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Introduction by Tuong Vu

Ja Ian Chong. *External Intervention and the Politics of State Formation: China, Indonesia and Thailand, 1893-1952.* New York: Cambridge University Press, 2012. ISBN: 9781107013759 (hardback, $104.99); 9781107679788 (paperback, $32.99).

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Introduction by Tuong Vu, University of Oregon

The modern state is the most fundamental unit of international politics but the literature on comparative state formation has relatively recent origins.[1] This literature builds on Western European cases and has slowly expanded its comparative scope to cover Asia, Africa, and the Americas. Scholars have debated the role of various factors such as war, religion, geography, and elite politics that shape or drive the state forming processes. Yet few have attempted to theorize about how foreign intervention may affect those processes.[2] Ja Ian Chong's book will thus be particularly helpful for those who study state formation in the global periphery. The book is also useful for diplomatic historians interested in understanding the range of impacts foreign intervention can theoretically have on the emergence of new states during colonization, decolonization, and the Cold War.

All three contributors to this roundtable praise Chong for focusing on external intervention as a key variable of state formation in weak polities. In this framework, foreign actors competing for access to a particular territory calculate the costs of intervention compared with other options. If those costs are too high, actors are willing to accept sovereign statehood for the territory as a way of denying their rivals exclusive access. Feng Zhang views Chong's argument that such intervention can facilitate rather than obstruct sovereign state formation as "bold and original." Joseph Parent agrees that Chong fills a gap in the literature that commonly neglects the role of external forces in shaping the form of new states. Thomas Pepinsky further suggests that Chong's argument poses a challenge to "autonomous history" approaches to the origins of modern states.

The contributors are generally positive about the book but they also make three major criticisms ofit. First, Zhang argues that Chong's focus on foreign intervention causes him to neglect local agency. Zhang points out that Chong's account is essentially a structural one in the sense that the calculation and interaction among powerful foreign actors formed a particular structure of international politics that constrained local state builders. However, Chong does not show how sovereign states actually emerged in his cases, which would require him to take into consideration the role played by local agents.

A second criticism concerns the empirical basis of Chong's theoretical account. Parent complains that "Chong does too much telling and too little showing. Rarely are debates quoted or decision-makers' views laid out in their own words." The rational calculations of costs and benefits of intervention made by foreign actors are simply not shown to have actually occurred, which makes it difficult to verify Chong's claim that such calculations mattered. For Indonesia, Pepinsky argues that Chong's logic leads one to assume that the U.S. and the Soviet Union supported independence simply because it would have been

[1] See Tuong Vu, "Studying the State through State Formation," *World Politics* 61:1 (2010): 148-175.

[2] A notable exception is Ian Lustick, "The Absence of Middle Eastern Great Powers: Political "Backwardness" in Historical Perspective," *International Organization* 51:4 (1997): 653-683.

costly for those two superpowers to colonize Indonesia. No historical evidence exists that indicates any U.S. or Soviet intention to do so.

Third, Zhang takes issues with what he views as Chong's overemphasis on the positive effects of foreign intervention on the formation of sovereign states. Zhang argues that while foreign intervention may have facilitated the emergence of a Chinese sovereign state in the 1930s, foreign intervention was also responsible for China's gradual loss of autonomy since the 1830s. Zhang thus expects the book to be controversial in China where official historiography regards foreign intervention as the culprit of all the travails Chinese have experienced during the last century. However, if one assumes that the modern sovereign state is a form more suitable to modern time than the pre-modern empire, foreign intervention during the nineteenth century that destroyed the Chinese empire made a positive contribution because imperial destruction paved the way for the modern state to emerge not only in China but also in China's suzerain states such as Japan, Korea, Mongolia, and Vietnam. Perhaps in a future project Chong can extend the timeframe of his book to the whole nineteenth century to see whether his theory still applies.

Participants:

Chong Ja Ian is Assistant Professor of political science at the National University of Singapore. His research crosses the fields of international relations and comparative politics, with a focus on security issues pertaining to China and East Asia and issues at the nexus of domestic and world politics. He is currently working on two projects. One examines how aggregate non-leading state responses to power transition affect regional order and stability in Asia. Another explores the effects of major power support for authoritarian rule on alliance politics after political liberalisation. Chong is author of *External Intervention and the Politics of State Formation: China, Indonesia, Thailand—1893-1952* (Cambridge, 2012), winner of the 2013 International Security Studies Section Best Book Award from the International Studies Association. His articles have appeared in *International Security*, the *European Journal of International Relations*, *Security Studies*, *Twentieth Century China*, and *Asian Affairs*. He received his Ph.D. in politics from Princeton University.

Tuong Vu is an Associate Professor of Political Science at the University of Oregon, and has held visiting fellowships at the National University of Singapore and Princeton University. His research focuses on the politics of state formation and ideological movements in East Asia. His book, *Paths to Development in Asia: South Korea, Vietnam, China, and Indonesia*, received a 2011 Bernard Schwartz Award Honorable Mention. Recent publications include "Socialism and Underdevelopment in Southeast Asia," in *Handbook of Southeast Asian History*, ed. Norman Owen (2013), and "Workers under communism: Romance and Reality," in *Oxford Handbook on the History of Communism*, ed. S. A. Smith (2014). He just completed a book manuscript about the Vietnamese revolution as a case of radical movements in international politics.

