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Introduction by Stacie E. Goddard

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**Mark Jarrett. *The Congress of Vienna and Its Legacy: War and Great Power Diplomacy after Napoleon*.** London: I.B. Taurus, 2013. ISBN: 9781784530563 (paperback, \$27.00).

**Jennifer Mitzen. *Power in Concert: The Nineteenth Century Origins of Global Governance*.** Chicago: University of Chicago Press, 2013. ISBN: 9780226060088 (cloth, \$90.00); 9780226060118 (paper, \$32.50).

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Introduction by Stacie E. Goddard, Wellesley College

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This year marks the bicentennial anniversary of the Congress of Vienna. From September of 1814 to June of 1815, over 200 representatives met in the Austrian capital to rebuild the foundations of European diplomacy, which lay in shambles after over twenty years of war. It was the great powers, the “Pentarchy” of Austria, Britain, France, Prussia, and Russia, who dictated the territorial and political agreements that formed the core of a European grand settlement. And more importantly, at Vienna these powers laid the groundwork for what Mark Jarrett calls “an audacious experiment in international cooperation” (205): a congress system, in which powers would engage in “habitual confidential and free intercourse between the Ministers of the Great Powers *as a body*” in hope that “many pretensions might be modified, asperities removed, and causes of irritation anticipated and met” (205).

The Congress system has long captured the attention of historians and political scientists alike. Scholars in both these fields are thus fortunate to have two outstanding new contributions to this already venerable scholarship. Jarrett’s work offers a spectacular narrative of the origins and operation of the Congress system, and is breathtaking in its scope and in its depth. As the reviewers here note, Jarrett is a gifted storyteller, and he deftly weaves together meticulous archival research in a book that Charles Maier argues will “become our generation’s authoritative study of the peace settlements of 1814-1815.” An international relations theorist, Mitzen takes the Congress as an opportunity to challenge and revamp scholars’ approach to “global governance.” Here, she focuses on how the “forums” of the Congress—the public institutional space in which the European ministers met—ultimately allowed, and sometimes forced, statesmen to pursue their collective intentions, even when their more parochial interests would suggest otherwise. It is a work, as Brian Vick argues, that provides an “insightful and potentially important contribution to international relations theory, with policy implications for diplomatic practice in an era of aspiring global governance.”

Rather than summarize the exceptional commentaries provided in this roundtable, this introduction will focus on a specific theme, namely, what is that these two works say about the ongoing dialogue between international relations scholars on the one hand and historians on the other. There is perhaps no better substantive ground on which these two disciplines should meet. Historians of the Congress and Concert system, including Paul Schroeder, Enno Kraehe, and of course Charles Webster, consciously and carefully used the terms of international relations theory and law to build their masterful works.<sup>1</sup> Jarrett is no different, engaging with levels-of-analysis debates and regime theory throughout his account. Likewise, Mitzen joins the rank of theorists such as John Ikenberry in carefully

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<sup>1</sup> Paul W. Schroeder, *The Transformation of European Politics, 1763-1848* (New York: Oxford University Press, 1994); Enno Kraehe, *Metternich’s German Policy, Vol. II: The Congress of Vienna, 1814-15* (Princeton Univ. Press, 1983); Charles K. Webster, *The Congress of Vienna, 1814-1815* (New York: Barnes & Noble reprint, 1963).

situating her theoretical innovations within the existing nineteenth century European diplomatic history.<sup>2</sup>

It is somewhat dismaying, then, that the reviewers seem so pessimistic, even frustrated, by what these two books suggest about dialogue between historians and political scientists. George Lawson bemoans that “rather than serving to bring the two subjects closer together, the books reveal more differences than commonalities.” Vick echoes this concern, noting that “given the still-obvious gulf between the styles and approaches of the fields of history and international relations reflected in these two works, I do have doubts as to how far either book will prove able to engage scholars in the respective other discipline.” This is not the fault of the authors, the reviewers stress. There are, as Lawson points out, myriad forces driving historians and political scientists apart; differences in method, in aims, and in analytical approach. The contributors here, while impressed with the individual contributions of the authors, see little material with which to bridge the chasm separating international relations theory and history.

This of course begs the question of whether there might be any starting point for an interdisciplinary dialogue, and here I think the reviews suggest three possible points for productive exchange. The first regards questions of method and methodology. Most of the reviewers remark on the difference in the sources underpinning Jarrett’s and Mitzen’s studies. Whereas Jarrett’s work is grounded in multi-archival, multi-linguistic research, Mitzen, as Vick notes, “relies solely on secondary literature in English by historians and political scientists and does not reference published document collections or archives.” This is critique with far-ranging implications, as many international relations scholars, even those who consider themselves “historically-inclined” often rely heavily on secondary source material to buttress their theoretical claims.

While historians might condemn this practice out of hand, international relations theorists would defend their methods. On the one hand, the goal of most of these works is theory development and testing, not novel historical research. On the other hand, international relations’ scholars often produce works that compare multiple cases, spanning historical time and geographic space, and conducting primary research into all of these cases would be impossible.<sup>3</sup> Rather than treat this as an irreconcilable difference between the disciplines, perhaps this is a moment where historians and political scientists might attempt to work together to define when and how reliance on secondary sources might be acceptable in academic scholarship. It may be, for example, that international relations scholars might draw exclusively from secondary sources, so long as they both situate their arguments within the complete historiography and explicitly reflect upon the

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<sup>2</sup> G. John Ikenberry, *After Victory: Institutions, Strategic Restraint, and the Rebuilding of Order after Major Wars* (Princeton, New Jersey: Princeton University Press, 2001).

<sup>3</sup> See, for example, Jack Snyder’s defense of his use of secondary sources in *Myths of Empire*, (Cornell University Press, 1993), 64.

methodological limitations of this practice. For their part, historians could be clearer as to when there is serious need for primary research in international relations scholarship.<sup>4</sup>

Second, this roundtable should prompt historians and political scientists to reflect on the relationship between theory and history in the study of world politics. There is of course a long-standing trope that while international relations scholars are interested in theory building, historians reside in the realm of “just the facts.” But as the reviews here remind us, this distinction is inaccurate, if not absurd. Mitzen’s work would hardly be compelling if it did not draw from respected literature on the Concert, nor would Jarrett’s work be particularly interesting if it did not offer an intriguing causal story about the origins of the Congress system and of international orders more generally. To deny the significance of history and theory in both books is to do justice to neither.

But at the same time, the reviewers indicate areas where both political scientists and historians could be more mindful of each other’s work. Much of Jarrett’s discussion of the Congress system, for example, focuses on the “individual level” of analysis, the critical role of leaders such as British Foreign Secretary Lord Castlereagh, Austrian Foreign Minister Clemens von Metternich Metternich, and Russian Tsar Alexander I in constructing this political order. It would be fascinating if Jarrett could use his research to comment on broader debates over the role of individuals in the international system, especially scholarship that asks under what conditions agents prove capable of shaping political order beyond what the “international structure” would dictate. Political scientists, for their part, should be extremely cautious in treating historical work as a repository for “facts” that can be separated from broader theoretical claims. It is surprising, for example, that Matthew Rendall uses Jarrett’s narrative as evidence for a “rationalist” explanation for Congress diplomacy, given how much Jarrett’s theoretical argument relies on concepts of legitimacy, ideology, and personal beliefs.

Perhaps the biggest cause for optimism about the future of disciplinary engagement lies in the following observation: both Jarrett and Mitzen’s books demonstrate that international relations theorists and historians continue to care deeply about the “big questions” of world politics. As many of the reviewers note, both authors seek in the Congress system answers, not only about emergence of this peculiar historical moment, but about the origins and future of the contemporary liberal order. In both books, the authors confront us with questions of whether order and peace can persist in the face of disparate and even irreconcilable strategic interests. Mitzen and Jarrett ask their readers to consider whether global governance is possible without a fully-institutionalized supra-national Leviathan to regulate the system. And, as Maier writes so eloquently in his review, these books should compel all of us with an interest in international affairs to evaluate the justice of any system—liberal or otherwise—that stifles democratic impulses in the name of order and peace.

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<sup>4</sup> In his contribution here, Vick nicely articulates where a broader engagement with historiography might be useful.

There is, in other words, plenty of common ground for these two disciplines to meet. Perhaps what is needed, to borrow from Mitzen, are forums. Disciplinary boundaries often seem impermeable precisely because there are so few opportunities for real dialogue: in journals, in conferences, and in departments, historians and political scientists are compelled to speak to their prospective audiences. Fortunately there are several interdisciplinary events on the horizon. This February at Columbia University, scholars will convene to reflect on the bicentennial of the Congress and its ongoing implications for history and political science.<sup>5</sup> At the International Studies Association meetings of 2015, a new section devoted to the historical study of international relations will convene several panels on the Congress system and European diplomacy. Perhaps such forums can provide the necessary institutional space to provoke a truly interdisciplinary conversation on the study of world politics.

### Participants:

**Stacie Goddard** is the Jane Bishop '51 Associate Professor of Political Science at Wellesley College. Her research explores issues of identity, legitimacy, and conflict in international relations. Her articles have appeared in *International Organization*, *International Security*, *International Theory*, and the *European Journal of International Relations*. Her book, *Indivisible Territory and the Politics of Legitimacy: Jerusalem and Northern Ireland*, was published by Cambridge University Press in 2009. She is currently working on a book that explores rising powers and legitimacy in international politics.

**Mark Jarrett** received a B.A. from Columbia University, an M.A. in international history from the LSE, a J.D. from UC Berkeley, where he was an editor of the school's law review and Order of the Coif, and a Ph.D. in history from Stanford University. He was an attorney in the San Francisco office of the international law firm of Baker & McKenzie. He is currently completing a book, *Castlereagh and Counter-Revolution: Enlightened Conservatism and the Political Order*, to be published by I.B. Tauris next year. His article "The Struggle for Poland at the Congress of Vienna" was published in the December 2014 issue of *History Today*.

**Jennifer Mitzen** (Ph.D., University of Chicago) is Associate Professor of Political Science at Ohio State University, where she has taught since 2004. She has research and teaching interests in the areas of international relations theory, global governance, and international security. Her book, *Power in Concert: The Nineteenth Century Origins of Global Governance* (University of Chicago Press, 2013) develops and illustrates empirically a theoretical framework for studying global governance rooted in the concept of collective intentionality. A second area of research is oriented around the idea of ontological (as opposed to physical) security, whether for individuals or for states. Mitzen's work has appeared in the *American Political Science Review*, *European Journal of International*

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<sup>5</sup> <http://europe.columbia.edu/events/congress-vienna-1814-1815-making-peace-first-global-way-day-1/>

*Relations, Journal of European Public Policy, and Security Studies* (co-authored with Randall Schweller).

**George Lawson** is an Associate Professor of International Relations at LSE. His research focuses on the interface between International Relations and Historical Sociology, and on processes of radical change, most notably revolutions. He is the author of *Negotiated Revolutions* (2005) and, with Barry Buzan, *The Global Transformation: History, Modernity and the Making of Modern International Relations* (2015). He is also the editor, with Chris Armbruster and Michael Cox, of *The Global 1989: Continuity and Change in World Politics* (2010).

**Charles S. Maier** is the Leverett Saltonstall Professor of History at Harvard, where he currently teaches courses in global history, the era of the world wars, and political trials. He received his A.B. and Ph.D. from Harvard in 1960 and 1967 and has taught as well at Duke, Bielefeld, and the Ecole des Hautes Etudes en Sciences Sociales. From 1994 to 2001 he directed the Center for European Studies at Harvard, and with Sven Beckert he currently directs the Weatherhead Initiative in Global History. His first book was *Recasting Bourgeois Europe* (Princeton, 1975) and most recently he published, *Leviathan 2.0: Inventing Modern Statehood* (Harvard, 2014). He is currently writing a history of territoriality, tentatively entitled *Once within Borders: The Territorial Imagination since 1500*.

**Matthew Rendall** is Lecturer in Politics and International Relations at the University of Nottingham, and holds a Ph.D. in Political Science from Columbia University. His earlier research focused on large-scale war and peace, including such topics as nuclear deterrence, whether there is a 'separate peace' among democracies, and collective security. Much of his work tested theories of war and peace through historical case studies, often drawing on original historical research. More recently he has been writing about intergenerational justice, climate change and various topics in moral philosophy.

**Brian Vick** is Associate Professor of History at Emory University. His major publications include the monograph *Defining Germany: The 1848 Frankfurt Parliamentarians and National Identity* (Cambridge, Mass.: Harvard University Press, 2002), and he has just completed an original study of Congress diplomacy, *The Congress of Vienna: Power and Politics after Napoleon* (Cambridge, Mass.: Harvard University Press, forthcoming September 2014).

## The eternal divide?

Over the years, the relationship between International Relations (IR) and History has taken many turns. As IR emerged as a discipline, figures such as E.H. Carr, Hans Morgenthau, Martin Wight, and Stanley Hoffman used history as a means by which to identify wider patterns and themes within world politics.<sup>1</sup> As the discipline became consolidated, the hegemony of U.S. political science saw history assume a secondary role as the ammunition for IR's laboratorians. In recent years, the rise of historically-oriented research programmes such as constructivism, neo-classical realism, and the English School, as well as the emergence of historical sociology, conceptual history, and the history of ideas, has seen history return to a place at the centre of the discipline.<sup>2</sup>

However, there is a tension that remains unresolved in the relationship between IR and history. Oftentimes, a range of fundamental distinctions is drawn between theory-building IR scholars and narrative historians.<sup>3</sup> These distinctions take many forms: over methods (a focus on secondary sources vs. primary sources); aims (the identification of regularities, mechanisms, and continuities vs. the highlighting of contingency, ambiguity, and change); orientation (nomothetic vs. idiographic); sensibility (parsimony vs. complexity); scope conditions (analytic vs. temporal/spatial); notions of causation (transhistorical vs. context specific); and levels of analysis (structure vs. agency). Taken together, these distinctions are often seen as constituting an 'eternal divide' between IR and history.<sup>4</sup>

Contemporary IR is home, therefore, to two contradictory trends. On the one hand, there is increasing recognition that history is central to the discipline. On the other hand, history is cut off from IR by dint of being seen as a distinct enterprise. It is into this contradictory context that Mark Jarrett and Jennifer Mitzen's books are published. Jarrett is a historian – his primary goal is to add value to existing historical accounts of the Congress of Vienna, even as he occasionally employs terms and concepts drawn from IR. Mitzen is an IR scholar – her aim is to contribute to theoretical debates about global governance and international order, even if she does so through detailed historical enquiry into the Concert of Europe.

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<sup>1</sup> E.H. Carr, *The Twenty Years Crisis* (Basingstoke: Palgrave Macmillan, 2001[1939]); Hans J. Morgenthau, *Politics Among Nations* (New York: Knopf, 1967); Martin Wight, *Systems of States* (Leicester: Leicester University Press, 1977); Stanley Hoffman, *Janus and Minerva: Essays in the Theory and Practice of International Politics* (Boulder: Westview, 1987).

<sup>2</sup> Overviews of these dynamics can be found in: John Hobson and George Lawson, "What is History in International Relations?" *Millennium* 37(2), 2008: 415–35; George Lawson, "The Eternal Divide? History and International Relations," *European Journal of International Relations*, 18(2), 2012: 203–26.

<sup>3</sup> See, for example, the contributions to: Colin Elman and Miriam Fendius Elman eds., *Bridges and Boundaries* (Cambridge, MA: MIT, 2001).

<sup>4</sup> Lawson, "Eternal Divide"

How well does each succeed? And what do their books tell us about the relationship between IR and history?

