

Statecraft Global
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INTERNATIONAL HISTORY POLITICS

Relations Empire
Change

An organized section of the American Political Science Association (APSA)

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International History and Politics (IHAP)

[Website](#)

Section Officers

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Vice Chair: Catherine Lu

Secretary -Treasurer: Andrew Yeo

2021 Program Chair: Marcos Scauso

Newsletter Editor: John Emery

Call for Announcements

Section members are invited to send their announcements about upcoming workshops, recent books, or anything else of general IHAP interest to the editor: John Emery—jremery@uci.edu

Newsletter Summer 2021 Volume 7, Issue 2

Message from the IHAP Chair
Stacie Goddard

We're looking forward at seeing you all at APSA next week, either virtually or in person. As you can imagine, with all of the uncertainty surrounding the Delta variant and the start of the academic year, we have been busier than usual putting the final touches on our schedule of panels, business meetings, and reception. This newsletter outlines a number of those details, though please reach out to me if you have any questions.

IHAP panels. First, I want to say “thank you” to this year’s program chair, Marcos Scauso, who has had the almost impossible task of assembling and then reassembling panels for this year’s conference. Despite all of the challenges, his heroic efforts have ensured a [full slate of panels](#). This year, we have panels on colonization and decolonization, historical great power politics, Eastern world orders, and religion and order, among others. I encourage you to attend as many as possible.

Business meeting. We have moved our virtual business meeting from its officially scheduled time on September 24 to **October 1 at 8:30 am PDT**. I will post a Zoom link to apsaconnect, and all are welcome to attend. Among other things, we will be voting our new Executive Council members, Carla Nörrlof, Marcus Scauso, and Jelena Subotic.

Reception. Our virtual reception **October 1 at 4:30 PDT**. The link is available here: <https://tinyurl.com/yzl3vt9d>. Last year, we made the reception a mentoring event for early career scholars. If you are an early career scholar, please think of a question for your colleagues. If you are a more senior scholar, please consider signing up to mentor and answer questions! I’ll be sending out a Google form to collect RSVPs and questions, but please do consider attending even if you don’t fill out the form.

Also, at our reception, we'll be recognizing our award winners! A special thanks here to our committee members who did some heavy lifting in a very heavy year.

Robert L. Jervis and Paul W. Schroeder Best Book Award
Committee Members: Etel Solingen, Ahmet Kuru, Jelena Subotic

Kyle Lascurettes, Lewis and Clark University

Orders of Exclusion: Great Powers and the Strategic Sources of Foundational Rules in International Relations. Oxford University Press

Dov Levin, University of Hong Kong

Meddling at the Ballot Box: The Causes and Effects of Partisan Electoral Interventions. Oxford University Press.

Honorable Mention: Lora Viola, Freie Universitat Berlin

The Closure of the International System. Cambridge University Press

IHAP Best Article Award

Committee Members: Timothy Crawford (chair), Sara Parkinson, Eric Hundman

Yasuhiro Izumikawa, Chua University

"Network Connections and the Emergence of the Hub-and-Spokes Alliance System in East Asia," *International Security*, Vol. 45, No. 2 (Fall 2020), pp. 7-50.

We look forward to seeing you next week!

All best,

Stacie Goddard

President

International History and Politics Section

IHAP at APSA 2021 – Promoting Pluralism

- At the 117th APSA conference in Seattle is scheduled for September 30 to October 3, 2021
- At the upcoming APSA conference, the division will put on 7 paper panels (2 in-person, 5 virtual) 1 poster session, and 1 author-meets-critics event (in-person) (on Ayse Zarakol's *Before the West: The Rise and Fall of Eastern World Orders*)
- We look forward to seeing all of you virtually at the IHAP business meeting, reception and at our sponsored panels both virtually and in-person.

IHAP Panels, Posters, & Roundtables Schedule at a Glance:

Friday October 1st

“Contending Explanations of China’s Behavior”

Fri. October 1, 8:00 to 10:30am PDT Location: WSCC, 620

[[More info](#)]

****International History and Politics Business Meeting****

– Business Meeting on Fri. September 24th 12:00 to 1:30pm PDT the direct link to our virtual reception: <https://tinyurl.com/yzl3vt9d>

“Religious Agency and Conflict Under Colonialism” Fri. October 1, 2:00 to 3:30pm PDT Location: Virtual Platform, Virtual Room 33 [[More info](#)]

“Religious Nationalism” Fri. October 1, 4:00 to 5:30pm PDT Location: Virtual Platform Full Panel Pre-Recorded Session [[More info](#)]

“Author Meets Critics–Before the West: The Rise and Fall of Eastern World Orders” Fri. October 1, 4:00 to 5:30pm PDT Location: Sheraton Grand Ballroom C [[More info](#)]

International History and Politics Virtual Reception Fri. October 1, 4:30 to 6:00pm PDT Location: Virtual at: <https://tinyurl.com/yzl3vt9d>

Saturday October 2nd

“Factors and Causes Shaping Great Powers” Sat. October 2, 6:00 to 7:30am PDT Location: Virtual Platform, Virtual Room 10 [[More info](#)]

“State Behavior Throughout History: Diverse Factors of Influence Virtual Poster Session” Sat. October 2, 7:00 to 7:30am PDT Location: Virtual Posters [[More info](#)]

“Agency and Marginalization in Constructions of Governance” – Sat. October 2, 4:00 to 5:30pm PDT Location: WSCC, 618 [[More info](#)]

Sunday October 3rd

“Paths of Legitimization and De-Legitimization” Sun. October 3, 6:00am to 7:30am PDT Location: Virtual Platform, Virtual Room, 32 [[More info](#)]

“The Emergence and Continuity of Colonialisms: Agency and Domination” Sun. October 3, 2:00 to 3:30pm PDT Location: Virtual Platform, Virtual Room 30 [[More info](#)]

Congratulations to the 2021 IHAP Section Award Winners!

