H-Diplo | ISSF
Forum, No. 24 (2020)

Editors: **Robert Jervis** and **Diane Labrosse** | Production Editor: **George Fujii**

John A. Vasquez. *Contagion and War: Lessons from the First World War*. Cambridge: Cambridge University Press, 2018. ISBN: 1-108-40427-3 (paperback, $34.99).

Published on 24 January 2020

https://issforum.org/to/Forum24

Contents

Introduction by Daniel Larsen, University of Cambridge

John Vasquez's book adds to the enormous mass of writings on the outbreak and spread of the First World War, with the centenary of the outbreak of the First World War having stimulated a further raft of historical scholarship.[1] Vasquez makes a fresh contribution to the subject, but investigates it anew using the tools of political science, and asks a very different question: how do wars—in general—spread? Using the First World War as an exemplary case study, he looks individually at each pair of countries that declared war on one another, not only during the July Crisis but also in the second and third waves of countries that entered the war in 1915-1916 and 1917-1918. Vasquez draws a sharp distinction between the *outbreak* and the *spread* of war, with his work focussing only on the latter, and he treats the First World War as beginning with the outbreak of a local war between Austria-Hungary and Serbia that subsequently spread across the globe.

As a political scientist who is only tangentially interested in the historical questions that drive the other recent work on the outbreak of the war, he ends up with an altogether different kind of book. There is no intention of archival work. Rather, the goal is to use the English-language historical literature on the First World War to offer a *proposal* for explaining how wars spread. He is explicit that he is not attempting provide an answer to his main question. Rather, the next step is to take the proposal he has developed based on the First World War, and to test it for other multi-state wars, such as the Napoleonic Wars or the Second World War.

His proposal is built around a classification scheme of six specific "contagion processes": alliances, contiguity, territorial rivalry, opportunity, economic dependence, and brute force. He categorizes each declaration of war according to one or more of these six processes. These six contagion processes are accompanied by twenty-nine specific hypotheses (eleven in Chapter 1 and eighteen in the conclusion). The reviewers discuss these contagion processes in detail, so there is little purpose in duplicating that summary here. The hypotheses, meanwhile, are rather disparate and defy summarization. They are typically attached to various contagion processes or specific declarations of war, and they take a highly general form. Looking at Japan's declaration of war against Germany, for example, Vasquez offers the hypothesis that "ongoing wars can put new territorial issues on the agenda where there had been no claims before, and these become a basis for intervention" (324).

This is a work of political science, not history. There is a healthy use of the sort of jargon and quantitative measures that historians typically eschew, and the book is structured around pairs of countries and their declarations of war, rather than chronological narrative. Relying on the work of historians, rather than archival work, the book has a very limited ability to contribute to historiographical debates. Where historical consensus exists, Vasquez relies on it; where it does not exist, he selects the interpretation he finds the most convincing, and occasionally makes theoretical arguments to attempt to reinforce it.

On the July crisis itself, Vasquez sides emphatically with those historians who argue that no one actually wanted a European war, and yet they nevertheless found themselves fighting one. Of the more recent historical writings on the crisis, he appears to align most closely with Thomas Otte, who argues that Germany and Russia mutually attempted to use the spectre of war to get the other side to back down, and war began after both failed to do so.[2] But where Otte's

[1] See Thomas Otte, *July Crisis: The World's Descent into War, Summer 1914* (Cambridge: Cambridge University Press, 2014); Christopher Clark, *The Sleepwalkers: How Europe Went to War in 1914* (London: Allen Lane, 2012); Margaret MacMillan, *The War that Ended Peace: The Road to 1914* (New York: Random House, 2013); Max Hastings, *Catastrophe 1914: Europe Goes to War* (New York: Knopf, 2013); Sean McMeekin, *The Russian Origins of the First World War* (Cambridge: Harvard University Press, 2011); William Mulligan, *The Origins of the First World War* (Cambridge: Cambridge University Press, 2010); Jack Levy and John Vasquez, eds., *The Outbreak of the First World War: Structure, Politics, and Decision-Making* (Cambridge: Cambridge University Press, 2014).

[2] Otte, *July Crisis*.

argument required a lengthy book of explication, Vasquez is able to use political science analysis to offer an elegant and concise articulation of this point of view—in which one can summarize it in a sentence, and explain it in a paragraph:

Vasquez argues that the great powers played a "coercive game" that failed, leading to a "security trap" that precipitated a "hostility spiral" (95, 353-356). No state wanted a general war, but instead sought to obtain a diplomatic victory. Except for Britain, the European great powers all hugged their allies tight, playing a dangerous game in which they sought to coerce the other bloc into backing down. Russia, with French support, sought to deter Austria-Hungary from striking Serbia. Germany aimed to deter Russia from getting involved, so that Austria-Hungary could humiliate Serbia without outside interference. But these mutual efforts at deterrence failed: neither side backed down. The failure of the game led to a "security trap": military mobilization, originally conceived of as a way to win the coercive game, caused both sides to "perceive an extreme threat due to the fear of imminent attack" (315). This intense fear set off a "hostility spiral," (356) as both sides abandoned the coercive game and their efforts to win a diplomatic victory. Instead, they went to war to prevent a military defeat, fearing that any delay would allow the other side to gain an irreversible military advantage.

The two reviewers in this roundtable come from very different perspectives. One is a political scientist, and the other is a historian. Perhaps reflecting their respective disciplines, the political scientist, Jack Levy, is wholly enthusiastic with some modest constructive criticism, while the historian, Talbot Imlay, is more ambivalent.

Levy is a political scientist who focusses on war and its causes, although his work has often taken on a significant historical dimension. He is also a past collaborator with Vasquez, with the two having co-edited a volume on the outbreak of the First World War, which was published for the centenary in 2014.[3] He is fulsome in his praise for Vasquez's book, which he calls "by far the best comprehensive treatment of the spread of the war." He gives a robust summary of Vasquez's six contagion processes and finds them a wholly persuasive theoretical model for explaining how wars spread. He does wonder, however, if it might be best to combine two of those processes into a single process—questioning whether "territorial rivalry" and "opportunity" are actually sufficiently different as to merit distinct categories.

Levy also takes issue with a few points concerning Vasquez's treatment of the July Crisis. Levy questions whether the summary of the crisis as a "coercive game" is historically accurate: not until the last two days of July 1914, he argues, did Germany and Russia start engaging in the kind of coercive bargaining that could be called a "coercive game." Nor does he think that the coercive game, on which Vasquez lays much emphasis, was as important as the alliance structure. Levy also questions whether the distinction between the *outbreak* of the war and its *spread* that lies at the heart of the book is actually valid. Depicting the war as one that started as a local war and then spread through contagion seems a bit dubious: Austria-Hungary, after all, would never have started the local war absent Germany's "blank cheque" offer of protection against Russia. Rather, the whole of the "First Wave" in 1914 is more properly treated as the outbreak of war. This is not, however, a major objection—it merely makes Vasquez's book an important contribution both to the causes as well as to the spread of war, and invites further analysis of the interdependence between the two.

Imlay is predominantly a historian of twentieth century European Socialism and of the Second World War, but has also published a significant review article in the *Historical Journal* on the outbreak of the First World War.[4] He frankly acknowledges that, as a historian, he found the book difficult to review. He notes it suffers from many of the problems that historians often identify in work done by Anglophone international relations scholars. Considering that the work engages with a significant number of more obscure pairs of countries that are little treated in the English-language historical literature, the absence of any multi-lingual work in the secondary literature adds considerable uncertainty to his findings. Additionally, he suggests that Vasquez never really engages with the historiographical debates over why a

[3] Levy and Vasquez, *Outbreak of the First World War*.

[4] "The Origins of the First World War," *Historical Journal* 49 (2006): 1253-1271.

particular country entered the war. He sometimes leaves readers with the impression that historians have reached consensus when they have not, or when he does note the existence of a debate, Vasquez merely selects one side of the argument, without actually making the case for it.

At the same time, however, Imlay is clear that historians must not fall into the trap of merely dismissing political science as bad history, but rather should engage with it seriously on its own terms. He welcomes further exploration of Vasquez's contagion processes in the French Revolutionary and Napoleonic Wars, as well as their application to the present, such as in a potential conflict in the South China Sea. He praises the clarity of the book's research design, its emphasis on causal mechanisms, its boldness in engaging with an extremely ambitious research question, and the balance it strikes between agency and structure. But he also challenges Vasquez's emphasis on pairs of countries, which has both benefits and costs, and wishes Vasquez had been franker about the latter.

In his response, Vasquez acknowledges the limitations on his research that Imlay mentions. He notes that he has read significant amounts of published and translated primary documents, but admits that there may be important works not in English that might have changed his analysis, especially in some of the more obscure country pairs that he writes about. He also suggests that further Russian and German archival work, centred on his idea of a "security trap," might help to more firmly resolve historiographical debates between those who fix a special German or Russian responsibility for the war, and those like himself, Thomas Otte, and Christopher Clark, who believe that neither side wanted war. At the same time, however, he notes that political scientists are not historians. It is for diplomatic historians to establish the facts, and the role of political scientists is to stand atop the work of historians to examine larger patterns.