### **The historian's craft**

In many ways, Mark Jarrett has written a rather traditional book. His *Congress of Vienna and Its Legacies* is a largely chronological account of how elite diplomats constructed the Congress system. Jarrett is a terrific storyteller, enlivening his narrative with memorable vignettes. Hence, even as the book is primarily concerned with diplomatic to-and-fro, Jarrett finds space to consider the sexual infatuations of the Austrian statesman Prince Metternich, the depressive episodes suffered by the British Foreign Secretary Lord Castlereagh, and the jousting tournaments, balloon ascents, and cultural events (including a symphony conducted by Beethoven) that were part and parcel of the Congresses. Jarrett also powerfully illuminates the scale of these meetings – over 200 plenipotentiaries and heads of state decamped to Vienna in the autumn of 1814; their retinues boosted the population of the city by a third. At Vienna and elsewhere, delegates were not just state emissaries, but representatives of what is now known as ‘global civil society’. Delegates at Vienna discussed issues ranging from the future of Poland to the rights of the German Jews, and from the reconstruction of Italy to the abolition of the slave trade. Although Jarrett’s story concentrates on the Pentarchy (Britain, France, Prussia, Austria, and Russia) that stood, as Friedrich Von Gentz (the Secretary of the Congress) aptly put it, as ‘the real and only Congress’, he is alive to the wider circulation of personnel and issues that made the Congresses such landmark events.

In an attempt to extend the scope of his narrative, Jarrett delves into IR debates around the balance of power, security communities, and related subjects. His use of these debates is accurate, if not novel. To this extent, the book is likely to appeal more to historians who are dipping their toes into these debates rather than died-in-the-wool IR scholars. The primary contribution of *The Congress of Vienna* lies not in its IR component; rather, it lies in the book’s comprehensive account of the Vienna settlement and its exemplary archival research.

This is not to say that there are not wider themes that can be extracted from Jarrett’s account. Particularly valuable is the way in which Jarrett hones in on the issue that most concerned the Pentarchy: revolution, Marx’s ‘sixth great power’. Although discussions leading up to Vienna were oriented around the need to contain France, the Congress soon assumed a more ambitious agenda: inoculating Europe against the revolutionary virus. This, in turn, prompted discussion about an issue central to the formation of modern international order: the rights and wrongs of intervention.<sup>5</sup> Whether or not Great Powers had the right, or even the responsibility, to intervene in cases of domestic unrest prompted a range of auxiliary concerns: the rights of peoples, particularly those inhabiting constitutional republics, to self-determination; the legal status of ‘belligerents’; and the

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<sup>5</sup> “Intervention and the Ordering of the Modern World,” *Review of International Studies Special Issue*, 39(5), 2013: 1039–1253.

relationship between domestic instability and international security. Such concerns, first expressed in early nineteenth-century deliberations about uprisings in Naples, Piedmont, Greece, Portugal, Spain and its Latin American colonies, have been central to international relations over the subsequent two centuries.

A second theme that emerges from Jarrett's book is the changing role of Great Powers during the early part of the nineteenth century. Rather than associating international order with a 'ranking of powers' based on precedence, title, and position, this period witnessed a shift towards the 'grading of powers' based on power capabilities.<sup>6</sup> The 'grading of powers' led to the formal recognition of 'Great Powers.' Great Powers possessed special rights (for example, over intervention) and responsibilities (such as a duty to maintain international order). They also agreed to recognize sovereignty among themselves. It was only with such mutual recognition, and a concomitant hardening of notions of inside and outside, that intervention as a discrete practice could emerge. The mutual recognition of territorial sovereignty acted as a brake on territorial transgressions. In turn, intervention became a specific right afforded *to* the Great Powers *by* the Great Powers. Intervention relied on a notion of 'dual authority' that tied together Great Powers horizontally, while constructing a vertical point of demarcation between them and other polities.<sup>7</sup>

In this way, the Congress system represents the first example of an international political order governed by 'legalized hegemony'.<sup>8</sup> After 1815, the Great Powers saw themselves, and were recognised by others as having, managerial responsibility for international order. This promoted a hierarchical international order stratified between 'greater' and 'lesser' powers. Since this coincided with the global expansion of Western power, this hierarchy operated not just in Europe, but around the world, buttressed by a 'standard of civilization' that divided the world into civilized, barbarian, and savage peoples.<sup>9</sup> Such a division was reproduced, often implicitly, in later forums of global governance, including the League of Nations and the United Nations. What began at Vienna became the norm in the twentieth century.

Although seeing Vienna as generative of some of these wider legacies, Jarrett is careful not to overplay his hand. Indeed he is clear that, in general terms, the Congress system had far less scope and reach than contemporary forums of global governance: it was *ad hoc* rather than permanent; it was socially thin rather than deeply embedded; it contributed little to the development of public international law; and it did not spread to issue-areas beyond

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<sup>6</sup> Edward Keene, "International Hierarchy and the Origins of the Modern Practice of Intervention," *Review of International Studies* 29(5), 2013: 1077-90.

<sup>7</sup> Ian Clark, *Hegemony in International Society* (Oxford: Oxford University Press, 2011), 96-7.

<sup>8</sup> Gerry Simpson, *Great Powers and Outlaw States: Unequal Sovereigns in the International Legal Order* (Cambridge: Cambridge University Press, 2004).

<sup>9</sup> "Rethinking the Standard of Civilization in International Relations" *Millennium Special Issue*, 42(3), 2014: 546-859.

security. In this sense, Jarrett is surely right to be cautious about the wider lessons that can be drawn from the Congress system.

### **The theorist's perspective**

Jennifer Mitzen's *Power in Concert* is a quite different book in both feel and content. Mitzen's primary concern is with theory development rather than (re)establishing the centrality of Vienna as a landmark historical event. Her contention is that Vienna ushered in a new system of governance – the 'Concert of Europe' – that marked a sea-change in how international political order was conceived and practiced. For Mitzen, the Concert was the first time that states purposefully constructed a legitimate "international public power" (5-11)." Public commitments, made through face-to-face forums (the Congresses), encouraged discussion, the formation of common norms and habits, practices of self-restraint, and the emergence of generalized interests. In this way, forums were participatory mechanisms that generated a thin social bond (a sense of 'us' or 'we-ness') that, in turn, fostered a sense of collective solidarity and shared purpose. The Concert was the result of the collective intentions of states to jointly share in the management of European international politics.

For Mitzen, the greatest innovation of the nineteenth century Congresses was that they were not just sporadic meetings concerned with dividing the spoils of victory. Rather, they were mechanisms through which to contain conflict, settle disputes, and solve shared problems – what Mitzen astutely calls the "visible hand" of international governance (101). Her interest in the broader order generated by Vienna and its successor Congresses means that her narrative extends well beyond the point where Jarrett's stops (the early 1820s), coming to an end only with the Crimean War of the 1850s.

There is a problem with this extended periodization. If forums rely on face-to-face interactions in order to promote discussion and generate shared interests, there was precious little of this after the early 1820s. The Verona meeting of 1822 (the same year in which George Canning replaced Castlereagh as British Foreign Secretary) exposed deep differences within the Pentarchy. Arguments over restoring Ferdinand VII in favour of Spain's revolutionary constitutionalists generated a split between the autocratic despots of Austria, Russia, and Prussia, and the constitutionalists of Britain and France. This split was never fully sutured. The relative paucity of inter-state forums and, indeed, inter-state cooperation during the second quarter of the nineteenth century serve to illustrate that the Concert was held together more by individual relationships than by systemic pressures or "group agency" (3-4).

An additional problem lies in the way in which Mitzen sees the primary orientation – and success – of the Concert as the prevention of inter-state war. This displays a common IR shortcoming: the elevation of war and the downplaying of revolution. As Jarrett shows, the Congresses were less about containing war than about thwarting revolution. Indeed, discussions revolved around whether to permit Great Powers to go to war precisely to prevent or overturn revolutionary uprisings. The revolutionary threat concerned all of the Great Powers, even if they had quite different takes on what to do about it. As such, if there was an end point to the Concert, it is marked not by Crimea, but by the 1848 revolutions,

which both posed a threat to *all* European polities and re-introduced a Bonaparte to European politics in the form of Louis Napoleon. Not for nothing did these events prompt Marx's quip about history repeating itself "the first time as tragedy, the second time as farce".<sup>10</sup> They also indicate the ways in which dynamics of revolution and counter-revolution were central to how modern international order emerged. Yet, in Mitzen's book, it is war rather than revolution that assumes centre-stage.

## History and IR

Both *The Congress of Vienna* and *Power in Concert* contain important insights into a key period in the formation of modern international order, introducing themes that will be of wide-ranging interest: the rights and wrongs of intervention, the legalized hegemony that lies at the heart of international governance, dynamics of revolution and counter-revolution, and more. In this way, both books shine a light on a theme that should be fundamental to IR, but which receives relatively scant scholarly attention: the extent to which IR's principal dynamics were forged within the nineteenth-century 'global transformation.'<sup>11</sup> Part of this story, as told by both Jarrett and Mitzen, lies in the advent of forms of proto-global governance. Another, perhaps bigger, part of the story lies in dynamics that do not feature prominently in their books: industrialization, technological changes, the rise of 'progressive' ideologies, the emergence of new strategies and weapons of war, and so on. If Jarrett and Mitzen help to reorient the international imagination towards this period – and this wider set of dynamics – their books will have performed a great service.

In terms of what the books tell us about the broader relationship between history and IR, I fear that their impact will be more limited. Indeed, rather than serving to bring the two subjects closer together, the books reveal more differences than commonalities. Whereas Jarrett is concerned primarily with a set of empirical questions, Mitzen aims to illuminate a theoretical puzzle. Although Jarrett displays a regard for "levels of analysis" (154), his narrative is predominantly oriented around individuals; for her part, Mitzen prefers a 'top-down' approach rooted in ideas of collective intentionality. If Jarrett's narrative is constructed from painstaking archival research, Mitzen's is derived from equally painstaking theoretical endeavor. Which approach readers favour is as much an aesthetic as an intellectual choice.

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<sup>10</sup> Karl Marx, *The Eighteenth Brumaire of Louis Bonaparte* (1852), available at: <https://www.marxists.org/archive/marx/works/1852/18th-brumaire/>

<sup>11</sup> Barry Buzan and George Lawson, "The Global Transformation: The Nineteenth Century and the Making of Modern International Relations," *International Studies Quarterly* 57(3), 2013: 620–34.

Review by Charles Maier, Harvard University

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2014, of course, is the centennial year of the great breakdown of Europe's long peace, which had prevailed certainly since the mid-nineteenth century wars of national reconstitution from 1853 to 1871 (the largest of which actually were civil wars fought in China and the United States), and by many reckonings since the almost twenty-five years of the wars for French containment (1792-1815). Thus 2014 is also the bicentennial of the encompassing settlements of 1814-1815, including the two treaties of Paris that ended the wars with France, the multiple territorial and dynastic settlements of the Great Powers at the Congress of Vienna. Those included the commitment to periodic great power congresses that functioned into the mid-1820s and evolved into the more fragmented cooperation that prevailed through 1849 and broke down with the Crimean War of 1853-56.

Scholars have discovered the intriguing puzzles of peace as well as the tragic dramas of war. Indeed peace and war can hardly be understood separately, as each condition emanates out of the other. The bases of international settlements – why they succeed or fail -- have taken their place in historical and political-science analysis as a major area of inquiry alongside the study of war and its origins. Charles K. Webster in 1919, Henry Kissinger in 1957, Paul Schroeder in 1994, John Ikenberry in 2001—all tackled what is loosely called the Vienna settlement, primarily to explain what they believed to be its relative success in constructing long-term peace.<sup>1</sup> Now Jarrett and Mitzen provide bicentennial interpretations from their respective disciplinary viewpoints.

Jarrett's massive and impressively researched volume promises to become our generation's authoritative study of the peace settlements of 1814-1815. Jarrett begins with the wars of the French Revolution, shows how they morphed into Napoleon Bonaparte's wars for empire from 1801 and 1805, then recounts how the adversaries of France fitfully coalesced, sometimes attempted their own alliances with Bonaparte, and finally came together to defeat his over-extended armies. He follows with great clarity the negotiation of the settlements and the decade of post-Vienna diplomacy and interventions from 1815 to 1825, which achieved hard-won cooperation among British Foreign Secretary Viscount Castlereagh, Klemens von Metternich, Foreign Secretary of the Austrian Empire, and even Russian Tsar Alexander I, and the Prussians, but finally broke down because of diverging ideological and strategic commitments. The powers agreed through the Congresses they convened at Aix-la-Chapelle (Aachen), Troppau, Laibach (Ljubljana), and Verona to allow interventions against revolutionary efforts in Naples and Spain, but Britain stood decisively against any effort to subjugate rebellious Spanish colonies in the Americas. The incoming British Whigs became increasingly uneasy about Russian power in the East as Russian

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<sup>1</sup> Charles K. Webster, *The Congress of Vienna, 1814-1815* (New York: Barnes & Noble reprint, 1963); Henry Kissinger, *A World Restored: Metternich, Castlereagh and the Problem of Peace, 1812-22* (New York: Houghton Mifflin, 1957); Paul K. Schroeder, *The Transformation of European Politics, 1763-1848* (New York: Oxford University Press, 1994); and John Ikenberry, *After Victory: Institutions, Strategic Restraint, and the Rebuilding of Order After Major Wars* (Princeton, N.J.: Princeton University Press, 2001).

aspirations encouraged by Metternich became far more reactionary. Jarrett follows these two crowded decades with admirable narrative clarity and thoroughness -- concluding with the last hurrah of successful great-power agreement by the 1830s to establish a Greek monarchy and to recognize a separate Belgian kingdom.

Given the frequent reshuffling of territories and regime forms and the proliferation of treaties made, broken, and remade, the book is a great achievement. Throughout, Jarrett presents the older interpretations that he is endorsing and often modifying. He supports by implication the idea that the struggle against Napoleon Bonaparte was a learning experience that compelled statesmen to abandon mere balance-of-power diplomacy for commitments to a common European order that might require short-term renunciations but promised longer-term security, an argument that Paul Schroeder advanced in his 1994 landmark volume on European international history, *The Origin of European Politics, 1763-1848*. There is biographical grounding enough in Jarrett's volume to satisfy the most insistent and traditional exponent of the individual's importance in grand historical events, but awareness, too, of long-term national aspirations. Jarrett clearly associates the approach of the commanding individuals -- British Prime Minister William Pitt the younger during the French wars, the great chameleonic survivor, French diplomat Charles-Maurice de Talleyrand, Castlereagh, Tsar Alexander I, Metternich, and the Prussian Hardenberg, and their major policy advisers including John Capodistrias, Russian Foreign Minister, and Alexander Stourdza (Capodistrias's secretary), and Friedrich von Gentz, an adviser to Metternich, as architects of both grand designs and tactical compromises. Intentionality prevails.

Intentionality, collectively arrived at, is a key concept for Jennifer Mitzen, who has a different agenda. She argues that Congress and concert diplomacy succeeded in averting war when great-power intentions were debated and resolved in the public forums provided by the Congresses -- "a visible hand short of a world state" (17). She contrasts this visible hand of negotiated outcomes with the invisible hand that allows markets to function cybernetically without economic planning. She also differentiates it from the hegemonic leadership that other theorists have posited is necessary and which in the post-1815 period they often (but too simplistically) ascribe to Great Britain. This desirable collective intentionality is achieved not merely through continuing exchanges of views and the willingness to cooperate for the greater good of peace (a view that usually separates 'liberal' theories of international society from the 'realist' models that see no possibility of overcoming rivalries and security dilemmas in the 'anarchic' world of states). Key to the procedure is what she terms "forum talk" (19) -- the appeal via visible summit diplomacy to the overriding interests of Europe, which decisively improved on balance-of-power jockeying and allowed the systemic interest of preserving peace to prevail. In her reading, 'forum talk' required a normative approach to resolving international conflicts that lay at the origins of today's efforts at global governance.

