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INTERNATIONAL HISTORY

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In This Issue

International History and Politics (IHAP)

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Message from the Chair

Dear IHAP Members

This excellent newsletter considers the challenge adapting standard IR theory to understand questions of import in other parts of the world. Together the contributions consider whether our discipline should be moving to less American and European IR perspectives—an apt question to ask right before the truly international ISA conference.

The contributions raise many good points, and after all who can defend insisting on the relevance of theories that are not helpful in understanding politics in other parts of the world? To play the devil's advocate, however, let me raise a concern. In IR, theory is primarily a heuristic used to generate expectations (or serve as a foil). We may as well expand the heuristic toolkit so as to better capture reality, but there is also a need to keep toolkit limited. I teach on the quarter system, which means we have 9 weeks for a graduate IR theory course. Since we want to also cover subjects, and not just cannons, we don't want 9 weeks of different theories. Nor do our graduate students want to double the potential explanations they need to investigate.

If expanding theories increases explanatory power, then there is little to lose in expanding our toolkit. But the challenge then becomes how to consolidate criticisms of American or Euro-centric approaches into one or maybe two new paradigms? Moreover, if the goal is to internationalize IR, these also need to be fairly simple paradigms that students around the world (e.g. non-native English speakers) can grasp on to.

In the other area I research—International Law—the challenge is of a different nature. Anthea Roberts's new book, *Is International Law International?*, considers how the "universalist" subject of international law is taught in different parts of the world, as well as the different careers of international legal academics around the world. Roberts—who has taught international law in the US, UK and Australia—collected syllabi, textbooks and CVs from different parts of the world, and she conducted interviews with leading international law faculty from the countries of focus. She found that legal ideas and the foundation international legal concepts are actually explained differently, and different topics are given different levels of emphasis. These differences increase the likelihood of conflict because legal advisors in different parts of the world understand their legal obligations differently. To be sure, some interpretations and teachings may be self-serving. One might expect, for example, that Chinese scholars are taught an international law that validates China's claim to the South China Seas, and invalidates the arbitral ruling that says otherwise. Yet, Roberts study suggests that an entire profession inside of China—lawyers, judges and academics—can come to see international law very differently from their European and American counterparts. The resentment which might then arise when lawyers "explain" to each other why the other's argument is wrong will not help to alleviate differences. The China situation is only somewhat different than what Lauri Mäilksoo describes when he considers Russia's arguments about the "illegal" sanctions being applied in light of its annexation of the Crimea. In short, there may not be any common set of "legal facts" or transnationally shared legal understandings.

If IR theory were understood differently in different parts of the world, the result may not be conflict. Rather, we may find that scholars from outside of the US and European orbit might have a difficult time publishing in the field's top journals, because reviewers may believe that non-Western scholars simply misunderstand IR theory. This raises the question of whether IR needs a book that asks *Is International Relations Theory International?* Or, maybe we don't need new paradigms as much as we need a map to what realism, constructivism, liberalism, practice theory etc. look like to scholars in different parts of the world?

This newsletter also contains thoughtful interviews with the winners of our section's book and article prizes. The authors are asked to offer tips to young scholars, to explain how they work historical insights and approaches into their work, and how their historically oriented work differs from that of historians.

Thanks to Peter and Tom for another excellent newsletter. I should note that this newsletter is a little less international than they had hoped; the lecturer strike in the UK has affected its content.

Happy Spring to all. One last thing—if you are interested in becoming more active in IHAP, we will be looking for new members of our executive committee. Please let me know if you are interested.

Karen J. Alter
Professor of Political Science and Law, Northwestern University

<u>Roundtable</u>
IR Theory across Space and Time

Introduction: IR Theory across Space and Time

By Peter Harris, Colorado State University,
Tom Le, Pomona College,
Hyeyoon Park, Colorado State University, and
Erika Sato, Pomona College

There is some irony in the fact that International Relations—the only academic discipline devoted in its entirety to the study of global interactions—should be so narrowly constituted in terms of its geography. Overwhelmingly, the ranks of IR scholars are drawn from white-majority Anglophone countries, with voices from the Global South being underrepresented in the discipline's prominent journals as well as in most conventional accounts of its intellectual history.

One consequence of IR's heavy skew in favor of white Anglophone authors is that the field has tended to rely on empirical evidence derived from the international histories of Europe and, to a somewhat lesser extent, North America when building and testing its vast corpus of analytical concepts and explanatory theories. In this roundtable, four contributors discuss whether this traditional empirical focus on the North Atlantic region has saddled the discipline of IR with a set of theoretical paradigms that are pathologically limited in their usefulness. What are the challenges and opportunities that arise when scholars generate a theory of international politics with reference to one region or time period and then to apply that theory to other regions or eras? Can IR theories be truly generalizable, or is their scope always bound by space and time? What are the general implications of "Eurocentrism" for IR scholarship?

Kick-starting the discussion, Pinar Bilgin ("Why Globalise International Relations?") suggests that, indeed, IR theory suffers from a marked Eurocentric bias. Bilgin explains that, inside the Western core of IR, the project of "globalizing" IR is often portrayed as the antidote to this problem of Eurocentrism. From this view, IR would be "fixed" if it could incorporate the theoretical innovations of scholars from a greater variety of cultures and countries. But Bilgin contends that globalizing IR will take more than just pluralism: it will also require scholars of IR to examine how the existing canon of IR-theoretical knowledge has already been influenced by non-Western voices, often in highly significant yet overlooked or (willfully) forgotten ways.

Next, May Darwich ("Analytical Eclecticism: Appraising the Study of Middle East International Relations") examines the capacity of IR theories for shedding light on the contemporary international politics of the Middle East. Drawing on Rudra Sil and Peter Katzenstein's work on "analytical eclecticism" in International Relations, Darwich contends that an eclectic approach to studying the Middle East can help push scholars in the direction of more problem-driven (instead of paradigm-driven) research. At the same time, she argues, "eclecticism as an emerging IR approach can benefit from the richness and complexity of cases in the Middle East to develop further connections and links among different theoretical traditions."

Graham Odell ("Mechanisms, Episodes and IR Theory") provides a direct answer to the question of whether IR theories can profitably be applied to empirical cases that span space and time. His injunction is for scholars to use theories as tools to explain discrete "mechanisms" and "episodes" rather than aggregated historical processes. While politics across space and time might vary considerably, Odell acknowledges, scholars need not entirely forsake the goal of producing generalizable knowledge about what drives political interactions.

Finally, David Kang ("The Challenge of East Asia for International Relations Theories") offers a focused analysis of one Western-generated IR theory (power-transition theory) and its applicability to one non-Western region (East Asia). Overall, Kang cautions that power-transition theory makes little sense when applied to historical East Asia. Different types of political regimes and, crucially, East Asia's distinct international-systemic context meant that the region simply experienced the rise and fall of great powers in a way unlike Europe. "Perhaps more of our theories are more specific and contingent than is commonly believed," Kang concludes, meaning that "carefully identifying those scope and boundary conditions will be critical moving forward."