Vasquez vigorously defends his work against Levy's criticisms about the July Crisis. He stoutly defends ranking coercive bargaining ahead of the alliance structure in the outbreak of the war. He also thinks it is entirely reasonable to portray the crisis as a local war that spread through contagion into a general one: many people at the time thought a likely outcome of the crisis would be a Third Balkan War. Separating the local from the general war, therefore, allows us to better appreciate diplomatic agency as it was set within the alliance structure, which helps to explain why that outcome did not happen.

Contagion and War is an important theoretical contribution to our understanding of how wars spread, but it is only a first step. Much work remains to determine if the categories and hypotheses it proposes are more universally applicable beyond the First World War. But he has given many people much food for thought: First World War historians, political scientists who study war and its causes, and policymakers as they manage often tense diplomatic relationships throughout the world.

Participants:

John A. Vasquez is the Mackie Scholar in International Relations at the University of Illinois, Urbana-Champaign. He has published twenty books, including *The War Puzzle* (Cambridge University Press, 1993)*; The Power of Power Politics* (Cambridge University Press, 1998)*; The Steps to War* (with Paul Senese; Princeton University Press, 2008); *Territory, War, and Peace* (with Marie Henehan; Routledge, 2011), and *In Search of Theory* (with Richard Mansbach; Columbia University Press, 1981). His edited books include *What Do We Know about War?* (Rowman and Littlefield, 2012), *Conflict, War, and Peace: An Introduction to Scientific Research* (with Sara M. Mitchell; CQ Press, 2014), and *Classics of International Relations* (Prentice-Hall, 1996). He has over forty-five articles in major journals. He has been President of the Peace Science Society (International) and the International Studies Association. He is currently working on a book on "War and Diplomacy: Lessons for World Politics."

Daniel Larsen holds a fixed-term University Lectureship in International Relations at the University of Cambridge, and a fixed-term College Lectureship in History at Trinity College, Cambridge. He previously held a Junior Research Fellowship at Trinity College, Cambridge, and he received his Ph.D. from Christ's College, Cambridge. His first book, re-examining American mediation diplomacy and British foreign policy in the First World War, is forthcoming with

Cambridge University Press, and he has published a number of journal articles in *Diplomatic History*, *Intelligence and National Security*, and the *International History Review***.**

Talbot Imlay teaches in the history department at the Université Laval in Québec (Canada). His most recent book is *The Practice of Socialist Internationalism: European Socialists and International Politics, 1914-1960* (Oxford: Oxford University Press, 2018). He is currently working on a book project on Clarence Streit and Atlanticist political currents in the United States during and after the Second World War.

Jack S. Levy is Board of Governors' Professor of Political Science at Rutgers University and Adjunct 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), and *The Outbreak of the First World War: Structure, Politics, and Decision-Making* (2014, co-edited with John A. Vasquez).

Review by Talbot Imlay, Université Laval

John Vasquez, a prominent and prolific international relations scholar, is perhaps best known for his book, *The War Puzzle*.[1] First published in 1993, the book offered a systematic inquiry into the origins of major inter-state war with the aim of working towards a "scientific explanation" of the subject. Analyzing existing empirical work principally in political science, it considered a variety of "underlying" and "proximate" causes that together forged a "path to war." While different paths led to different types of wars, Vasquez believed he had identified a path that explained major war in the "modern global system" dating from 1495.[2] The critical underlying cause was the existence of territorial disputes, particularly between contiguous states, although the interaction of an array of proximate causes (domestic politics, alliances, arms races, etc.) would determine whether the path ended in war. *The War Puzzle* belonged to a larger endeavour to study war scientifically in order not simply to understand the phenomenon in all its complexity, but ideally also to limit and even prevent its occurrence.

Vasquez's recent book, *Contagion and War*, constitutes another chapter in this research program, with the focus now on the expansion of war to include ever more belligerent countries rather than on its origins. Also unlike *The War Puzzle*, Vasquez in this book confines his study to the First World War, proposing that the contagion dynamics at work in this conflict possess a wider pertinence.

I admit that I found this book difficult to evaluate. I am not an international relations scholar by training or by practice; and though I try to keep somewhat informed of developments in this massive field (or perhaps more accurately to limit my ignorance), my perspective is that of an outsider. And this position no doubt colors my understanding of the book and hence my comments in this review.

Vasquez begins by justifying his choice of contagion as opposed to diffusion to describe the process by which the list of belligerent states lengthened between 1914 and 1918. He is well aware of the medical-biological uses, not to mention the negative connotations, of the term, but nevertheless prefers contagion because, it appears, of the importance accorded to geography (and geographic contiguity) in the book—as in *The War Puzzle*. In any case, Vasquez is more interested in the "contagion processes" (14) or "mechanisms" (40) by which war spreads, identifying several of them: alliances though formal commitments; alliances through "valence balancing" in which the enemy of my enemy becomes my own enemy; geographic contiguity through location (Belgium was in the way of Germany); geographic contiguity through alliances formed with the aim of forcing a two-front war on an opponent; geographic contiguity through time pressure (when faced with a neighbour with an imposing army, the pressure mounts to strike quickly); opportunity presented by the weakening of a rival; opportunity presented by the breakdown of the rules and norms governing international politics; economic dependence (one state becomes dependent on another state or one state becomes convinced that widening the war is the only way to alter the existing economic balance); brute force to compel a state to enter the war on one's side or, presumably if it is weak, to enter on the enemy's side; and of course territorial rivalry. Given the multiple mechanisms potentially at work, to say nothing of their

[1] All citations are from the updated version. See John A. Vasquez, *The War Puzzle Revisited* (Cambridge: Cambridge University Press, 2009).

[2] Vasquez, *The War Puzzle Revisited*, 4-7.

interactive effects, one might think that any great-power war that did not end quickly in decisive victory was bound to spread.

Having identified multiple "contagion processes," Vasquez launches into an extended examination of how they operated in a variety of case studies. For the most part, the emphasis is on high politics and decision-making: what led national leaders to decide to enter the war between 1914 and 1918. He outlines two principal findings. One is that contagion worked in three waves: an initial wave in 1914 that brought in the major European powers as well as some other states (Japan, Serbia); a second wave during 1915-1916 that brought in smaller European powers (Italy, Bulgaria, Romania); and a third wave in 1917 that brought in the United States most notably but also some South American and Scandinavian countries. The second major finding consist of eighteen hypotheses, presented in the final chapter, that are spun from his discussion of the different "contagion processes" during the First World War. Vasquez finishes with a call to other scholars to investigate further whether these hypotheses hold for other wars.

This call for further research obviously raises the question of the wider applicability of his findings, which in turn is related to the question of the representativeness of the First World War. Vasquez appears confident that research into other great-power wars will confirm at least some of his hypotheses, even if he remarks that the intrinsic interest of the First World War means that *Contagion and War* is not dependent on this being so. There are at least two ways to consider the matter. One way is look at past great-power wars to see how useful contagion is as a concept. If we take nineteenth-century great-power wars, the answer seems mixed. If the German Wars of Unification were too rapid for the "contagion processes" to have had much effect, the Crimean War presents a more promising case. Piedmont-Sardinia did enter the war for opportunistic reasons, and if the conflict had continued, Austria, and even Prussia, would likely have also been drawn in. But perhaps a better case is that of French Revolutionary and Napoleonic Wars, although here some problems arise. Are these wars best thought of as one long conflict or as a series of successive wars from the first coalition (1792-1797) to the seventh coalition (1815)? With states entering and leaving the successive coalitions, one might argue that contagion existed alongside de-contagion. In any case, it seems worthwhile to explore whether similar "contagion processes" were in effect from 1792 to 1815 and from 1914 to 1918.

The second way to consider the question of the book's wider applicability is to look at the present and the future. Even if it is always imprudent to say never again, great-power wars on the model of the First and Second World Wars (or the French Revolutionary and Napoleonic Wars) are for the moment difficult to imagine. One might, of course, point to a possible war between the United States and China, and there appear to be plenty of scholars and talking-heads eager to speculate on this possibility. And yet the United States' overwhelming military superiority, which, based on current military spending levels, promises to perdure well into the future, suggests that Chinese leaders would have to be extremely risk inclined to accept war. Of course, a conflict by inadvertence cannot be ruled out, but in this case would not Chinese leaders hesitate to escalate hostilities beyond clear limits? That said, it is arguably worth considering the extent to which Vasquez's "contagion processes" might be present in a Sino-American war as well as how they might influence such a war.

But perhaps the more important question is the extent to which Vasquez's war contagion is pertinent to non-great power conflict, of which there is a depressing abundance today: civil wars, proxy wars and/or asymmetric wars of various types. I don't have an answer, although here again the question of which if any "contagion processes" are operating seems worth posing. To take the American-led war on terror, a variety of states have signed up in one way or another for a variety of reasons; and a systematic study of why some did so and others did not would no doubt reveal much about the dynamics of international politics. In other cases, however, contagion might not be the best way to think about the processes at work, if only because the presence of non-state actors in many conflicts fits awkwardly with Vasquez's state-centric framework.