Joseph M. Parent is an Associate Professor of Political Science at the University of Miami. He is the author of several articles and two books: *Uniting States: Voluntary Union* *in World Politics* (Oxford, 2011) and (with Joseph E. Uscinski) *American Conspiracy Theories* (Oxford, 2014).

Thomas B. Pepinsky (Ph.D., Yale, 2007) is Associate Professor of government at Cornell University. He is the author of *Economic Crises and the Breakdown of Authoritarian Regimes* (Cambridge University Press, 2009) as well as articles on comparative politics and political economy in Southeast Asia and beyond in journals such as the *American Journal of Political Science*, *International Studies Quarterly*, *Pacific Affairs*, *World Politics*, and others. He serves as president of the American Institute of Indonesian Studies as well as co-convener of the Southeast Asia Research Group.

Feng Zhang is Fellow in the Department of International Relations, the Australian National University. He received his Ph.D. in International Relations from the London School of Economics, and has previously taught at Tsinghua University and Murdoch University. His work on China's historical role in world affairs and contemporary Chinese foreign policy has appeared in the *Chinese Journal of International Politics, European Journal of International Relations*, *International Politics*, *Review of International Studies*, and *Survival*. He is the author of *Chinese Hegemony: Grand Strategy and International Institutions in East Asian History* (Stanford University Press, forthcoming).

Review by Joseph M. Parent, University of Miami

Ja Ian Chong deserves our appreciation for writing a stimulating and helpful book. The work's puzzle is why Asian states settled on a European model of sovereignty rather than other forms of political organization. Chong's basic argument is that patterns of great power intervention are the key factor (2). European-style statehood among weak states is a middling goal (30-31). For great powers, the best result is to dominate an area, and the worst is to have rivals dominate an area. When no outsiders can secure their favorite outcome, they settle on the sovereign state to avoid their least favorite outcome.

Since at least the time of Charles Tilly's work, we've known that international forces are crucial in state building and modern sovereignty.[1] Yet to date few works have theorized about how different constellations of external forces yield different polities. Chong begins filling this gap. He usefully disaggregates state form along three dimensions—political centralization, territorial exclusivity, and external autonomy—and ventures intricate contentions about how shifts in external powers' positions affect the domestic composition of weak states.

In essence, his claims boil down to opportunity costs and access denial. Great powers weigh the value of intervening in a territory—based on either the economic resources it contains or its strategic value to transit networks—against the likely costs incurred to deny others access to that territory, and then compare these costs and benefits to other grand strategic commitments they could make. He tests his hypotheses on three least likely cases, China, Indonesia, and Thailand, and finds that his argument fares better than three alternative views: nationalist ideology, war-making as state-building, and institutional commitment. Chong also does a good job examining cases in the round, that is, from the perspectives of all the relevant players.

Admirable as it is, no work is perfect and this one is no exception. First, the core concept is defined decently, but it is not measured well. Opportunity costs are "the understandings political leaders hold of costs, capabilities, and the compromises that pursuing various goals may entail" (29), and to track opportunity cost the author considers "debates amongst policymakers over the expected net returns from investing a given set of capabilities towards intervention in a polity relative to the net gains from committing to other objectives." (31). Unfortunately, Chong does too much telling and too little showing.

Rarely are debates quoted or decision-makers' views laid out in their own words. Chong makes plausible circumstantial cases for how countries may have assessed when and how much to intervene, but by the terms of his own argument he does not buttress his claims. In addition, he wisely chooses not to delve into the origins of exact perceptions, but then unwisely fails to assert that perceptions tend to gravitate around underlying fundamentals.

[1] Charles Tilly. "International communities, secure or otherwise," in Emmanuel Adler and Michael Barnett, eds., *Security Communities* (New York: Cambridge University Press, 1998). See also Hedley Bull and Adam Watson, *The Expansion of International Society* (New York: Oxford University Press, 1985).

This would have strengthened his case in the few instances in which he discussed shifting trade statistics or the price-tag of military action. Even had he done these things, it is still not clear empirically how he draws lines between high, medium, and low opportunity cost.

This makes it tough to verify Chong's claims. We all know that people tend to do things because they think the benefits will outweigh the costs. But this is a truism. If we don't know how they do the accounting, and the calculations appear untethered to fundamentals, and we seldom see anyone actually lay out a ledger of their incentives, and we have little guidance on how to compare decisions, then much of the argument rests on appeals to authority. At times, various levels of support from various great powers seem to have contributed to sovereignty (e.g. 171). This may be correct, but with no specification of what evidence would disconfirm the argument it is hard to tell. Connected to this confusion is that the treatment of counterarguments starts out vigorously and ecumenically before shrinking to single paragraphs as the work progresses.

A related issue has to do with the case selection. Chong is most persuasive when speaking about China, but then the other cases feel tacked on. In fact, the discussion of China was more than two-and-half times longer than all other cases combined. There are good grounds for believing China to be a more important case, but China looks different than Indonesia and Thailand in so many ways during the period under consideration that I was uncomfortable with comparisons without more, and more precise, control factors (20-23).

So, too, there are issues with scope—how applicable is the argument? Sometimes, it seems Chong finds his logic to be valid in all weak states (33). This lumps a lot of disparate states together. Other times, he confesses with becoming modesty that his explanation is not universally valid, and may not account for changes in the same state at various periods (45). But this ambiguity has serious policy consequences. If Chong wishes to speak to state formation in Iraq and Afghanistan—and the text is littered with attempts to do so—he needs to tie them more tightly to his argument. If both countries are weak states and domestic factions in weak states are highly dependent on outside aid (3, 33), why has the United States spent vast sums to little avail? What exactly should we learn from China, Indonesia, and Thailand that would help solidify Iraq and Afghanistan? Plus, it is hard to reduce recent events in Afghanistan, at the very least, to a tale of outsiders competing for access, however broadly construed.