In many ways, International Relations as a discipline is undergoing an experience that parallels events in the real world: just as "globalization" as a project of social, political, and economic homogenization is being met with local resistance around the world, so too is the idea of a "one-size-fits-all" International Relations coming under increasing strain. Old truisms about the veracity and universal applicability of traditional IR theories no longer seem secure. And even if the response need not be anything as extreme as the fragmentation of IR into multiple country- or region-specific IRs, careful reflection about local circumstances, knowledge, and historical geographies is likely to reveal that IR still has a lot to incorporate. The discipline will be the better for it.

there is no need to globalise IR, for it is already global—not only in terms of its reach but also by virtue of producing universally applicable 'scientific' knowledge about world politics.

Why Globalise International Relations?

By Pinar Bilgin, Bilkent University

Is not the study of international relations (IR) already global? After all, academic departments at universities around the world are devoted to the study of world politics; annual meetings of the International Studies Association consistently attract 5000 or more researchers from all around the globe; and regional IR conferences convene regularly in myriad locales. In terms of its reach, International Relations as an academic discipline is already global.

Over the years, sociological analyses have shown that IR is studied differently in different parts of the world. Yet, the point of globalising International Relations is *not only* to inquire into the different ways that world politics is studied and understood in the global South; *but also* to interrogate critically the way IR is studied inside the global North. While IR as a field of study may be global in terms of its reach, the same cannot be said for its substance.

"Not everybody is convinced of the need to globalise IR."

That said, not everybody is convinced of the need to globalise IR. After all, some sceptics say, IR produces universally applicable 'objective' knowledge about world politics by virtue of being a 'scientific' product. Global variations in the study and teaching of IR, they suggest, are a result of less-than-proper training in scientific methods. While IR may have originated in Western Europe and North America, they insist, this is only an accident of history and therefore not consequential for the production of knowledge about world politics. Accordingly, some sceptics conclude, there is no need to globalise IR, for it is already global—not only in terms of its reach but also by virtue of producing universally applicable 'scientific' knowledge about world politics.

However, IR's origins in the US and Western Europe is not inconsequential for what we know about world politics. Accordingly, considering the implications of such locatedness and seeking to address the limitations that follow is no mere subject of academic curiosity. This is a point raised earlier by E.H. Carr who noted that the study of world politics had, for long, reflected the perspectives and concerns of the 'mighty'. The state of IR at the time was not sustainable, argued Carr. He expected the less powerful to begin to make their voices heard in world politics as well as its scholarly study.[1] In the early 1980s, K.J. Holsti echoed Carr when he invited students of IR to take stock of the field and see '*who* does the theorising?' [original emphasis], for he expected the findings to have significant implications for the study of world politics. This was because, Holsti wrote, IR 'reflected the historical experience of the European state system in the past, and the Cold War more recently' and that one should expect 'serious challenges' to come from those who did not share these experiences or experienced them differently. 'The problem of what *kind* of theories we use to understand and explain the world of international politics is not divorced from *who* does the theorising', he concluded.[2]

To recap, there are two related dynamics behind the calls for globalising IR. First, sociological analyses of the field of IR revealed it to be 'not so international'.[3] That is to say, there are relatively few global South contributors to IR publications. Second, IR's understanding of 'the international' is less-than-sociological.[4] The point being that existing body of studies do not reflect interactions between the global North and the global South in the constitution of the realm called the 'international'. Prevalent conceptions of the international have come to prevail by overlooking the experiences, contributions and contestations of peoples and states from the global

[1] E.H. Carr quoted in Tarak Barkawi and Mark Laffey, "The Postcolonial Moment in Security Studies," *Review of International Studies* 32, no. 2 (2006): 329-352, at 349.

[2] Kalevi J. Holsti, *The Dividing Discipline: Hegemony and Diversity in International Theory* (Boston: Allen & Unwin, 1985), 118.

[3] Ole Waever, "The Sociology of a Not So International Discipline: American and European Developments in International Relations," *International Organization* 52, no. 4 (1998): 687-727.

[4] Stephen Chan, "Cultural and Linguistic Reductionisms and a New Historical Sociology for International Relation," *Millennium—Journal of International Studies* 22, no. 3 (1993): 423-442; and Justin Rosenberg, "Why Is There No International Historical Sociology?" *European Journal of International Relations* 12, no. 3 (2006): 307-340.

South. Hence the concept used in reference to the latter: 'constitutive outside'.[5]

'Constitutive outside' refers to the ideas and experiences of those people and states in the global South who have shaped the global North even as the latter are not always aware of and/or acknowledge what they owe the latter.[6] This is often due to the prevalence of Eurocentric narratives on world history that do not always reflect the contributions and contestations of the global South.[7] Accordingly, the concept of constitutive outside highlights a contradiction that is central to thinking about the relationship between the global South and the global North. That said, this is not a contradiction to be resolved, but only acknowledged and thought through. For, the global South's ideas and experiences have shaped world politics and yet these contributions and contestations have not been acknowledged explicitly in scholarly studies on the international. What is absent, in other words, is not contributions from the global South per se, but their due recognition in scholarly studies on world politics.

Consider, for example, Siba Grovogui's archival study on the contributions of African intellectuals to European debates on the post-World War II order in Europe.[8] While these intellectuals' contributions and contestations shaped debates during World War II, Grovogui showed, their contributions were not always acknowledged when the intellectual history of this period was written. Nor was their advice regarding the post-war order given due value, noted Grovogui. Once the war was concluded in a way that was favourable to the allies, the camaraderie between European and African intellectuals that was formed during the war came to an abrupt end. The point being that understanding the global South as 'constitutive outside' of the global North is not a contradiction to be resolved, but only acknowledged and thought through as regards their implications for the study of world politics. Those who are 'outside' are outside not always because they are physically far away (i.e. in the global South) but because they have been left outside of conventional narratives on world history due to the prevalence of Eurocentrism in history writing.[9]

Following Blaney and Inayatullah, I suggest that our strategy for globalising IR should be one of 'excavation' and not one of adding another body of theory.[10] As well as asking 'Why is there no non-Western IR Theory?',[11] we could also ask, as Arif Dirlik did: why is it that we search for the kind of IR theory that 'we' are accustomed to when we look at other parts of the world.[12] Such a question would then highlight how our inquiries into IR scholarship around the world are conditioned by pre-existing definitions as to what counts as IR 'theory'. The point being, identifying the issue as one of 'absence' of IR theory outside the US and Western Europe identifies the problem in self-centred terms by formulating the problem in terms of others not doing things (theorizing) in the way that 'we' are accustomed to. As captured by the notion of 'constitutive outside', what is 'absent' may not be theorizing per se, but due recognition and acknowledgement of the ways in which others' ideas and writings have already shaped 'our' thinking about the international.

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[5] David L. Blaney and Naeem Inayatullah, "International Relations from Below, " in Christian Reus-Smit and Duncan Snidal, eds., *The Oxford Handbook of International Relations* (Oxford: Oxford University Press, 2008)

[6] Stuart Hall, "When Was 'the Post-Colonial'? Thinking at the Limit," in Iain Chambers and Lidia Curti, eds., *The Post-Colonial Question: Common Skies, Divided Horizons* (London: Routledge, 1996).

[7] Pinar Bilgin, "Beyond the 'Billiard Ball' Model of the International," *European Political Science* 15, no. 1 (2016): 117-119.

[8] Siba N. Grovogui, *Beyond Eurocentrism and Anarchy: Memories of International Order and Institutions* (New York: Palgrave Macmillan, 2006)

[9] Bilgin, "Beyond the 'Billiard Ball' Model of the International."