Returning to *Contagion and War*, in examining the working of multiple "processes of contagion" during the First World War, Vasquez opts for a case study approach centred on dyadic analysis. Basically, each case of a state entering the war (over 40 in total) is treated as the result of a bilateral interaction. Vasquez recognizes that this approach might appear odd for a conflict waged, in part at least, by alliance blocs. But he insists that the analytical benefits outweigh the costs, especially in terms of pinpointing the particular factors at work fueling contagion processes. I am not sure what to make of this argument. I suspect that something important is lost, for example, in considering Greece's entry into the war in 1917 in terms of a series of separate, albeit interconnected, interactions with Austria-Hungary, Germany, Bulgaria and the Ottoman Empire. No less odd is the fact that, in theory at least, Vasquez's dyadic approach treats one dyad the same as any other: it is one more decision to analyze. Thus Italy's entry (or entries against Austria-Hungary, the Ottoman Empire and later Germany) into the war is set on par with that of Bulgaria, or Brazil or ever Liberia. Yet surely some states matter more than others. In any event, more discussion of the costs and not simply the gains of Vasquez's dyadic approach would be welcome.

One advantage of Vasquez's approach, at least for historians, is the emphasis on qualitative analysis. In chapters 3 through 5, Vasquez examines the dyadic interrelations in several cases, many of which are little known: Germany/Portugal, Japan/Germany, Rumania/Austria-Hungary, and Brazil/Germany, for instance. For each case, he presents a well-structured and readable narrative based on English-language secondary sources. Yet, here again, there are questions. One concerns the use of sources. Historians often criticize Anglophone political scientists for limiting their reading to English-language sources. By itself, this criticism can be unfair: few scholars of international history are able to work in all the relevant languages. Also, it is not enough to remark that Vasquez does not consult Russian sources, for example; one also needs to indicate how doing so would alter one of his narrative case studies or contradict his conclusions. That said, given the limited attention English-language scholars have devoted to some cases (Brazil, Bulgaria, Montenegro, etc.), an exclusive reliance on English-language sources undercuts confidence in the findings. But arguably a greater problem stems from Vasquez's tendency to assume the existence of a consensus among historians on highly contested questions—a tendency that is all too common in international relations scholarship. Take the case of the United States' entry into the war. Vasquez portrays President Woodrow Wilson as reluctant to enter the conflict, doing all he could to avoid this outcome. Everything changed, however, with the German decision in early 1917 to try to starve Britain into defeat. "The key factor", Vasquez writes, "that led to the U.S. entering the war was the German decision on January 9, 1917 to resume unrestricted submarine warfare" (230). In some ways, this is obviously true: American entry would not have occurred, or at least not at that time, without this German decision. Yet the reasons motivating Wilson (and Vasquez adopts a very Wilson-centric view) in 1916-1917 are the subject of considerable scholarly debate. Vasquez relies heavily on John Milton Cooper Jr., who is a leading Wilson scholar. But a different Wilson emerges from say Adam Tooze's study, *The Deluge*; and still another Wilson from Trygve Throntveit's *Power without Victory*.[3] The point is not to argue that Vasquez is wrong or Tooze or Throntveit are right, but rather that consensus on the

[3] John Milton Cooper, The United States" in Richard F. Hamilton and Holger H. Herwig, eds., *The Origins of World War* I (Cambridge: Cambridge University Press, 2003), 415-442; Adam Tooze, *The Deluge: The Great War, America and the Remaking of the Global Order, 1916-1931* (London: Penguin, 2015); and Trygve Throntveit, *Power Without Victory: Woodrow Wilson and the American Internationalist Experiment* (Chicago: University of Chicago Press, 2017).

question of why the United States entered the war is likely to remain elusive. Of course, Vasquez, like any scholar, is free to decide which interpretation—or which Wilson—he finds the most persuasive. But some recognition that this is a choice would also be welcome.

Given that Jack Levy, another prominent international relations scholar, is also writing a review for H-Diplo, I will end with a few comments on the book's potential interest for historians, even though I do not pretend to speak for the latter as a group. All too often, I think, historians criticize political science scholarship for being bad history—too schematic, insufficiently rooted in primary sources, limited to English-language sources, etc. Such criticism no doubt reaffirms the confidence of historians in their own methods and craft. The problem, though, is that political scientists are not historians. Criticizing them on these grounds simply closes off any possibility of dialogue. This is not to say that historians need to think like political scientists, let alone do political science. Rather, historians, I would suggest, should take an opportunistic view of political science scholarship (and any other non-history scholarship for that matter), asking themselves how can it be useful to their own research projects. But this requires making a good-faith attempt to understand what political scientists do.

If I apply this perspective to *Contagion and War*, I find several elements to be useful. One, already mentioned, is the series of historical narratives on the decision of various states to enter the war. Another concerns research design, in which Vasquez's study, like the work of many political scientists, offers something of a model in its clarity. There is the identification of a phenomenon (contagion in war); the explication of causal mechanisms (how factors interact to produce an outcome); and the ambition to ask big questions, however tentative the answers arrived at may be. A final and related element concerns one of the big questions Vasquez's asks indirectly—that of the balance between agent and structure. Although the analysis is focused on high-level decision-making, which would seem to favour agency, Vasquez leaves room for more structural factors (not least that of geographic contiguity). For historians, it might be worth exploring how the concept of contagion, which works through contact between individuals (or individual states) but is also influenced by environmental factors, offers an intriguing framework for integrating structure and agency.[4]

[4] I am indebted to William Mulligan (University College Dublin) for this point.

Review by Jack S. Levy, Rutgers University

The theoretical literature on the expansion or spread of war is underdeveloped relative to that on the outbreak of war. The same can be said about work by both historians and political scientists on the spread and outbreak of particular wars, including the First World War. There is an older literature on the contagion or diffusion of war, but its focus is primarily correlational, with little attention to the causal mechanisms through which wars expand.[1] This leaves a significant gap in our theoretical understanding of war, and thereby limits our understanding of some of history's most consequential and destructive wars. In developing a theoretical framework for the study of contagion and in applying that framework to the First World War, John Vasquez takes a major step forward in filling this void. In terms of its systematic, comprehensive, and detailed treatment of the spread of the First World War, *Contagion and War* is unparalleled.

The book follows a clear organizational structure. After a useful discussion of contagion and related concepts,[2] Vasquez identifies a number of analytically distinct causal paths through which a dyadic war can expand to draw in additional belligerent states. He then uses these hypothesized processes of contagion to guide detailed historical narratives of the entry of each state into the First World War (and the non-entry of neutrals), from the outbreak of the war between Austria-Hungary and Serbia in 1914 to the entry of the United States and Greece in 1917.[3] In the process, Vasquez identifies common patterns across cases and new factors of unexpected causal importance, which he uses to refine his hypotheses and develop new ones.[4]

Vasquez develops his initial theoretical propositions, or "*ex ante* theoretical expectations" (1), in chapter 1. These propositions build on Vasquez's earlier work on the expansion of war, the role of territorial disputes in international conflict, his 'steps-to-war' model, and his collaborative 'ConflictSpace' research program.[5] These propositions also build on a wide range of empirical findings on war and peace, based on Vasquez's unrivaled knowledge of the quantitative-empirical literature on war. He identifies six 'contagious processes' or 'models' of contagion, which are essentially distinct

[1] For an excellent review of the diffusion literature, see Benjamin A. Most, Harvey Starr, and Randolph M. Siverson, "The Logic and Study of Diffusion of International Conflict," in Manus I. Midlarsky, ed., *Handbook of War Studies* (London: Unwin-Hyman, 1989), chap. 5). For a more recent attempt to explain the expansion of war by focusing on the revelation of unexpected information during wars, see Zachary C. Shirkey, *Is This a Private Fight or Can Anybody Join?* (Burlington: Ashgate, 2009).

[2] Vasquez defines contagion as the "spread of war from one set of parties to include new ones" (9). He prefers the term contagion to diffusion because of contagion's negative connotations and association with medical conceptions of infectious disease. It is useful to distinguish between the horizontal *expansion* or spread of war and the vertical *escalation* of an existing war between current belligerents, the latter in terms of means, goals, and other characteristics.

[3] Vasquez defines neutrality as a formal (declared) condition of "abstaining from active fighting in an ongoing war and not providing substantial support favoring one side over the other" (272). By including neutrals, Vasquez can explore whether the hypothesized causes of contagion are absent and whether other factors are present, providing a useful comparison with cases of contagion.

[4] This organizational structure parallels the logic of theory construction, cases study analysis, and theory reformulation first emphasized by Alexander L. George and Richard Smoke, *Deterrence in American Foreign Policy* (New York: Columbia University Press, 1974).

[5] John A. Vasquez, *The War Puzzle Revisited* (New York: Cambridge University Press, 2009), chap. 7; Paul D. Senese and John A. Vasquez, *The Steps to War: An Empirical Study* (Princeton: Princeton University Press, 2008); John A. Vasquez and Marie T. Henehan, *Territory, War, and Peace*. London: Routledge, 2011). On the ConflictSpace framework, see John A. Vasquez, Paul F. Diehl, Colin Flint, Jürgen Scheffran, Sang-hyun Chi, and Toby J. Rider, "The ConflictSpace of Cataclysm: The International System and the Spread of War 1914-1917," *Foreign Policy Analysis* 7:2 (April 2011): 143-68.

causal paths or mechanisms leading to the expansion of war.[6] These processes involve alliances, contiguity, territorial rivalry, opportunity, economic dependence, and brute force. Vasquez describes these processes in considerable detail, and develops more specific hypotheses on the conditions under which these factors have the greatest causal weight and on the way they interact with other causal factors. He elaborates further as he applies these models to the various cases of contagion in the First World War. Here I summarize briefly.