The 2021 Outstanding Article in International History and Politics

The Outstanding Article Award in International History and Politics recognizes exceptional peer-reviewed journal articles representing the mission of the International History and Politics Section of the American Political Science Association, including innovative work that brings new light to events and processes in international politics, encourages interdisciplinary conversations between political scientists and historians, and advances historiographical methods. The Outstanding Article Award is given to a published article that appeared in print in the calendar year preceding the APSA meeting at which the award is presented.

Committee Members: Timothy Crawford (Chair), Sarah Parkinson, and Eric Hundman

The winner of the 2021 Outstanding Article Award is: **Yasuhiro Izumikawa** (Chuo University) for his article: "Network Connections and the Emergence of the Hub-and-Spokes Alliance System in East Asia," *International Security* 45 no. 2 (Fall 2020): 7-50.

The 2021 Robert L. Jervis and Paul Schroeder Best Book Award

The Robert L. Jervis and Paul Schroeder award is for the best book on International History and Politics. The award may be granted to a single-authored or multi-authored book, or to an edited volume, and will be given to works published in the calendar year prior to the year of the APSA meeting at which the award is presented. The copyright date of a book will establish the relevant year.

Committee Members: Etel Solingen (Chair), Ahmet Kuru, and Jelena Subotic

The co-winners for Robert L. Jervis and Paul Schroeder Best Book Award are:

Kyle Lascourettes (Lewis and Clark) *Orders of Exclusion: Great Powers and the Strategic Sources of Foundational Rules in International Relations* (2020 [Oxford University Press](#))

AND

Dov Levin (University of Hong Kong) *Meddling at the Ballot Box: The Causes and Effects of Partisan Electoral Interventions* (2020 [Oxford University Press](#))

With an Honorable Mention for:

Lora Viola (Freie Universitat Berlin) *The Closure of the International System: How Institutions Create Political Equalities and Hierarchies* (2020 [Cambridge University Press](#))

Interviews with the Award Winners

Outstanding Article in International History and Politics

Yasuhiro Izumikawa's article: "Network Connections and the Emergence of the Hub-and-Spokes Alliance System in East Asia" *International Security* 45 no. 2 (Fall 2020): 7-50.

How did you become interested in the intersection between international history and politics?

You can say that historical and cultural contingencies really shaped my interests in linking international history and politics. I was born and educated in Japan, where even today the study of international politics is more historical rather than theoretical. (This fact has both negative and positive ramifications.) With this background, I naturally became interested in diplomatic history first. But since when I started studying at U.S. graduate schools, my exposure to theory has immensely impacted my intellectual developments. This is how I have become interested in qualitative methods of studying international politics.

In your article you ask why the so-called hub-and-spokes alliance system emerge in East Asia after World War II instead of a multilateral alliance. You argue that in East Asia, three U.S. allies—Japan, South Korea, and Taiwan—contributed to the emergence and shape of the hub-and-spokes system, which came into being as an unintended consequence of their interactions. What led you to challenge prominent realist or constructivist accounts of this case, and what inspired you to tackle this issue?

When the seminal article by Hemmer and Katzenstein on this subject appeared in *International Organization* in 2002, I felt that their argument could not be reconciled with the existing historical works, conducted mostly by scholars outside the United States, that showed the United States did pursue a multilateral alliance in East Asia and that its efforts were blocked by its allies. I was also familiar back then with social exchange network approach, which I had used for my own dissertation, and thought

that it could do better in explaining the formation of the hub-and-spokes system. Then came a surprise; Victor Cha, who is one of my dissertation advisors at Georgetown University, published his article on the so-called powerplay theory in 2010. While I liked some elements of Victor's work, the notion that the United States intentionally created the hub-and-spoke system still contradicted what I knew from the aforementioned historical works. This is how I became motivated enough to write my own account.

Are there any scholars that you look to as role models? Or pieces of scholarship that you view as being templates for excellent research?

Among many scholars whose works I admire; Robert Jervis and David Baldwin stand out. I still regard Baldwin's *Economic Statecraft* as one of the best works in our field, and I highly respect Jervis for his ability to move beyond different theoretical boundaries and to weave his theoretical notions with detailed historical details. If you ask me who really influenced me, however, it's my dissertation advisors at Georgetown. My mentor George Shambaugh, and my three advisors—Andy Bennet, Victor Cha, and Bill Wohlforth—all are my role models even today. One might be able to discern the elements of all the four in my works.

How do you navigate the tension between detailed historical research and macro theoretical claims, between contingency and generalizability?