[10] Blaney and Inayatullah, "International Relations from Below."

[11] Amitav Acharya and Barry Buzan, "Why Is There No Non-Western International Relations Theory? An Introduction." *International Relations of the Asia-Pacific* 7, no. 3 (2007): 287-312.

[12] Arif Dirlik, "Culture in Contemporary IR Theory: The Chinese Provocation," in Robbie Shilliam, ed., *International Relations and Non-Western Thought: Imperialism, Colonialism and Investigations of Global Modernity* (New York: Routledge, 2011).

Analytical Eclecticism: Appraising the Study of Middle East International Relations

By May Darwich, Durham University

In the aftermath of the Arab Uprisings, scholars of Political Science, in general, were impelled to rethink their theoretical tools and concepts in the study of the region. As IR scholars of the Middle East pursued the same endeavour, they found themselves in the same old debate of whether the Middle East is a region where normal rules of IR theory applies.[1] IR scholars with a particular interest in the Middle East have constantly found themselves torn between two clusters: International Relations (IR) literature with little interest in empirical evidence from the region and regional analyses with little interest in theories. Revealing a lack of cross-fertilization between IR theories and region-focused analyses for decades, some scholars — such as Mark Tessler,[2] Morten Valbjørn,[3] and Andrea Teti[4] — have called for an academic enquiry that moves beyond the "Area Studies Controversy" and that is in favour of a dialogue between IR Theory and Middle Eastern Studies.

This inauspicious start notwithstanding, gradually more sophisticated enquiries of international relations of the region emerged. A current review of recent scholarship on the international relations of the Middle East suggests that a different direction is being taken and that both fields are gradually engaged in serious interchanges. Middle East scholars, in particular, have been more keen to engage with IR theoretical approaches. In their endeavor to combine IR theory and empirical puzzles without discounting regional complexities and particularities, many scholars have opted for a *modified IR Theory applied to the Middle East.* This adaptation has followed several strategies. Some scholars have contextualized sociological IR approaches — such as Historical Sociology[5] and the English School[6] — to fit the alleged exceptional characteristics of the Middle East. Most scholars have, however, developed eclectic theoretical frameworks that combine insights from several traditional paradigms to capture the complexity of regional politics.[7] This eclecticism does not neglect assumptions within paradigmatic traditions but rather engages with them in pursuit of empirical and conceptual connections to account for the complexity of international life in the region that no single research tradition can. In the remainder of this piece, I examine the nature of eclectic scholarship on Middle East international relations while linking it to similar trends within IR. I also expound on what the use of eclecticism in the region may reveal about the strengths and weaknesses of this approach and its utility in comprehending Middle East International Relations.

Eclecticism is considered as any approach that moves between and beyond research traditions to explain real-world puzzles. Katzenstein and Sil[8] define analytical eclecticism as "any approach that seeks to extricate, translate, and selectively integrate analytic elements — concepts, logics, mechanisms, and interpretations — of theories or narratives that have been developed within separate paradigms but that address related aspects of substantive problems that have both scholarly and practical significance". Whereas eclecticism has been adopted — sometimes self-consciously but most of the time unconsciously — in the study of the Middle East for decades, IR scholars have only recently started to consider eclectic frameworks as a serious research approach.[9]

[1] Morten Valbjørn, "Strategies for Reviving the International Relations/Middle East Nexus after the Arab Uprisings," *PS: Political Science & Politics* 50, no. 03 (July 2017): 647–651.

[2] Mark Tessler, Jodi Nachtwey, and Anna Banda, *Area Studies and Social Science: Strategies for Understanding Middle East Politics* (Bloomington: Indiana University Press, 1999).

[3] Morten Valbjørn, "The Meeting of the Twain: Bridging the Gap between International Relations and Middle East Studies," *Cooperation and Conflict* 38, no. 2 (2003): 163–173.

[4] Andrea Teti, "Bridging the Gap: IR, Middle East Studies and the Disciplinary Politics of the Area Studies Controversy," *European Journal of International Relations* 13, no. 1 (March 2007): 117–145.

[5] Raymond Hinnebusch, "Toward a Historical Sociology of State Formation in the Middle East," *Middle East Critique* 19, no. 3 (2010): 201–216.

[6] Barry Buzan and Ana Gonzalez-Pelaez, eds., *International Society and the Middle East: English School Theory at the Regional Level* (Basingstoke: Palgrave Macmillan, 2009).

[7] Raymond Hinnebusch, *The International Politics of the Middle East* 2nd ed. (Manchester: Manchester University Press, 2015); F. Gregory Gause, "Balancing What? Threat Perception and Alliance Choice in the Gulf," *Security Studies* 13, no. 2 (2003): 273–305.

[8] Rudra Sil and Peter Katzenstein, *Beyond Paradigms: Analytic Eclecticism in the Study of World Politics* (New York: Palgrave, 2010), 10.

[9] Peter Katzenstein and Nobuo Okawara, "Japan, Asia-Pacific Security, and the Case for Analytic Eclecticism," *International Security* 26, no. 3 (2001): 153–185; Rudra Sil, "The Foundations of Eclecticism: The Epistemological Status of Agency, Culture, and Structure in Social Theory," *Journal of Theoretical Politics* 12, no. 3 (2000): 353–387; Samuel Makinda, "International

In a region with an extremely high level of militarization, neorealist scholars considered the Middle East a typical example where anarchy and insecurity are predominant. Constructivists also found in the incongruence between the norms of sovereignty and identity a puzzle to illustrate the validity of their worldview. Within IR debates, scholars often outline the three mainstream approaches (realism, constructivism, and liberalism) or their variants and then show that one of these approaches is right and the other two are wrong. Some scholars argue that none of these work and opt instead for a new theory or theoretical framework.[10] Scholars of the Middle East have approached these debates from a different perspective. Instead of uncovering the "absolute truth" or developing "universal theories", they are concerned with solving empirical questions and unravel puzzles with substantive significance for understanding the region.

In this endeavor, scholars of the Middle East have often faced with complex phenomena where material structure are as important as normative ones. Henceforth, ME scholars have relied on eclectic frameworks that combine concepts and elements from realism and constructivism. Gause, for example, offers a theoretical framework based on combining several material and ideational elements: the anarchic structure, the distribution of power, state-society relations, and the region's economic integration.[11] Elsewhere, he argues that an understanding of threat perception in the Middle East combines neorealism, constructivism, as well as domestic politics.[12] Along the same lines, Hinnebusch[13] offers a theoretical framework to explain political outcomes in the IR of the region based on combining concepts from neorealism, constructivism, and a variant of Marxist structuralism.[14]

This eclectic standpoint in approaching the international relations of the Middle East has produced problem-driven rather than paradigm-driven research. Despite the numerous benefits from such approach, it entailed costs for the IR-Area Study dialogue. The most common result is that the recent dialogue between IR and the Middle East has been unidimensional where the interchange was limited to theory testing, application, and adaptation to produce sophisticated and complex analyses of various regional phenomena. Yet, the Middle East remains largely invisible in IR theory development, and theoretical debates have hardly engaged with empirical evidence from the region.[15] The Middle East remains missing from major IR textbooks compared to other regions in the world system. In addition, the Middle East is surprisingly missing from the emerging literature on eclecticism in IR.[16]

"Eclecticism does not neglect assumptions within paradigmatic traditions but rather engages with them in pursuit of empirical and conceptual connections to account for the complexity of international life in the region that no single research tradition can."