Alliances can lead to the spread of war through two different paths. One is by structuring commitments that shape the "coercion game" (14), in which allies of two belligerents each use threat and intimidation in an attempt to coerce the other into backing down from supporting its ally involved in war. Although coercive threats sometimes work, they occasionally lead to threats and counter-threats through a 'contagiousness of coercion,' drawing each ally into the war.[7] The Germany-Russia dyad of 1914 is one example. Alliances can also lead to the spread of war through "valence balancing" (18), based on the related logics that the enemy of my friend is my enemy and that the enemy of my enemy is my friend (18-23).[8] Here a state enters a war, or at least formally declares war, despite having no significant grievances with the enemy, no contiguous borders, and often no expectations of engaging the other side on the battlefield. Nevertheless, it often enters the war "without hesitation and without much deliberation" (153). Its aim is simply to support its ally. Vasquez emphasizes the psychological as well as strategic component of valence balancing, based on the emotional discomfort of not opposing a friend's enemy. A long list of dyads entered the First World War through valence balancing, including Serbia-Germany, where neither had significant grievances with each other before the war.

Contiguity is a second path through which wars can spread. A state can be drawn into war because it is "in the way" of a belligerent state's military operations, as Belgium was in 1914. This pattern of contagion interacts with alliance structure, and in particular the tendency for alliances to display a checkerboard pattern. Contiguity can also spread war by creating time pressure for military action, especially if military technology and geography create a first-mover advantage that favors preemption. This was true for both Germany and Russia in 1914.[9] One interesting implication of this, as Vasquez emphasizes, is that this time pressure can create a split within the elite. Military organizations tend to have shorter-term time horizons and focus primarily on maximizing the chances for military victory, while civilian decision-makers typically have long-term time horizons and prioritize crisis management and the avoidance of war. He points to Germany and Russia in the last few days of the crisis.

A third process of war contagion involves territorial rivalry, defined as a situation in which "states are focused primarily on territorial claims and disputes" (27). Vasquez defines rivalry as "a relationship characterized by extreme competition," but differs from some rivalry scholars by emphasizing that the competition usually involves "psychological hostility," which sometimes leads states to "deviate from strict cost-benefit analysis" (27). Vasquez points to the territorial rivalry between Serbia and Austria-Hungary as the one that "started the First World War," and also to those between Italy and Austria-Hungary and between other Balkan states. Vasquez emphasizes that these long-standing territorial disputes do not themselves cause war, because the states involved were not in a position to fight on their own. What the existing war

[6] Students of Comparative Politics give more attention to causal paths (or pathways) than do students of International Relations. For my view, see Jack S. Levy, "The 'Paths-to-War' Concept," in John A. Vasquez, ed., *What Do We Know about War*, 2nd ed. (Lanham: Rowman & Littlefield, 2012), 281-90.

[7] The term is from Stuart A. Bremer, "The Contagiousness of Coercion: The Spread of Serious International Disputes, 1900-1976," *International Interactions* 9:1 (1982): 29-55.

[8] See also the analysis of "Alignments and Consistency" in Robert Jervis, *System Effects: Complexity in Political and Social Life* (Princeton: Princeton University Press, 1997), chap. 6.

[9] Jack Snyder, *The Ideology of the Offensive: Military Decision Making and the Disasters of 1914* (Ithaca: Cornell University Press, 1984), chap. 4-7.

does, however, is "remove an obstacle" that blocked war in the past (43), and create an "opportunity to fight for them *now*" (29).

The mechanisms involved with the territorial rivalry model overlap with the next one, 'opportunity.' Here Vasquez focuses on "how ongoing events remove obstacles" (30) that states face in using military force to achieve their objectives. Vasquez identifies two sets of obstacles, one material and one more ideational. First, if a state has an ongoing dispute with a militarily stronger adversary, the adversary's involvement in a war elsewhere effectively reduces its military capability and creates an opportunity for the first state to advance its objectives through military force. This contagious process helps explain Japan's attack against Germany's colonies in China and the western Pacific within three weeks of the start of the war in August 1914. Second, an ongoing war may contribute to the "breakdown of the political order" (32) and the dissolution of norms against violence, including great power restraints against the use of force by minor powers to advance their political interests. Vasquez places Italy-Austria-Hungary and two other Balkans cases in this category.

A fifth war contagion process involves economic dependence. In a war between two states, if one is assisted in its military effort by trade with a neutral third state, then its opponent in war may attempt to eliminate or minimize the problem. It may either attack the neutral trading partner directly or attempt to interfere with the trade (through blockade, for example), which can trigger a chain of events drawing the neutral third state into the war. The U.S.-Germany case in 1916-1917 is a classic example, given the U.S. contribution to Britain's military effort against Germany. In a second variation of the economic dependence mechanism, a third party that is economically invested with one belligerent in an ongoing war, especially through the extension of credit, may intervene in the war to tip the scales in favor of its economic partner, for fear that it would suffer intolerable economic losses should its economic partner lose the war. Some argue that extensive American loans to Britain, along with political pressures from domestic banks making those loans, was an important cause of U.S. entry into the First World War.[10] This argument is discounted by most historians, and Vasquez argues that this second path involving economic dependence is less likely to occur than the first.

The sixth path to spreading war involves what Vasquez calls "brute force" (39-40). In this pattern, one belligerent state, after failing to persuade a neutral third state to join the war on its side, intervenes militarily in that state in order to force a change in policy. This pattern is most likely to occur if the third party is split internally, with outside intervention helping to bring to power a faction supporting intervention in the ongoing war. Vasquez's one example is Greece's entry into the war against the Central Powers in 1917, after an Anglo-French military intervention in the politically divided country triggered a partial coup d'état that ended the Greek stalemate over intervention.

After describing the six contagion processes, Vasquez provides an excellent chapter describing the research design through which he applies his models of contagion to the complex case of the First World War. Although the book's comprehensive treatment of all of the states entering the war and its extraordinary attention to historical detail should be of great interest to historians, Vasquez makes it clear that "this is a work of political science rather than history" (52). He notes his reliance on the rich secondary literature on the war rather than archival sources, concedes that he has not provided new "facts," and goes on to say that "The contribution is not in elaborating the historical record, but in the application of new concepts and a theoretical framework for explaining aspects of that record" (52). Vasquez's case studies of the First World War provide a test of the validity of his propositions in that historical case, but he does not claim that his propositions are generalizable to other cases. He emphasizes that *Contagion and War* is an exercise in theory construction, not theory testing, and that his original as well as his revised theoretical propositions must be regarded as "untested hypotheses" that must be tested in other cases. This discussion of the epistemic status of the book is a good example of the methodological self-consciousness that runs throughout the chapter and in fact the entire book.

[10] Charles Callan Tansill, *America Goes to War* (Boston: Little Brown, 1938).

Vasquez goes on to justify his focus on the First World War as an "exemplar" of war contagion (47), noting that for theory construction, as opposed to theory testing, there are few clear rules for case selection. He lays out the specific theoretical questions that will guide each of the case studies, and explains the benefits of focusing on the dyad as the unit of analysis.[11] Vasquez argues that the entry of new states into a war is best understood by focusing on the decision-making processes of political leaders, and that this is best accomplished through process tracing in narrative case studies. It is important to note, however, that the case-study oriented *Contagion and War* is part of Vasquez's larger, multi-method study of the spread of war, as he is engaged in a separate collaborative project that involves a large-N network analysis.[12]

Although *Contagion and War* adopts a case study approach, the case studies of each dyad and how they entered the First World War are far from traditional narratives. Each case study is supported by quantitative descriptions of the history of militarized interstate disputes, alliances, arms races, territorial claims, and rivalry involving the dyad.[13] This data is presented in the form of histograms and text boxes (tables). Vasquez provides a detailed discussion of his data sources, along with a guide for reading the histograms and text boxes. This provides extremely useful background information on structural variables and historical context that might otherwise be marginalized in a case study approach that focuses on decision-making.

The case studies themselves are divided into "The Local War and the First Wave" in 1914, "The Second Wave" in 1915-1916, and "The Third Wave" in 1917, and organized by theoretical category within each wave. They are closely guided by Vasquez's theoretical framework and supported by impressive historical detail and extensive references to the secondary historical literature. In the final chapter, on "How Contagion Actually Worked," Vasquez provides a useful summary of contagion processes in each dyad, and he highlights unexpected findings that lead to new contagion hypotheses. He ends with an analysis of the theoretical lessons of his empirical study.