Ever since Charles Webster wrote the history of Vienna to guide diplomats at the negotiations of 1919 and then Kissinger published *A World Restored* in 1957, the post-Napoleonic settlements have been a major font for trying to fuse theory and history. Aspiring to join this tradition, Mitzen's book is still a curious construction. Resting

exclusively on English-language reflections and accounts, it begins by setting out contending theories of great-power action, then follows how the Great Powers managed disagreement by continually referring back to the normative commitment to European peace. But it devotes most detail to comparing the treatment of the Greek rebellion in the early 1820s, when Russia was kept from going to war on behalf of the Greeks, with the run-up to the Crimean War, when forum talk failed and Britain and France went to war in 1853. At the end of the day major theoretical outcomes hinge on the divergent outcomes of the two Greek-Russian crises.

I am not in a position to assess the details of Mitzen's narrative. The theoretical framework, however, seems to me to verge on tautology. For Mitzen intentions are the crucial intermediate step between preferences and policy; intentionality requires an assessment of possible success; it acts like Freud's ego with respect to Freud's id. But how do we assess intentions? Do we judge them from the written texts designed to persuade monarchs or wider publics – the traditional approach largely adopted by Jarrett? Do we equate them with the actions finally taken as an analog of what economists would term revealed preference? If so intention becomes an almost tautological concept and compromises the lessons drawn from Mitzen's accounts of Congress diplomacy.

Second, what she calls 'forum talk' may be as much the *result* of a negotiable context as it is the instrument creating one. Mitzen endeavors to eliminate rival explanations for the different outcomes with respect to Russia in the 1820s and 1850s; crucial was the ability to 'group' Alexander in the early 1820s as part of a tutelary public power, and the inability to do so in the 1850s. But so many factors evolved in the thirty years between the Greek revolt and the pre-Crimean crisis that the failure of 'forum talk' was as much the outcome as a cause. Containment of Russia became more critical for activist British policy; French policy was in the hands of an emperor always aware of his uncle's conquests; Austria was vulnerable on two fronts, Italy and the Balkans. Although forum talk was attempted in the early 1850s it succeeded only in 1856 after three years of an exhausting war.

Forum talk, understood here as a public normative commitment to the peace of Europe, moreover, is doubtless easier to generate after exhausting warfare, especially war provoked by a would-be hegemon: Two decades of French revolutionary and imperial expansion were needed to summon into being the Concert of Europe; Nazi conquests and the devastation of World War II, and the threat of Soviet expansion set the stage for the European Union. Concertation requires inculcating long-term time horizons rather than the short-term reactions that usually govern state behavior, and without a prior conqueror it is hard to alter the temporal calculus. Most every organized state today shares an interest in ending the Syrian horrors, much as they did the Turkish horrors in the nineteenth century; and certainly there is a potential forum in the United Nations, but without a threat to outside powers there is no general interest.

More generally – and this applies to many of the historians who praise the outcome of Vienna -- I think it problematic to describe concert diplomacy as unambiguously a positive outcome. If the international realm is considered as an autonomous and overriding political domain, then the idea of equilibrium that Henry Kissinger so compellingly ascribed

to Castlereagh and Metternich as both means and end serves as an adequate explanation for action. But as Jarrett understands, after the French Revolution, the international domain could never remain autonomous: revolutionary transformation in the middle of Europe linked domestic upheaval to international reverberations.

Jarrett emphasizes that Castlereagh defined his problem as militant Jacobinism. Any international settlement had simultaneously to be a social settlement in the participating states; indeed this is what the actors described as 'legitimacy.' Legitimacy meant, of course, government by monarchs – tempered by aristocracy and parliaments in some places, bureaucratically advised in others – and the absence of any violent challenge to landed gentry. Equilibrium and legitimacy might accommodate gradual change but not violent change, although increasingly the problem became that of channeling upheavals (as in Greece or what became Belgium) rather than simply shooting or hanging those who had led them. Between 1822 and 1848 the 'concert of Europe' broke down over those different approaches.

It is the reluctance to consider the sociopolitical premises of the Vienna settlement that renders many accounts incomplete. In the works that both authors reference, the failure or success of the international orders is judged only by the absence or recurrence of war. Indeed that must count as a major success or failure. But does it let us understand the dynamics of ongoing relations? Schroeder argued that it is unfair to expect statesmen to solve social problems. That may be, but to build international orders on vulnerable social systems that cannot readily meet widespread grievances or aspirations is to make them hostage to internal upheaval as well as to the usual frictions that arise between states. The problem of bracketing out the sociopolitical, I believe, becomes even more disabling with the theories that underlie Mitzen's account of the origins of so-called global governance. From this historian's point of view, the difficulty can be chalked up to the international-relations discourses that she takes as her intellectual universe.

A further issue, I believe, remains obscured by the histories of the period after 1815 – and by today's advocates of a normative international society. It is the frankly inegalitarian or hierarchical nature of the international order that is prescribed. European peace, both histories claim, requires a small group of leading states to create the order of the whole by stabilizing the status quo or at least supervising the incremental change that is permissible. This usually means managing or repressing any disruptive domestic change within states but also controlling the challenges from state actors outside the circle of Great Powers. Such challenges can be met – we can go from G-8 to G-20; we might enlarge the Security Council -- but at any given point in time international governance, global governance, governance within states seems to entail a model of tutelage and control that has to be anti-democratic in principle. Keeping a hierarchy of power is crucial. The men who made the Vienna settlement and the Congress system, certainly Castlereagh and Metternich, accepted this principle. Tsar Alexander claimed at times to represent a more populist or even democratic alternative, but as political theory from antiquity on might have predicted, his vision was coupled with autocratic claims. Perhaps the time has come to acknowledge that the idea of governance is basically non-democratic if not anti-democratic. The strong police the weak on the basis of their alleged wisdom. The neo-Kantian idea of the

‘democratic peace’ is challenged in real life by the Congress idea of the ‘non-democratic peace.’

The democratic peace, of course, refers to the internal ordering of states, whereas the non-democratic peace (more precisely the inegalitarian global order) refers to international cooperation. The history of settlements after major wars does suggest that for international harmony there must be agreement on the fundamental principles of internal order – every international settlement entails a social settlement within the constituent national units. Metternich and Castlereagh preferred that it should be roughly the same aristocratically based social settlement within each of the constituent states. But they recognized that there could be variance as long as each state had a stable traditional organization. What was requisite was that within no state should there arise a revolutionary challenge to its respective social order. Revolution triggers intervention, and also produces the social fervor that has often produced astounding and unexpected military success. Yes, the Congress of Vienna settlement was more successful in delaying renewed warfare than the Versailles settlement. And precisely because it admitted the defeated power into the inner circle of administering states very rapidly, it avoided one of the major difficulties of 1919’s settlement. But *pace* Mitzen and the appealing idea of concerted governance, is the institution that best expresses her aspirations the forum or the club? And is the key principle one of concerted purpose or systemic exclusion?

How do international regimes work? Two centuries on, the Concert of Europe remains an interesting case. While it was less institutionalized than contemporary regimes, the era of conferences upon which both these books focus involved unusually intimate and ongoing interaction among leaders, “giving to the counsels of the Great Powers,” according to one leading participant, “the efficiency and almost the simplicity of a single State.”<sup>1</sup> Moreover, we have the advantage of being able to study their interaction in great detail. The Concert came late enough that states were keeping and preserving systematic records; it came early enough that even the most sensitive documents are declassified. Mark Jarrett draws on these resources to produce a thoroughly researched and detailed history. Jennifer Mitzen proposes an ambitious theory of how regimes promote cooperation, and uses the Concert to illustrate it.

Rationalist regime theory emphasizes how regimes can establish standards for cooperation, reduce transaction costs, and increase transparency. Mitzen claims that regimes can do more than this to promote cooperation, without requiring states to change their original preferences or become friends (57-58, 225). Once agents start to work together on common projects, the need to justify their actions as contributing to the project constrains even those with parochial and conflicting interests. Agents come to see themselves as a group pursuing certain shared interests, and making mutual commitments to achieve them. That the commitments are shared gives them the right to expect others to comply with them and to explain and defend their actions with reference to these standards (37-40).

This dynamic arises through forums, defined as “any locale where more than two actors who have entered into a commitment meet face to face to discuss issues of common concern” (50). The meetings produce “forum effects”: Agents come to be expected to justify their actions to the group, and the need to give reasons in public encourages them to act in ways that can be impartially justified, and are consistent with arguments they have invoked in other cases. The group converges on certain principles, and “actions will be seen as legitimate only if they can be justified in these terms” (51-54; quoted passage at 54). Even outside the forum agents must abide by their arguments or risk appearing hypocritical (54-55). To evaluate her theory, Mitzen examines the Concert of Europe, first with a case study of the 1820 Congress of Troppau, and then with a more extended analysis of great power policy toward the Greek Revolution. She concludes that “forums were crucial to the success of the Concert.” A practice of public reason-giving developed, constraining participants to state their reasons in a fashion “acceptable to all” (215-16).

This is an attractive picture. As Mitzen points out, far from having reached the ‘end of history,’ the actors in international relations and their values seem to be becoming more heterogeneous. If regimes can establish normative constraints even in the absence of

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<sup>1</sup> Lord Castlereagh, quoted in Charles Webster, *The Foreign Policy of Castlereagh, 1815-1822: Britain and the European Alliance* (London: Bell, 1963; originally published 1925), 153.

ideological convergence, this would be very good news (227). The theory also seems *prima facie* plausible. After all, people generally have a strong desire to be able to justify their behavior, as T. M. Scanlon observes, and “are willing to go to considerable lengths...in order to avoid admitting the unjustifiability of their actions and institutions.”<sup>2</sup> Moreover, a basic feature of normative judgments is that they are universalizable: What is right for one agent must be right for any other relevantly similar agent in relevantly similar circumstances.<sup>3</sup> If policy makers know that they will have to defend their actions, it seems possible that this could constrain their actions to those for which they can offer impartial justifications. To test the findings further, Mitzen suggests that we “might want to further open up the black box of the interstate conversations” (215). That is what this review will try to do.

Mitzen’s first example is Austrian policy toward the 1820 revolution in Naples, which by all accounts the Austrian Foreign Minister, Prince Metternich, would have liked to suppress quickly and unilaterally. Yet earlier that year he had argued against intervention against rebels in Spain on the grounds that counterrevolutionary interventions were always a failure, and had Austria now simply gone into Italy, Mitzen argues, he would have exposed his motives as “blatantly parochial” and given other states a pretext for meddling. Instead, Metternich sought to obtain the sanction of the other Great Powers to intervene, eventually forcing him to hold an international congress at Troppau to obtain their consent. “He did not want a great power meeting,” Mitzen observes, “and it is difficult to think of any reason he would have acquiesced to a ‘full dress congress’ other than being trapped by his own rhetoric and constrained to act consistently” (114).

Is it really so hard? The obvious reason, as Jarrett’s analysis makes clear, is that Russia insisted on a meeting, and Metternich feared that Petersburg could make serious trouble if Austria intervened without first having it on board (241-42).<sup>4</sup> Both he and his close collaborator Friedrich von Gentz told others that if Austria could have crushed the revolt quickly, it would have acted without any power’s consent.<sup>5</sup> “Of all evils,” Metternich wrote to his ambassador in London, “the greatest would be to see the Emperor Alexander abandon the moral tie which unites us” and turn to supporting the forces of change. Ultimately, he told the Austrian emperor, it was better to risk Britain’s displeasure than

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<sup>2</sup> T. M. Scanlon, “Contractualism and Utilitarianism,” in *Utilitarianism and Beyond*, ed. Amartya Sen and Bernard Williams (Cambridge: Cambridge University Press, 1982), 103-28, at 117.

<sup>3</sup> R. M. Hare, *Freedom and Reason* (Oxford: Oxford University Press, 1965); Joerg Schroth, *Die Universalierbarkeit moralischer Urteile* (Paderborn: Mentis, 2001).

<sup>4</sup> See also Paul W. Schroeder, *Metternich’s Diplomacy at Its Zenith, 1820-1823* (Austin: University of Texas Press, 1962), 49, 54-55. This was also the interpretation of Britain’s ambassador in Vienna. See Guillaume de Bertier de Sauvigny, *Metternich et la France après le congrès de Vienne* (Paris: Hachette, 1968-1971), 347.

<sup>5</sup> Hans W. Schmalz believes Vienna would actually have done so, whereas Paul Schroeder doubts that Metternich would have risked it. See *Versuche einer gesamteuropäischen Organisation, 1815-20: Mit besonderer Berücksichtigung der Troppauer Interventionspolitik* (Aarau: H. R. Sauerländer, 1940), 59 and 59 n. 1; Schroeder, *Metternich’s Diplomacy*, 43 n. 51.

“the free and voluntary defection of Emperor Alexander.”<sup>6</sup> This is not to deny any evidence of ‘forum effects’: One leading authority speculates that Metternich’s extended delay after the revolution before consulting Russia arose from “the extreme embarrassment of having to justify such a complete about-face,” and the Russian ambassador thought that he wanted to avoid a congress where he could be accused of pursuing Austria’s narrow interests.<sup>7</sup> But the evidence that Metternich’s chief concern was not to antagonize Russia is overwhelming, as much of Mitzen’s account suggests. Moreover, when push came to shove, Metternich *did* contradict his own earlier argument about Spain by demanding intervention in Italy.<sup>8</sup>

Another piece of evidence Mitzen presents is Metternich’s invitation to the King of Naples and other Italian monarchs to the Congress of Laibach the following year. In so doing, she argues, the Austrian Foreign Minister ran the risk of diplomatic complications for the sake of “creat[ing] the semblance of a fair process” (119, 122-23; quoted passage from 119). That is true, but what Mitzen does not take into account is that Metternich also stood to gain directly from inviting the King. If the revolutionaries in Naples released the King, he told his minister in Turin, the latter would be able to revoke the concessions he had made under pressure; if they did not, it would prove he was being held against his will. In inviting the other monarchs, he took up a proposal which Russia seems already to have put on the table, and which might have been adopted in any case. Moreover, the invitation was *pro forma* from the start. When it was discussed at Troppau, a French negotiator shrewdly asked whether the Italians would actually be allowed any serious role.<sup>9</sup> As Jarrett’s account of the ensuing Concert of Laibach shows, they were given a seat at the table only once the key decisions had been made (270, 275).

Mitzen’s second case is Russian policy toward the Greek revolution of 1821. News of the revolt came during the Congress of Laibach. Despite the fact that Turkey, in its efforts to suppress the rebellion, was interfering with Russian trade and killing Orthodox Christians, Tsar Alexander I condemned the revolt and initially adopted a policy of non-interference. As the year wore on, however, Petersburg showed signs of wanting to intervene. At this point, Austria and Britain made a coordinated push to persuade the tsar that Greece was a European matter, and planned a conference to deal with the problem. “That the Concert frame, including a specific set of arguments and the prospect of meeting in the Concert forum, restrained Russia,” Mitzen maintains, “is evident from the set of meetings in February 1822 and the congresses of Vienna and Verona in autumn 1822” (133). This argument does not seem to match the chronology of events. By “the set of meetings in February 1822,” Mitzen appears to mean Alexander’s sending of Count Dmitrii Tatishchev

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<sup>6</sup> Schroeder, *Metternich’s Diplomacy*, 54-55.

<sup>7</sup> Bertier de Sauvigny, *Metternich et la France*, 323-24; F. de Martens, *Recueil des traités et conventions, conclus par la Russie avec les puissances étrangères* (St Petersburg: Ministère des voies de communication, 1874-1909), 4/1: 275.