Were Chong only partially right about the Chinese case alone, his book would still render a valuable service. My sense is he has done better than that and made a significant advance. Regardless, he has reopened a promising field of inquiry and thrown down an enticing gauntlet. I look forward to his future work.

Review by Thomas B. Pepinsky, Cornell University

There are two broad contributions in Ja Ian Chong's important new book *External Intervention and the Politics of State Formation*. For historians, Chong offers an interpretation of state development in early modern China in which the interactions of foreign powers determined the course of state formation. For political scientists, Chong provides a theory of state formation in the global periphery and conceptualizes multiple possible state forms that vary across the dimensions of territorial exclusivity, external autonomy, and political centralization. The argument is comparative in scope and theoretically nuanced, but also grounded in extensive historical research. It is a welcome interdisciplinary contribution that shows how the disciplines of history and political science can inform one another to make sense of truly foundational questions about the origins of the modern state system.

Chong's contribution is also interesting, not least because it rests on some provocative claims that challenge nationalist narratives in China, Indonesia, and Thailand. It powerfully undercuts narratives about each state's founding moments by asking—appropriately—why nationalist state-builders adopted a common state form rooted in the Westphalian tradition. The answer, for Chong, is that nationalist state-builders were not alone in building their states. In the Thai case, Chong highlights the roles that the British and French played in providing the core functions of the Siamese state. In the Indonesian case, the Dutch are portrayed as having been driven out of the Indies not by Republican forces, but by U.S. pressure. In the Chinese case, the centerpiece of the book, Chong inverts the standard narrative of external interference *weakening* the Chinese state by arguing that the key to maintaining the *coherence* of an exceedingly fragile polity was external powers' joint unwillingness to intervene more decisively in Chinese affairs. Scholars of Chinese political and diplomatic history in the nineteenth and twentieth centuries will be better placed than this reviewer to evaluate the strength of Chong's case on empirical grounds. But together, these claims make Chong's argument about the fundamentally international politics of state formation a challenge to "autonomous history" approaches to the origins of modern states.[1]

Chong's conceptual architecture raises some questions from a political science perspective. A core element of Chong's theoretical argument is the concept of "opportunity costs." Opportunity costs—or more precisely, anticipated or expected opportunity costs—are the variables that shape the decisions of external powers deciding whether and how to intervene in a given territory. High opportunity costs dissuade states from intervening, while low opportunity costs encourage intervention. Some confusion about this concept is avoided upon realizing that Chong's use of opportunity costs does not follow the standard notion of 'opportunity costs' in economics to denote the implicit costs that are associated with taking one action and not another. What Chong means, at root, is simply 'costs.' The anticipated costs of intervention are subjective, of course, but they also interact with one another, such that the costs of intervening in China for Britain after 1914 would have been

[1] I borrow the term "autonomous history" from John R.W. Smail, "On the Possibility of an Autonomous History of Modern Southeast Asia," *Journal of Southeast Asian History* 2 (1961), 72-102.

lower had the anticipated costs for the Japanese been any higher. This raises a host of theoretical questions about how states conceptualize their choice sets and the costs associated with them when they understand that their decisions are interdependent. Chong is refreshingly forthright about the difficulties in gauging states' understandings of subjective costs of intervention using historical material, but it is likely even harder than he acknowledges. Game theoretic representations of such interdependent non-cooperative interactions often emphasize the importance of bluffs, signals, and costly irreversible actions. In this application, states would be expected to understand that they can change one another's anticipated costs of intervention through their own strategies of communication.

Conceptually, the argument lacks a clear link between what Chong terms "the aggregated pattern of foreign intervention" and the state form that obtains. Table 2.2 lists four possible outcomes based on four possible aggregated patterns: subordinated statehood/nonstatehood, sovereign statehood, feudalized/fragmented statehood, and state disintegration. However, the typology of eight state forms in Figure 1.1 includes sovereign statehood, conquered territory, and six other alternatives. Still other state forms are possible. Is 'nonstatehood' or 'state disintegration' the same as 'conquered territory?' Does 'subordinated statehood' correspond to 'colonial state,' 'feudal state,' or 'vassal state'? Are there multiple pathways to the same state form? A fuller theoretical treatment would have provided links between external intervention and the range of potential state forms that might obtain. This is not to criticize Chong's reasonable decision to focus on the rise of sovereign statehood as a particularly interesting state form. However, given that most aggregate patterns of foreign intervention are predicted to produce something other than sovereign statehood, and the importance of conceptualizing sovereign states as entities that maximize all three dimensions of stateness, a tighter fit between concepts and predictions would clarify matters.

Like many theoretical arguments in the field of international relations, Chong's argument is pitched at a very general level. For a non-specialist, the analysis of the Chinese case is compelling, but it is harder to find wider application, and the Indonesian case illustrates why. The basic contours of this case are not in dispute. After the Japanese surrendered in 1945, Dutch forces effectively regained control over most of the Indies after a series of victories over republican forces. By the late 1940s, though, significant U.S. pressure on the Dutch forced them to accept Indonesian independence.