The discussions of "unexpected findings," which are scattered throughout the case studies and again in the concluding chapter, raise new theoretical ideas that demonstrate the value of the inductive component of Vasquez's research design. One set of findings concerns the relative causal importance of the different contagion processes. Among other things, Vasquez suggests the important hypothesis that when alliances and territorial disputes push in opposite directions, territorial concerns usually win out (29). The case studies also reveal new causal factors that Vasquez had not identified in his original models. One is the importance of a strong leader in state decisions to enter a war.[14] The absence of a strong leader in the First World War cases was often associated with a divided and hence stalemated government, which is another important causal factor. Still another is battlefield conditions, which were important in several ways in 1914-18, especially for neutrals seeking the best deal but having to discount that preference with the probability that the state making the best offer would win the war.

[11] On the limitations of dyadic analysis, see Paul Poast, "(Mis)Using Dyadic Data to Analyze Multilateral Events," *Political Analysis* 18 (2010): 403-425.

[12] Vasquez et al., "ConflictSpace of Cataclysm."

[13] The treatment of rivalry includes not only its presence and absence but also the primary issue of the rivalry and its spatial or positional character. On the later see William R. Thompson, "Principal Rivalries," *Journal of Conflict Resolution* 39:2 (June 1995): 195-223.

[14] Examples include, among others, the role of Japanese Foreign Minister Katō Takaaki in the Japanese decision for war against Germany, and of Argentine President Hipólito Yrigoyen in keeping his country out of the war. Closely related to the "strong leader" category is Vasquez's occasional reference to "astute diplomacy" – for example, in his explanation of how Sweden, Norway, and Denmark managed to stay out of the war (292) – though theoretically that might involve the foreign ministry as well as the executive.

Vasquez's provisional theory of war contagion is quite useful, and his case studies are simply unparalleled in their scope and historical detail. One can find more detailed and authoritative treatments of the entry of individual states into the First World War, but this is by far the best comprehensive treatment of the spread of the war. It is well-grounded in the historiography of the war, and Vasquez does a good job of acknowledging interpretations that differ from his own. The fact that the case studies are guided by a well-defined theoretical framework enhances their value as comparative history and, for the international relations scholar, as an important contribution to theory building on a relatively neglected topic.

In this context, most of my concerns are rather limited. In terms of the historical analysis, my own expertise limits me to commenting on the immediate spread of the war, to which I turn later, while leaving it to others to comment on Vasquez's treatment of the second and third waves of contagion. Beginning with some theoretical issues, there is some overlap in Vasquez's models of contagion. This overlap complicates the task of assessing the relative causal weight of these models, though at the same time it helps identify interaction effects among multiple causal factors, which Vasquez does effectively. One such overlap, which I briefly noted earlier, is between territorial rivalry and opportunity. Vasquez argues that for both the territorial rivalry model and opportunity model of contagion, an ongoing war "removes obstacles" (43, 30) that previously prevented a state with grievances from achieving its foreign policy goals through the use of force. Since most of Vasquez's examples of opportunities involve territorial objectives, this raises the question of whether these models/processes of contagion should be combined in a single category

Ongoing wars sometimes create multiple opportunities for neutral states, and they must decide not only whether to enter an ongoing war, but on which side. Italy in the First World War is a classic example. Vasquez provides a strong theoretically-driven historical analysis. He notes that minor states are in a particularly good position to conduct a favorable 'auction' when both belligerents in war seek their allegiance. Vasquez provides an excellent discussion of Italian decision-making leading to its joining the Triple Entente, invoking both variants of the 'removing obstacles' mechanism of the opportunity contagion model: the war both weakens the capability of a rival and creates a breakdown in the political order that previously constrained minor states from the use of force. This is a persuasive analysis. I wonder, however, whether the phenomenon of a neutral conducting an auction in search of the best deal is sufficiently different than other phenomenon in the opportunity category to warrant a separate category of contagious process.

The discussion of the potential impact of an ongoing war in diminishing the restraining effects of norms against the use of military force, especially by smaller states, is an important theoretical contribution. I think Vasquez could have done more to specify the criteria by which we can identify a political order and its restraining norms, but his treatment of individual cases is usually convincing, especially when he invokes the counterfactual of what would have happened in the absence of restraining norms. For example, when he argues that Germany's involvement in the First World War created an opportunity for Japan to move against German colonies, and that this opportunity was a "prerequisite for Japan's actions" (323), Vasquez argues that in the absence of war "the prevailing political order would not otherwise have permitted it" and that the major powers would have diplomatically opposed military action by Japan. This strikes me as a very plausible historical argument.[15]

Vasquez makes good use of counterfactual analysis elsewhere, in his theoretically important discussion of the contribution of strong leaders to contagion processes. He argues that given Britain's highly divided cabinet, "A strong leader [Foreign Secretary Edward Grey] is the main factor that brought Britain into the war" (320). He then asks what would have happened if Grey had lost the argument. He notes that if the Liberal government had fallen the Conservatives would have come to power and entered the war. He also argues, correctly I think, that had the Liberals

[15] On counterfactual analysis see Jack S. Levy, "Counterfactuals, Causal Inference, and Historical Analysis," *Security Studies* 24:3 (September 2015): 378-402.

stayed in power the outcome would have been contingent on developments on the battlefield, with any significant turn against the French creating "pressure for British entry regardless of the Foreign Secretary."

I now return to the" failure of the coercive game" mechanism of contagion through alliances. Vasquez provides substantial evidence about threats and counter-threats between Germany and Russia in 1914 and how those threats escalated.[16] This discussion, however, raises two questions in my mind. First, I think Vasquez attaches too much causal weight to the failure of coercive bargaining and not enough to the underlying alliance commitments that led to coercive bargaining strategies in the first place. How do we measure the "value added," in causal terms, of coercive bargaining relative to the alliance commitments and the interests that generated coercive strategies? If two states each have formal alliance commitments to intervene in support of a weaker ally in the event of war, and war breaks out between the two weaker allies, there is a good chance that the two outside states will intervene, with or without coercive bargaining. The escalation of coercive bargaining may increase the probability of entry into the war, but a prior alliance commitment makes that probability high in the first place.

Vasquez's discussion of the failure of the coercive game between Germany and Russia raises another issue. For those of us whose image of crisis bargaining has been influenced by Thomas Schelling, Robert Jervis, James Fearon, and others, with an emphasis on political leaders supporting threats and counter-threats with credible and costly signals of resolve, then Russo-German bargaining in the July crisis looks very odd.[17] Although the crisis was escalating, the great powers made relatively few efforts to send costly signals of resolve, at least before the last two days of July 1914.

The July 20-23 meeting in St. Petersburg significantly stiffened the determination of French and Russian leaders to stand firm and together in any crisis, but this resolve was not communicated to Germany or Austria-Hungary, and in fact some mixed messages emerged.[18] After learning of the Austro-Hungarian ultimatum to Serbia on July 23, the Tsar soon initiated the "Period Preparatory to Mobilization," which many historians regard as an early mobilization. Russia did its best, however to keep these measures secret, rather than incorporate them into a strategy for signaling resolve. German intelligence quickly learned of the Russian actions,[19] but the German response was muted. Undoubtedly one reason is that until approximately July 27, German Chancellor Bethmann-Hollweg and other German leaders were reasonably confident that Russia would not intervene in an Austro-Serbian war.[20] When the Kaiser learned of Serbia's nuanced

[16] Vasquez also classifies the Germany-France and Britain-Germany dyads under the coercive game category (see the tables on 15, 313), but this is misleading. I'll leave the first aside, as Vasquez describes France as "not a direct player" in crisis bargaining" (102). For the British-German dyad, however, the problem was precisely the opposite of the failure of coercive bargaining. It was the failure of Britain to clearly warn Germany in a timely fashion that Britain would enter the war in response to a German invasion of France. Many (but not all) analysts conclude that such a warning would have been sufficient to avoid even a local war. Fritz Fischer, *Germany's Aims in the First World War* (New York: W.W. Norton, 1967 [1961]), chap. 2; Jack S. Levy, "Preferences, Constraints, and Choices in July 1914," *International Security* 15:3 (Winter 1990/91): 151-186, here 163-170.

[17] Thomas Schelling, *Arms and Influence* (New Haven: Yale University Press, 1966), chap. 1-3; Schelling, *Strategy of Conflict* (Cambridge: Harvard University Press, 1960); Robert Jervis, *The Logic of Images in International Relations* (Princeton: Princeton University Press, 1970); James D. Fearon, "Signaling Foreign Policy Interests: Tying Hands versus Sinking Costs," *Journal of Conflict Resolution* 41:1 (February 1997): 68-90.

[18] Christopher Clark, *The Sleepwalkers: How Europe Went to War in 1914* (New York: Harper, 2013), chap. 9; T.G. Otte, *July Crisis: The World's Descent into War, Summer 1914* (Cambridge: Cambridge University Press, 2014), 218.

[19] Ulrich Trumpener, "War Premeditated? German Intelligence Operations in July 1914," *Central European History* 9:1 (March 1976): 58-85.