Furthermore, even though recent scholarship on the international relations of the Middle East reveals that research in the area is characterised by serious interchanges with universal theoretical approaches from IR Theory, the scholarship has lacked a sense of progress in understanding the Middle East; in the

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Society and Eclecticism in International Relations Theory," *Conflict and Cooperation* 35, no. 2 (2000): 205–216.

[10] Katzenstein and Sil, "Eclectic Theorizing in the Study and Practice of International Relations," in Christian Reus-Smit and Duncan Snidal, eds., *The Oxford Handbook of International Relations*, (Oxford: Oxford University Press, 2010), 110.

[11] Gause, "Systemic Approaches to Middle East International Relations," *International Studies Review* 1, no. 1 (1999): 11–31.

[12] Gause, "Balancing What? Threat Perception and Alliance Choice in the Gulf."

[13] Hinnebusch, *The International Politics of the Middle East*.

[14] For other examples of eclecticism, see Bassel Salloukh, "Regime Autonomy and Regional Foreign Policy Choices in the Middle East: A Theoretical Explanation," in Rex Brynen and Bassel Salloukh, eds., *Persistent Permeability?: Regionalism, Localism and Globalization* (Aldershot: Ashgate, 2004), 81–104; Fred Lawson, *Constructing International Relations in the Arab World* (Stanford: Stanford University Press, 2006); May Darwich, "Ideational and Material Forces in Threat Perception: The Divergent Cases of Syria and Saudi Arabia During the Iran–Iraq War (1980–1988)," *Journal of Global Security Studies* 1, no. 2 (2016): 142–156.

[15] Two books are unique in this aspect: Stephen Walt, *The Origins of Alliances* (Ithaca: Cornell University Press, 1987); Michael Barnett, *Dialogues in Arab Politics: Negotiations in Regional Order* (New York: Columbia University Press, 1998).

[16] Despite the rich, eclectic tradition in Middle East International Relations, not a single example from the region has been cited in Sil and Katzenstein, *Beyond Paradigms: Analytic Eclecticism in the Study of World Politics*.

sense that it is not providing cumulative knowledge about unexplained phenomena. The attempts to combine theory with empirical evidence are often random, unsystematic, and lacking methodological reflections. In other words, the eclectic attempts are often driven by intuition rather than building on previous knowledge to make the field better.

Therefore, the emergence of 'analytical eclecticism' within IR debates bears many potentials for studying the Middle East and other regions of the Global South. On the one hand, the theoretical development of 'analytical eclecticism' can provide Middle East scholars with an invaluable toolkit to capitalize on emerging connections between IR paradigms. Furthermore, a self-conscious eclectic approach to the Middle East will enhance the visibility of the region in the IR literature. As analytical eclecticism can provide an umbrella for intriguing analyses of the Middle East, eclecticism as an emerging IR approach can benefit from the richness and complexity of cases in the Middle East to develop further connections and links among different theoretical traditions.

Mechanisms, Episodes and IR Theory

By Graham F. Odell, Chapman University

Whether Western-derived IR theory is applicable to other world regions is a core debate in IR scholarship. For instance, there has been considerable disagreement on the validity of using such paradigms as realism and liberal-institutionalism to explain East Asian international relations. This debate often treats these paradigms as whole entities in competition with each other, with their truth claims wholly applicable or not at all. Scholars disagree, for instance, on whether balancing theory explains East Asian IR. A related debate deals with whether regional dynamics are best viewed through a Westphalian or hierarchical lens. If paradigms and their associated theories are left as whole entities comprised of general claims, then this debate may be left unresolved. However, I argue that theories can be most relevant to formulating explanations for diverse contexts through their disaggregation into mechanisms. Such disaggregation enables analysts of specific empirical cases to construct explanations that do not contort social reality into preconceived theoretical lenses while still recognizing the potential for at least some degree of generalization.

"This emphasis on mechanisms and episodes, rather than general theory, points a way forward for transcending disagreements over the (in)applicability of IR theory to non-Western regions."

This approach takes inspiration from Sil and Katzenstein's advocacy for analytic eclecticism[1], but most directly draws from McAdam, Tarrow and Tilly's work on mechanisms and episodes. The latter trio of scholars argues in favor of "shifting the search away from general models that purport to summarize whole categories…and toward the analysis of smaller-scale causal mechanisms that recur in different combinations with different aggregate consequences in varying historical settings."[2] McAdam et al. structure their approach around a number of concepts, though I focus on their definitions of mechanisms and episodes. Episodes are specific instances of some concept that the analyst wants to explain or understand. Mechanisms, in turn, are "a delimited class of events that alter relations among specified sets of elements in identical or closely similar ways over a variety of situations."[3] The identification of a single mechanism is not enough to explain a particular episode. But wherever the mechanism is operational, a clear pattern should emerge concerning the immediate consequences for relations between phenomena. Any given episode can potentially be explained by the identification of several mechanisms operating in conjunction and/or in series. McAdam et al. emphasize formulating explanations for specific episodes, treating "recurrent uniformities" with skepticism.[4] In other words, their purpose is not to develop general theories, but instead to account for variations across specified historical instances of their social phenomenon of interest.

This emphasis on mechanisms and episodes, rather than general theory, points a way forward for

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[1] Rudra Sil and Peter J. Katzenstein, "Analytic Eclecticism in the Study of World Politics: Reconfiguring Problems and Mechanisms across Research Traditions," *Perspectives on Politics* 8 no. 2 (2010): 411-431.

[2] Doug McAdam, Sidney Tarrow, and Charles Tilly, *Dynamics of Contention* (Cambridge: Cambridge University Press, 2004), 74.

[3] McAdam, Tarrow, and Tilly, *Dynamics*, 24.

[4] McAdam, Tarrow, and Tilly, *Dynamics*, 29.

transcending disagreements over the (in)applicability of IR theory to non-Western regions, a goal shared by other scholars.[5] In other words, a mechanistic-episodic approach encourages the analyst to systematically compare phenomena across diverse contexts, without being hamstrung by the need to develop a parsimonious, general theory. In the remainder of this essay, I use this approach to discuss aspects of the Sengoku Period of Japan (1477-1615), specifically the developments that occurred between 1550 and 1590. This era of Japanese history is especially appropriate for a discussion of the (in)applicability of Western-inspired IR theory because of its peculiar dynamics. Unlike the anarchic, competitive state system of early and late modern Europe that has served as the backdrop for both realist and liberal-institutionalist theories, Sengoku Japan was an anarchic, institutionally-unstable subsystem embedded within a regional hierarchy dominated by China. This two-level structure of international relations, coupled with the persistence of the Japanese emperor as a symbol of (idealized) political unity, renders this context distinctive from standard IR systems. Another special feature is that the era concluded with fundamental system change, as a hegemon emerged who was capable of reuniting Japan under a single authority structure. All of these notable differences suggest that it should be difficult to employ standard IR theories to explain the era's dynamics. On balance, I argue that this is the case, though key mechanisms of many IR theories can in fact be observed in the processes that defined and shaped the interactions between actors. Below, I discuss the applicability of three mechanisms drawn from IR theories in explaining two Sengoku-era episodes organized around the careers of two prominent samurai warlords – Oda Nobunaga and Toyotomi Hideyoshi, respectively. Thus, I employ this era to illustrate the utility of the mechanistic-episodic approach in explaining IR system dynamics.