But the purpose of Chong's argument is to show why the result of this process was a sovereign state in Indonesia *rather than something else* like a vassal state or a colony. And if we faithfully apply Chong's argument, we are left with the intriguing claim that U.S. pressure on the Dutch to leave Indonesia resulted in a sovereign state in Indonesia not just because the Dutch faced high costs of intervention but also because the U.S. and other powers found it too costly to make Indonesia into a colony or something else. It is true that it would have been prohibitively costly for the Soviet Union or the United States to have colonized the Indies. However, unlike the Chinese case in the early 1900s, there is no evidence that anything other than sovereign statehood for Indonesia was ever considered, even briefly, by non-Dutch powers after 1945. Indonesia became a sovereign state after the

Dutch left because that is what former colonies were doing upon independence, and because Indonesia's independence leaders wanted it that way. (However ahistorical their understanding of sovereign statehood in the context of the pre-colonial Indonesian polities, Sukarno and other independence leaders did truly believe that a sovereign state was the only appropriate state form for an independent Indonesia.) Chong's argument is consistent with literature on the role of external powers in the collapse of the Dutch colonial regime,[2] but it does not fundamentally change the standard historical interpretation of the events.

This reveals the limits of Chong's argument. It is possible to argue that the cost variable for various foreign powers has the correct value ('high' or 'prohibitively high') for many postcolonial cases that ended up as sovereign states. But evidence of the mechanism linking that cost calculation to foreign powers' intervention strategies remains limited outside of the Chinese case. These arguments are confounded by the simple fact that the global trend towards decolonization after the Second World War meant that Britain, the United States, Japan, and other powers were not seriously entertaining colonial control as a strategy in the way that they had previously.

A different case selection strategy might have proven more fruitful to illustrate the general applicability of Chong's theory. Ideally, we would like a case in which multiple states demonstrated their willingness to intervene in the domestic affairs of a target state in the postcolonial era. The obvious cases in Asia are not colonial-era Thailand or revolutionary Indonesia, but Korea and Vietnam during the Cold War. In these two cases, we have evidence of substantial variation over time in the willingness of multiple external powers to intervene directly in each country's domestic affairs, and can use these to develop a better sense of how changing costs of foreign intervention shape state forms that ensued. We can even observe the outcomes of 'fragmented' or 'subordinated' statehood in two entirely postcolonial contexts. It would be especially interesting to see how Chong's theory would make sense of Vietnam under Ngo Dinh Diem, wrestling with the difficult question of U.S.-South Vietnamese relations that continues to generate important new research.[3] Other useful cases might include Algeria in the early 1950s, the Portuguese empire in Asia and Africa (which persisted longer into the postcolonial era than did the British and French empires), or contested territories that remain nonstates today, such as Tibet or Indonesian Papua. Fortunately, Chong has provided us with a clear enough template for comparative, cumulative research that future scholars can probe the limits of his conceptual and theoretical framework with relative ease.

[2] See, for example, George McTurnan Kahin, *Nationalism and Revolution in Indonesia* (Ithaca: Cornell University Press, 1952), esp. Chapter 13; Anthony Reid, *The Indonesian National Revolution, 1945-1950* (Hawthorn: Longman, 1974).

[3] See, recently, Edward Miller, *Misalliance: Ngo Dinh Diem, the United States, and the Fate of South Vietnam* (Cambridge: Harvard University Press, 2013).

Review by Feng Zhang, The Australian National University

Structure and Agency in the Formation of Sovereign Statehood in East Asia

Ja Ian Chong's *External Intervention and the Politics of State Formation* makes a bold and original argument: foreign intervention can foster, rather than hinder, the formation of sovereign statehood in weak polities.[1] He thus turns on its head the conventional wisdom that foreign intervention is a disruptive force for division in weak polities. His analytical focus on the effect of foreign intervention on state sovereignty offers a new perspective on the various causes of sovereign statehood. As Chong notes, the relationship between external intervention and state formation is overlooked by both international relations (IR) and comparative politics scholarship. By examining this relationship with theoretical innovation and historical depth, through three case studies of sovereign formation in China, Indonesia, and Thailand from 1893 to 1952, his book makes an important contribution to the established political science literature on state sovereignty.

The book proposes a new theoretical framework to account for the influence of foreign intervention on state forms in weak polities. The key innovation is the concept of expected costs of intervention, taken as the central variable affecting different state forms. Chong argues that sovereign statehood develops in a weak polity when foreign actors uniformly expect high costs to intervention in that polity. Unable to intervene vigorously but still hoping to prevent their worst fear of domination of that polity by a rival, these actors will try to abet a local nationalist group that seems most able to guarantee equal access to all outside actors. In this way that polity can avoid being dominated by a hostile foreign power and will presumably treat the assisting foreign actors at least as equally as it will treat all the others. This is seen as the best outcome for foreign actors expecting high costs of intervention, one that can protect their interests in the weak polity with minimal capability commitments.

Chong uses a wide range of primary and secondary sources to evaluate his argument in the case studies. The three East Asian case studies are a most welcome addition to the sovereignty literature since most existing studies have focused on state sovereignty in Europe, Africa, and Latin America. The book's historical research on the China case, buttressed by extensive primary sources, is particularly impressive. It is in fact an exemplar of how in-depth historical research on the experiences of China and East Asia can contribute to the development of IR theory[2]—in this case a theory of state formation that has gone beyond the well-researched field of Western Europe.