[20] Otte, *July Crisis*, 270. William Mulligan, *The Origins of the First World War* (Cambridge: Cambridge University Press, 2010), 213.

response to the ultimatum, he proposed the 'Halt-in-Belgrade' plan to diffuse the crisis. The German military was getting nervous, however, and on July 29 Prussian War Minister Erich von Falkenhayn proposed the declaration of *Kriegsgefahrzustand*, the 'threatening danger of war.' This surely would have sent a credible signal of German resolve, but after a strong objection from Helmut von Moltke, Chief of the General Staff, no action was taken. Later that evening, after news of the Russian partial mobilization triggered calls for German mobilization, Bethmann-Hollweg rejected those calls. If the Russian partial mobilization sent a signal, its relatively quick cancellation sent the opposite signal. Bethmann-Hollweg did make a significant threat later that evening, but it was directed to Vienna, not St. Petersburg. In a 3am telegram on the 30th, the Chancellor essentially threatened to abrogate the blank check unless Austria-Hungary adopted a more moderate stance and agreed to mediation.[21] By the morning of July 30 Moltke had adopted a stronger line and joined the war minister in support of a declaration of the threatening danger of war, but Bethmann-Hollweg refused. Up to this point, the week-long sequence of events does not fit the standard model of costly signaling and coercive bargaining.

The 'contagion of coercion' did kick in later on July 30, however, with the Tsar's proclamation of general mobilization. This triggered the growing influence of the military in German decision-making, fitting another Vasquez hypothesis, and led to Bethmann-Hollweg's ultimatum to Russia that he would declare threatening danger of war at noon the next day if Russia did not reverse course. I agree with Vasquez that the failure of coercive bargaining played a significant role in drawing Germany and Russia into war, but think that this coercive game did not reach a significant degree of intensity until July 29 or 30.

This leads me to another issue. Vasquez argues that an "important theoretical assumption" underlying *Contagion and War* is that "the causes that start a war and bring the initial belligerents into the war are different from the causes that make a war spread.… Why war spreads is seen as a theoretically different question from that of what causes war" (311).[22]

This may be true of many wars, and it is probably true of the spread of war to minor powers in the second and third waves of contagion in the First World War. If there is any case for which this assumption is *not* true, however, it is the spread of the declared war between Austria-Hungary and Serbia to additional great powers in the first week of August 1914. In contrast to Vasquez's theoretical argument that "war spreads because of the consequences of its own dynamics," which are "not often anticipated" (311), I think that each of the great powers had a pretty good idea as to how an Austro-Serbian war might spread. They understood that there was a good chance that Russia would enter the war in support of Serbia, and that in anticipation Germany would inevitably enter the war against Russia and in the process bring France into the war. Whether the war would expand to include Britain, and if so when, was less certain.

Moreover, these expectations as to how the war might spread affected the processes leading to the outbreak of war in the first place. Austro-Hungarian leaders, in deciding on war against Serbia, knew very well that Russia would intervene unless deterred by Germany, which is why Vienna sought assurances from Berlin. German leaders recognized that a local war in the Balkans might lead to a continental war against Russia and France, and that Britain might come in, though they hoped, and for a while expected, that the fact that Russia and France were unprepared for war (or so they believed) would keep them out of the war.[23] There is little doubt, however, that if Berlin had not given Vienna a blank check,

[21] *German Documents on the Outbreak of the World War*, collected by Karl Kautsky (New York: Oxford University Press, 1924), no. 395 & 396, 343-346. Bethmann-Hollweg, however, did not sustain his threat.

[22] The ConflictSpace research program makes a similar assumption. Vasquez et al, "ConflictSpace of Cataclysm," 144. My remarks here build on my critique of that project, in Jack S. Levy, "The Initiation and Spread of the First World War: Interdependent Decisions," *Foreign Policy Analysis* 7:2 (April 2011): 183-188.

[23] On Russia's lack of readiness for war 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.

Austria-Hungary would not have moved against Serbia. Thus many have argued that the German blank check was a necessary condition for a local war and thus for a larger war, at least in 1914.[24] Vasquez basically accepts this argument when he says that Austrian Emperor Franz Josef "insisted on German support as a pre-requisite to the war" (132).

German leaders granted the blank check because they believed that an Austro-Serbian war would advance their interests. Moltke believed that it would precipitate the preventive war against Russia that he wanted. Bethmann-Hollweg, also driven by preventive logic induced by the growing power of Russia, hoped and expected a local war, which he somehow believed might lead to a split in the entente,[25] but was willing to risk a continental war in which he was reasonably confident that Germany would be victorious. For Germany, the factors leading to the blank check that enabled the local war were the same as the factors leading to the war with Russia.[26] Thus contrary to Vasquez's argument the outbreak and spread of war are analytically distinct, in the case of the First World War they were intimately interconnected.

My argument about the analytic inseparability of the initiation and spread of war has focused on the immediate spread of the First World War. It probably does not apply to the subsequent spread of war after the first week in August 1914. Whether it is theoretically generalizable to other wars is an interesting empirical question, but one that I leave to others to investigate.

Despite Vasquez's problematic assumption that the spread of war involves a separate set of causes than does the outbreak of war, his framework of six models or processes of contagion does not really rely on that assumption. Consequently, the causal separability assumption does not significantly detract from *Contagion and War*. In some respects, the possible interdependence between the initiation and spread of war actually increases the value of the book, by generating some important questions about the relationship between the outbreak and contagion of war. To what extent and in what ways do political leaders incorporate expectations about the likely spread of war into their crisis bargaining strategies and decisions to initiate a war? Which of Vasquez's contagion factor do they incorporate, how do they weigh those factors, and do they consider other important factors that Vasquez leaves out? What about the wars that never occurred because leaders anticipated that the war they were considering would spread in ways contrary to their interests? These are important empirical questions that scholars need to explore in other cases. Thus Vasquez's theoretical model can inform theories of the initiation of war as well as of the contagion of war. In doing so, it can also provide useful guidance to policy makers facing consequential difficult decisions of war or peace.

To summarize, *Contagion and War* is an important and likely enduring contribution to both theories of international conflict and to the study of the First World War. If ever I have a question about the expansion of the First World War, this will be the first place I will look. I have already made plans to assign parts of the book to my fall 2019 graduate course on Theories of War and Peace. I suspect that I will continue to do so for as long as I am teaching the course. The theoretical question will remain with us, and I doubt very much that anything else will come along to replace *Contagion and War* anytime soon.

---

[24] Luigi Albertini, *The Origins of the War of 1914*, vol. 2, trans. Isabella M. Massey (Westport: Greenwood, 1980 [1953]), 162; Levy, "Preferences, Constraints, and Choices," 155-156; Otte, *July 1914*, 102-103.

[25] Fischer, *Germany's Aims*, 60; Konrad H. Jarausch, "The Illusion of Limited War: Chancellor Bethmann Hollweg's Calculated Risk, July 1914," *Central European History* 2:1 (March 1969), 58.

[26] Although Vasquez recognizes the importance of preventive war logic for Moltke, he underestimates its importance for Bethmann-Hollweg and for the Kaiser. Vasquez develops his argument more fully in John A. Vasquez, "Was the First World War a Preventive war? Concepts, Criteria, and Evidence," in Levy and Vasquez, eds., *The Outbreak of the First World War: Structure, Politics, and Decision-Making* (Cambridge: Cambridge University Press, 2014), 199-223. For my view, see Jack S. Levy, "The Sources of Preventive Logic in German Decision-making in 1914," in Levy and Vasquez, *Outbreak*, 139-166.

Response by John A. Vasquez, University of Illinois, Urbana-Champaign

Political Science, History and July 1914

My thanks to Daniel Larsen for writing the introduction to the forum and to Talbot Imlay and Jack Levy for their generous reviews, which raise some important issues. In this response I will emphasize those points that are most significant for further study about the onset and spread of the First World War.

Imlay points out that any study of the First World War is limited by the sources and by previous work that shapes the author's point of view. In my work, and that of many political scientists, the reliance on single-language sources is an important constraint that the reader must keep in mind. Although I have relied on English-language sources, I do need to mention that I looked at a number of translated secondary sources and, more importantly, translated documents and primary sources, including Annika Mombauer's 2013 compilation[1] as well as those previous efforts beginning with the original release of the governments' 'white,' 'yellow,' and 'blue,' books. In addition, I examined all the declarations of war. Certain documents have been the focus of extensive commentary by various scholars, for example, the famous December 1912 war council emphasized by Fritz Fischer and most recently Dale Copeland.[2] Many of the commentaries include analysis of the original language to get at a clearer meaning. All of this has aided me, as with other political scientists, who do not have the broad language range of diplomatic historians. Nonetheless, it is a limitation that must be kept in mind by the reader.

How much difference it made in the final product is difficult to say. In my case I felt most deprived by not having a translation of Stefan Schmidt's 2007 book, so I had key parts translated for me.[3] No doubt there were many other foreign sources that I could have benefitted from reading. Likewise, even though it has been slightly over one hundred years since 1914, not all documents have been translated. Some sources, like those in Eastern Europe and the Balkans, still remain elusive except to country experts. Slowly, however, these have come under analysis, Mustafa Aksakal, for instance, has mined much of the Ottoman documents in a sensitive and objective fashion.[4] Imlay specifically raises the question of whether my interpretation would have been improved had I used more recent Russian sources. I will have to leave this to the reader to decide, but I did use Dominic Lieven's *The End of Tsarist Russia,* which utilizes documents released in the post-Cold War era.[5]

Having made these apologies let me mention three examples where I think more work with original sources could be useful in moving forward the debates about the war. In terms of Russian sources, having a finer and more detailed knowledge of what went on in what I call "the mobilization crisis" of July 29-August 1 would help us decide whether the war between Germany and Russia came about through escalation because of mutual fear generated by a *security trap* that enveloped both sides, as I argue, or whether the war resulted from a pre-planned German preventive war. Similarly, on

[1] Annika Mombauer, ed., *The Origins of the First World War: Diplomatic and Military Documents* (Manchester: Manchester University Press, 2013).