Maintaining a big theoretical picture and delving into the details of historical materials is indeed challenging. When I read historical materials, I always try to ask myself what this piece of evidence, a diplomat's statement to his/her counterpart, may mean for my and competing

hypotheses. Is it supporting what I aim to claim or not, or is it just irrelevant? What kind of evidence can I find in the documents that I am looking at? Without consciously reminding ourselves of what we really try to get at, we may be easily carried away because reading historical materials is really exciting! It is also important to keep in mind that historically significant evidence and theoretically important one may be different.

What was the most challenging aspect of working with the historical material used in the project?

The toughest challenge was to collect and go through empirical evidence regarding South Korea's and Taiwan's decision-makings. As a native Japanese who went through U.S. postgraduate education, I can use Japanese and English sources without too much trouble. On the other hand, I don't speak Korean or Chinese, so collecting Korean and Taiwanese materials were tough. Luckily, my partner is a historian who utilizes Chinese sources, and I was able to visit Taiwan's diplomatic archive many times accompanying her. And interestingly, so many documents were in English because when Taiwanese diplomats communicated with their counterparts in other states, they usually did so in English, right? As for Korean materials, I was not so lucky but was still able to read the English translations of many important materials available at Wilson Center's database. While language and national barriers still exist for historical research, the barriers have become much lower than before, thanks to those historians who spend so much time making documents available to a wide range of audience.

What was the most unexpected thing you found in conducting your historical research?

When I visited Taiwan's diplomatic archive, I was struck by the level of communications between Seoul and Taipei from the late 1940s to the 1950s. It is well known that Rhee Syngman and Chiang Kai-shek had similar policy preferences regarding their respective relations with the United States, so it should not be so surprising. Nonetheless, the depth of their communication regarding security cooperation

was far beyond the existing research published in English suggest. On the other hand, the differences did exist between South Korea and Taiwan, and US policymakers back then often failed to notice such differences and tended to put them in the same basket. It was fascinating to compare Taiwanese and US documents to see what Washington correctly and incorrectly inferred about South Korea and Taiwan. If you are only looking at US documents, you may not get that picture.

What do you think are the major differences in how political scientists and historians "do" history?

In addition to the several features pointed out by the previous research, I would like to point out two less-frequently mentioned differences. First, while historically-oriented political scientists tend to focus on one or a few "key" documents/evidence, historians rarely rely too heavily on such thing. Since political scientists like myself aim to test or propose specific theoretical hypotheses, they inevitably but sometimes overly highlight the evidence relevant to such hypotheses. Second, historically-oriented political scientists sometimes err in overemphasizing the significance of archival documents, whereas historians know the importance of balancing archival documents and other sources, such as oral history records, diary, or even biography. For these reasons, I try to tell myself that my works are still different from historians' works in some important ways.

What would you like to see more of in terms of research into international history and politics, either methodologically or substantively?

I would like to see more research that utilize non-US or non-Western archival sources. We often hear the criticism that the existing IR theories have US/Western biases, and that may be true. On the other hand, I do not think that seeking what is dubbed "non-Western" IR theories is fruitful. We can learn a lot from research that utilizes Asian, Latin American, or African archives to test the existing theories or propose new hypotheses. We can more effectively expand

our knowledge by doing so rather than revolutionize it.

What do you think are the biggest lessons that publics and/or governments should take from your work?

My article shows the importance of network connection—the degree of one relation’s tightness positively or negatively influences the tightness of other relations in a network—in explaining the origin of the alliance system in East Asia. Applying this idea to today’s East Asia, the relative decline of US ability to provide security prompts its allies, such as Japan and Australia, to contribute more than ever to regional security. US policymakers should keep this in mind and take advantage of the trend in order to maintain US leadership in East Asia. Policymakers in US allies should use this logic to procure domestic support for contributing more to regional security. In this sense, the logic laid out in my article may help resolving the “burden sharing” problems.

What tips would you give graduate students or junior scholars interested in historical methods?

If you like historical/qualitative research, don’t give it up for “strategic” reasons and do the type of research that you enjoy doing. I would like to say this in particular to foreign students from non-English speaking countries. Sometimes, they give up qualitative/historical research because it is time-consuming to do so in English or because using quantitative methods/rational choice is easier for some foreign students. That is very, very unfortunate. In addition, the fact that you speak languages other than English, non-Western languages in particular, gives you the advantages that not many others possess, such as the ability to read books in your home language or archival documents of your home country. Think of it as your strength and use it for doing what you really want to do. Our community will all benefit from your doing so. After all, what is strategically rational changes depending upon your time horizon and other factors.

The Robert L. Jervis and Paul Schroeder Best Book Award

Dov Levin *Meddling at the Ballot Box: The Causes and Effects of Partisan Electoral Interventions*
[Oxford University Press](#)

How did you become interested in the intersection between international history and politics?

That was actually one of the key things that got me interested in International Relations in the first place. I’m a big history buff, especially of political and military history, and in my undergraduate degree I double majored in history and political science hoping to get a better understanding of the broad patterns of international history.