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[1] Ja Ian Chong, *External Intervention and the Politics of State Formation: China, Indonesia, and Thailand, 1893-1952* (Cambridge: Cambridge University Press, 2012).

[2] It thus joins a number of earlier works in this area: Alastair Iain Johnston: *Cultural Realism: Strategic Culture and Grand Strategy in Chinese History* (Princeton: Princeton University Press, 1995); Victoria Tin-bor Hui, *War and State Formation in Ancient China and Early Modern Europe* (Cambridge: Cambridge University Press, 2005); Brantly Womack, *China and Vietnam: The Politics of Asymmetry* (Cambridge: Cambridge University Press, 2006); David Kang, *East Asia Before the West: Five Centuries of Trade and Tribute*

Nevertheless, one wonders whether the book's central argument has somewhat overemphasized the role of international structure at the expense of local agency. Chong claims that "Shifts in the organisation of rule in fragile states come about from the machinations of outside actors trying to forward their interests under changing international systemic constraints" (2). How exactly can foreign intervention bring about changes in state forms in weak polities? The book's theory establishes the conditions under which foreign powers may take different approaches toward intervention in a weak polity. That is, when the expected opportunity costs of intervention are high, foreign actors will seek nonprivileged access over the polity. When moderate, they will seek access regulation over a certain area. And when low, they will seek complete access denial over the area. The theory also suggests the mechanisms through which different access strategies may lead to different state forms. Under the strategy of nonpriviledged access, foreign powers will sponsor local actors to pursue sovereign statehood. Under the strategy of access regulation or access denial, in contrast, they will foster local actors to support collaboration in a colonial project.

The theory is thus a structural theory emphasizing the role of international intervention for explaining state forms in weak polities. Chong justifies the underemphasis of local agency by arguing that the effects of local agency on state formation depend on foreign support (3). He seems to claim that there is not much that local actors can do to affect state forms without foreign sponsorship. This almost completely takes agency out of the structure-agency relationship that is in most cases necessary to explain outcomes. And one wonders whether that is a fully justifiable move. It is certainly true that "what gave nationalist groups the financial, military, and political wherewithal to persist against the challenges they faced was often patronage by foreign powers" (231). But even if foreign intervention played a disproportionally large role in the formation of sovereign statehood, the actual formation of sovereignty would require the efforts of local agents, even if with indispensable support from foreign actors.

We need a theory that can explain the mechanisms through which foreign intervention interacts with local agency to produce distinct state forms. The book suggests such mechanisms, as mentioned above. But the theory and case studies have focused heavily on the structural dynamics of the variations of foreign intervention to sideline how particular intervention worked with local agency to affect state forms. As Chong acknowledges, his analysis "concentrates on opportunity cost expectations, access denial strategies, and other considerations *leading up* to intervention efforts in a target polity by each relevant outside actor" (44, emphasis in original). So it appears that the book documents not so much how sovereign statehood formed in target polities as how foreign actors calculated their different approaches to intervention in these polities and how this affected the state formation of these polities. The theory, it seems, is actually a theory of how the structural factor of foreign intervention made possible or allowed for sovereign statehood, not of how

(New York: Columbia University Press, 2010); Yuan-kang Wang, *Harmony and War: Confucian Culture and Chinese Power Politics* (New York: Columbia University Press, 2011).

sovereign statehood actually emerged. It establishes a vital structural condition of the emergence of sovereignty, but not so much of the agential processes producing it.

This raises an additional question about the type of causal claim that the book is making. Is the book claiming that foreign intervention *caused* different state forms? Judging from the terms that the book has used to describe the relationship between foreign intervention and state forms, including "results from," "affect," "sustain," "foster," "promote," "allow for," and "sponsor," the answer seems ambiguous.[3] One may think that the book solidly establishes an essential international condition—foreign intervention—for state formation without, however, fully demonstrating how this structural condition allowed local agency to create particular state forms. Foreign intervention created permissible conditions for producing distinct state forms, but the actual development of state forms lay in the efforts of local agents.

Overemphasis on the positive influence of foreign intervention on state sovereignty sometimes obscures the multiple effects of foreign intervention on weak states. For example, Chong argues that the major outside powers enabled China to retain a relatively high degree of external autonomy between 1893 and 1922. British, American, and intermittent Japanese and Russian insistence on the autonomy of the Chinese polity allowed central governments from the Qing court to the Xu Shichang government to remain fully responsible for official external relations (73). This, however, raises a counterfactual question: would Chinese external autonomy have been substantially reduced without foreign intervention? Before foreign intervention into China took place after the mid-nineteenth century, the Qing empire too had external autonomy without being subject to any higher source of authority in its foreign relations. The question of external autonomy, it seems, largely arose with the advent of foreign intervention. It was foreign intervention that limited Chinese autonomy in the first place. And if foreign powers subsequently insisted on China's autonomy, this indicates that they did not damage Chinese sovereignty. From the perspective of these powers, they were enabling Chinese autonomy. From the Chinese perspective, however, they may be seen as doing what they ought to have been doing in their relationships with China—that is, treating China as an independent and autonomous entity in foreign affairs.

A similar example is the argument that foreign powers restricted the degree of partitioning that could undermine China's territorial exclusivity by cooperating over a limited partitioning of China between 1893 and 1922 (109-110).Yet, whatever the degree, that act itself undermined China's territorial exclusivity. It is not clear why foreign partitioning of China is seen as having safeguarded China's territorial exclusivity.