[2] Fritz Fischer, *Germany's Aims in the First World War* (New York: W.W. Norton, 1967), Dale C. Copeland International Relations Theory and Three Great Puzzles of the First World War in Jack S. Levy and John A. Vasquez, eds., *The Outbreak of the First World War: Structure, Politics, and Decision-Making* (Cambridge: Cambridge University Press, 2014), 167-198.

[3] Stefan Schmidt, *Frankreichs Aussenpolitk in Julidrise 1914: Ein Bettrag zur Geschichte des Ausbruch des Esten Weltkrieges*. (Munich: Oldenbourg, 2007).

[4] Mustafa Aksakal, *The Ottoman Road to War in 1914* (Cambridge: Cambridge University Press, 2008).

[5] Dominic Lieven, *The End of Tsarist Russia: The March to World War I and Revolution* (New York: Viking 2015).

the Russian side, having more detail regarding Sean McMeekin's argument that the Straits were a clear motivation for Russian entry would be useful for assessing his position compared to mine.[6] I argue that both Kaiser Wilhelm II of Germany and Tsar Nicholas II of Russia did not want war and resisted internal pressures. What these pressures were like and which had more of an impact is still an open question. Finally, in terms of detail, it would be useful to have had a *theoretical* investigation of the August 1 incident along a Robert Jervis style of analysis[7] of the psychological effect of the Aug 1 "misunderstanding" between the Kaiser and Edward Grey, Foreign Secretary of Britain, which ultimately led the Kaiser to throw in the towel to Chief of the German General Staff, Helmuth von Moltke and say to him, as Sean McMeekin put it, "Now you can do what you want."[8]

Likewise, more details on the discussions and especially the thinking that went on in the decision-making in cases that have already been analyzed in the literature in Bulgaria, Romania, Greece, and of course Serbia, would be informative.[9] This would be even more so in the cases that are not widely analyzed in English— for instance, Portugal, Ethiopia, Persia, and the Latin American neutrals.

On the whole, I provided narratives by analyzing each pair of countries that declared war, what political scientists call the 'dyad.' I was led to do this primarily because data analyses in international relations have been very productive when analyzing dyad data as opposed to that of individual countries or the system as a whole. Some scholars object to this breaking down and argue in favor of looking at all of the actors in ensemble. This approach is the typical one. I chose a dyadic perspective because in the end decision makers decided to go to war with specific states, even if they knew that war with another party would likely. To compensate for this artificial division, I often looked at third parties within a case. Sometimes, as in the Greek case, there were so many parties and it was so late in the game that the dyadic approach became cumbersome, and, as Imlay points out, a broader perspective would be useful. His view on Greece is well taken. In the end, what I found most interesting about Greece, from a theoretical perspective, was how France and Britain intervened to overthrow a legitimate monarch to pursue their war. A larger lesson here was how the stalemated Western front pressured the major states to expand the war. So even though I employed a dyadic analysis, I did not permit myself to be overly constrained by it in complicated cases.

A specific point that Imlay discusses is my treatment of President Woodrow Wilson. He mentions in passing that I relied on a consensus view, citing my use of John Milton Cooper.[10] I should make it clear to readers that it was not my intention to provide a consensus view of the principals in the narratives, a task which, as Imlay points out, is difficult. Rather, I gave my own views on the role they played. I may have ended up seeing Wilson as a strong and pivotal leader, but this was my interpretation and not necessarily a consensus (based on Cooper). My interpretation was also informed by Thomas Knock, David Stevenson, Adam Tooze, and others cited.[11]

---

[6] Sean McMeekin, *The Russian Origins of the First World War* (Cambridge: Belknap Press, 2011).

[7] Robert Jervis, *Perception and Misperception in International Politics* (Princeton: Princeton University Press, 1976).

[8] Sean McMeekin, *July 1914: Countdown to War* (New York: Basic Books, 2013), 349.

[9] See Richard F. Hamilton and Holger H. Herwig, eds., *The Origins of World War I* (Cambridge: Cambridge University Press, 2003).

[10] John Milton Cooper, Jr. "The United States" in Hamilton and Herwig (2003), 415-442.

[11] Thomas J. Knock, *To End All Wars: Woodrow Wilson and the Quest for a New World Order* (Princeton: Princeton University Press, 1992), David Stevenson, *1917: War, Peace, Revolution* (Oxford: Oxford University Press 2017), Adam Tooze, *The Deluge: The Great War, American and the Remaking of the Global Order*, 1916-1931 (New York: Viking, 2014). I did not have a

In terms of my interpretation of decision makers I tried to avoid the 'blame game' that has colored so much of previous scholarship.[12] I tried to assiduously outline the empirical role each dyad played in spreading the war, and how the decision-making led it to play that role without discussing who was responsible for the war. In doing so I have sometimes given different interpretations of key actors, like Wilhelm II, that might serve as the basis for a more revisionist account of July 1914. Such revisionist accounts will help lead us beyond "the German paradigm." in the words of Samuel Williamson.[13]

Last, Imlay discusses the generalizability of my models and hypotheses to other cases past and future as a way of looking at the relevance of a book about a war that occurred 100 years ago. His reference to a possible U.S.-China war is on the mark, especially in terms of how a militarized dispute in the South China Sea might spread. In terms of the present, I also mentioned that current events in Syria regarding Turkish intervention can be traced back to contagion resulting from the 1980 Iran-Iraq war that led to the invasion of Kuwait and subsequent Persian Gulf War and War in Iraq and the rise of ISIS, the *Islamic State of Iraq and Syria* (370). These cases could be used to investigate what generalizations will hold based on the new hypotheses in the concluding chapter.

In the end much of the contribution of the book rests on it providing a set of concepts and theoretical analyses that make it possible to compare the First World War to general patterns. Political scientists, unless they do archival work, cannot really add new facts to the historical record. That is primarily the purview of diplomatic historians. Political scientists have the advantage of standing upon the hard work of historians to see what larger patterns and forces were at work to bring about the war and peace.

Levy's review tackles mainly this theoretical aspect of the book. One of the great assets of Levy's approach is that it dissects the different aspects of possible casual processes to uncover paths that we may have missed or underestimated. I will concentrate on two of his major points where we disagree, both of which deal with the July 1914 crisis. The first is on the role of alliances and the second is my separating the local war from the general war in August as a form of contagion. With regard to alliances, one of the key points he raises in my discussion of the July 1914 crisis is the precise role of alliances versus bargaining in the onset of war. He states:

"If two states each have formal alliance commitments to intervene in support of a weaker ally in the event of war, and war breaks out between the two weaker allies, *there is a good chance that the two outside states will intervene, with or without coercive bargaining* (emphasis added)."

There are two analytical questions here: the first is to what extent the previous alliance commitment prime the parties for war that the subsequent bargaining plays almost no role in bringing about escalation to war, and the second is whether the diplomacy of the crisis played a significant causal role. In terms of the first question, the disagreement is over the magnitude of the alliance effect. We both agree that alliances increase the probability of joining alliances; the disagreement is over the magnitude of this probability.

---

chance to look at Trygve Throntveit, *Power without Victory*, which came out in July 2017 as I was submitting the final manuscript. If I had, I would have included some of what he had to say about the League of Nations.

[12] See Christopher Clark, *The Sleepwakers: How Europe Went to War in 1914* (New York: HarperCollins, 2012), 560.

[13] Samuel R. Williamson, Jr. "July 1914 Revisited and Revised: The Erosion of the German Paradigm," in Levy and Vasquez (2014), 30-62.

As a general finding, we know from an early study by Randolph Siverson and Joel King that being allied with a belligerent before the outbreak of a war increases the likelihood of entering the war.[14] In a study that examines all states that had alliances with a belligerent from 1815-1965, they find that 75 entered the war and 233 did not. Conversely, there were 2,150 states without an alliance with a belligerent who did not enter and only 113 that did. This means that alliances increases the probability of entering a war, Q=.73 (Siverson and King, Table 2: 40). However, most states that had an alliance with a belligerent (233 out of 308) did not enter.

Two subsequent studies refine this relationship. Aan Ned Sabrosky finds that about 75% of alliances are *un*reliable in that states that had an alliance with a belligerent did not enter a war where that ally was involved.[15] Ashley Leeds, Andrew Long, and Sara McLaughlan Mitchell further refine that analysis, controlling for whether the alliance treaty specifically bound the ally to come to the defense of the attacked state, i.e. whether this involves a defense alliance.[16] When reliability is examined in this more legal context, they find the opposite--about 75% of the alliances were reliable and the state entered the war. Still this means that in 25% of the cases allies did not intervene the way Levy might have anticipated. I read this evidence as saying that there is a statistically significant probability that allies of a belligerent come to the aid of attacked parties if they had a defense pact, but even in these strict legal conditions, in a large proportion of cases—at least 25%--allies would not join the war.