<sup>8</sup> Cf. Schroeder, *Metternich’s Diplomacy*, 254.

<sup>9</sup> Bertier de Sauvigny, *Metternich et la France*, 384-85, 410-11.

to Vienna to negotiate with Metternich. But as Jarrett shows, the decision to avoid war and continue to work with the allies was evidently taken before dispatching Tatishchev, and the mission was a result, not a cause of it (304-5).<sup>10</sup> It may well be that the Vienna negotiations and the prospect of a conference in the autumn strengthened the tsar's resolve, but evidence would be required to show this. This is not to deny that Alexander's commitment to the allies and the concert affected his decision—they clearly did. What is needed is evidence that presenting arguments in a forum—or the prospect of doing so—played a role.

Mitzen's third example is the collaboration of Britain, France and Russia in reaching a Greek settlement between 1826 and 1832. In the 1827 Treaty of London, the three powers agreed to offer their mediation between Turkey and Greece, forswore unilateral gains of territory, influence, or trading privileges, and provided for ongoing negotiations among their representatives.<sup>11</sup> The treaty "marks a clear turning point," Mitzen argues, "where the great powers backed down from the brink of conflict and committed to treating the Greek Question as a European issue, linking its resolution to a forum." Britain rejected several Turkish approaches seeking a bilateral entente, and after Russia declared war on Turkey in April 1828, it restrained its military operations in response to pressure from the allies and continued to treat Greece as a multilateral matter. The treaty Russia concluded with the Turks in 1829 after driving nearly all the way to Constantinople was moderate, and again left Greece as a matter to be resolved collectively. "Despite the Ottoman offer to grant Greece the status of the Principalities and despite the prospect of a fallen Constantinople," Mitzen concludes, "Russia fought and won the Russo-Turkish War as a limited war within its sphere of influence, while keeping the Greek Question separate. It therefore is reasonable to attribute to the Treaty of London a causal role in constraining the meaning of the Russo-Turkish War and keeping the Greek Question out" (155-167; quoted passage at 167).

As before, there is no reason to doubt that allied pressure and negotiations helped restrain the Russians and contributed to a moderate settlement. The question is whether 'forum effects' played a role in doing so. The alternative, rationalist explanation is that the talks clarified expectations, established red lines, and helped the Russians gauge how far they could go without provoking great-power conflict. There is plenty of evidence of that. In October 1828, Petersburg delayed a blockade of the Dardanelles after British protests. The Russian Foreign Minister, Count Nesselrode, wrote that disputes with Britain and France

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<sup>10</sup> See also Nesselrode to Golovkin, 31 January 1822, in Anton Freiherr von Prokesch-Osten, *Geschichte des Abfalls der Griechen vom Türkischen Reich im Jahre 1821 und der Gründung des Hellenischen Königreichs* (Vienna: Carl Gerold's Sohn, 1867), 3:290-96; Webster, *Foreign Policy of Castlereagh*, 388; Schroeder, *Metternich's Diplomacy*, 185; V. N. Vinogradov et al., *Mezhdunarodnye otnosheniia na Balkanakh, 1815-1830 gg.* (Moscow: Nauka, 1983), 156.

<sup>11</sup> Text of the treaty in Edward Hertslet, *The Map of Europe by Treaty* (London: Butterworths, 1875-91), 1:769-74.

should be put off until military successes afforded more leverage.<sup>12</sup> Russia's agreement that the allies might resume diplomatic relations with Turkey was also an effort to hold the alliance together and buy time for military successes. It freed Russia from the risk of European opposition during the forthcoming campaign, Nesselrode explained, and "maintain[ed] among the three courts the appearance [*simulacre*] of union whose moral effect is so salutary for the general tranquillity of Europe," while "leav[ing] in the last analysis, the solution of the Greek question in the hands of Russia."<sup>13</sup> This language reflected strategic calculation, not the need to justify Russian actions for its own sake. Nor is that surprising. Neither Nesselrode nor Nicholas I took part in the negotiations, and ultimately it was the tsar himself to whom Nesselrode was concerned with justifying decisions, not his foreign counterparts.

If 'forum effects' encouraged cooperation, we would expect at least the direct participants to be by and large advocates of restraint. For most of the period in question, Russia's Ambassador in London, Prince Lieven, did indeed advocate cooperation with the allies. The justifications he gave to his home government for cooperating, however, typically invoked the need to block the formation of hostile coalitions.<sup>14</sup> In June 1829 Lieven and his deputy in London, Count Matuszewic, advised Petersburg that "our chief interest is in preserving the last remnants of the alliance established by [the Treaty of London] for as long as possible, gaining time, and assuring the security necessary for conducting our military operations." In their view "[t]he peace treaty should be signed in our military camp; after its conclusion Europe should learn its conditions. At that point it will be too late to object..."<sup>15</sup> Nor were Lieven and Matuszewic consistent advocates of cooperation.<sup>16</sup> As Russian troops approached Constantinople, both men called for a drive on the capital.<sup>17</sup>

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<sup>12</sup> Nesselrode to Lieven, Pozzo-di-Borgo, Tatishchev, Alopeus, Capodistrias, Ribeaupierre, Bulgari and Grand Duke Constantine, 3/15 October 1828, confidential, in *V[neshniaia] p[olitika] R[ossii] xix i nachala xx veka: dokumenty Rossiiskogo Ministerstva inostrannykh del* (Moscow: Gosudarstvennoe izdatel'stvo politicheskoi literatury, 1960-), 15: 625-44, at 630; Nesselrode to Capodistrias, Ribeaupierre and Bulgari, 14/26 October, 1828, *ibid.*, 16:23-27; Muriel E. Chamberlain, *Lord Aberdeen* (London: Longman, 1983), 210-11.

<sup>13</sup> Nesselrode to Nicholas I, 10/22 December, 1828, VPR, 16:63-68; Nesselrode to Lieven, 19 April/1 May 1829, *ibid.*, 166-71, quoted passage at 168.

<sup>14</sup> Lieven to Nesselrode, 16/28 November 1828, VPR, 16:39-43, at 41; Martens, *Recueil*, 11:389-90; VPR, 15:668, n. 98; VPR 16, 577 n. 77.

<sup>15</sup> Quoted in Vinogradov et al., *Mezhdunarodnye otnosheniia*, 211.

<sup>16</sup> In late 1827, against his orders, Lieven resisted a British proposal that the allies agree in the event of a war to observe the provisions of the Treaty of London and not seek territorial acquisitions or exclusive advantages, eventually accepting it only under British pressure. Martens comments that "Prince Lieven evidently held to a policy of 'free hands' and foresaw a change of circumstances which could require Russia to claim compensation for losses incurred in a war against Turkey." *Recueil*, 11:367-68.

<sup>17</sup> Martens, *Recueil*, 11:410-11.

To sum up, while Mitzen's theory is deductively plausible, her cases, on closer inspection, do not seem to support it. A case that *Power in Concert* does not examine—the Belgian crisis of 1830-32—provides some evidence for her theory. Here France agreed to allow the German Confederation to drive pro-Belgian rebels out of Luxembourg and re-establish Dutch rule, despite its support for Belgium during the crisis. The reason seems, in large part, to have been the Confederation's irrefutable legal case for doing so. Had the new regime in Paris rejected these reasons and resisted intervention, it would have branded itself a rogue state at a time when it was trying hard to demonstrate its legitimacy.<sup>18</sup> Abiding by norms that could be internationally justified signalled France's moderate intentions. As with the Russo-Turkish War, however, rationalist theories seem sufficient to capture the Concert's role: establishing standards for cooperation and helping states signal their cooperative intentions.

To signal cooperative intentions, it helps to have them in the first place. Peace would probably not have lasted so long after 1815 had not four of the five Great Powers been basically content with the European status quo. One of the strengths of *The Congress of Vienna* is its emphasis on the leaders' deep-seated suspicion and fear of revolution. Britain's Lord Castlereagh shared the conservative sympathies of his European counterparts, but the continental powers' desire for counterrevolutionary intervention led Britain to distance itself by the early 1820s. This story has been told before, but Jarrett's account is distinctive in the detail with which it shows how an explicit policy of counterrevolution proved incompatible with British parliamentary politics. Nevertheless, the four erstwhile allies against Napoleon continued to resist French attempts to revise the status quo in ways that could have shifted the balance of power. In the 1850s, that finally changed. When France provoked a minor dispute in the Near East, and Russia overreacted, Britain blamed Petersburg rather than Paris. As Mitzen describes, it challenged Russia in Turkey and the Balkans, seeking to destroy Russian influence in the region and refusing a compromise settlement. The result was the Crimean War.

What went wrong? Russia faced a problem that still afflicts it today—its size and power inevitably raised suspicions, and even when its intentions were moderate, its opaque autocratic regime made it hard to signal them.<sup>19</sup> Moreover, there seems to have been a shift in the balance of power: the Western powers had resented previous Russian interventions in Turkey, but whereas it is not clear that they could have resisted Russia in those earlier cases even if they had tried,<sup>20</sup> this time they saw their chance. Most important, however,

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<sup>18</sup> Matthew Rendall, "A Qualified Success for Collective Security: The Concert of Europe and the Belgian Crisis, 1831," *Diplomacy & Statecraft* 18/2 (2007): 271-95, 280-81, 287. The intervention never took place.

<sup>19</sup> On the difficulties authoritarian regimes face in signaling their intentions, see Kenneth A. Schultz, *Democracy and Coercive Diplomacy* (Cambridge: Cambridge University Press, 2001).

<sup>20</sup> Matthew Rendall, "Russia, the Concert of Europe, and Greece, 1821-29: A Test of Hypotheses About the Vienna System," *Security Studies* 9/4 (2000): 52-90, 76-77; Rendall, "Restraint or Self-Restraint of Russia:

Britain's objectives had changed, with domestic politics now driving social imperialism.<sup>21</sup> The Vienna system functioned so long as only France challenged it. Confronted now with two—or even, if one counts Russia, three revisionist states—it fell apart. As Mitzen observes, “If strong parties aim to undermine or destroy a commitment, public talk alone cannot preserve it” (211). Nor could any other mechanism of the Concert of Europe.

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Nicholas I, the Treaty of Unkiar Skelessi, and the Vienna System, 1832-1841,” *International History Review* 24/1 (2002): 37-63.

<sup>21</sup> Jack Snyder, *Myths of Empire: Domestic Politics and International Ambition* (Ithaca: Cornell University Press, 1991), chapter 5.

This roundtable is conceived in the spirit of interdisciplinary dialog between international relations and history, and the two works selected lend themselves admirably to the purpose. Mark Jarrett offers a well-told narrative of the diplomatic events leading up to the Congress of Vienna and through the Congress of Verona (1822) that at the same time carries an analytical dimension assessing the era's international system in engagement with the international relations literature on the subject. Jennifer Mitzen makes an insightful and potentially important contribution to international relations theory, with policy implications for diplomatic practice in an era of aspiring global governance, and she does so by working through the historical example of the Congress system and the Concert of Europe, again in engagement with the literature from both fields. In the end, however, and with all due praise for the efforts of both authors to incorporate insights from the respective cognate discipline, I come away from these books with a sense of just how deep the divide between the disciplines really is. It will be interesting to see how far Jarrett is able to make an impact on the international relations literature, or Mitzen on the historical. In the spirit of a thousand flowers blooming, or of providing independent and complementary perspectives on the past and present practice of interstate relations, such robust disciplinary diversity is perhaps a good thing, but it does make it harder to spur interdisciplinary dialog.

In the first two chapters, Mitzen sets forth a meticulous theoretical framework for her argument in which she establishes the possibility of “collective intentionality” (2) and of concerted action among disparate states in order to address specific problems or to exercise global governance in the name of a common or higher interest that transcends the immediate interests of the single states. She further describes what she calls “forum effects” (19), whereby the fact of coming together in public venues for multilateral face-to-face discussion of problems deemed common begins to put pressure on the actors to maintain their “joint commitments” (2) and to identify and solve problems from the perspective of the broader interest. Putting these claims together, Mitzen contends that political effects emerge at the systemic level that cannot be reduced to the interests of the single state actors, and that they still do so in the absence of a unitary institutional agent, of an ideological consensus among the actors, or even of a shared belief or preference among the actors showing that they have somehow internalized new norms. This represents one of the ways in which Mitzen depicts her work as a middle path among the main competing schools of international relations theory: rational choice realists, (usually liberal) institutionalists, and constructivists. Taking this focus on process and system above individual intention and agency back to its Kantian or even Mandevillian roots, but avoiding realist notions of an “invisible hand,” (12) Mitzen pithily concludes, “In global governance, good outcomes are possible without good intentions” (57).

Two further chapters trace the rise of the preconditions for such diplomacy in the course of the eighteenth century and the coalitions against French military and political leader Napoleon Bonaparte. Mitzen then arrives at the crux of her argument in the following two chapters, which offer a comparison of the diplomacy surrounding relations with the

Ottoman Empire in the period of the Greek revolt during the 1820s and early 1830s with that in the run-up to the Crimean War after 1848. This is the most empirically compelling part of the book, and she makes a strong case that it was the lack of a joint commitment rather than the failure of the forum system that led to the collapse of the peace among Great Powers that had held since the Vienna Congress. She stresses that high-level multilateral talks are no “silver bullet” (179) when the main actors have already decided on war as the preferred policy option, but as the Crimean case shows, even then the existence of such talks can put a brake on conflict and render it more difficult to justify war. On the larger scale, and with reference to our own times, Mitzen argues that forum talk makes it easier to maintain joint commitments once entered into; determining how joint commitments get forged in the first place, or how and why they fall apart, of course then becomes another problem, to which she devotes less attention. That Mitzen puts her finger on an important and neglected element of international relations seems clear, even if a historian might suggest that setting forum talk alongside the study of ‘political languages’ and claims-making of the sort associated with the Ideas in Context approach of Quentin Skinner and J. G. A. Pocock could take the analysis deeper and put its insights on an even more solid footing.<sup>1</sup>

Mark Jarrett’s work takes its place as the most reliable and detailed narrative of the period of Congress diplomacy from 1814 to 1822, surpassing the older works of Harold Nicolson and Henry Kissinger as well as the newer volumes of Jacques-Alain Sédouy.<sup>2</sup> One should still read these too, plus the studies of Charles Webster, while for the Congress of Vienna itself the best chronological narrative probably remains that of the late Enno E. Kraehe.<sup>3</sup> Jarrett’s impressively-researched study commands a wide range of sources, including in German and French, and he is concomitantly balanced in his judgments of the main actors, even if British Foreign Secretary Lord Castlereagh – the subject of his doctoral dissertation – still emerges as closest to his heart and most central to his coverage, and Austrian Foreign

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<sup>1</sup> On Ideas in Context, see James Tully, ed., *Meaning and Context: Quentin Skinner and His Critics* (Princeton: Princeton University Press, 1988); J. G. A. Pocock, *Political Thought and History: Essays on Theory and Method* (Cambridge: Cambridge University Press, 2009).

<sup>2</sup> Harold Nicolson, *The Congress of Vienna: A Study in Allied Unity: 1812-1822* (New York: Compass, 1961 [1946]); Henry Kissinger, *A World Restored: Metternich, Castlereagh and the Problem of Peace 1812–1822* (London: Phoenix, 2000 [1957]); Jacques-Alain de Sédouy, *Le Congrès de Vienne: L’Europe contre la France 1812-1815* (Paris: Perrin, 2003), and *Le concert européen. Aux origines de l’Europe 1814-1914* (Paris: Fayard, 2009).