Finally, a point can be raised about the normative implications of the book. Such implications are not explicitly discussed in the book, but readers inside China steeped in official historiography may react by asking whether the book wants to drive home the

[3] Chong, *External Intervention and the Politics of State Formation: China, Indonesia and Thailand, 1893-1952,* 2, 30, 35, 105, 150, 151, 156, 171, 172.

point that foreign intervention was actually 'good' for the Chinese nation, assuming that sovereign statehood is 'good' for the Chinese polity. Chong points out that the book challenges the conventional wisdom about the effects of foreign intervention in China. In his words, "That foreign powers in effect sponsored the creation of the sovereign Chinese state contrasts understandings that pit the Chinese nation against outside forces and see external involvement as a driver of division" (172). This is a statement likely to stir some controversy inside China. 'Sponsor' is certainly the right word, but the claim still seems somewhat too general, without differentiating between the different foreign powers and without specifying how foreign sponsorship enabled local agents inside China to strive for sovereign statehood. Some Chinese readers might ask: what then was the role of the Chinese themselves in achieving Chinese sovereign statehood? More attention to agency may reduce the room for misunderstanding.

Notwithstanding these reservations about the place of agency in the explanatory framework, I find *External Intervention and the Politics of State Formation* a hugely admirable book in its theoretical innovation, historical depth, and bold challenge to received wisdom. In terms of theory, it shows how Chinese and East Asian materials can advance IR theory building, which until recently has been based almost entirely on trans-Atlantic experiences. In this way it has enriched existing studies on sovereignty by drawing on East Asian history. Of equal importance, , it compels us to rethink the role of foreign intervention in the formation of sovereign statehood. In particular, it challenges the long-established and extremely negative Chinese view of foreign intervention in China. A Chinese translation of the book will help to stimulate a new debate and rethinking on the subject inside China.

Author's Response by Ja Ian Chong, National University of Singapore

Rethinking External Intervention and State Formation: A Response[1]

It is a rare honor for a work to receive thoughtful, sustained criticism and feedback, especially from three top scholars in the field. In the case of my book, *External Intervention and the Politics of State Formation: China, Indonesia, Thailand—1893-1952*, the three reviewers, Joseph Parent, Thomas Pepinsky, and Feng Zhang do an exceptional job of highlighting areas that I either did not address, or failed to cover sufficiently, in part due to limitations of space, time, and resources. These observations lay the ground and specify the scope for subsequent research, especially in the domains of foreign intervention, state formation, and nationalism. Let me begin by responding to common areas of concern raised by the reviews before I provide some reactions to some of the observations that are more specific to the individual reviewers.

Anticipated Opportunity Costs

Joseph Parent and Thomas Pepinsky take some issue with my use of opportunity costs, or more precisely expected or anticipated opportunity costs, to ascertain the approach an outside power takes toward a potential target for intervention. Parent argues that I engage in "too much telling and too little showing," making it difficult to verify and falsify my claims. Pepinsky observes that assessing expected opportunity costs is even more difficult than I acknowledge given that states "would be expected to understand that they can change one another's anticipated costs of intervention through their own strategies of communication."

For me, expected opportunity costs of intervention is based on the degree to which leaders of a potential intervening state anticipate net returns from investing a given set of capabilities toward denying rivals access in a target polity outweigh those from the next best goal. (33) The more leaders expect returns from denying rivals full access to a target polity to outweigh gains from the best alternative, the lower the anticipated opportunity cost of intervention and the more extensive intervention attempts are likely to be. This comes down to how leaders subjectively anticipate future net gains, and they are more than willing to accept negative net gain over extended periods to realize such returns.

Just as individuals willingly take on debt several times their assets to finance property, or companies borrow heavily for mergers and acquisitions, governments and leaders are quite ready to pursue development plans, intervention, expansion, and war through loans and bonds. Trade and investment numbers or even bases and alliances cannot fully account for expectations about gains and losses in the future. Efforts to apply prospect theory to

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[1] The author would like to thank Subashish Ray for his helpful comments. All errors are the author's own.

world politics quite clearly illustrate the difficulties of extrapolating expectations directly from concrete economic figures and strategic considerations, for instance.[2]

To address the challenges of measuring and evaluating opportunity costs, I surveyed as much of the secondary literature covering major power decisions leading up to and during intervention in a target state. I augmented these with primary evidence where possible. My discussion in the relevant chapters sought to present as accurate a summary of my findings as I could muster, and I also listed references to my sources for others to verify my measurements. Admittedly, the sheer number of intervening powers, linguistic limitations, and resource restrictions meant that my efforts in this regard could not be truly comprehensive. Given the importance of replicability and testing for establishing confidence in social science research, I welcome attempts to evaluate my efforts at assessing opportunity costs expectations.

Finding Causality

Pepinsky and Feng Zhang additionally highlight the concern that I do not sufficiently develop the causal relationship between external intervention attempts and the range of outcomes on state form. I leave readers to judge how well I make my causal case. However, I wish to make it clear that I agree with Pepinsky that multiple pathways to the same state form are possible. I do not claim to monopolize explanations about state formation. In fact, my work comes as a response to unexplained variation on state formation left by existing accounts developed by others.[3] I do not reject these perspectives as wrong, but view them as not fully addressing how cross-boundary interactions between foreign and local actors can shape state forms, especially when indigenous political institutions are weak.