The beauty of case analysis is that it gives us a chance to deal with these questions both in terms of the actual written record and counter-factual reasoning. My interpretation of the July 1914 crisis in terms of the German-Russian dyad is that ultimately it was the bargaining and the failure of coercion to work that led the two sides to go to war, and not simply their previous alliance commitments. The bargaining was not just ancillary to the previous alliances and all the factors alliances put into play, which were considerable (346-353). In the end, the two principal players in each state—Wilhelm II and Nicholas II—wanted to avoid war, but were unable to do so despite their best efforts, and it was this failed negotiation that brought about the war. Why they were unable to do so tells us a great deal about what actually happened in the causal chain to war.

I argue in the book that the coercive game got out of hand over the question of mobilization. This mobilization crisis was a crisis within a crisis that resulted from a *security trap* that produced the fear that the mobilization efforts of the other side would give their opponent an overwhelming initial advantage. This led the generals and hard-liners on each side to become increasing frantic in pressing their leaders to fully mobilize and go to war. For me, it was the insecurity produced by the dynamics of the crisis rather than specific grievances or the previous alliances that brought about the escalation to war within this dyad (95-96): I write that "Mobilization was the single most crucial decision taken in the July crisis that brought war about in the sense that there was no turning back" (96). Hence, unlike Levy I place the failure of the coercive game as being causally more immediate than the alliance structure, even if the structure was important in terms of setting up the sides and playing the coercive game to the hilt (92).

Coercion failed because both sides thought that the other would back down in face of superior power, but neither did. As this coercion game was ongoing there was another game—the peace game that the Kaiser, Grey, and to a certain extent the Tsar were playing to avoid war. The Kaiser's Halt-in-Belgrade plan, whereby Austria-Hungary would occupy

[14] Randolph M. Siverson and Joel King, Alliances and the Expansion of War, 1815-1965 in J. David Singer and Michael Wallace, eds., *To Augur Well* (Beverly Hills: Sage, 1979), 37-49.

[15] Alan Ned Sabrosky, Interstate Alliances: Their Reliability and the Expansion of War, in J. David Singer, ed., *The Correlates of War, Vol II* (New York: Free Press, 1980), 161-198.

[16] Ashley Brett Leeds, Andrew Long, and Sara McLaughlin Mitchell, "Reevaluating Alliance Reliability: Specific Threats, Specific Promises, *Journal of Conflict Resoluti*on 44:5: 636-699.

Belgrade and stop there, was a useful way of doing this but it was not easily implemented because of the opposition of Leopold Berchtold, Foreign Minnister of Austria-Hungary.

It was thus the failure to successfully coerce in the first instance, and then to reach some bargain to avoid a general war in the second instance, that led immediately to the onset of hostilities. Alliances played a role in setting up the coercive game and making each side think the other would back down, but the failed bargaining subsequent to this 'structural' standoff in the context of mutual efforts at mobilization is what brought about the war. This is not to deny the tremendous theoretical importance of alliances. The 1914 case is a prime example of deterrence failure, as is 1939, which indicates that world war is connected with the failure of alliances to prevent war, as I discuss at length in the book (353-356).

In the end, several factors, alliances, failed coercion, failed bargaining over a peace plan, and the mobilization crisis, were all vectors pushing the two sides to war. In addition, the military and certain hard-liners within Germany and Russia were pushing the two leaders to decide in favor of war. Against this host of vectors were only the Kaiser and the Tsar, both of whom wanted to avoid war, along with Grey. Events on the ground, in terms of the mobilization crisis and the fear it generated, undid their efforts.

This view of the escalation of the July crisis is very different from the view that 'Germany' (as if there ever was such a black-boxed actor) wanted a preventive war and used the assassination of Archduke Franz Ferdinand of Austria for that greater purpose. It also differs from the view that the crisis involved a finely played game that examines payoffs in light of consistent unchanging objectives of unitary 'rational' actors. The Kaiser and the Tsar really wanted to avoid war. The former in particular was not interested in taking advantage of the situation to fight a war now rather than later. He wanted a negotiated solution. Once it became clear that Britain would intervene, Bethmann-Hollweg, Chancellor of Germany, was on the same page as the Kaiser and the Tsar.[17]

The dynamics of the bargaining, complete with its individual contingency and accidents, introduced a certain inadvertency to the onset of the war between Germany and Russia, which, in turn, brought the other cards down. The latter, was, in fact, a result of alliances that offered a contagion mechanism, as I outline in the book, but this factor was only activated once the coercive game failed. The coming of the war was more probabilistic and more indeterminate and filled with uncertainty than the alliance vector implies. Hence, I place more emphasis on the coercive game. I do this in turn, because this factor has generally been underemphasized, if not ignored, in much of the literature.[18]

Let me now turn to the second major point of Levy's critique—that the July 1914 crisis is best seen as a single event that caused the First World War rather than as two separate events—a local war that spread to a general war. Thus, while he sees the concept of contagion as being relevant to the subsequent spreading of the war, he does not see this as applicable to the actual July crisis that culminates in the guns of August.

[17] Levy argues that many analysts think that if Grey had made it clear that Britain would intervene, the war would have been avoided. What is crucial about this British deterrent argument is that it is entirely moot. It could not have been made by Foreign Secretary Edward Grey because the Cabinet would not have permitted it and the government would have fallen. Thus, it could never have played a role in the crisis. Nor am I sure that a clear commitment to defend France would have prevented the war. When it was obvious that Britain would enter, this did force Bethmann-Hollweg to pressure Berchtold to follow the Kaiser's peace plan, but he and the Kaiser were not ultimately deterred from going to war by the realization that Britain would enter.

[18] A related point that Levy makes is that, in general, allies will intervene with or without coercive bargaining. Coercive bargaining did occur in 1914 and in 1908 (Bosnia), 1938 (Munich), and 1939 (Poland), whether these are exceptions is an empirical question that deserves more systematic investigation.

I do not disagree that the later entrants to the war involve clearer cases of contagion, but I do think there is some merit in looking at July in terms of a local war that expanded through contagion to a general war. The first argument for doing so is that there are in history many dyadic wars between neighbors over territory that do not spread. The theoretical question for July 1914 is why this war between neighbors spread to become a world war. Also, at the time, the local war was seen as distinct from the wider general war (both continental and world war). It was often thought that an Austro-Hungarian/Serbian war could have been a Third Balkan War and not a general war. Thus, theoretically, and based on contemporary views, separating the local war is not unreasonable, and I do come up with some new hypotheses on why this dyadic war expanded (314-316).

The second argument is a matter of what the conceptualization adds—what does it allow us to see that we did not see before (or at least that we underemphasized). It places the origins of the war clearly within the dynamics of the crisis on what the principal decision makers did to each other and how they acted, rather than the more fixed underlying structure. It emphasizes agency but within the context of structure. The implication is that the war could have remained local, and that diplomacy could have avoided a general war. It permits us to see why this did not happen. It helps us understand how some of the more underlying variables that Levy emphasizes overwhelmed the two most authoritarian leaders in the crisis, but it does this by examining the dynamics of the crisis itself. It points out that the German-Russian dyad was the keystone dyad and that the war would not likely have occurred if it had simply involved a matter of Germany and Russia or Britain and Germany. It provides a new interpretation of the role of the Kaiser as someone sincerely trying to avoid the war, and describes his actions as following a well-known pattern of his behavior; namely, bravado followed by backing away from war. It opens the black box and shows how the bureaucratic interests of the military in both countries created immense time pressure to act quickly. The time pressure in turn was generated by the close proximity of large armies. It permits us to see that a *security trap* brought about by the mobilization crisis played a key causal role in spiraling the crisis into war in the last days of July and the beginning of August. These were full days filled with tension where men (and they were exclusively men) were tired, took actions in the middle of the night, saw their best efforts fail, and their militaries grow frantic. Examining the crisis that made the local war spread allows us to recognize the possibility that these individual-level variables could have played a key causal role. It then argues that once they made the crisis spiral out of control, the strong contagion mechanism of alliance made the war diffuse rapidly and inexorably. But before that happened, a general war, while in the cards, did not have to occur.

The contribution of this analysis of the July crisis is that first, it shows how different motivations of different leaders contradicted each other. The preventive war hypothesis makes most sense for Moltke, but not for the Kaiser. Second, the analysis looks at the role of contagion—how the ongoing crisis spread the conflict to other parties. It did this through alliances, as Levy states, but the addition of the concept of contagion to July permits us to see alliances not simply as attempts at deterrence, but as a strong mechanism for the diffusion of war.

This is a fascinating set of decisions that historians have painstakingly reconstructed. Political science can aid our understanding of events by supplementing the concepts and hypotheses that historians have used to explain events. The decisions of 1914 and during the war years (as more states joined) are so complicated that this topic will not soon go away. Nonetheless, by moving away from the 'blame game' we have turned an important corner in our search for understanding.