<sup>3</sup> Enno E. Kraehe, *Metternich’s German Policy*, vol. 2, *The Congress of Vienna* (Princeton: Princeton University Press, 1983); Charles K. Webster, *The Congress of Vienna 1814-1815* (London: Bell, 1950 [1919]), *The Foreign Policy of Castlereagh, 1815-1822: Britain and the European Alliance* (London: Bell, 1925), and *The Foreign Policy of Castlereagh, 1812-1815: Britain and the Reconstruction of Europe* (London: Bell, 1931). On the Concert, also see Matthias Schulz, *Normen und Praxis: das Europäische Konzert der Grossmächte als Sicherheitsrat, 1815-1860* (Munich: Oldenbourg, 2009); and Wolfram Pyta and Philipp Menger, eds., *Das europäische Mächtekoncert: Friedens- und Sicherheitspolitik vom Wiener Kongreß 1815 bis zum Krimkrieg 1853* (Cologne: Böhlau, 2009).

Minister Clemens von Metternich also comes off with high marks for his “mastery of diplomatic tactics” (266).

Jarrett insightfully contends that the origins of the Congress system lay not in the Vienna Final Act of June 1815 but rather in the accords signed later that year in Paris after Napoleon’s second defeat at Waterloo. He by no means overlooks the previous thinking along these lines among the main protagonists going back to 1804-1805 and continuing through the Vienna Congress – where ideas of future meetings and consultation to steer the international ship of states had already been enunciated – but he emphasizes that these schemes were not put into their final form and signed into treaty until the autumn of 1815, in reaction to the changed situation following Napoleon’s Hundred Days after his return from exile on the island of Elba and the renewed outbreak of what the leaders perceived to be a continuation of the French Revolution. Similarly, for Jarrett, it was the follow-up Congress of Aachen (Aix-la-Chapelle) in 1818 rather than the Congress of Vienna that marked the real beginning of “an audacious experiment in international cooperation,” as “the first great peacetime summit conference” (180). Having led the reader through the origins of Congress diplomacy between 1812 and 1815, Jarrett then provides an expert guide to the subsequent course of negotiations to deal with outbreaks of revolution in Italy and Spain and with the question of whether to attempt to intervene in the mushrooming independence movements in Latin America, all in the years between 1818 and 1823. Across the whole span, Jarrett knowledgeably interweaves the historical context and background, in a way that should serve the needs of general readers while also offering something for period specialists. Jarrett brings in international relations literature, and even suggests that a “levels of analysis” (154) methodology that takes into account individual actors and intentions, geopolitical realities, and systemic effects might be the best approach. On balance, though, as he himself observes, he focuses on the first level, in the mainstream historical tradition.

Jarrett and, to a greater extent Mitzen, both aim to connect their historical analyses to ongoing debates about international relations and global governance in our own times. Mitzen states that her book “was framed as a critique of global liberal governance,” to which she contrasts her model of “international public power” (227). She makes clear that she does not oppose liberalism as such, and indeed considers its “toleration of difference” (228) a crucial part of the process. She regards insistence on a global ideological consensus of the sort that she claims characterizes neoliberal notions of global governance, however, as an obstacle precisely to the ability to accommodate and negotiate differences. In a world in which no single regime-type or hegemon seems on the horizon, and in which states remain disinclined to surrender sovereignty to supranational institutions, finding a way to concert power for common ends seems critical to her. Jarrett, for his part, highlights the historical roots of present-day institutions such as the UN Security Council in the Congress system and the Great Power pentarchy of Austria, Britain, France, Prussia, and Russia – several of the originators of the later institutions had been students of the Congress and Concert systems, not least Sir Charles Webster. He also draws attention to the continuing problem of reconciling disparate state and nonstate actors and of squaring the partly competing goals of social justice, the rule of law, and international peace. Quoting Alex Bellamy, Jarrett asks whether it is possible to move “from an international system to an

international society, or from an international society to a world society” (377). For Mitzen, the question is rather whether one can move from an international society – one that is already partly in place – to a system of global governance.

Mitzen relies solely on secondary literature in English by historians and political scientists and does not reference published document collections or archives. This narrows or pre-selects both the range of available information from the periods she examines and the spectrum of possible interpretations of those primary sources. Some misapprehensions do creep into Mitzen’s account. She asserts, for example, that the ambassadorial-level London conferences on Greek affairs from 1827 to 1832 were “unprecedented” (169), but in fact ministerial conferences to deal with specific problems had already been established among the Great Powers in London and Paris following the Paris treaties in 1815, as “a sort of permanent European Congress” in Castlereagh’s words.<sup>4</sup> Mitzen also maintains that the creation of an independent neutral state by the powers was “something they never had done before” (144), but the Congress of Vienna had already created the neutral Republic of Cracow in 1815, the constitution of which was drawn up by a commission of Russian, Austrian, and Prussian officials of slightly lesser rank than those involved in the London negotiations regarding Greece. The constitution was then finalized in 1818 with some, though limited, input from the Poles in Cracow, again as in the Greek case of the later 1820s. For that matter, insofar as the original plans for Greek autonomy aimed at making it a “tributary vassal state” of the Ottoman Empire (171), the powers’ creation of the Kingdom of Poland under Russian suzerainty in 1815 also suggests itself as a precursor case of the role of national self-determination at the Vienna Congress.

Mitzen also indicates that the Ottoman Empire had “sent an emissary” to the Congress of Vienna but was ultimately “kept out” by the Powers (95); actually, the Sublime Porte had been encouraged to send one but elected simply to leave the existing chargé in Vienna as an observer of the great diplomatic gathering. In this context too it is significant that the failure to introduce a full guarantee of the Vienna Settlement into the Final Act of June 1815 had much to do with disagreements between Russia and Great Britain about if or how the Ottoman realm would be included in such a guarantee (as Jarrett observes).<sup>5</sup> Given the central role that the Eastern Question plays in the later portion of Mitzen’s argument, more precision on these matters for the period of the Congress itself would have been useful. None of this, however, need weaken her basic argument about the importance of collectively intended joint commitments and public forums; if anything, being able to point to the prior instances would strengthen the case for the effect of multilateral consultative diplomacy later on, as certain practices and norms began to be established within these institutional spaces.

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<sup>4</sup> Jarrett, 426, n. 225, citing Webster, *Foreign Policy of Castlereagh, 1812-1815*, 420.

<sup>5</sup> Emphasizing Metternich’s role, also see Miroslav Šedivý, *Metternich, the Great Powers and the Eastern Question* (Pilsen: University of West Bohemia, 2013), 40-44.

Jarrett suggests that the Great Powers' "collective hegemony" was "concealed behind a mask of seeming restraint" (180). For Mitzen, on the other hand, the restraint was real, and at least partly caused by the effects of meeting in international forums to discuss the ways and means of realizing their joint commitment to uphold European stability. Mitzen takes the desire to mitigate interstate conflict and war to be the European concert's chief aim after the wars against Napoleon, whereas Jarrett, on the contrary, argues that the Great Powers' primary concern was to oppose the spread of revolution, as the greatest danger to international stability, a worry that led them to a condemnable degree of repression and repressive violence. I would emphasize that the system was constructed to address both potential problems, and that they were at least partly seen as interconnected. I agree with Mitzen that the Concert and even the Holy Alliance were not as reactionary as they have usually been portrayed, but there still needs to be recognition of just how central stopping the spread of revolution was to the Great Powers' and elites' concerns in this period. This even bears implications for Mitzen's argument that similarity of regime type or ideology is not required in order for states to be able to form and uphold institutionalized joint commitments of the sort whereby forum effects begin to function, insofar as both Mitzen and Jarrett underestimate the degree of relative consensus about the causes of and potential solutions to the problem of revolution. Most leaders understood that a fully reactionary return to royal absolutism sustained by repression was unlikely to be a successful governance strategy; instead, moderate reforms and the rule of law, with at least some recourse to local representative institutions and provincial or national consultative government, were perceived as the way forward. Such views even held for Austria's chief minister, Prince Metternich. The oft-claimed division between a 'constitutional' western bloc around Britain and France and an autocratic eastern one around Austria, Prussia, and Russia was thus much slower in arising, really not until the 1830s, and even then, as Jarrett concedes, with residual glitches such as the support throughout the decade by Louis-Philippe's July Monarchy in France for Mehmet Ali's Egypt against the Russian- and British-backed Ottoman Empire.

Mitzen is, however, quite right to draw attention to the importance of the fact that the leaders of the Great Powers recognized the need to avoid Great Power warfare as one of the most pressing problems of the postwar settlement, and that they believed a shift to framing diplomacy in terms of European interests rather than of individual national interests had to be part of the remedy. Putting specific diplomatic issues in that European rhetorical frame, and into the institutional frame of face-to-face multilateral diplomacy, could then to some extent take on a life of its own or build up its own momentum as a seemingly successful practice over the coming years. Revolution constituted one of the main causes of war that leaders thought they needed to address – an easy lesson to have learned from the previous decades – but additionally they thought they needed to replace the balance-of-power politics based on states' self-interest of the sort taken to have characterized eighteenth-century international relations. This recognition was clearest in the drafting of the Holy Alliance, with its invocation of fraternity and charity, which is part of the reason that the treaty should be included among the elements of the Vienna Settlement and system, but it

shaped the other components of the system as well.<sup>6</sup> Not least among the errors of the previous regime figured the partitions of Poland. Thus, the Vienna and Paris treaties were designed both to make revolution less likely and to maintain cooperative rather than competitive relations among the main powers. Jarrett downplays the degree of self-restraint exhibited by the Congress system and considers it “more of an incidental by-product . . . than an essential characteristic of the system” (360). For Mitzen, too, self-restraint was a by-product, but it was consequential rather than incidental, and it was built into the system from the outset.

Scholars have often treated the Congress system and the Concert of Europe as two distinct stages of the Vienna system, the latter running up until the Crimean War or even the First World War. Jarrett does too, but tends to emphasize the sharp disjuncture between the “end” (344) of the Congress system in 1822 and the developments that came after, even if the later period still saw the continued use of multilateral ambassadorial conferences to manage international crises. Mitzen, on the other hand, while of course noting the end to the actual congresses after Verona in 1822, tends to treat the whole period under the same rubric, with forum effects still partly operative even after the breakdown of any joint commitment to keep the peace in the run-up to the Crimean War. Jarrett may partly overstate the caesura, but Mitzen could also pay more attention to the transition between the two phases in the 1820s. Along these lines, Mitzen could consider how far the forum effects differ when the venue is a meeting at the ambassadorial or second-tier diplomat level compared to the resplendent summits of rulers and foreign ministers at the full congresses, not to mention their large military and diplomatic entourages, and the greater degree of publicity that usually accompanied them. In general, both Mitzen and Jarrett could delve more into the nature of the talk and of the social environments involved in these face-to-face meetings, whether the etiquette and exchanges of court and salon culture or the role of festivities and display. This would allow them to draw more fully on the scholarship of the new international history and cultural history, alongside diplomatic history or international relations scholars, a line of inquiry I pursue in my own work on the Congress of Vienna.<sup>7</sup> Mitzen herself points to the attempt “to further open up the black box of interstate conversations” (215) as one of the desiderata for future research. I certainly agree, as the diplomacy did not take place in a black box, and this would be one way to investigate those wider spaces and deeper levels of interaction.

Although largely accepting the division between a western constitutional and an eastern autocratic bloc, Jarrett insists on the multipolar framework and “collective hegemony” of

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<sup>6</sup> Philipp Menger, “Die Heilige Allianz – ‘La garantie religieuse du nouveau système Européen?’” in Pyta and Menger, eds., *Das europäische Mächtekoncert*, 209-236; Wolfram Pyta, “Konzert der Mächte und kollektives Sicherheitssystem. Neue Wege zwischenstaatlicher Friedenswahrung in Europa nach dem Wiener Kongress 1815,” *Jahrbuch des Historischen Kollegs* 2 (1996): 133-173.

<sup>7</sup> Brian E. Vick, *The Congress of Vienna: Power and Politics after Napoleon* (Cambridge, Mass.: Harvard University Press, forthcoming 2014); “Der Kongress tanzt, und arbeitet trotzdem. Festkultur und Kabinettpolitik,” in Helene Maimann, Wolfgang Maderthaner, and Thomas Just, eds., *Die Erfindung Europas. Der Wiener Kongress 1814/15* (Vienna: Gerold, forthcoming 2014).

the Vienna system. Hence Jarrett's work distinguishes itself both from those scholars who emphasize a bipolar hegemony between Britain and Russia such as Paul Schroeder, and from those who stress Britain's role as the "leading state," as with G. John Ikenberry.<sup>8</sup> Jarrett to some extent steers a middle course in the debates regarding how far the Vienna system either moved past eighteenth-century competitive notions of balance-of-power to adopt ideas of collective security, as Schroeder and those following him insist, or saw precisely the continuation of such pursuit of state interests and counterweight alliances as the basis of nineteenth-century international relations, as Enno Kraehe and particularly Alan Sked argue (Sked, it should be noted, wrote the Foreword to Jarrett's book).<sup>9</sup> Mitzen, as one would expect, comes much closer to Schroeder's interpretation of the new regime of collective security and to his focus on the system level to explain international restraint. She does, however, offer a different explanation with her emphasis on emergent forum effects rather than the absorbing of new norms in a post-Napoleonic learning process.

Again, given the still-obvious gulf between the styles and approaches of the fields of history and international relations reflected in these two works, I do have doubts as to how far either book will prove able to engage scholars in the respective other discipline. But I hope I am wrong, and it is certainly a dialog that needs to happen. Books like these at least make it somewhat more likely to occur, and putting them together in a discussion forum such as this brings it a step nearer.

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<sup>8</sup> Paul W. Schroeder, *The Transformation of European Politics, 1763-1848* (New York: Oxford University Press, 1994); G. John Ikenberry, *After Victory: Institutions, Strategic Restraint, and the Rebuilding of Order after Major Wars* (Princeton, New Jersey: Princeton University Press, 2001), Ch. 4.

<sup>9</sup> See for example the contributions by Paul W. Schroeder, Enno E. Kraehe, Robert Jervis, and Wolf D. Gruner in the Special Forum "Did the Vienna Settlement Rest on a Balance of Power?" *American Historical Review* 97:3 (1992): 683-735; and those by H. M. Scott, Charles Ingrao, T. C. W. Blanning, Jack S. Levy, and Paul W. Schroeder in the special issue, "Paul W. Schroeder's International System," *International History Review* 16:4 (1994): 663-754; Alan Sked, *Metternich and Austria: An Evaluation* (Basingstoke: Palgrave Macmillan, 2008), 55-57, 63.

Author's Response by Mark Jarrett, Independent Historian

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Let me first thank the reviewers for taking the time to tread so carefully through our books and for their insightful comments.

Just as the year 1989, the bicentennial of the French Revolution, was accompanied by its own spectacular revolutionary events, culminating in the fall of the Berlin Wall, the year that has just ended (2014), both the centennial of the outbreak of the First World War and the bicentennial of the start of the Vienna Congress, has turned scholars and the world at large towards reflection on the origins of and prospects for international order, as demonstrated by such recent works as Mark Mazower's *Governing the World* and Henry Kissinger's *World Order*, as well as our own. Public concern with international order has only been intensified by a seeming return to multipolarity and an unprecedented number of new global challenges.<sup>1</sup>

Few serious scholars would disagree that the years 1814-1815 actually gave birth to the first attempt at multilateral global governance. "Focusing on the Concert makes sense," writes Jennifer Mitzen, "first of all, simply because the Concert is widely acknowledged as the first modern international security institution and thus a precursor to international governance as we know it today." (19) And yet this "audacious experiment" (as I call it in my own book) remains largely unknown outside a limited circle of international relations experts and historians of Europe. I therefore intended *The Congress of Vienna and its Legacy* to serve as a simple introduction and my overriding goal remains that of stimulating interest in a much-neglected period. In short, can we drum up the same enthusiasm for the events of 1814-1815 that we have recently witnessed for 1914?