Balancing, a core mechanism of neorealist IR theory,[6] was a common feature of interunit dynamics throughout Oda Nobunaga's career. During the onset of his rise to power in the 1550s and 1560s, for instance, balancing heavily structured interactions between major samurai families that dominated the region just east of the capital of Kyoto, where the Oda clan got its start.[7] These families not only engaged in external balancing by forming shifting alliances to counter the rising strength of any one that became too powerful, but also pursued internal balancing through encouraging economic development and strengthening political control over their territories. Though he eliminated one of his neighboring rivals (thus ending this local-area balancing system), Nobunaga was confronted with balancing by new foes as he extended his activities into new regions from 1564 through the 1570s.[8] Indeed, his assassination in 1582 may very well have been an effort to retaliate against his self-aggrandizement.[9] Thus, balancing was a prominent mechanism throughout the episode of Nobunaga's rise and fall.

However, other mechanisms also played a role in the dynamics of the Nobunaga era. In fact, Nobunaga himself engaged in strategies that challenge the expectations of balancing. Though his interactions with other samurai warlords were often structured by this mechanism, his confrontation with the militant Buddhist temple on Mt. Hiei was an instance of *domination*.[10] He launched a war of complete destruction against the temple, razing it to the ground in 1571.[11] Other powers did not balance against Nobunaga in response to this particular campaign of his, suggesting that this mechanism operated within certain bounds. Indeed, Tsang argues that the political-conceptual frameworks of the Buddhist sects and the samurai warlords were irreconcilable.[12] Balancing was thus an option for warlords competing against other samurai, but not against other types of organizations.

Our second episode, Toyotomi Hideyoshi's rise to hegemony from 1582 to 1590, shows that balancing in Sengoku Japan was temporally bounded as well. The weakened salience of this mechanism is clear as Hideyoshi, a former Oda vassal, encountered much more limited balancing than did his predecessor. Hideyoshi did launch a series of large-scale military campaigns across the country, but each was directed at a specific front and did not receive a balancing

[5] See, for example, Sil and Katzenstein, "Analytic Eclecticism."
[6] Kenneth Waltz, *Theory of International Politics* (New York: McGraw-Hill, 1979).
[7] Elizabeth Berry, *Hideyoshi* (Cambridge: Council on East Asian Studies Harvard University, 1982), 36.
[8] Berry, *Hideyoshi*, 38-51.
[9] Berry, *Hideyoshi*, 42.
[10] Victoria Tin-bor Hui, *War and State Formation in Ancient China and Early Modern Europe* (New York: Cambridge University Press, 2005).
[11] Berry, *Hideyoshi*, 46.
[12] Carol Richmond Tsang, "'Advance and Be Reborn in Paradise...': Religious Opposition to Political Consolidation in Sixteenth-Century Japan," in John A. Ferejohn and Frances McCall Rosenbluth, eds., *War and State Building in Medieval Japan* (Stanford: Stanford University Press, 2010): 91-109.

response from powerful elites ensconced in adjacent regions. All but one of his foes accepted terms of defeat that guaranteed their survival but also their removal from their established bases of support. Moreover, these subdued samurai warlords carried out Hideyoshi's hubristic invasion of Korea in the 1590s despite their strong reluctance to do so.[13] Their behavior, in other words, was in many ways the opposite of balancing.

Rhetorical action, a mechanism drawn from Schimmelfenig's work on EU expansion,[14] can be used to explain the diminished importance of balancing in this second episode. Rhetorical action, briefly, is a mechanism that involves the utilization of symbolic resources to garner support for public efforts. I argue that rhetorical action was a mechanism that Hideyoshi employed to weaken resistance to his expansionism and consequently undermine the balancing mechanism that afflicted Nobunaga's campaigns. In Sengoku Japan, the emperor served as a normative symbol that actors could utilize to legitimize their self-interested efforts. The samurai class in particular continued to recognize the normative relevance of the emperor and his representation of Japanese unity despite the country's long-running fragmentation.[15] Thus, Hideyoshi's active and continual use of imperial symbolism (including an elaborate system of court ranks and titles) can be described as an instance of rhetorical action that diminished the salience of balancing and, consequently, enabled a transformation from competitive anarchy to consolidated hierarchy.

For both episodes of late Sengoku history (Nobunaga's rise and Hideyoshi's ascension), warfare was a regular and essential feature. Yet, balancing only played a major role in the first, while rhetorical action played a core role in the second. Thus, two mechanisms that have been derived from the European experience can be usefully employed to address non-Western system dynamics. Though the above discussion is far from a satisfactory explanation of how Sengoku Japan operated and ultimately reunified, it points the way forward for how mechanisms drawn from disparate IR theories can be employed in the construction of contextualized explanations for a variety of episodes.

The Challenge of East Asia for International Relations Theories

By David C. Kang, University of Southern California

Can International Relations theories be applied across time and space? Are some theories more time- or space-bound than others? It might seem intuitively obvious that different regions of the world, with vastly different religions, social structures, cultures, political systems, economic systems, and geography, would have different historical patterns of foreign relations, as well. Yet this seemingly obvious point is often masked by the confident assertion inherent in many conventional IR theories developed from the European experience that they are in fact deductive and universal. Yet perhaps a little more humility is in order, and more attention to the scope and boundary conditions for when a theory applies would be a positive step forward. As an example, in this short essay I will sketch the problems with applying power transition theory outside of the European experience in which the theory was originally formed.

There is a view that power transitions between a rising power and a declining hegemon are particularly volatile, and that a war between China and the U.S. could be possible or even likely as a power transition draws near. Scholars and policymakers are increasingly worried about such a possibility. The application of power transition theory to contemporary Asia relies heavily on the analogy of a few key historical cases. For example, Susan Shirk argues, "History teaches us that rising powers are likely to provoke war. The ancient historian Thucydides identified the fear that a rising Athens inspired in other states as the cause of the Peloponnesian War."[1]

But what historical record is Shirk referring to? By far, the most commonly examined case studies of power transition in the scholarly literature are the Peloponnesian War (431-404 BCE) and the rise of Germany under Bismarck and Anglo-German rivalry of the 19th and early 20th centuries.[2] In fact, although

[13] Berry, *Hideyoshi*, 209-213.
[14] Frank Schimmelfennig, "The Community Trap: Liberal Norms, Rhetorical Action, and the Eastern Enlargement of the European Union", *International Organization* 55 no. 1 (2001): 47-80.
[15] Lee Butler, *Emperor and Aristocracy in Japan, 1467-1680: Resilience and Renewal* (Cambridge: Harvard University Asia Center, 2002), 4.
[1] Susan L. Shirk, *China*: *Fragile Superpower* (New York: Oxford University Press, 2007), 4.
[2] This essay draws on David Kang and Xinru Ma, "Domestic Threats, Selection Bias, and East Asian Power

“Researching historical East Asia provides an opportunity to seek out genuine comparisons of international system systems and their foundational components.

