Evelyn Goh, "Contesting Hegemonic Order: China in East Asia," *Security Studies* 28:3 (2019): 614–644.

<sup>75</sup> It is therefore ironic that the word 'revisionism' is often deployed in prevailing U.S. discourse in exactly the opposite way of its customary meaning. A China that professes to uphold and wants to maintain the traditional Westphalian principles of sovereignty and non-interference in other countries' domestic affairs is labeled 'revisionist,' whereas a U.S. that challenges and undermines these principles (such as in openly advocating 'regime change' abroad) is described as a 'status quo' country. Thus, the concept of revisionism is often 'weaponized' as a rhetorical device rather than employed as an analytic concept.

<sup>76</sup> Rosemary Foot and Andrew Walter, *China, the United States and Global Order* (Cambridge: Cambridge University Press, 2011).

<sup>77</sup> Offensive realists would surely contend that states would never be satisfied with the power they already have and would always want more.

installing the post-1945 liberal international order,<sup>78</sup> but this order can also be introduced and sustained by a multilateral regime such as the Concert of Europe after the Napoleonic Wars (that is, a single dominant power is not necessary to create and support this order as is suggested by hegemonic stability theory).<sup>79</sup> Britain never achieved the same level of preponderance as the U.S., and it had only the status of *primo inter pares* in the Concert of Europe.<sup>80</sup> Finally, as Charles Kupchan has argued,<sup>81</sup> an international order—specifically, Europe’s Concert system—can come to its demise not because of challenges from conservative or autocratic governments; in this case the Concert was rather deliberately undermined and dismantled by its leading liberal members (Britain and France). If I understand Wang correctly, some such important issues have often been dispensed with by facile assumptions and blanket assertions in many quantitative studies relying on aggregate data even when these assumptions and assertions are contradicted by historical facts.<sup>82</sup>

I am indebted to Levy not only for his comments for this roundtable but also for his past scholarship, which has had a great deal of influence on my own thinking about systemic wars, power transitions, democratic peace, the preventive motivation, prospect theory, and the differences in other states’ reactions to regional and global powers and to those with primarily a maritime versus continental orientation. My intellectual debt to Levy’s work is too substantial to cite here (as this essay is already too long) but was amply documented in my book. Naturally, I find much to agree with and little to dispute in his review of my book. I would mention here only two minor points. I thought about modern international relations starting from 1815 without, however, having made this dating explicit in my book. Because of this dating and power-transition theory’s concern with only international relations during the industrial era, I omitted discussions of the Anglo-Dutch and Hapsburg-French rivalries. My other point pertains to the Anglo-American dyad. Specifically, some of the standard reasons given for this peaceful transition strike me as possibly post hoc constructions. How can *constant* factors such as a common heritage, strong economic interests, and geographic distance explain *change* in this relationship (it was turbulent, even confrontational, earlier but became amicable later)?

Finally, I very much concur with Levy’s point that British grand strategy was motivated by a consistent (and constant) desire to prevent the rise of a hegemonic power on the European continent. Mearsheimer’s<sup>83</sup> argument in the same vein also resonates with me, specifically, with the United States having established its own regional hegemony in the Western

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<sup>78</sup> G. John Ikenberry, *After Victory: Institutions, Strategic Restraint, and the Rebuilding of Order after Major Wars* (Princeton: Princeton University Press, 2001).

<sup>79</sup> On hegemonic stability theory, see the classic study by Charles P. Kindleberger, *The World in Depression: 1929-1939* (Berkeley: University of California Press, 1973).

<sup>80</sup> On the Concert system, see Kissinger, *A World Restored: Metternich, Castlereagh, and the Problems of Peace: 1812-22* (Boston: Houghton Mifflin, 1957).

<sup>81</sup> Charles Kupchan, *How Enemies Become Friends: The Sources of Stable Peace* (Princeton: Princeton University Press, 2010).

<sup>82</sup> Naturally, not all people hold all the following views (implicitly or explicitly) all the time, views that need not be consistent either logically or empirically: that the dominant power can almost unilaterally decide the rules of international order; that if the U.S. disengages from the world, China will be able to rewrite these rules; that the provision of public goods requires a single dominant power; that by sustaining ‘the’ international order, the dominant power provides public goods (and therefore, with its decline this provision is jeopardized); that the rules of international order are rigged to benefit the dominant power; that power transition presages order transition (except, of course, in the case of U.S. ascent to global primacy); that international order is threatened by dissatisfied rising powers; that the dominant power is steadfast in its support of this order even when it experiences decline; that because it has the largest stake in the existing international order, the dominant power cannot ever become revisionist; that although the existing order has facilitated the rise of latecomers, they will remain dissatisfied with it even though they now have a larger stake in it; and that war is the primary mechanism for transforming the international order.

<sup>83</sup> *The Tragedy of Great Power Politics*.

Hemisphere, a consistent and continuing feature of its diplomacy has been to prevent the rise of another regional hegemon whether in Europe (Germany or the USSR), the Middle East (Iran versus Iraq, and Iran versus Saudi Arabia), South Asia (India versus Pakistan), or East Asia (Japan or China). This explains London and Washington's history of alternating their support for the weaker of the regional contenders when the other one threatens to become too powerful. The more important theoretical insight and policy question raised by Levy, however, indicates that one should not assume the rise of a regional power *necessarily* means that it is challenging the existing hegemon's global position. Very different dynamics are involved in regional versus global competition, and this difference entails different policy responses. Both world wars have been misinterpreted by power-transition theory as Germany's challenge to Britain's global position when it represented in fact Berlin's bid for regional dominance (the same can be said about Japan's war with the U.S. in the Pacific). Levy's probing question is highly relevant for contemporary Sino-American relations: "Is this a competition between a rising regional power and a global power for regional dominance, or a competition for dominance in the global system?" My answer to his question should be obvious. As he says, "the [latter], but presumably, not the former, might involve a remaking of the rules and norms of the global system that Organski posited was at the heart of power transitions." The danger is to misconstrue or misrepresent a possible regional contest as a global struggle, thereby to exaggerate the danger posed by the newcomer and the stake involved in this competition.<sup>84</sup> One may also ponder what would occur if the shoe were on the other foot. Benjamin Schwarz has remarked,

Hardliners and moderates, Republicans and Democrats, agree that America is strategically dominant in East Asia and the eastern Pacific—China's backyard. They further agree that America should retain its dominance there. Thus U.S. military planners define as a threat Beijing's efforts to remedy its own weak position in the face of overwhelming superiority that they acknowledge the United States holds right up to the edge of the Asian mainland. This probably reveals more about our ambitions than it does about China's. Imagine if the situation is reversed, and China's air and naval power were a dominant and potentially menacing presence on the coastal shelf of North America. Wouldn't we want to offset that preponderance?<sup>85</sup>

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<sup>84</sup> Of course, officials have been known to deliberately inflate foreign threats or scapegoat foreigners for domestic political gains. See, for example, Michael P. Colaresi, *Scare Tactics: The Politics of International Rivalry* (Syracuse: Syracuse University Press, 2005).

<sup>85</sup> Benjamin Schwarz, "Comment: Managing China's Rise," *The Atlantic Monthly* (June 2005), <http://www.theatlantic.com/magazine/archive/2005/06/managing-chinas-rise/303972/>, accessed 10 June 2006.