During the course of studying for my undergraduate degree, I increasingly realized, as I took courses in both fields and saw each side’s perspectives, that the type of big picture

historical questions that interested me were much less likely to be studied by historians and much more likely to be studied by IR scholars. That was one key reason why, when I finished my B.A. degree, I went for a Ph.D. in IR instead of history.

In your book, you focus on six intervention cases in which electoral interventions were carried out or seriously considered by a foreign power. What was your criteria for case selection and were there any cases that you wanted to include but did not make the final cut?

As part of my research on partisan electoral interventions, I collected a dataset of such meddling (PEIG) by the US and the Soviet

Union/Russia between 1946 and 2000. Accordingly I chose my case studies based on the expert advice on how to choose qualitative cases for in depth analysis from a dataset. To avoid getting into excessive technical jargon, each one of the six cases represents one of the four possible combinations that my two explanatory variables which concurrently explain when an electoral intervention occurs- whether a would-be intervener perceives a particular domestic actor in the target as implacable or not and whether another domestic actor in the target wants or is willing to receive such electoral aid or not. At the same time I tried to keep other possibly important 'control' variables like the period these cases occurred in and the identity of the would be foreign intervener as identical as possible.

I was hoping to also include the case of the American intervention in the 1964 Chilean elections- the lesser-known prequel to the (in)famous US intervention in Chile's elections in 1970. Unfortunately, when I arrived to the relevant archives, I discovered that, despite the fact that more than 50 years have passed since this intervention, many of the key documents related to the JFK and LBJ administrations' decision to intervene were still classified or heavily retracted- and my FOIA's and MDR's failed to secure any significant declassifications after a few years of waiting. Accordingly, I was unfortunately forced to omit this case.

How do you navigate the tension between detailed historical research and macro theoretical claims, between contingency and generalizability?

It is always a challenge to find the correct balance between generalization on the one hand and, on the other hand, the richness and complexity that is an inherent part of real-life historical events. My focus in this regard was to make sure to provide a historically accurate, detailed narrative on how events developed in each case while making sure to always note with extra details the key aspects in the narrative related in some manner to my theoretical arguments. In other words, give as accurate a picture of the key

developments as possible but also make it as simple as possible for readers to easily notice and follow the most relevant information.

What was the most challenging aspect of working with the historical material used in the project?

The key challenge was to identify the "lay of the land". In many of the electoral intervention cases that I studied there was little previous secondary literature about the bilateral relations and the little that existed focused on other issues with the electoral intervention getting a short shrift. Likewise, the archival documents naturally were written by diplomats in the thick of the events who usually didn't need to give each other much exposition. Accordingly, much of the work with the archival materials involved trying to better comprehend the underlying reasons for any U.S. concerns, who exactly were the key domestic players in the target and their political situation- the wider political and historical context that would enable me to understand why a decision to intervene in an election was or was not made in a certain case.

What was the most unexpected thing you found in conducting your historical research?

Two such things: one specific to electoral interventions and one more general. The first was how some factors widely believed to be in the public discourse on such meddling to encourage it did not play a major role in practice. For example, a belief that another unfriendly foreign actor is intervening for one side in a particular election is not usually a major or sufficient reason to lead the US (or other rivals) to intervene in that election as well.

The second, more general, one was how different sometimes were the perspectives of contemporaries about the range of possible or plausible domestic future political developments in a particular country or of plausible international developments compared to many later retrospective analyses of said countries or developments. For example, I was quite surprised to discover how plausible the possibility that Western Europe would soon have a common army- a development yet to occur

nearly seventy years later- seemed to many serious people in the 1950s. When I later came across French philosopher's Henri Bergeson's warning to people who seek to understand the past about making sure to avoid "the illusion of retrospective determinism", his words rang very true.

What do you think are the major differences in how political scientists and historians "do" history?

Most historians look to explain and describe a certain particular historical event or era with the main goal being seen as explaining the particularistic causes of that specific event. Wider explanations are often eschewed or seen as a secondary concern. In contrast most political scientists see political history as the output of immutable or wider dynamics which apply beyond any particular era and seek to use highly detailed informed about this "output" to identify the history generating dynamics behind it. Both are valuable in my opinion, but I am naturally biased towards the latter perspective.

What would you like to see more of in terms of research into international history and politics, either methodologically or substantively?

We need more historical research in political science into less well known and non-western cases. Our understanding of international politics has been heavily affected by a handful of extremely famous international events (such as WW2, the Cuban Missile Crisis, or the end of the Cold War) in general and the foreign policy experiences of the US in particular. However, in order to derive a better understanding of international politics, both less well-known events, and the foreign policy experiences of non-western powers, need to be historically analyzed and insights from them integrated into our theories. Over the last two decades there has been a growing effort by scholars to do both, which has indeed improved the situation in this regard- but more still needs to be done.

What do you think are the biggest lessons that publics and/or governments should take from your work?

A few such lessons. The first lesson is that foreign interventions in elections are a common phenomenon frequently used by various foreign powers for centuries for very similar reasons. As a result, there is little that was new or unprecedented about the Russian intervention in the 2016 US elections except for the use of the internet-which was largely a high-tech twist on a traditional electoral intervention method. Likewise, many of the conditions that lead to such meddling, such as local political actors with strong political incentives to request foreign assistance or accept such foreign electoral aid offers are expected to continue to be present in many democracies, both western and nonwestern, the near future. Accordingly, we should expect many more such interventions in the future in the US and in other democratic countries.