Two of the reviewers have suggested that Mitzen and I may have launched proverbial ships that passed one another in darkness. George Lawson refers to this as the "eternal divide," the title of his own article on the subject, while the historian Brian Vick fortuitously employs nearly identical language: "I come away from these books with a sense of just how deep the divide between the disciplines really is."

It is therefore this question that I first wish to address. Surely it cannot be accidental that E.H. Carr, a former diplomat whose *The Twenty Years Crisis* is often identified as the seminal work in the emergence of international relations as a discipline, was also the author of *What is History?* And, not surprisingly, Carr pointed out that with an infinite number of facts surviving from the past, a historian needs some way of selecting and organizing those facts—in effect, a theory or organizing principle.<sup>2</sup>

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<sup>1</sup> Mark Mazower, *Governing the World: The History of an Idea* (New York: Penguin Press, 2012); Henry Kissinger, *World Order* (New York: Penguin Press, 2014).

<sup>2</sup> Edward Hallett Carr, *The Twenty Years' Crisis, 1919-1939: An Introduction to the Study of International Relations* (London: Macmillan, 1946) and *What is History?* (New York: Random House, 1961). Lynn Hunt chronicles the flirtation of postwar historians with theory, ranging from Marxism, sociology and

As a graduate student at Stanford, I was fortunate enough to serve as a teaching assistant to the late Alexander George, a political scientist whose *Deterrence in American Foreign Policy: Theory and Practice* was actually a winner of the 1975 Bancroft Prize in history.<sup>3</sup> George was especially interested in the case-study method and what he called the method of structured, focused comparison, best exemplified in his books on deterrence and coercive diplomacy. His work was based on the integration of history and theory—in general, he asked a series of identical questions of each historical case in order to make systematic comparisons and to build more general theories. Later, he produced a well-known collaborative work with historian Gordon Craig, *Force and Statecraft: Diplomatic Challenges of Our Time*, based on the Stanford course they co-taught.<sup>4</sup> Deborah Welch Larson's *Origins of Containment* and Matthew Rendall's articles on the "Eastern Question" are other examples of superb historical work by political scientists who do not limit themselves to the use of secondary sources.<sup>5</sup>

Doubtless some International Relation (IR) approaches are more closely aligned to the historian's craft than others. The closer one moves to a decision-making model with a focus on the primary policy-makers, the closer the picture comes to a 'traditional' history. But the theory that a political scientist develops—the body of testable generalizations and principles that emerge—are only as valuable as the facts upon which they rest, whether based on the political scientist's own research or that of professional historians. Thus, Rendall writes that "while Mitzen's theory is deductively plausible, her cases, on closer inspection, do not seem to support it." This is not to agree or disagree with Rendall—only to point out that his method of testing Mitzen's hypotheses is to return to the historical record. There can be no clearer demonstration of the relevance of history to IR than this. "Theory loses value," concludes political scientist Ned Lebow, "when it does not engage empirical evidence or does so in a self-serving and selective way."<sup>6</sup>

In fact, if we turn to Lawson's own article—as opposed to his review—we discover that what he actually seeks are "more durable links" between history and political science.

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modernization theory to identity politics, cultural anthropology and globalization, in her recent book *Writing History in the Global Era* (New York: Norton, 2014).

<sup>3</sup> Alexander L. George and Richard Smoke, *Deterrence in American Foreign Policy: Theory and Practice* (New York: Columbia University Press, 1974).

<sup>4</sup> Alexander L. George and Gordon Alexander Craig, *Force and Statecraft: Diplomatic Problems of Our Time* (New York: Oxford University Press, 1983).

<sup>5</sup> Deborah Welch Larson, *Origins of Containment: A Psychological Explanation* (Princeton: Princeton University Press, 1985); Matthew Rendall, "Russia, the Concert of Europe, and Greece, 1821–29: A Test of Hypotheses about the Vienna System," *Security Studies* 9:4 (Summer 2000) and "Defensive Realism and the Concert of Europe," *Review of International Studies* 32:3, (2006), 523-540.

<sup>6</sup> Richard Ned Lebow, "What can international relations theory learn from the origins of World War I," *International Relations* 28:4 (December 2014): 387-410, at 409.

While he criticizes the ahistorical use of history as a social laboratory in which political scientists test their theories shorn of context, he identifies four ways in which history and social science can be meaningfully linked—through context, eventfulness, narrative, and ideal-typification. For Lawson, narrative may in fact be the glue that holds these two ways of viewing the world together—“both historians and social scientists are concerned with establishing ‘causal narratives,’ structured stories that explain events and make them intelligible to others.”<sup>7</sup>

If international relations scholars are, in one way or another, reliant on history, can historians remain aloof from international relations theory? I believe this would be a mistake. The task of the historian is to try to reconstruct and explain past events. In the process of reconstruction, the historian may sift through thousands of published and unpublished primary and secondary sources—from contemporary letters, newspaper accounts, and official reports, to the writings of later historians. As Jo Guldi and David Armitage point out in the *The History Manifesto*, it is typical for historians to focus their investigations on periods of five to fifty years—what Guldi and Armitage call the “short past”—in order to master the published and unpublished archival sources, and then to synthesize what these reveal.<sup>8</sup>

This preoccupation with “new” archival findings and the “short past” can generate its own biases. It becomes tempting to interpret documents at face value, or at least to view events from the perspective of contemporaries. In this way, historians are able to develop a necessary empathy and an appreciation for the choices that actors make. But there is a danger that the outside forces and larger patterns that placed the actors in these circumstances to begin with—Pierre Renouvin and Jean-Baptistes Duroselle’s *forces profondes*—may be lost sight of.<sup>9</sup> International relations theory provides a necessary corrective to the professional myopia of the historian. We come to see possible

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<sup>7</sup> George Lawson, “The Eternal Divide? History and International Relations,” *European Journal of International Relations* 18(2) 2010: 203–226. Lebow reaches a similar conclusion, arguing that “structuralist theories and rationalist accounts, which by definition exclude ideas, agency, path dependence, confluence, and emergence from their analysis can never be more than starting points for causal narratives in which the particular must be combined with the general.” Lebow, *op. cit.*, 409–410. On the importance of narrative, see Hunt, *Writing History in the Global Age*, 123–128; on the similarities and differences between history and IR generally, see Thomas W. Smith, *History and International Relations* (London: Routledge, 1999); Colin Elman and Miriam Fendius Elman (eds.) *Bridges and Boundaries: Historians, Political Scientists and the Study of International Relations* (Cambridge MA: MIT Press, 2004), especially the essays by Robert Jervis and Paul W. Schroeder; and Andrew J. Williams, Amelia Hadfield and J. Simon Rofe, *International History and International Relations* (Milton Park: Routledge, 2012).

<sup>8</sup> Jo Guldi and David Armitage, *The History Manifesto* (Cambridge: Cambridge University Press, 2014); see also Paul W. Schroeder, “International History: Why Historians Do It Differently than Political Scientists,” in Paul W. Schroeder, *Systems, Stability and Statecraft: Essays on the International History of Modern Europe* (New York: Palgrave Macmillan, 2004), 285–295.

<sup>9</sup> Pierre Renouvin and Jean-Baptiste Duroselle, *Introduction à l’histoire des relations internationales* (Paris: Librairie A. Colin, 1964) and *Introduction to the History of International Relations* (New York: Praeger, 1967).

connections that we might not otherwise have drawn. “In dealing with these and other problems of recent diplomacy,” Gordon Craig once observed, “we may gain in analytical sophistication if we overcome our congenital distrust of theory and our insistence upon the uniqueness of the historical event. . . [I]n this spirit some of our colleagues in political science have reminded us that one can, after all, on the basis of similarity, treat unique cases as members of a class or type of phenomenon and, by appropriate methods of analysis, discover correlations among different variables that may have causal significance or, at the very least, serve as indicators of predictive value.”<sup>10</sup> The idea that, as a mental experiment, one might change the identities of the individual decision-makers and still reach the same results can be especially salutary.

This is not to discount, however, the role of contingency, of the individual actor or of freedom of choice. In some cases—from Napoleon to Bismarck to Hitler—the presence of a particular individual may have been especially decisive. Would the Congress System have been launched if George Canning had been British Foreign Secretary instead of Lord Castlereagh? In other situations, random events may appear to have impeded or influenced the operation of long-term factors. Would Tsar Alexander have succumbed to Prince Clemens von Metternich’s arguments at Troppau if his beloved Semenovsky Regiment had not chosen that moment to rise up against the harsh discipline of Colonel F.E. Schwartz? We can never know.

Or, as Lawson puts it:

By focusing on events, by ordering and sequencing these events into intelligible narratives, recognizing how people act within certain contexts, contexts that can only be discerned from the vantage points of researchers’ historically situated positions, history does not abhor social science —rather, it requires it. As such, the choice is not one between a historical enterprise which can do with or without theory, but acceptance of the fact that history is a social science. It is an approach that emplots, narrates and analyses causal stories. In this way, history takes its place as an indispensable part of the panoply of social sciences just as social science appears as one among many story-telling enterprises. Both are necessarily implicated in each other, something made clear by a focus on context, eventfulness, narrative and ideal-typification.<sup>11</sup>

That said, it may well be that I could have done more to integrate political science perspectives into my narrative. I might have explored, for example, how the postwar territorial settlement at Vienna and the subsequent attempt at global governance during the Congress System could be used as evidence in the continuing debate between

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<sup>10</sup> Gordon Craig, “The Historian and the Study of International Relations,” Presidential Address to the 97th Annual Meeting of the American Historical Association, December 1982.

<sup>11</sup> Lawson, “The Eternal Divide?,” 221. Robert Jervis likewise concludes that in works of history, analytical explanations are “embedded in the description.” See Robert Jervis, “International History and International Politics,” in Elman and Elman (eds.) *Bridges and Boundaries* 396.

neorealists, neoliberal institutionalists, and constructivists. There is sufficient grist for each of these approaches—for neorealists, the attempts of Britain and Austria, as satiated powers, to obstruct the further expansion of Russia, Prussia and France, and the creation of the Congress System as a “security regime” or “security community” to protect the Great Powers; for neoliberals, the role of the Congress System as a system of international cooperation and as a precursor to later institutions; for constructivists, the role of ideas from Sully and the Abbé St. Pierre to Tsar Alexander and Castlereagh, and equally, the abhorrence of revolution that became nearly universal among European elites. The events of this critical period are simply “fundamental,” as Lawson points out, to our understanding of international relations as a whole.

But the truth is that I saw *The Congress of Vienna and its Legacy* as an introduction, not the final word. I made use of ideal-typification to explain the four possible approaches to the postwar settlement (84-85), reviewed the assessments of the territorial settlement by both historians and political scientists (149-157); and again analyzed their interpretations of the later Congress System (357-363). More than this would have simply been beyond the compass of the present work.

So, what is actually new about the *Congress of Vienna and its Legacy*?

For one thing, it attempts to sketch the background by familiarizing the reader with the basic contours of eighteenth-century Europe, and then to explain how this structure was challenged by the events of the French Revolution. As Charles Maier notes, the wars of the French Revolution “morphed into Napoleon Bonaparte’s wars for empire” while “the adversaries of France fitfully coalesced, sometimes attempted their own alliances with Bonaparte, and finally came together to defeat his over-extended armies.”

These events, confusing enough in themselves, set the stage for much of what was to follow. After reading this introductory section, students and general readers alike should be in a position to judge the degree to which the statesmen of 1814-1815 were responding to earlier changes.

The book pays particularly close attention to the Polish-Saxon Question at the Congress of Vienna, but also summarizes the many other issues discussed there, ranging from the German Confederation and reconstruction of Italy to the Swiss federal constitution, the rights of German Jews, and the British attempt to end the slave trade. Of particular importance was the consideration of a possible general guarantee of borders, which was ultimately rejected.

The book then turns to the Hundred Days and how, in the aftermath of the defeat of Napoleon, the Congress statesmen attempted innovative methods of global governance. In my view, as Vick acutely observes, the apex of this system was reached not at the Congress of Vienna, but at the Congress of Aix-la-Chapelle, when the Great Powers re-admitted France into the Congress System and attempted to create an embryonic world government. It might be said that *The Congress of Vienna and its Legacy* really tells the story of this Congress System—how it was created, how the leaders of the Great Powers then deployed

it in response to the post-Napoleonic unrest that shook the governments of several states (the German states, Spain, Portugal, Naples and Piedmont), how this caused divisions among the powers, and finally, how the system collapsed.

After more than twenty years of carnage and war, the allied leaders were surely devoted to strengthening the prospects for peace. Nonetheless, it is my contention that the main impetus for the foundation of the Congress System was the desire to combat revolution. This was not necessarily a dichotomy: the Congress statesmen closely identified revolution and war in their own minds. The fact remains that they established the Congress System not at Vienna but in Paris after the Hundred Days. For historians, chronology is important.

The next chapters take the story through the Congresses of Troppau, Laibach and Verona; how the Tsar deserted liberalism in favor of repression; how Britain rejected open use of the alliance for counter-revolutionary purposes; how Castlereagh and Metternich were reconciled over the Eastern Question; and how, after the suicide of Castlereagh, French unilateral intervention in Spain and the death of Tsar Alexander, the Congress System finally fell apart. It ended, as I say, with a whimper not a bang, and no one quite realized it was over at the time. But I do see, for reasons explained in the book, a break between the Congress Era and the later Concert of Europe. One has only to read the boasts of Canning in his correspondence (344) or to consider the fact that no more peacetime congresses of top world leaders were held.

The conclusion discusses the reasons why this first attempt at global governance failed, but then traces its legacy through the nineteenth century to the international institutions we possess today, in particular the five permanent members of the Security Council of the United Nations. They are the true heirs of the Congress System—the wartime alliance carried over into peacetime.

Even in this well-trodden field, I have uncovered some new documents or references not previously found in English, which alter our understanding of these events: for example, a report from Prince Schwarzenberg on possible Prussian and Austrian troop movements at the height of the Polish-Saxon crisis at the Congress of Vienna; Prince Karl August von Hardenberg's diary entry in mid-December conceding that much of Saxony was already lost to Prussia; Lord Mulgrave's condemnation of the Secret Treaty of January 1815; new details on the drafting of the Quadruple Alliance in November 1815; details on the drafting of the Holy Alliance based on Stella Ghervas' ground-breaking work (still only available in French, Russian and Romanian)<sup>12</sup>; the Tsar's revealing interview with Castlereagh at the Congress of Aix-la-Chapelle on the importance of parliamentary management for the preservation of the alliance; the memorandum by Johann Peter Friedrich von Ancillon discussed at Aix-la-Chapelle, which called for an allied guarantee of borders; the evidence provided by historian Guillaume de Bertier de Sauvigny that alleged sessions of the allied leaders with the King of Naples at the Congress of Laibach were fictitious; Castlereagh's

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<sup>12</sup> Stella Ghervas, *Reinventer la Tradition: Alexandre Stourdza et l'Europe de la Sainte-Alliance* (Paris: Honoré Champion, 2008).

willingness to countenance Russian intervention against the revolution in Piedmont in 1821 (despite the strictures of his State Paper of May 5, 1820); and new insights from Baron Charles Joseph de Boisilecomte's journal, written at the Congress of Verona.