“East Asia’s history was nothing like the European experience.”

power transition theory has become a widely-accepted research program, the overwhelming majority of empirical cases examined in the literature are from the European historical experience, while scholars have paid almost no attention to the fairly clear methodological problems in case selection of power transition theory. From the time of Organski and Kugler onwards, almost all cases are drawn from 1816-1975, and by far the two most studied cases are Anglo-German rivalry or the Peloponnesian War. It is troubling that the empirical cases that IR scholars use to derive their theories are essentially all European.

After all, the rise and fall of Chinese dynasties are all potential examples of power transitions, not to mention those in Vietnam, Korea, Japan, and elsewhere. What scholars have not yet directly addressed is whether there are other power transitions in history, and whether power transition theory itself can be transposed outside of the European experience. There is no scholarship that actually asks whether all other regions of the world fall inside of the scope and boundary conditions for the theory to apply.

It could be that mainstream IR scholars have actually included all of the relevant empirical cases of power transition in their research, and in fact there was not one single case of a power transition in Asia. Indeed, it appears that for most of East Asian history, the conditions for power transition theory actually did not obtain, despite the rise and fall of Chinese, Korean, Vietnamese, Cham, Siamese, and Japanese power over time.

The best way to study the contemporary international system is to compare it to something truly different. Because of the triumph of the nation-state system, it is forgotten that other international orders have existed, and might exist again. The current international system is actually a recent phenomenon in the scope of world history, but to date it has generally been studied from within: that is, scholars studied European history to explain how this European model for international relations developed over time.

Although many scholars have extended and refined the theory, Organski and Kugler’s early definitions of a power transition war remains the simplest and most intuitive.[3] Organski in 1968 examined hegemons and challengers; in 1980 Organski and Kugler refined the theory, arguing that a power transition war occurs if three scope conditions are met:

1. At least one major power is involved on either side of the conflict
2. The losing side loses territory or population
3. Battle deaths reach higher than any previous war

Yet even a cursory glance at East Asian history would reveal that the conditions Organski and Kugler identified have almost never been obtained by countries in East Asian history. Although Chinese power waxed and waned over the centuries, “China among equals” was a rare occurrence (Table 1).

Table 1. European and East Asian share of world GDP, 1000-1820

	China	Western Europe	Japan
1000	22.68	6.90	2.63
1500	24.89	15.47	3.10
1600	28.98	17.11	2.90
1700	22.31	19.05	4.15
1820	32.96	20.39	2.99

Western Europe = Austria, Belgium, Denmark, Finland, France, Germany, Italy, Netherlands, Norway, Sweden, Switzerland, UK (12 W. Europe)
Source: The Maddison Project (2013)

Rather, the biggest problem facing this type of replication study is that the form of political regime, their survival, and transition in East Asia differed from that experienced in Europe. Perhaps most

Transitions in History” (Unpublished Manuscript, in the author’s possession, January 2018).
[3] See the discussion in Indra de Soysa, John Oneal, and Yong-Hee Park, “Testing Power-Transition Theory Using Alternative Measures of National Capabilities,” *Journal of Conflict Resolution* 41, no. 4 (1997): 509-528.

importantly, the four most long-enduring countries in the region – China, Korea, Japan, and Vietnam – were characterized by political regimes that have been called "dynasties." These dynasties were remarkably long-enduring, and while there were occasional wars between these regimes, much of the violence was internal, not external. Most strikingly, only 3 out of 20 regime transitions in China, Vietnam, Korea, and Japan from 500 CE to 1900 CE came as a result of war. The three external transitions were the Tang/Silla alliance that crushed Koguryo in 668, the Mongol conquest of both Song dynasties in 1274-79, and the Ming intervention in Vietnam in 1407 on behalf of the Tran dynasty against the Ho.

Moreover, the Mongol and Manchu conquests of China did not occur in classic power transition fashion. The Mongols did attack the northern Song; but the Manchus did not advance on Beijing until the Ming had collapsed from internal rebellions, which were unrelated to any external attack. Indeed, it appears that internal instability was far more dangerous for these dynasties over the centuries than was external challenge. Exploring why this is so for such a large swath of the world can provide a much more nuanced way to study what are the scope and boundary conditions for when power transition theory applies.

In this way, researching historical East Asia provides an opportunity to seek out genuine comparisons of international system systems and their foundational components. East Asia's history was nothing like the European experience. East Asia historically was characterized by hegemony – a powerful, culturally influential China – as opposed to the routine bellicosity of balance of power Europe. Had our IR theories been derived from the Asian experience, it is almost impossible to imagine that we would have concluded that balance of power is a natural and inevitable phenomenon. As far back as the rise of unified Han dynasty in 221 BCE, Asia's predominant pattern has been concentrated power, not balance of power. China rose and fell over the centuries, for sure, but the concentration of power in East Asia provides a stark contrast to the fragmentation of the European experience.

Power transition theory is difficult to apply to premodern East Asia. The theory may be "right" or "wrong," but it does not apply in a vast geographic region over a remarkably long period. That in itself should lead us to ask why this is the case – especially because power transition theory is largely considered to be a deductive and universal theory with a logic that is intuitive and self-evident. Perhaps more of our theories are more specific and contingent than is commonly believed, and if so, carefully identifying those scope and boundary conditions will be critical moving forward. Given the dramatically different context from Europe within which political regimes rose and fell in East Asian history, it is also perhaps worth being more cautious about applying the "lessons of history" to contemporary East Asian security dynamics.

UCU Strike for USS

Editors' Note

This roundtable was slated into include a contribution from Branwen Gruffydd Jones, Head of Politics and International Relations at Cardiff University and one of the discipline's foremost experts on postcolonial International Relations theory and African politics.

Due to the recent strike action in the UK, which has been organized by the University and College Union (UCU) in response to proposed cuts to university staff pensions, Professor Gruffydd Jones was, quite understandably, unable to complete her intended contribution. We wish her and all of our colleagues in the UK good luck, and hope for a fair settlement that protections their pensions and other rights.

At time of writing, more strikes are planned for April 2018. Information on the strike action, and instructions on how to contribute to the UCU's fighting fund, can be found at the following link:

https://www.ucu.org.uk/strikeforuss

Q&A: The 2017 IHAP Award Winners

Each year, the IHAP section awards the **Robert L. Jervis and Paul W. Schroeder Best Book Award** and the **Outstanding Article Award in International History and Politics**. In 2017, the winners of these awards were **Rosella Cappella Zielinski** (for her book, *How States Pay for Wars*) and **Lisa Blaydes and Christopher Paik** (for their article, "The Impact of Holy Land Crusades on State Formation: War Mobilization, Trade Integration, and Political Development in Medieval Europe" in *International Organization* 70, no. 3).

The IHAP newsletter team interviewed the award winners. What follows are their responses.

Book award winner: ***Rosella Cappella Zielinski (Boston University):*** **How States Pay for Wars** ***(Cornell University Press, 2016)***

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

Rosella: I did not anticipate going down the route of international history and politics. When I entered graduate school I planned on working in the realm of international political economy using quantitative methods. Things started to change while I was working on my dissertation. I realized that a) I enjoyed really big questions that spanned both time and space and b) via attending the Summer Workshop on the Analysis of Military Operations (SWAMOS), I came to appreciate the historical manner in which strategic studies scholars approached their research questions. Yet, I wanted to continue working on questions from a political economy angle. The works that I felt melded the two approaches best were those that addressed war and state building. Inspired by such works as Charles Tilly's *Coercion, Capital and European States, A.D. 990-1992* and Thomas Ertman's *Building Leviathan: Building States and Regime in Medieval and Early Modern Europe*, I turned to war finance.