The second lesson is that electoral interventions have significant effects on the results, enough in many cases to determine the results of the intervened elections. Accordingly, fears of many American and Western policymakers of such meddling are justified- as is the search for policies designed to detect such meddling attempts or dull their effects.

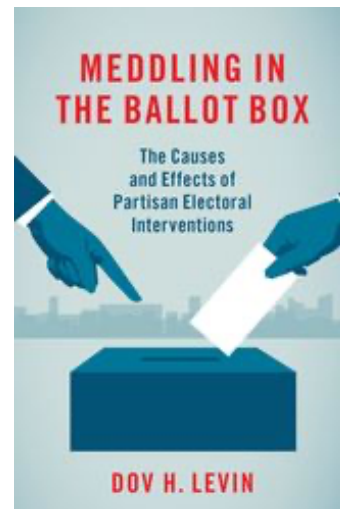
What tips would you give graduate students or junior scholars interested in historical methods?

First, don't assume that if qualitative research doesn't involve statistical packages or fancy math you don't need to carefully learn how to do it. If your department doesn't teach classes on this topic, consider attending the MQMR workshop on qualitative methods in general or, more specifically, the Summer Institute on Conducting Archival Research at George Washington University. At the very minimum make sure to carefully read good guidebooks on doing such research and proper case selection such as Marc Trachtenberg's *The Craft of International History*, John Gerring's *Case Study Research* and of course Gary King, Robert Keohane and Sidney Verba's *Designing Social Inquiry*.

Second, before diving into the primary sources look for and acquaint yourself with any other historical or secondary literature on that or related topics, if available. That will speed up getting into the thicket of things and help make sure that you don't miss important documents or key insights from reading them.

Third, it's harder and more time consuming than it looks from reading the "final product". For example, every paragraph in a well- done historical case study involves first studying carefully a large number of archival documents with the citations referring only to the handful most relevant documents in this regard. Likewise, as you work on a case, you will sometimes discover that you need to investigate some related aspects (say learn more about a certain issue in dispute between two countries) in order to better understand decision-makers

behavior or that you'll need to provide additional exposition to readers, so they understand the key developments. Accordingly, budget your time to make sure that you have sufficient time to write such cases properly.



Co-Winner of the Robert L. Jervis and Paul Schroeder Best Book Award

Kyle M. Lascurettes *Orders of Exclusion: Great Powers and the Strategic Sources of Foundational Rules in International Relations* [Oxford University Press](#)

How did you become interested in the intersection between international history and politics?

I don't think I would have ever become interested in international politics had I not been allowed and encouraged to make grand comparisons across history. I remember signing up for an American foreign relations class as an undergraduate, then not really being excited about talking about the details of the controversial foreign policy topics of the day. It was only when we turned to drawing huge comparisons across decades and centuries of American diplomacy to look for major commonalities and discontinuities that I became truly excited about the subject. I know some people find macro-historical studies to be too speculative, but I feel the opposite. To me, a lot of political science feels too focused on

narrow slivers of time and incredibly specific structural circumstances. I know those studies are still useful, but I personally wouldn't be in this profession if I couldn't ask huge questions that span time and space. In sum, I think I've always thought of history as a vital part of what the study of international relations is.

In your book, you explore nine key moments of foundational rule writing throughout history, from 1648 to 1991. What was your criteria for case selection and were there any cases that you wanted to include but did not make the final cut?

I chose the nine case studies of *Orders of Exclusion* by looking at the aftermath of two types of shock to the international system: major wars and great power deaths. I categorize the periods following instances of

each as “order change opportunities” because they are the historical moments when preponderant powers have the capacity and inclination to undertake significant order changes. It was important for me to examine “opportunities” for order change rather than simply “order changes” themselves, however, so that I could include negative cases where change was possible but not chosen. This allowed me to examine both logical sides of each theoretical coin I was testing, since the presence of X causing Y should also imply that the absence of X will result in an absence of Y. More concretely, it allowed me to show how low or declining perceptions of threat (the absence of X in my theory) led to the decisions against significant order changes (the absence of Y) in 1848, 1856, and 1989 even as the relevant powers had opportunities to pursue such changes at these moments. Originally, I had hoped to include several additional cases—order building at and after the Hague Conferences and Soviet order building in their own sphere after World War II, for instance. But because I worked hard to use deduction (using logical reasoning to determine the type of cases to look at) rather than induction (choosing cases I already knew to be important based on their historical legacies) in selecting the case studies, I had to exclude cases like these that are fascinating to me but didn’t fit my deductive criteria for inclusion.

Are there any scholars that you look to as role models? Or pieces of scholarship that you view as being templates for excellent research?

The scholars and works I most enjoy are those that strike the right balance between the comparable and contingent aspects of their historical work and analysis. They command enough of a master narrative to keep the focus on the most important and generalizable aspects of each case most germane to their argument. Yet they also recognize that great history—even historical case studies—must remain interesting and readable. So they include enough “side trips” off of the main narrative thruway to allow the reader to explore the nuance, context, and richness of each historical moment or era. John

Ikenberry’s *After Victory* is exemplary in this regard. And some books of the last decade or so that I think do this especially well are John Owen’s *The Clash of Ideas in World Politics*, Dale Copeland’s *Economic Interdependence and War*, Seva Gunitsky’s *Aftershocks*, and Stacie Goddard’s *When Right Makes Might*. I quite literally kept these books close by as I was writing and revising my own case studies in *Orders of Exclusion*.