Guldi and Armitage call for historians to ask bolder questions and to look to the future as well as the past. I may not have succeeded, but these were among the objectives of this book: to set the events of diplomacy in their broader social context; to consider ideas reaching back to the French Wars of Religion and forward to the composition of the Security Council today; to appreciate how new international organizations often arise in response to specific challenges; to emphasize that the post-revolutionary era marked a great turning point, perhaps one of the greatest in modern history, when the established powers finally came to terms with the changes of the Enlightenment and the French Revolution; to reject the idea that the great powers gave up the pursuit of selfish interests in the name of norms, while recognizing that they did agree to act cooperatively in the Congress System; to give Tsar Alexander and his assistants Czartoryski and Capodistrias more of a central place alongside Castlereagh, Metternich and Gentz—rather than relegating them to the lunatic fringe.

The reviewers are correct in singling out my views on the relationship between *Innenpolitik* and *Aussenpolitik*, which differ significantly from Kissinger's in *A World Restored*.<sup>13</sup> The post-Napoleonic period was a critical turning point in the domestic histories of several nations as well as for international relations as a whole. As Maier writes: "Jarrett understands [that] after the French Revolution, the international domain could never remain autonomous: revolutionary transformation in the middle of Europe linked domestic upheaval to international reverberations." Or Lawson: "As Jarrett shows, the Congresses were less about containing war than about thwarting revolution." Or Rendall: "One of the strengths of *The Congress of Vienna* is its emphasis on the leaders' deep-seated suspicion and fear of revolution."

Vick is surely right to point out that there was also a significant concern to avoid another war. After all, the Tsar reputedly had tears in his eyes when the plan for a guarantee of the peace settlement was read to him (148). And these hopes reached back to eighteenth-century dreams of perpetual peace (35-42). But the fears of war that dominated at Vienna were not powerful enough to create an operational Congress system; fears of revolution after Napoleon's temporary return to France were. "The sole aim of the alliance is that for which it was formed," the Tsar later told the French ambassador at the Congress of Verona, with only slight exaggeration: "to combat revolution" (326).

The final issue, raised by Vick, is whether the great powers acted with restraint. John Ikenberry praised the powers for their use of strategic restraint after the Napoleonic Wars,

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<sup>13</sup> Henry Kissinger, *A World Restored: Metternich, Castlereagh and the Problems of Peace, 1812-1822* (Boston: Houghton Mifflin, 1957).

as the title to his famous work indicates.<sup>14</sup> Paul Schroeder's view of the transformation of power—of respect for new norms replacing the *ancien régime* game of the balance of power—tells a similar story.<sup>15</sup> On the other hand, scholars such as Alan Sked, Adam Zamoyski and Pamela Pilbeam see the great powers as continuing to pursue their own selfish interests after Vienna, much as before. Where do I stand on this continuum?

The problem is a complex one and raises questions inherent to the interpretation of documents and identifying the motives of actors. Almost by definition, a system of collective action can be expected to exercise some restraining force. The parties subject their views to common discussion, and may be persuaded or coerced in this process; they may also feel they have to live up to any promises made in concert (this, crudely stated, is the thesis of Mitzen). But it could also be that the participants see the system as just another way of pursuing their existing objectives—in other words, a continuation of self-interest by other means. For example, when Castlereagh told the British Cabinet that it would be wiser to have the Prince Regent adhere to the Holy Alliance as a way to restrain the Tsar (177), and Metternich told Kaiser Franz the same (177), should we then say that, as a member of the Holy Alliance, Austria was subsequently acting with restraint?

Or take the following statement from Castlereagh, also in my book (68). I had seen it earlier in published form, but was especially moved when, sitting in the Haus- Hof- und Staatsarchiv in Vienna, I came across the actual copy that Castlereagh had sent to Metternich, still preserved in a folder of official papers on the Polish Question. I read aloud this same excerpt at a conference alongside Christopher Clark, the author of *The Sleepwalkers* (on the origins of World War I).<sup>16</sup> Clark ends his book with this justly renowned sentence: “In this sense, the protagonists of 1914 were sleepwalkers, watchful but unseeing, haunted by dreams, yet blind to the reality of the horror they were about to bring into the world.” Contrast Clark's characterization of the sleepwalkers of 1914 with Castlereagh's wisdom a century earlier, which provides the perfect counter-point:

The great military Powers of the Continent who have triumphed in the War, should recollect, that they avowedly fought for their own liberties, and for those of the rest of Europe, and not for an extension of their dominion[s]. . . . [I]f the Allied Powers act liberally towards each other, and indulgently to other states, they may look forward to crown a glorious war by a solid, and lasting peace; and posterity will revere their names, not only

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<sup>14</sup> G. John Ikenberry, *After Victory: Institutions, Strategic Restraint, and the Rebuilding of Order after Major Wars* (Princeton, 2001). Ikenberry shows, however, that the British took advantage of the situation to establish their hegemony.

<sup>15</sup> “Here there was unmistakable structural change. A competitive balance-of-power struggle gave way to an international system of political equilibrium based on benign shared hegemony and the mutual recognition of rights underpinned by law.” Paul W. Schroeder, *The Transformation of European Politics, 1763-1848* (Oxford: Clarendon Press, 1994) 580.

<sup>16</sup> Christopher Clark, *The Sleepwalkers: How Europe Went to War in 1914* (New York: Harper, 2013).

for having delivered by their arms the world from a tyrant and conqueror, but for having restored, by their example and by their influence, the reign of moderation and justice. (68)

Here we encounter, after twenty years of war, no sleepwalker but a far-sighted visionary. Like the Tsar, I was overcome with emotion. But diplomatic discourse can be a double-edged sword. This paragraph comes from Castlereagh's memorandum to the Tsar during the negotiations over Poland. Was Castlereagh genuinely urging all the powers to act with meaningful restraint? Or, since Britain had already decided which colonial outposts it would keep and had secured the independence of the Low Countries and the union of Belgium with the Netherlands, was Castlereagh cynically urging restraint on the other powers because Britain had already achieved its own objectives? If so, where was the restraint?

In the following years, were the Austrians and British the great restrainers because they believed in norms and self-restraint in principle, or because they were satiated powers terrified of further accretions of power by either France, Russia or (in Austria's case) even Prussia? Was it only Russia that actually restrained itself? If so, was this the consequence of the Tsar's religiosity, and later of his fear of revolution? And yet if we consider the language that Castlereagh chose to convey his argument, I cannot accept that these were simply guileful, empty phrases. The riddle may never be solved, which is why I wrote that I considered self-restraint to be an incidental by-product but not an essential characteristic of the system (360).

Would I change anything after reading these reviews, other more recent publications, and attending conferences on the Congress of Vienna in Cambridge (Massachusetts), Amsterdam, Salzburg and Vienna itself? In attempting to keep the text a reasonable length, I am doubtless more guilty of sins of omission than commission. If I had the opportunity to revise the book tomorrow, I might pay more attention to the decline of the Spanish, Portuguese and Dutch empires and to the rise of the British one; to the role of international finance; to the perspective of a world-system in transition; to the role of women; to the role of smaller states and wider publics; to the antics of the King of Denmark at the Congress; to the Principality of Liechtenstein's successful strategy for survival; to the creation of the Central Commission for the Navigation of the Rhine; to post-1815 changes to the borders of the Netherlands; to the issue of French refugees who fled to Belgium in 1815, providing the first test case of the use of the ambassadorial conference in Paris; to the independence of the Latin American colonies; and to how the events of the Vienna Congress and the Congress System might be viewed through the prism of a neorealist, neoliberal and constructivist. And I would certainly evaluate the application of Mitzen's theory of "forum effects," public power and the need for a visible hand. Yet even without these additions, I believe *The Congress of Vienna and Its Legacy* remains a valuable introduction to the subject.

In *The History Manifesto*, Guldi and Armitage issue a clarion call to historians to look at longer periods of time, to ask larger questions, to reflect on current significance—looking to the future as well as the past—and to attempt to convey findings and enter into a dialogue with the wider public. My goal was to at least make every effort in this direction.

Although it would be wrong to mechanistically derive a set of lessons from the decade of 1812 to 1822 for today's diplomats, it is nonetheless remarkable how many of the issues they once faced remain with us today: what should be the role of the great powers towards each other and other states? Are they entitled to special roles with special privileges? (This is the issue of hierarchy that Maier raises at the end of his comments.)

When is armed intervention in the domestic affairs of another nation justified? Under what conditions is it likely to succeed? Can sovereign nation-states put aside their individual interests to concert for the common good? Can democratic states deal effectively with long-term problems like global climate change? Can societies achieve major socioeconomic reform while preserving peace and the rule of law? And if so, how can they best do so? Can the international system undergo major transitions peacefully? In particular, can peace be preserved in a multipolar, post-hegemonic environment?

These are surely questions for historians, political scientists, and practitioners alike; one would like to think that they are also issues in which general readers would take a genuine interest. Hopefully, Jennifer Mitzen and I have nudged the discussion an inch or two forward by bringing the communities of historians and political scientists closer together rather than moving them further apart.

Author's Response by Jennifer Mitzen, Ohio State University

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I want to thank H-Diplo for inviting me to be part of this forum, which I understand is an unusual format in that it treats two books from different disciplines side by side. Such a format places a high burden on the reviewers, and I am grateful to Thomas Maddux and James McAllister for putting it together and to George Lawson, Charles Maier, Matthew Rendall, and Brian Vick for participating and taking seriously the cross-disciplinary terms of engagement. It is fair to say that Mark Jarrett and I approached the Congress of Vienna and Concert of Europe with the broadly shared goal of shaking up disciplinary common sense about early nineteenth-century European Great Power politics. In addition, diplomatic history and International Relations (IR) theory that draws on qualitative case studies are natural allies, with more in common with each other than each has with some of the scholarship in their own disciplines. But in many ways Jarrett's and my books are quite different – we bring to our projects different disciplinary training, we are working from different orienting questions and literatures, and we have different anticipated audiences. That is, at the end of the day, Jarrett's is a work of history and mine is one of IR theory. Apart from the merits or drawbacks of each book on its own terms, the question in juxtaposing them is what added value or new insights can be gained by reading them together. More generally, what can IR theory learn from Concert History and what can Concert History learn from IR Theory?

The four reviewers take different stands on the relationship between the two disciplines. Brian Vick (a historian) and George Lawson (an IR scholar), treat History and IR as separate but equal, and agree that it is difficult for the two fields to speak to each other. For Vick, it is nonetheless a dialogue that needs to happen. Lawson is more circumspect and suggests that “which approach readers favour is as much an aesthetic as an intellectual choice.” For Matthew Rendall (an IR scholar) and Charles Maier (a historian), on the other hand, History seems to be the baseline and IR Theory an epistemically dependent junior partner. In that vein, Rendall offers archival evidence that leaders acted based on self-interest, which in his view undermines my argument that collective intentions existed and had effects; and Maier suggests that “so many factors evolved in the thirty years” between 1828 and 1853/4 that the Great Power crises are too incomparable to warrant my focused comparison in chapters four and five.

The difficulties in navigating the relationship between IR theory and history are similar to those characterizing the relationship between theory and observation in science more generally. Contemporary philosophy of science tells us that theories must be examined in light of evidence but that evidence is never by itself decisive because observation is always theory-laden. The best we can do is something like a three-cornered fight between rival theories and evidence.<sup>1</sup> With this in mind, the point I will develop here, particularly in

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<sup>1</sup> Imre Lakatos, “Falsification and the Methodology of Scientific Research Programs,” in Imre Lakatos and Alan Musgrave, *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970), 91-195.

response to Rendall's and Maier's reviews, is that although their reviews appeal only to historical facts, neither Rendall nor Maier approaches my arguments from a theory-free zone, and their own theoretical commitments, which are implicit but discernible from their reviews, inform their critiques in ways that make it difficult for them to fully engage with my argument on its own terms. That is, the fact that their theoretical standpoints remain implicit creates the appearance of a two-cornered fight between my theory and historical evidence, when what is really going on is a three-cornered fight between our rival theories and historical evidence. Viewed from this perspective, I will show that my theory does not fare as poorly as their reviews suggest.

Let me first consider the reviews by Vick and by Lawson. My aim was to contribute to two debates in IR theory, and I could not do that without relying on the work of historians. These two reviews rightly point out that taking my theoretical goals on my own terms, there are important ways that being a better historian would have enriched my theory.

First, there is a longstanding debate about the power of international institutions between IR realists, liberals and constructivists, in which the Concert is used as a data point. One role of my empirical chapters is to tell a different story about governing from the same 'data' about cooperation. That is, I re-read the main sources used by IR scholars who have written on the Concert and offer a different reading of what happened. Thus, while a fair bit of my research involved immersing myself in historians' debates about the Concert, my bibliography includes only English language sources, and mostly secondary ones. Relying on such sources is not uncommon in IR and even highly influential accounts of the Concert such as John Ikenberry's *After Victory* are vulnerable to this critique.<sup>2</sup> But as especially Vick and Maier point out, this limits my gaze, and Vick is certainly correct that it "preselects ... the range of information and spectrum of interpretations."

The second debate I orient the book around is IR scholarship on contemporary global governance, in which there is an unquestioned premise that liberalism and liberal states are a necessary condition for global governance (if states are to remain the focus at all). Both the theoretical tools I develop and the Concert application aimed to disrupt that common sense. It is jarring to see nineteenth-century autocrats doing something we today pride ourselves on being newly able to do – jointly manage violence in the international system. That jarring can force us to see our practices differently. If what they did together in the Concert was governing, then today's great-power governing must be partly like theirs – hierarchical, club-like, and repressive perhaps (as Maier rightly emphasizes) – but also successfully managing violence in the system.

Lawson and Vick wonder about my stress on states and ideological consensus. First, in light of the decline of face-to-face Congresses after 1822, each raises in different ways the question of the role of states in global governance. Vick points out that many face-to-face

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

conversations after 1822 took place through ambassadorial-level diplomacy and not full Congresses. He is probably right that these “wider spaces and deeper levels of interaction” helped sustain the collective intentions of the Concert. These spaces appear but are not thematized in my study. Drawing them in would have deepened my account, though I am heartened by the fact that Vick does not think this would have undermined my larger argument. Lawson proposes that relationships among individual leaders might have had more to do with successful governing than I allow. On that, I am less sure; but taking diplomatic culture into account might provide a way to assess that proposition, insofar as this diplomatic fabric potentially mitigates the importance of individual relationships.

Second, they wonder whether there was really a lack of ideological consensus among the great powers, and feel I under-stress the Concert consensus on stopping revolution. As Lawson argues, the “dynamics of revolution and counter-revolution were central to how modern international order emerged,” and not granting these full consideration leads me to overlook the extent to which 1848 truly marked the Concert’s downfall. Although my concern was not so much to identify the sources of modern international order as it was to highlight the modern practice of governing, Lawson is right. Since governing rests on and requires order, the two are not as separable empirically as they are theoretically. This is roughly in sync with Vick’s concern that by underplaying the extent to which stopping revolution was a common theme for Great Power leaders and elites, I overlook the “degree of relative consensus about the causes and potential solutions to the problem of revolution.” I argue that states can sustain joint commitments irrespective of ideological divides. But if the consensus on the problem of revolution goes deeper than my analysis suggests, I might need to qualify that claim. Examining the content and depth of the Great Power consensus would deepen the understanding of the mechanism and of our understanding of global governing more generally. One way to do this might be along the lines of what Vick suggests, which is taking the approach of Quentin Skinner and others who look at rhetoric in the context of strategies of legitimation.

Nonetheless, granting all this, I would press two points in response. First, ideological consensus on one issue is not the same thing as a harmony of interests across the board, since the Great Powers also had many other geopolitical interests and flashpoints where they remained rivals. Second, given that rivalry, even status quo powers can find themselves sucked into war and in fact there was a great deal of concern in the 1820s that Russia would intervene on behalf of the Greeks, leading to Great Power war. Ideological consensus notwithstanding, then, there was still the separate problem of keeping the peace.