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

Rosella: Go straight to the documents and visit an archive. I cannot tell you how much I get out of my archival visits. While always initially daunting, you find so much more than you imagined by going. Moreover, some of my best findings have been non-findings, when you are certain there will be evidence for your argument but there is not. Only going to the archive yourself can provide you such information. More importantly, enjoy the archives. There is nothing better than looking at documents, especially those that have never been opened and you are surprised you are allowed to handle them. One of my favorite moments was when I was at the National Archives at Kew looking at the original handwritten Boer War cost accounting ledger that had not been checked out before. Finally, go as much as you can. Once you become a junior faculty member, time is limited and archival research travel is harder to do.

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

Rosella: There are so many I hate to choose just a few! At the top of my list would be Robert Gilpin's *War and Change in World Politics*. From the first time I read it to each time I teach it, I am continually impressed with the boldness of this work in regards to the question he asks, the parsimony of the answer he posits, and the breadth of time he addresses. Second on my list would be Daniel Carpenter's *The Forging of Bureaucratic Autonomy: Reputations, Networks, and Policy Innovation in Executive Agencies, 1862-1928*. His case study on the United States Postal Service still resonates with me today. He did an excellent job taking something seemingly esoteric and presenting a well-researched, informative, and compelling narrative of bureaucratic evolution. Third on my list would be a two-way tie between Adam Tooze's *Wages of Destruction: The Making and Breaking of the Nazi Economy* and Richard Bensel's *Yankee Leviathan: The Origins of Central State Authority in America, 1859-1877*. Both works are excellent examples of historical pieces that address the means, causes, and consequences of war finance.

My peers doing excellent historical work also inspire me. My co-author Ryan Grauer, at the University of Pittsburgh, for example, has spent countless hours in various national and military archives looking to understand how military organization affects performance on the battlefield. My dear friend Barbara Elias at Bowdoin College, formally at the National Security Archives, has built an impressive database of documents accounting for the interaction of co-belligerents fighting counterinsurgencies.

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

Rosella: Before I answer this question I lament that such a tension exists within the discipline of political science. Perhaps I am optimistic or naïve, but I think it is a false dichotomy as you can have macro theoretical claims supported by detailed historical research. The ability to do so, however, is no easy task for the author. In my case, the creation of a broad theoretical framework to understand war finance and account for war finance variation was paramount. However, to convince others I had to wage a multi-method defense in depth. In addition to six case studies, which included visits to multiple archives, I created a dataset capturing how interstate wars have been financed since 1800.

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

Rosella: In retrospect, I think the most challenging aspect of working with historical material was word count. I only put a fraction of the evidence I collected into the book.

The other challenge was time. While visiting archives is time consuming, the real challenge is sorting and cataloging your findings. For example, I took almost a thousand pictures at the Harry S. Truman Presidential Library alone. Creating a system to catalog findings and to do so in such a manner that was searchable took weeks. It is critical, however, to annotate all documents because you will return to them years later. I was grateful for taking the time when I was going through my copy edits for the book and found some quotes had missing citations.

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

Rosella: The most unexpected finding was limits of monetary policy – the tools central banks use to influence the amount and price of money in a state's economy – as a means to control war inflation, particularly in the United States experience since World War I. In light of works touting Central Bank independence and personally being influenced by the anti-inflation regime of the Paul Volcker era, I wrongly assumed the Federal Reserve would privilege anti-inflationary policies during wartime. The opposite was true. I found that in multiple wars the Fed was unwilling and or unable to raise interest rates to ward off inflation. Instead, the Fed privileged cheap war debt to keep the costs of war down by pegging interest rates to low levels. More importantly, the chairperson of the Federal Reserve knowing the limits of monetary polity to control prices often encouraged the president to raise taxes not to pay for the war but as an anti-inflation measure.

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

Rosella: I would like to see more work (and jobs) in the fields of American Political Development and Economic History. I get the impression that both are disappearing subfields. The works by Stephen Skowronek and Ira Katznelson, in the field of American Political Development for example, and Benjamin Cohen, in the field of economic history, are critical to understanding how the institutions that shape our political environment today came to be.

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

Rosella: Raise taxes to pay for war. In addition to avoiding deficit spending, a progressive war tax mitigates inflation and decreases inequality through the redistribution of wealth. For example, during the Korean War, Truman felt the Roosevelt Administration did not raise taxes enough. Secretary of the Treasury John Snyder stated, "Unless prompt action is taken to increase taxes, we will be repeating the pattern of World War II financing which resulted in a permanent increase in the public debt. The President has properly said that we must not make this mistake again." President Truman's aggressive tax increase stabilized rising prices and yielded a budget surplus in 1951 and raised enough revenue to reduce gross federal debt that fiscal year. In contrast, during the Vietnam War, President Johnson and his advisors explicitly rejected the lessons from the previous war with Chairperson of the Council of Economic Advisors stating, "The analogy with Korea simply does not hold water" and war inflation was unlikely.

In turn, President Johnson proclaimed that the United States was a "rich nation" that could "afford to make progress at home while meeting obligations abroad." He explained, that it was for "this reason, [he had] not halted progress in the new and vital Great Society programs in order to finance the costs of our efforts in Southeast Asia."

While contemporary leaders are deterred from raising taxes to avoid political costs - the Bush and Obama Administrations implemented multiple tax cuts during the Wars of Iraq and Afghanistan – they should consider the statement made by President Johnson's advisor McGeorge Bundy to the president in March 1968, "I now understand, as I did not when I got here, that the really tough problem you have is the interlock between the bad turn in the war, the critical need for a tax increase, and the crisis of public confidence at home. If I understand the immediate needs correctly, the most important of all may be the tax increase, simply because without it both the dollar and the economy could come apart—and with them everything else."

Article award winners: Lisa Blaydes (Stanford University, blaydes@stanford.edu) and Christopher Paik (NYU Abu Dhabi, christopher.paik@nyu.edu): "The Impact of Holy Land Crusades on State Formation: War Mobilization, Trade Integration, and Political Development in Medieval Europe," ***International Organization*** **70, no. 3 (2016): 551-586**

How did you become interested in the intersection between international history and politics? How did you become interested in war mobilization and state formation?

CP: History provides us with valuable insights and lessons on understanding political processes. I always find myself drawn to past events that come at critical moments and have lasting impact. They offer clues to where we first start seeing political and economic divergence. I was initially looking at the Neolithic Revolution in effort to trace the first rise of state formation, and eventually found myself connecting my research endeavor with related topics in more recent history, on war mobilization and state formation in Europe.

LB: I first became interested in the study of history as an undergraduate when I took a 4-semester sequence in the History of Occidental Civilization. Yet as foundational as that course work was for me, labeling the history of the Western world as the study of "Occidental Civilization" draws an immediate, implicit contrast with the streams of history left untaught. The "Occident-Orient" characterization belies the interconnected, highly dependent nature of cross-cultural interactions, a point that we try to make in our paper. Cross-cultural conflict provides one way by which these interactions take place but, of course, economic ties, environmental interconnetions and disease exchange are other avenues of connection across regions.