How do you navigate the tension between detailed historical research and macro theoretical claims; between contingency and generalizability?

This is a great question, and I do not have a great answer. All I can really say is that I navigate these perils by being as honest and transparent as possible about the major aspects and minute details of each case study that fit with my overarching argument and those that do not.

What was the most challenging aspect of working with the historical material used in the project?

The hardest part was interpreting which issues of each case period constituted the “order” aspects of the case in terms of affecting the more general rules of international relations. And then beyond that, figuring out how much adjustment was necessary in these rules to code a case as one of “order change” instead of “order continuity.” On these challenges as well as others, I found the best remedy was simply being as transparent as possible in explaining my coding decisions within each case and then briefly justifying these decisions. People won’t always agree with every single interpretation you have made, but I’ve found that they are much more understanding if you explain the reasoning behind your choices.

What was the most unexpected thing you found in conducting your historical research?

I think it is assumed that the elites of more recent times have been more farsighted when it comes to international order building than

those of the past. It is certainly true that order building became more complex and complicated in the 20th century, and particularly after World War II. But over the course of my research, I was surprised by how strategic and farsighted even those statesmen at Westphalia, Utrecht, and Vienna were in prior centuries. They might not have known they were inventing core concepts like “sovereignty” and “balance of power.” But they understood that they were crafting postwar settlements with the potential to last far beyond the specific circumstances of the most recent war. So the degree to which they actually understood that they were making or remaking “international order” was surprising to me.

What do you think are the major differences in how political scientists and historians “do” history?

I think that historians are often able to explore and highlight the nuances and discontinuities of particular historical episodes without necessarily having to worry about those things contributing to an overarching argument or theory. By contrast, political scientists are supposed to remain relentlessly focused on only those aspects of history that contribute to the larger argument or theory. It is probably not surprising for me to say that I think each camp can learn a lot from the other. Political science would be richer for highlighting more of the historical nuance and contingency that makes historical work so interesting, while diplomatic history would at times benefit from being a bit more systematic about abstracting away from the particularities of an episode or era and advancing a more general argument.

What would you like to see more of in terms of research into international history and politics, either methodologically or substantively?

I think it’s time for much more IHAP work off the beaten path of the Westphalian states system of Europe, America, and the traditional great powers of the Global North. I don’t necessarily mean no longer focusing on those

actors and cases, but doing a better job of systematically comparing them to far less studied eras, systems, and actors. There is of course scholarship that already does or is doing this well—Victoria Tin-bor Hui’s *War and State Formation in Ancient China and Early Modern Europe*, Andrew Phillips and J.C. Sharman’s *International Order in Diversity*, and Hendrik Spruyt’s *The World Imagined* to name but a few excellent examples—but in my opinion much more needs to be done.

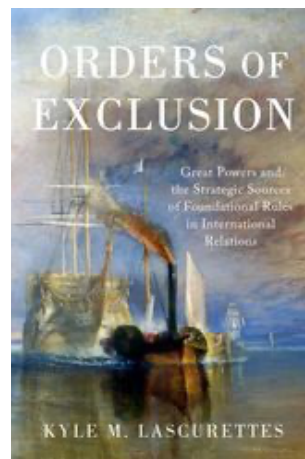
What do you think are the biggest lessons that publics and/or governments should take from your work?

First, and when it comes to international order, great powers are not magnanimous even in triumph. Instead, they have often seized moments of opportunity throughout history to revise the rules of order in ways that will hamstring their perceived challengers and rivals in the future. Second, the United States has been no exception to this approach, and American-led orders have been as much about constraining perceived rivals as prior ones. Third and most importantly, it is wishful thinking to view ‘international order’ as a force that will ameliorate growing tensions between the United States and China. Instead, exclusionary rule writing on the international stage will be one of the major arenas in which Sino-American competition will play out. The larger implication here is that international order cannot serve as a lifeboat for fixing the relationship between China and the United States. Rather than a repudiation of *realpolitik*, international orders have been built throughout history to serve as its very instruments.

What tips would you give graduate students or junior scholars interested in historical methods?

If historical work is what interests you, don’t worry about how “marketable” it is. There will always be a market and audience for great work regardless of how “fashionable” the methodology or topic is considered to be. So, if historically big and unwieldy questions are the ones that fascinate you, don’t be afraid to

ask them! At both the dissertation and book phase, I was sometimes advised against pursuing such a historically ambitious project. I ignored that advice, less because I disagreed with it and more because I couldn't imagine being interested enough in a less ambitious project to pour my heart and soul into getting it done. There were days (and maybe years) when I regretted it. But in the end, I'm so glad I stuck with it.



Honorable Mention for of the Robert L. Jervis and Paul Schroeder Best Book Award:

Lora Viola *The Closure of the International System How Institutions Create Political Equalities and Hierarchies* [Cambridge University Press](#)

How did you become interested in the intersection between international history and politics?