A number of outcomes are possible under my formulation for state forms, but the book pays special attention to the 'sovereign state,' 'feudal state,' and 'colonial state.' Other possibilities that I do not explore in detail include 'conquered territories' completely incorporated into another entity, such as the Native American lands that became part of the

[2] See, for example, Daniel Kahneman and Jonathan Renshon, "Why Hawks Win," *Foreign Policy* (2007), Rose McDermott, *Political Psychology in International Relations* (Ann Arbor: University of Michigan Press, 2004), Rose McDermott, "Prospect Theory in International Relations: The Iranian Hostage Rescue Mission," *Political Psychology* (1992), Rose McDermott, *Risk-Taking in International Politics: Prospect Theory in American Foreign Policy* (Ann Arbor: University of Michigan Press, 1998), Ch. 2, George A. Quattrone and Amos Tversky, "Contrasting Rational and Psychological Analyses of Political Choice," *American Political Science Review*, vol. 82 no. 2 (September 1988).

[3] See, for example, Perry Anderson, *Lineages of the Absolutist State* (London: NLB, 1974), Miguel A. Centeno, *Blood and Debt: War and the Nation State in Latin America* (University Park: Pennsylvania State University Press, 2002), Jeffrey Herbst, *States and Power in Africa: Comparative Lessons in Authority and Control* (Princeton: Princeton University Press, 2000) Victoria Hui, *War and State Formation in Ancient China and Early Modern Europe* (Cambridge: Cambridge University Press, 2005), Hendrik Spruyt, *The Sovereign State and Its Competitors: An Analysis of Systems Change* (Princeton: Princeton University Press, 1994) , Joseph Strayer, *On the Medieval Origins of the Modern State* (Princeton: Princeton University Press, 1970), Charles Tilly, *Coercion, Capital, and European States, AD990-1992* (Cambridge: Blackwell, 1992).

United States or the Ryukyu Kingdom that is now part of Japan. 'State disintegration' where a polity dissolves into separate and distinct entities that each demonstrate different attributes of state-likeness is another possibility, as is seen with the former components of the Austro-Hungarian and Ottoman Empires after World War I.

I do not wish to rehash in this response each type of state form I consider in the book, but I want to note that differences in state form helped me to think through the implications of my research and serve as a reminder of the historical variety in organizing governance. I accept that state forms can never be fully discreet types and that overlaps exist. Like all conceptual typologies, my classification of state forms offers radical simplifications of reality that, at best, provide heuristic devices to think about different ways to organize politics and governance. The main point I seek to make in the book is that external intervention can play a major role in shifts in state form and that this process results from the multidimensional competition among local and domestic actors.

State formation does not have to result from the internal mobilization and reactions against external pressures or efforts at internal consolidation that do not involve actors outside some geographically-defined political boundary. The ways foreign actors contest in and over a polity can have very direct influences on how local actors approach the tasks of organizing coercion, extraction, and the distribution of benefits that are fundamental to a polity. Given the current pervasiveness of sovereign states in world politics and their historical contingency, I made explaining the development of sovereign statehood in a region where such an approach to organizing politics is relatively novel the focus of my project. The relative wealth of information on my cases was helpful as well. Looking at the development of sovereign statehood calls for investigations of the immediately preceding conditions, which means state forms such as 'colonial states' and 'feudalized states' feature more prominently.

Cases and Case Selection

Since all three reviews query the wisdom of my case selection, it is an issue I should address. Parent and Pepinsky question the appropriateness of my cases—China, Indonesia, and Thailand—for making more general points about external intervention and state formation. A point of commonality for all three cases for much of the periods I study is that they were weak states in that they had relatively modest institutionalization of governance along with high levels of domestic contestation over not only who wielded political authority but the form it should take.

Parent points to the fact that my China case is far more extensive than the Indonesia and Thai cases to highlight his discomfort with my case selection short of "more, and more precise, control factors." My rationale for case selection controls for under-institutionalization, domestic contestation, threat of invasion, nationalism, availability of loans and arms from external sources, and arguably culture, while allowing variation on configurations of external intervention. (pp. 16-24, 43-4) These aim to account for drivers of state formation that are most common in the literature while isolating the variable of interest to my theoretical explanation. I welcome the opportunity to be educated on how to more extensively and precisely control for alternative considerations in my case selection.

Consequently, my explanation is potentially applicable to other polities that have similar characteristics, such as the cases of contemporary Iraq and Afghanistan I allude to in the book. Pepinsky proposes candidate cases such as Korea and Vietnam during the Cold War, Algeria, and parts of the former Portuguese empire. Other more contemporary possibilities include Somalia and the states that emerged in the Balkans and Central Asia following the end of the Cold War. The degree to which my claims hold in such cases and the conditions that limit my explanation are empirical questions that only rigorous attempts to test my conceptual account in these different cases can more fully resolve. I invite others to test the veracity of my arguments in these instances, as well as other comparable situations.

Pepinsky further contends that the former Dutch East Indies would have become a sovereign Indonesia anyway, given that it was the inclination of the nationalist elites and the global trend toward decolonization after World War II. Zhang similarly asks whether there would be a reduction of China's external autonomy absent foreign intervention, given that external actors should be perceiving China as an "independent and autonomous entity in foreign affairs" in any case. Moreover, Zhang finds issue with my claim that foreign agreement to limit the partitioning of China helped preserve territorial exclusivity since the division of China into spheres of influence, leasehold territories, and even outright colonies occurred regardless.