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

LB: I would encourage students and junior scholars to read broadly and pursue opportunities for interdisciplinary engagement, whenever possible. Humanists are increasingly intrigued by the opportunities presented by growth of the Digital Humanities. Within the field of economics, economic history has also enjoyed a higher profile in recent years. Political Science students and scholars have the potential to play an important role in bridging these interdisciplinary conversations as humanists see the opportunities presented by developments in data science and economists engage with the core questions of historical economic development.

CP: It is an exciting field with no shortage of topics to explore, since it is interdisciplinary in nature and speaks to scholars not only in history and political science, but also economics and sociology. But this also means having to frame the topics with the right motivating questions and convincing scholars in various fields, which are always challenging. I would still encourage people to look for regions and events in history that have not been explored- either in political science, history, or either. Holy Land Crusades are written about and deemed an important topic among medieval history scholars, but we have not seen much work with empirical analysis of their impact in the literature.

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

CP I would think that political scientists tend to focus on macro theories and generalizable implications, for which history often provides apt context and testing ground. For historians, the priority may be in understanding the significance of particular moment and place in depth, and their scope of analysis defined in a narrower and more focused frame.

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

LB: David Stasavage, Timur Kuran, Romain Wacziarg, James Fearon and James Kung are some of the senior colleagues working on comparative political economy, economic history and long-run growth and institutions. I have looked up to their works when formulating my research ideas.

CP: Janet Abu-Lughod's Before European Hegemony: The World System A.D. 1250-1350 stands out in my mind as an example of outstanding, engaging scholarship. Patricia Crone's writing and research has also had an important influence on my work.

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

CP: This tension makes the writing process more difficult in a fruitful way. In some ways it is inevitable, as research methodologies differ across disciplines. I appreciate constructive criticism from scholars in other fields, but also try to convince them the value added in pursuing empirical research embedded in historical context, and proposing testable claims that can then be either refuted or supported with data.

LB: This is a very difficult issue. Within a single paper, I think that it can be quite challenging to balance these concerns. My feeling is that close attention should be paid to historical detail with regard to causal mechanism. Social scientists tend to be most highly concerned with the processes or pathways by which political change occurs. Historians are typically much better at providing a detailed account of the state of the world.

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

CP: That history matters! This goes for any works on exploring the legacies and long-run impact of past events. The crusades happened a long time ago, but their impact on trade, urbanization and political development help to explain the subsequent series of events that unfolded in Europe. Factors involved in the proposed mechanism, including religious fervor and perceived external threat, taxation, trade integration and ruler stability all continue to be relevant today, as they were centuries ago.

It seems hard for governments and democratic publics to learn from history. What do you think we could do differently to communicate international historical research to "real world" actors?

CP: Policy makers and academics are often at odds on what should be done on the ground. We are trained to provide positive analyses, while normative implications and policy suggestions are perhaps often set aside from our priorities. One way to engage a broader audience might be by providing more guidance for the general reader on the relevance of the topic at hand with regard to current events. This may involve extending relevant discussions on our papers, writing op-eds specifically on the importance of historical research on understanding deep-rooted government and public issues, and training students to appreciate historical research in similar regard.

The data collected for use in your article impressed a lot of people. What was the most challenging aspect of working with the historical material used in your article?

LB: We tried to be exhaustive in our collection of crusader mobilization as well as other key explanatory variables, like the location of medieval cathedrals. What was more difficult, in my opinion, was operationalizing development of the state, our outcome variable. This led us to try a number of different strategies.

CP: We had different waves of crusader mobilization and changing state borders over time, so a main data challenge was using the information collected and actually deciding on the proper unit of analysis for our empirical study.

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

CP: How much impact the crusaders actually had on subsequent political development, urbanization and trade, and their long-term implications. Our initial motivation was to simply try to understand the role of the crusaders and their place in history, in between the spread of feudalism and the rise of modern sovereign states in Europe. We were pleasantly surprised by the various significant results that we tested for.

LB: I would like to see a greater focus and interest in understanding the history and politics of world regions outside of Europe.

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

CP: I am encouraged to see that both general political science and economics journals are publishing topics on international history with advanced research methods. Substantively, I would like to see more research done on Central Asia, Southeast Asia and East Asia excluding China. These regions offer rich contextual insight, and in many cases, wealth of data ready to be explored. There are yet many works to be done. One is to re-test the various hypotheses forwarded in the literature hitherto based on the European and North American context, and to see whether these claims can be generalized under different geographic settings (ex. warfare and state formation in Europe vs. Asia). Another is to explore the unique context that these regions offer and propose alternative theories (ex. Asian tributary state systems compared to Western territorial state systems, Japanese colonization and its impact on former colonial states). Yet another is to work on the various topics that connect these different regions (inter-regional trade and political fragmentation of empires, for example).

Upcoming Events and Workshops

April 2018

76th Annual Midwest Political Science Association
April 5-8, Chicago IL, USA
More information

ECPR 2018 Joint Sessions of Workshops
April 10-15, Nicosia, Cyprus
More information

New England Political Science Association Annual Meeting
April 19-21, Portsmouth NH, USA
More information

June 2018

Midwest Public Affairs Conference
June 6-8, Chicago IL, USA
More information

5th Annual European Workshops in International Studies
June 6-9, Groningen, Netherlands
More information

Measuring Leadership Conference
June 12-13, Warsaw, Poland
More information

Research Workshops in International Studies (RWIS)
June 12, Bath, UK
More information

43rd British International Studies Association Annual Conference
June 13-15, Bath, UK
More information

Spanish, Portuguese and Latin American Studies [SPLAS] Postgraduate Community Forum —
June 22-23, University of Nottingham, UK
More information

Development Economics and Policy
June 28-29, Zürich, Switzerland
More information

July 2018

Pluralism(s) in Emergencies: Movement, Space, and Religious Difference
July 11-12, Tunis, Tunisia
More information

FLACSO-ISA Joint International Conference
July 25-27, Quito, Ecuador
More information

ECPR Summer School in Methods and Techniques
July 26-August 10, Budapest, Hungary
More information

August 2018

Asia and the Anthropocene Workshop
August 23-27, Ann Arbor MI, USA
More information

114th APSA Annual Meeting & Exhibition
August 30 - September 2, Boston MA, USA
More information

September 2018

ISA West Annual Conference
September 21-22, Pasadena CA, USA
More information

October 2018

ISA South Annual Conference
October 12-13, Ashland VA, USA
More information

November 2018

The Second International Conference on Well-being
November 1-2, Singapore University of Social Sciences, Singapore
More information

ISA Northeast Annual Conference
November 2-3, Baltimore MD, USA
More information

ISA Midwest Annual Conference
November 16-17, St. Louis MO, USA
More information

The Ethics of Business, Trade & Global Governance
An Interdisciplinary Conference
November 30 – December 1, New Castle NH, USA
More information

March 2019

60th International Studies Association Annual Convention
March 27-30, Toronto ON, Canada
More information

June 2019

44th British International Studies Association Annual Meeting
June 12-14, London, UK
More information

August 2019

115th APSA Annual Meeting & Exhibition
August 29 – September 1, Washington DC, USA
More information