If I reflect on the core puzzle that has motivated my research so far, I would characterize it as trying to understand the dynamic relationship between continuity and change in political institutions. By training I am a political scientist, and not a historian, so I address this issue from a distinctly political science perspective. But there is no way to identify continuities and changes—not to mention explain them—without thinking about history. In my work on historical institutionalism, for example, I am interested in questions like: How can we conceptually define and empirically distinguish change from continuity? When is change fast or slow and how does sequencing matter for outcomes? How much of the past influences the present and in what ways? How do the timeframes we choose affect the patterns of change and continuity that we identify and the explanations that we find convincing? In *The Closure of the International System*, my interest in history is motivated by the way a long timeline can relativize or denaturalize hegemonic theories and bring larger patterns into relief.

In your book, you challenge the conventional view that international institutions are modes of inclusion, democratization, and expansion, and argue instead that the history of the international system is a series of institutional closures. What was your driving impetus in writing this book to show that inclusion and exclusion were acting in tandem all along?

The Closure of the International System is a response, in the first instance, to the dominant narrative of progressive change towards more open, inclusive, and democratic global governance institutions that is more or less explicit in much of the institutionalist literature within IR. I think that this narrative of progress is skewed or, at least, incomplete because it typically is based on a relatively short time frame (post-1945) and a narrow reference point (the Western sovereign nation state as the main unit of analysis). I don't dispute that there has been a meaningful expansion of rights for certain actors, but I want to highlight that this is only part of the story. What I hope to show is that when we take a longer time horizon and a broader reference point of who might potentially count as a legitimate international actor (e.g., individuals, civil society groups, religious communities, indigenous communities, non-governmental organizations, private firms, and so on), then the

progress we seem to observe has come at the cost of political exclusions that have narrowed and homogenized the set of actors considered rights-holders at the international level. Put even more strongly, I argue that the institutionalization of political equalities and inequalities are not antithetical but—for both constitutive and causal reasons that I lay out—the achievement of political equalities for some is the result of a dynamic of exclusion that institutionalizes political inequalities for others. Although the book does not engage in normative theorizing, I think this point is ultimately important for thinking about questions of global justice.

How do you navigate the tension between detailed historical research and macro theoretical claims, between contingency and generalizability?

This is, of course, a point that has been much debated by historians, especially historians of global history. From my point of view, I don't see a genuine tension between micro and macro levels of inquiry because both are necessary for understanding political developments. While any single study will likely have to emphasize one or the other of these levels, and this often entails a choice in the logic of inference, they are usefully thought of as mutually reinforcing. Contingencies can bring powerful insights especially as they relate to larger patterns, and macro perspectives often rely on insights gained from the micro level. My own work in *The Closure of the International System* clearly takes the macro perspective and focuses on identifying large-scale patterns in the spirit of historical sociology, but in doing so it rests on examples from particular periods, institutions, events, and thinkers that were chosen because of their significance to the overall development of the international system.

What was the most challenging aspect of working with the historical material used in the project?

Historians often specialize in a specific period, and in researching my chapter on early modern diplomacy in the Italian city-states, I came to appreciate why a contextual understanding of a period is so important. I think we are generally

prone to read and interpret historical material in terms of our own political moment, our own political knowledge. But this is particularly problematic when trying to understand periods and places that are not at all—socially, politically, culturally—like our own experiences. This challenge was most apparent to me in researching early modern European diplomacy. The very concepts we use to organize political thought today—the sovereign nation-state, the international system, the separation of public and private spheres, for example—are not meaningful in that period and anachronistically using those concepts to understand the politics of the period risks distorting how we understand historical trends and developments. Historians have done important work in mapping diplomatic and political relationships from that period and situating them in their social and cultural context, and I came to lean on that work even as I applied my political science lens to it.

What was the most unexpected thing you found in conducting your historical research?

My research did not aim to unearth any unknown historical facts, but one of the things that I found surprising is how many different claims are made in contemporary IR literature about the periodization of sovereign equality as an idea and as a practice. Many political scientists argue that sovereign equality became a principle of the international system relatively recently, but there is much disagreement about when. Some scholars argue that the principle of sovereign equality became central in the nineteenth century, others argue that this only happened when it became enshrined in the UN Charter in the mid-twentieth century, and still others argue that the principle was only “really” achieved after decolonization. As I discuss in the book, the idea itself can be traced back to seventeenth- and eighteenth-century jurists who were formulating early international law. I think an argument can be made that even in this early formulation, the principle had significant ordering effects. As a practice, however, sovereign equality has always been aspirational rather than “fully” achieved. Indeed, I argue that the incomplete application of the sovereign equality principle is not merely an institutional shortcoming or an empirical failure,

but precisely part of what international institutions are designed to do.

What do you think are the major differences in how political scientists and historians “do” history?

In my view, the conventional wisdom that political scientists tend to focus on theory-based explanations that can be generalized, and historians tend to focus on empirical narratives in their context and complexity, still broadly holds. At the same time, though, the increase in inter- and trans-disciplinary research over the last few decades has grown awareness for how others work and has fostered cross-fertilization. One result, I think, is that many political scientists are aware of the different uses of history beyond using the past as a context-less source of evidence for verifying hypotheses, while many historians are drawing on methods and theories from the social sciences for their research.