I believe evidence from my case studies address these concerns. The development of sovereign statehood in Indonesia was far more contingent and uncertain going into the 1940s even if it seems inevitable in retrospect. As late as the early 1940s, it was unclear if Indonesian nationalists would prevail. Through much of the 1930s, top nationalist leaders like Soekarno, Mohammad Hatta, and Soetan Sjahrir were either in exile or in prison, and the nationalist movement was in disarray. These three top nationalist leaders were later to become the Republic of Indonesia's first President, Vice-President, and Prime Minister, respectively. Japan's domination of the archipelago between 1942 and 1945 was not in doubt, Japanese sponsorship of the Indonesian nationalists notwithstanding. Moreover, a reason for the fall in major power support for colonialism after World War II had to do with expectations about the declining returns of such projects given the need to invest in fighting the Cold War, especially in Europe. In fact, American support for the Indonesian cause spiked only in 1948 when the fact that Dutch recolonization efforts hampered reconstruction in the Netherlands and the anti-communist credentials of the Indonesian Republicans became clear to Washington. (187-8, 194-6)

Like the Indonesian case, it is easy for observers to take China's ability to exercise external autonomy for granted and to take an erosion of territorial exclusivity by outside actors as precluding the possibility of foreign support on this dimension. Japan's creation of the Manchukuo puppet state in 1933 and a subordinate Republic of China government in 1940, as well as Moscow's establishment of a Mongolian client state in 1945 indicate that outside actors were quite ready to limit Chinese external autonomy when it suited them. Just as Qing China forcefully absorbed the Dzungars in 1755 or Prussia other German states after the Franco-Prussian War, the fact that more powerful actors at times subjugated weaker ones is no surprise in world politics.[4] China's relations with other actors are not exceptional in this regard.

That said, not all world politics is about war and destruction, and actors can limit, even calibrate, the way they intervene in a target polity. Major powers active in China supported territorial exclusivity insofar as they settled on preventing the polity's disintegration. Alternative policies are an imposed division of an original polity into distinct autonomous entities as in the cases of Korea and Germany during the Cold War or territories subject to different outside powers, as with the parts of the Ottoman Empire after World War I. The point here is that since territorial exclusivity can exist in degrees and shades, so too can efforts to bolster it. Undermining territorial exclusivity is only the opposite of support for the same if territorial exclusivity is a discreet, dichotomous phenomenon, which it clearly is not.

Local Agency and Normative Concerns

Another concern for Zhang is the issue of local agency, which he sees as lacking in my accounts of actors in China. Ironically, I started research on the project expecting a much larger role for local agency, but this did not bear out either theoretically or empirically. Indeed, there are a large number of local actors in my China cases. However, until the final victory of the Chinese Communists, the acuteness of competition among these actors and their relative equality made them depend quite heavily on external assistance to make a difference domestically. The more limited sources of foreign support meant that local actors had to constantly worry about foreign backers channeling support to rivals, as happened with the Duan Qirui government and Japan in 1917. (69-70, 94-6, 107-8, 227) This gave external actors disproportionate influence in determining the nature of relations with their local partners, diminishing the effect of local agency.

Finally, Zhang questions the normative implications of suggesting that foreign intervention may have been "good" for the Chinese nation, "assuming that sovereign statehood is 'good' for the Chinese polity." This is more of an issue for those who see sovereign statehood as a normative good, and one that somehow results from local efforts. These are not assumptions that I make. I see sovereign statehood—and indeed other state forms—as means of organizing governance, arranging politics, and institutionalizing authority, which can be for normatively positive or negative ends. My agnosticism toward sovereign statehood comes from recognizing it as a highly permissive institutional form. Sovereign states can provide protection for life, liberty, and property as much as they can be vehicles for repression and great violence. In this respect, it may be a relief not to pin too much hope on the sovereign state as a normative "good."

[4] James Millward, *Eurasian Crossroads: A History of Xinjiang* (New York: Columbia University Press, 2007) p. 95, Peter Perdue, *China Marches West: The Qing Conquest of Eurasia* (Cambridge: Harvard University Press, 2005), 283-7.

Conclusion

Engaging with critical readings of my work is always a pleasure for me. It makes me think harder about what I have done. An opportunity to explicitly consider and lay out the implicit assumptions I made, clarify ideas I did not fully express, as well as go through the gaps and implications I did not fully develop is a welcome exercise. It gives me a 'second shot' at things I did not execute as well as I should in the book, and invites me to think harder about my own work. I could not have asked for more careful and rigorous reviewers than those here, who were invited by Tom Maddux of H-Diplo. I hope that in going through the range of concerns and objections Parent, Pepinsky, and Zhang raised, I am able to give readers greater insight into *External Intervention and the Politics of State Formation*, as well as my broader considerations.

An objective in my writing the book is to encourage further discussion about the varied origins of sovereign statehood and the limitations of nationalism. In this sense, the book is a small part of a larger area of academic and policy interest and reflects my own intellectual concerns. In helping to move the conversation forward along these lines, the reviews and, with some luck, this response can point to the gaps in this vein of research that continue to require attention. I am immensely grateful for the reviewers for their help, support, and intellectual generosity in providing feedback. Ultimately, only time can tell where this line of investigation will lead, if it goes anywhere at all. I am grateful to have had the opportunity to at least try and provide some sort of contribution.