When it comes to Rendall’s and Maier’s reviews, we seem to be talking past each other. Each makes some fair points, but neither convinces me there is something wrong with my theory of global governance because neither grapples with the key concept I’m introducing, collective intentions among states, much less suggest how it might relate to rival concepts, which is the only way we can assess its applicability to the Concert period.

Taking Rendall's review first, I do not recognize my theory in the picture he offers. Rendall summarizes my argument as an amendment to rationalist, neo-liberal regime theory. According to Rendall, I argue that regimes can work without states changing preferences or becoming friends, as long as there are forums. Now, how regimes work is certainly a question that IR scholars have asked, but it is not the question that I asked. How states govern together is the problem I focus on, and it is distinct from the problem of cooperation that underlies regime theory. The cooperation problem is about how order can be achieved by a kind of invisible hand in the absence of a harmony of interests. Governing is about how order (and other goals too) can be achieved intentionally, by a group of states working together as a sort of visible hand. I distinguish the approaches to international order and governing in Chapter One (12-18). Given Rendall's tight focus on the cooperation problem, it is not surprising that he sees my theory as being not much of an improvement over rationalist regime theory. How could it be? Stripped of its core concepts of collective intentions and joint commitments, my theory becomes rationalist regime theory plus talking, and from that perspective, talking is not generally an effective way to provide information about preferences (57-61).

Rendall then argues that my theory is not borne out in practice, by dipping into three historical moments covered in the empirical chapters, to show that forums did little to produce Great Power self-restraint. First, whereas I argue that Austrian Foreign Minister Prince Metternich was trapped by rhetoric to hold a Great Power meeting in 1820, Rendall points out that Metternich had something to gain from a meeting and, because he could foresee benefits, he was not trapped. Second, regarding Russia's non-intervention in the 1821 Greek revolt, Rendall points out that Russia's decision to work with the allies was taken before the Tsar dispatched Count Dimitrii Tatishchev to Hanover. Because those meetings did not change anyone's mind, in Rendall's view, the prospect of a great-power conference in the autumn at most strengthened the Tsar's resolve. Third, regarding Russian restraint in the period from 1826-1832, Rendall argues that the direct participants were strategic calculators with status quo preferences, who did not feel "the need to justify Russian actions for its own sake."

All three of these historical points are well taken. Among IR scholars, Rendall is undoubtedly the best historian of the Concert period. I respect the skills and sustained work that stand behind his ability to intervene by offering evidence gleaned from archival work and primary sources in several languages. In the book I address the arguments Rendall has made in his published work about these cases (144; 165-168). But I do not believe the three points he makes in his review undermine my theory. Of course Metternich had something to gain from a meeting (why else would he have attended, much less organized it?). My argument is not that actors ignore their self-interests when they act together, but that actors faced with mixed motives can work together when they make joint commitments linked to forums. It might be true that Russia wanted to cooperate before sending Tatishchev to Hanover. But is Rendall suggesting that the Tsar had erased all doubt over what to do and that Great Power cooperation would have proceeded smoothly from there, without meetings? Since these states had never before managed to act together, I am not convinced that Tsar Alexander's desires at one particular moment in

time are the smoking gun that Rendall suggests they are. And I can agree that Russia was a status-quo state in the 1820s, but the 'status-quo' motive is a coarse trump card with vague empirical content. None of the powers in the 1820s wanted Great Power war. But security dilemma theory teaches us that status-quo states can nonetheless end up going to war anyway.

As is suggested by the way he has modified my central question into that of rationalist regime theory, Rendall is apparently approaching the historical data from a standpoint in which states are assumed to be rational, unitary actors motivated by instrumental self-interest. Such actors create institutions in order to realize joint gains, and because of the competitive environment of anarchy these institutions always will be fragile. Talking to one another can convey information but it is often cheap because in the competitive environment of anarchy, states have incentives to misrepresent.

This theoretical baseline structures Rendall's review in that it changes my central question and leads him to look for evidence of instrumentalist self-interest that remains unchanged by talking in forums. In my view it limits his gaze in at least three ways. First, Rendall's argument that the Concert was a regime is (ironically) a-historic. The concept of international regime was developed in the 1980s by rationalist IR scholars, yet by treating the forum as a locale for cheap talk, which might but did not necessarily convey information about intentions, Rendall is reading a twenty-first century view of how inter-state forums work into the thought processes of these nineteenth century leaders. This way of treating forums makes it hard to grasp their importance in the Concert period specifically, because it overlooks that in the early nineteenth century, formerly rivalrous Great Powers meeting together to manage problems on the ground was a new practice. It was not clear how to work forums into strategic calculations, because it was not clear that the practice meant the same thing to all the participants. Metternich could not have been certain what would happen. In short, from Rendall's starting point it is difficult to see a key aspect of my argument, which has to do with the institutional novelty of the forum, and the uncertainty that this novelty, as a novelty, itself generated.

Second, Rendall's baseline for considering interests and preferences is expected utility theory, which both conflates intentions and preferences and treats preferences as well defined and stable. The starting point of my theory, however, is to point out that intentions and preferences are different. Unlike preferences, intentions are action-oriented and reflect a commitment to do something (see 32-35). To assert that Russia had status quo preferences and even wanted to cooperate does not tell us that it would actually form the intention to do so – that it would commit to act on that preference when the time came. Moreover, when it comes to preference stability, there is a great deal of psychological research showing that expected utility theory is a poor predictor of behavior and arguing that preference reversals are common and staying on track is hard to do.<sup>3</sup> This is particularly the case in complex decision-making environments (like international security

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<sup>3</sup> Paul Slovic, "The Construction of Preferences," *American Psychologist*, 50, no. 5 (1995), 364-371.

politics) where decision makers routinely face conflicting pressures. Rendall seems to be suggesting that the analyst's task is to discover evidence of a decision maker's true beliefs and desires. But actors often have mixed and changing motives, and beliefs, too, are not constant. Even if at one moment a decision maker has a clearly defined preference, it is hard to say whether or how that preference will be manifest in action.

Third, Rendall's review never mentions the core theoretical concept of the book, collective intention, or considers how it relates to his own rationalist, expected utility baseline. As such, it overlooks the fact that collective intentions are discursive structures that operate at the macro level. Macro level structures do not have unique microfoundations. That is, collective intentions exist relatively independently of the preferences and beliefs in the heads of actors and are consistent with different configurations of motives. Collective intentions are joint commitments that, when combined with forums can steer action toward jointly desired outcomes. As structures, they are more helpful for explaining patterns of effects than particular decisions. The influence of a collective intention on outcomes is measured by specifying the boundaries of outcomes that are made possible (or not) by the terms of the intention, and then looking at actual outcomes and counterfactual possibilities, and looking for the particular mechanisms of collective intentions (forum effects). My proposition is that collective intentions among states can operate over time, despite changes in decision-makers. I might be wrong. But Rendall's evidence does not undermine this claim. Discerning an actor's true beliefs, even if such a thing were possible, would not count as falsifying evidence against a theory of collective intentions.

In sum, Rendall's history is valuable but it does not shed light on my theory, much less disprove it. For that to happen he would have had to have made his own theory of the Concert explicit and to have systematically compared it to mine, including acknowledging the costs of his rationalist approach to historical institutions and weighing those against the benefits of an approach that captures the novelty and contribution of the forum. It also would have had to acknowledge the differences between the explanatory logic of collective intentions versus that of expected utility theory.

Turning to Maier's review, he found my particular "fusion" of theory and history awkward and the book "a curious construction." This is partly because he found the theory to be tautological. In terms of the internal logic, Maier claims that intentionality as I define it "requires an assessment of possible success; it acts like Freud's Ego with respect to Freud's Id." Since I never reference Freud it is difficult to see what he might be talking about. I imagine he is referring to my discussion of intentions in Chapter two, but what I am doing in those sections is less mentalist than Freud and hardly mysterious. Indeed, I explicitly bracket debates about whether intentions are mental states or whether we can access motivational states (e.g., 34-5; 43), a move justifiable by my focus on collective intentions. Maier asks whether, because intentions are close to action, we can only know them by examining actions, which he rightly points out would make the theory tautological. But whatever the difficulties of discerning individual level intentions, I can bracket those in the case of collective intentions because there are observable indicators separate from participants' actions on the ground: public commitments in the historical record and the

content of forum talk (e.g., 61-63). The indicators of intentions are separate from actions; tautology is avoided.

Maier also doubts the power of forum talk, insofar as it “may be as much the result of a negotiable context as it is the instrument creating one.” It is easy to account for cooperation after the Napoleonic Wars, he writes, because actors have longer term time horizons just after destructive wars; while the Crimean War was caused by deep factors that evolved in the thirty years after 1828 that erased the negotiation context and made the failure of 1850s diplomacy a foregone conclusion. True, the Napoleonic Wars left a favorable negotiation context, but that must also have been the case after 1648, 1713, and 1763, and none of those peace settlements led to long time horizons, or more importantly, to inter-state governing. And while the thirty years following 1828 certainly brought many changes, as I discuss especially on pages 204-209, these did not make it inevitable that the Russo-Turkish War of 1853 would spiral into the Crimean War. There is ample evidence that these states did not desire to fight each other and had a precedent of managing the Eastern Question together, and a goal of the chapter is to uncover the contingency of that war in order to show the role that commitments can play and thus the tragedy of that particular case. It seems ironic that I, the IR theorist, am arguing for the role of contingency against a historian. In light of Maier’s skepticism, it was heartening to read that Vick, the other historian reviewer, found the case comparison to be “the most empirically compelling part of the book.”

Maier has two deeper concerns with the book that are more political than theoretical. He suggests that I valorize Concert diplomacy and overlook how it operated as a structure of domination: “pace Mitzen and her appealing idea of concerted governance, is the institution that best expresses her aspirations the forum or the club? And is the key principle one of concerted purpose or systemic exclusion?” I don’t disagree with Maier that the Concert was a structure of domination in which stability among the Great Powers was produced by restraining liberalism and democracy. Maier also is quite right that I do not dwell on these social forces and my focus on the change from Great Power war to Great Power management brackets the dark side of their collective intention.

But bracketing domination is not the same thing as sanctioning it, and I was surprised at Maier’s framing of these as my “aspirations” because at no point in the book do I claim that the Concert was an unmitigated good for Europe. Indeed I distance my argument from that claim up front (5). The points I develop in the book are rather, first, that the relations among Great Powers have relative autonomy from the social forces Maier mentions and it is important to understand the dynamics of those relations on their own terms. To take an analogy, it is true that we cannot write a history of slavery by looking only at the masters. But if we do not look at the masters at all or think about the constitution of their domination, then we cannot fully understand the trajectory of slavery as a social system. In the international case, the Great Power ‘masters’ were armed to the teeth not only against the ‘slaves’ but against each other, with longstanding geopolitical rivalries driving their interactions. In other words, Great Power domination was constituted not only vertically but horizontally, and so to understand that system of domination we must pay analytic

attention to the horizontal, interstate context. Second, I also suggest in the book that Great Power concerting was important because it made the practice of Great Power domination in nineteenth century Europe more visible, which can be seen as a necessary condition for resistance and, ultimately, accountability. Third, it also is true that European Great Power war was itself a kind of domination from which all of Europe suffered and which Great Power concerting held at bay. In its time, from a European perspective, and not just that of the autocrats but also the perspective of those who suffered most from those wars – their subjects – Great Powers working together to avoid war among themselves, while not an unmitigated good, was an improvement over how they had acted before (see 11, 216-7, 224-5, and 228).

Finally, Maier argues that while both books gloss over the sociopolitical foundations of the Vienna Settlement, the omission is more problematic in my case because of the IR theoretical context in which I write. Many IR scholars treat inter-state relations or the ‘third image’ as a distinct systemic sphere with causal power. But to Maier, after the French Revolution “the international domain could never remain autonomous.” Maier here is treating the explanation of international outcomes in dichotomous terms – they are either autonomously produced by international forces or reducible to causal processes at the domestic level – and he is taking what IR scholars would call a reductionist, second-image approach to post-French Revolution European politics. But while no one would dispute the reverberating importance of the social forces unleashed with the French Revolution, there is no reason to treat these forces as if they were in a zero-sum relationship with states or the sphere of international political relations. The choice is not so stark; there is a middle path. Indeed, Kenneth Waltz’s great contribution to the field was to draw out the relative autonomy of this inter-state sphere, i.e., the dynamics that produce international effects irrespective of state regime type or ideology.<sup>4</sup> Waltz focused only on mechanistic logics, but following many IR scholars I believe the relative autonomy of the third image carries over to more social structures, too, such as international law and international society. That is why in chapter three I argue that the possibility of states talking to one another arose in the seventeenth and eighteenth centuries with the development of diplomacy, law, and balance of power/systemic thinking.

Maier’s claim that the international domain could no longer be autonomous after the French Revolution overlooks the social structures of diplomacy and international law, and it even seems to erase the mechanistic causal powers that Waltz and other third image realists attribute to anarchy. As such, in my opinion it is a perspective that few IR scholars would agree with. I think it is too simple to say that after 1789 the interstate sphere gave way to the social forces from below. To this day, the causal power of international society is not reducible to and is relatively autonomous from the sub-state social forces Maier is talking about. At least, that is a key argument of my book. By asserting that the rise of bottom up social forces overrides longstanding geopolitical logics, Maier’s review does not engage that argument, much less refute it.

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<sup>4</sup> Kenneth Waltz, *Theory of International Politics* (Reading MA: Addison-Wesley, 1979).

In closing, I'd like to step back from the particulars of the reviews and return to the larger question raised by this forum about the relationship between IR Theory and Concert History. When it comes to individual scholars choosing what kind of work to do, I agree with Lawson that it is an aesthetic choice which approach one prefers, that of the IR scholar or historian. But because this forum was set up to foster dialogue, I also want to offer a few thoughts about what the goal of our interdisciplinary dialogue should be. Admittedly, before this forum I had not given this question much thought. But reflecting on what I've learned from these four reviews, I would say that a dialogue is surely needed, if only because we cannot escape each other. History and IR theory are not independent, and so even the diplomatic 'separate but equal' approach seems to go not quite far enough.

IR Theory can certainly learn from history: dialogue with diplomatic historians would help me become a better theorist, as exemplified by Vick's point above that the Concert's collective intention may have been sustained by practices at the ambassadorial level and below and not merely by decision maker forums. That point also, incidentally, links up nicely with the 'practice turn' in IR theory, which is recently being applied to diplomatic studies.<sup>5</sup> But I would not go so far as to treat the relationship as one of dependency, where history sets the terms of the relationship and theory yields to history. History is not the arbiter of historical truth; our observations are always theory-laden. Indeed the theory-ladenness of 'what really happened' is what makes this interdisciplinary dialogue potentially fruitful in the first place.

In other words, it is not the case that only IR theorists have something to learn. Historians could improve their practices through dialogue with IR theorists as well. Theoretical lenses make some evidence more salient than other evidence, and historians rely on lenses irrespective of whether they acknowledge that reliance. With this in mind, I found it regrettable that not one of the reviews stresses the importance of theory for understanding history, nor do any of them suggest that Jarrett's book would have benefited from greater attention to IR theory (whether my theory or anyone else's).

So we are left by this exchange with the strong impression that by and large IR theorists are supposed to learn from diplomatic historians, while historians have little if anything to learn from IR theorists. The former is certainly true, and Jarrett's book sounds like one all scholars interested in the Concert would benefit from reading. But for there to be a genuine, productive conversation between our disciplines, both sides need to be prepared to do some extra work. The bridge, if there is going to be one, has to be built from both sides.

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<sup>5</sup> E.g., Iver Neumann, Vincent Pouliot and Ole Jacob Sending, eds., *Diplomacy: The Making of World Politics* (Cambridge: Cambridge University Press, forthcoming 2015).