What would you like to see more of in terms of research into international history and politics, either methodologically or substantively?

Analyzing events over long periods of time and paying attention to temporal development offers significant explanatory leverage. One of the ways it does this is by bringing to light alternatives, discontinuities, and contingencies in how events and structures unfold over time. A lot of this potential is lost in the field of IR, however, because of its relatively narrow focus on the post-1945 period. In the case of the international institutions literature, apart from some important exceptions, much of the cutting-edge research is focused on the post-1990 period. I think that this research would be enriched by taking a significantly longer time horizon into account in order to contextualize and historicize political trends, their causes and consequences. Recent research on institutional design, regime complexity, institutional legitimacy, or on rising powers and hegemonic decline, for example, are areas where long time-horizons can help to trace dynamics of change and continuity and to identify how endogenous and exogenous factors interact to motor that change. There is already

important work being done in this direction, but I'd like to see more of it.

What do you think are the biggest lessons that publics and/or governments should take from your work?

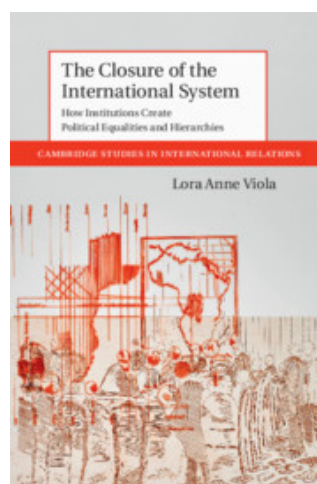
The Closure of the International System has a few implications for politics. First, the book argues that the question of acceptable grounds for extending rights cannot be separated from the question of acceptable grounds for restricting rights. In this light, the book asks us to reconsider 1) who should be a legitimate rights-holder at the international level and, in particular, whether rights ought to be extended beyond the sovereign state, and 2) how the rules of international institutions reinforce political and material inequalities among existing rights-holders. Second, the book suggests that mitigating the institutionalization of political inequality requires revisiting the role of property rights in creating material incentives for inequality and, in particular, that this requires a politics of global redistribution. Third, the book asks us to reconsider the widely held view that international institutions provide global public goods and to consider, instead, how the extent of the “public” that has access to collective goods is endogenous to the rules and designs of institutions. This is practically relevant for decisions about which actors get access to what kinds of globally relevant resources—an issue we've recently seen in decisions about the production and distribution of COVID vaccines. While the book does not offer concrete policy prescriptions, it does suggest that current political challenges to the liberal international order and to globalization could be opportunities to open discursive and political space for renegotiating existing distributions of resources and rights.

What tips would you give graduate students or junior scholars interested in historical methods?

Historical methods are not usually taught as methods in political science programs, and there is often the misunderstanding that there is no “method” to doing historically-oriented work. So, I would encourage students of political

science who intend to do historical research to, first of all, reject the notion that they do not have a method and to reflect on the epistemological foundations of their research. One of the most promising aspects of cross-disciplinary research is mixed-methods analysis, and much historical work draws on a range of methods, including statistics, discourse analysis, and ethnography. Political scientists interested in history are particularly well-positioned to engage in mixed-methods analysis, but this might require reaching out beyond the political science department to get additional training from historians, geographers, ethnographers, and other related experts. Beyond any specific technique, though, using historical methods means engaging in historical thinking, which I would describe as an analytical approach that pays special attention to multiple contextual

frames, historical complexity, contingency, and change over time.



Highlighting Recent Publications in International History and Politics

The following recent publications have been written by IHAP members or are of interest to those who study international history and politics. If you would like your publication featured in the fall newsletter, please email the newsletter editor John Emery: jremery@uci.edu

- Davidson, Jason. *America's Entangling Alliances: 1778 to the Present*. Washington, DC, [Georgetown University Press](#), 2020.
- Emery, John R. "Moral Choices Without Moral Language: 1950s Political-Military Wargaming at the RAND Corporation" *Texas National Security Review* 4 no. 4 (2021):
- Maass, Richard W. *The Picky Eagle: How Democracy and Xenophobia Limited U.S. Territorial Expansion*. [Cornell University Press](#), 2020
- Schulhofer-Wohl, Jonah. 2020. *Quagmire in Civil War*. [Cambridge University Press](#), 2020.
- Sterling, Brent. *Other People's Wars: The US Military and the Challenge of Learning from Foreign Conflicts*. [Georgetown University Press](#), 2021.
- Stewart, Megan A. *Governing for Revolution: Social Transformation in Civil War*. [Cambridge University Press](#), 2021.
- Stewart, Megan A., and Karin E. Kitchens. "Social Transformation and Violence: Evidence from US Reconstruction." *Comparative Political Studies* (2021): DOI: 0010414021997164.
- Williams, Brad. *Japanese Foreign Intelligence and Grand Strategy: From the Cold War to the Abe Era*. [Georgetown University Press](#), 2021.
- Yeo, Andrew and Stacie Pettyjohn. "Bases of Empire? The Logic of Overseas U.S. Military Base Expansion, 1870–2016." *Comparative Strategy* 40, no. 1 (2021) 18–35.