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Introduction by Richard Ned Lebow

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Introduction by Richard Ned Lebow, King's College London

Barry Buzan and George Lawson have produced a book of grand scope that examines the multiple ways modernity has influenced the world and our theories about it. What they call the 'global transformation' brought about a shift from a polycentric world to a core-periphery order centered on the West. In the process, according to the authors, regional systems of international relations were integrated into a global one. In effect, international relations theories and the discipline of international relations are products of the long nineteenth century. They further contend, and more controversially, that these theories, and the discipline more generally, have neglected this 'global transformation.'

Buzan and Lawson argue that this transformation was the result of four fundamental developments: industrialization; rational state building; ideologies of nationalism, socialism, and racism; and the destabilization of any balance of power. This last development was in turn the product of rapid technological and social change. These four developments interacted in non-linear ways, which accelerated their effects and produced a radically unbalanced distribution of economic and military power in favor of the West. This imbalance was temporary, as globalization in the post-1945 era had a leavening effect.

The authors contend that we are living, and will for the foreseeable future, in a world shaped by the 'downstream consequences' of the transformation. IR's theoretical and substantive agendas reflect this transformation but in a largely unreflective way. The transformation must be made central to IR's framing and conduct of inquiry. This requires a new periodization of the last two centuries and more analysis of the transformation and its consequences.

The lion's share of the book is devoted to documenting the empirical claims of the authors concerning the nature and consequences of global transformation. The concluding chapters explore ways of rethinking international relations, and especially the concepts of power, security, and globalization. The authors make the case for engaging ideas and history more seriously. Above all, they want us to recognize the extent to which IR theory is not some abstract, purely scientific enterprise, but rather a product of the global transformation and thus rooted in our subjective understandings of why it occurred, what it means, and what longer term consequences it is likely to have.

The three reviewers are well disposed to the fundamental argument of the book and generally agree that IR must be an historical discipline. They reject the claim that the discipline has ignored the importance of the nineteenth century or failed to take the transformation it wrought into account. They point out that they and other scholars have written extensively about the nineteenth century and its importance for the practice and study of international relations. Some question the focus on 'modes of production' or how the concept is developed. Edward Keene in particular thinks the concept underdeveloped, insufficiently distinguished by the kinds of material and ideational factors central to Karl Marx and Max Weber, and the pre-modern modes that are said to precede it. Jennifer Mitzen questions the extent to which the nineteenth century should be treated as the crucible for today's world.

All the reviewers conclude on a positive note and consider the book a significant contribution in that it sensitizes the discipline to the extent that its paradigms and theories are historically situated and highly context dependent.

Participants:

Barry Buzan is a Fellow of the British Academy, Emeritus Professor in the LSE Department of International Relations and a Senior Fellow at LSE IDEAS. He was formerly Montague Burton Professor in the Department of International Relations, LSE. Among his books are: *People, States and Fear: The National Security Problem in International Relations* (ECPR, 1983); with Charles Jones and Richard Little, *The Logic of Anarchy: Neorealism to Structural Realism* (Columbia University Press, 1993); with Ole Wæver and Jaap de Wilde, *Security: A New Framework for Analysis* (Lynne Rienner, 1998); with Richard Little, *International Systems in World History* (Oxford, 2000); with Ole Wæver, *Regions and Powers* (Cambridge, 2003); *From International to World Society?* (Cambridge, 2004); with Lene Hansen, *The Evolution of International Security Studies* (Cambridge, 2009); *An Introduction to the English School of International Relations* (Polity, 2015); and with George Lawson, *The Global Transformation: History, Modernity and the Making of International Relations* (Cambridge, 2015). He is currently working on international relations in Northeast Asia, and English School theory.

George Lawson is Associate Professor in International Relations at LSE. His books include: *Global Historical Sociology*, edited with Julian Go (Cambridge, 2016); *The Global Transformation: History, Modernity and the Making of International Relations*, with Barry Buzan (Cambridge, 2015); *The Global 1989: Continuity and Change in World Politics*, edited with Chris Armbruster and Michael Cox (Cambridge, 2010); and *Negotiated Revolutions: The Czech Republic, South Africa and Chile* (Ashgate, 2005). He is currently working on a monograph entitled *Anatomies of Revolution*.

Richard Ned Lebow is Professor of International Political Theory in the War Studies Department of King's College London, Bye-Fellow of Pembroke College, University of Cambridge and the James O. Freedman Presidential Professor Emeritus of Government at Dartmouth College. He has published 30 books and over 200 peer reviewed articles in a career spanning six decades. In 2014, he published *Franz Ferdinand Lives: A World without World War I* (Palgrave-Macmillan); *Constructing Cause in International Relations* (Cambridge); and coauthored with Simon Reich, *Goodbye Hegemony! Power and Influence in the Global System* (Princeton). *Identity and International Relations* (Cambridge) will appear in 2016, as will two edited volumes, one on Max Weber and international relations, and the other, imaginary interviews with great thinkers.

Edward Keene is Associate Professor of International Relations at the University of Oxford, and Official Student in Politics at Christ Church. He is the author of *Beyond the Anarchical Society* (Cambridge University Press, 2002) and *International Political Thought* (Polity, 2005), as well as articles in, among other journals, *The European Journal of International Relations*, *International Organization* and *The Review of International Studies*.

Jennifer Mitzen is Associate Professor of Political Science at the Ohio State University. 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. Her work has appeared in the *American Political Science Review*, the *European Journal of International Relations*, *Journal of European Public Policy*, and *Security Studies*.

Ayşe Zarakol is a University Lecturer in International Relations at the University of Cambridge and a Fellow at Emmanuel College. She is the author of *After Defeat: How the East Learned to Live with the West* (Cambridge: Cambridge University Press, 2011) and has most recently published articles in *International*

Theory, International Studies Quarterly, European Journal of International Relations, Review of International Studies and Journal of Democracy. She is currently working on a collaborative book project entitled *Theorising Hierarchies in World Politics*.

Review by Edward Keene, Christ Church, The University of Oxford

Barry Buzan and George Lawson believe that the world we live in today was largely created during the ‘long nineteenth century’, from the American and French Revolutions to the First World War. They argue that this was when ‘global modernity’, in the sense of the conjunction of industrialisation, rational states, and ideologies of progress (notably liberalism, socialism, nationalism, and racism) established a new ‘mode of power’ that connected every part of the globe, divided it into a core and a periphery, and destabilised relations between the great powers within the core. They further contend that this crucial phenomenon has been overlooked by most International Relations (IR) theorists, and that as a result “IR needs to rethink many of its principal areas of interest and reconsider how it defines much of its contemporary agenda” (10).

This is an important argument, and, as I will discuss below, while I think that some of the more sweeping theoretical claims made here do not stand up to close scrutiny, it represents a formidable contribution to the already substantial literature on the historical development of the modern international system or society. Building on Buzan and Lawson’s earlier interesting statements on issues such as periodization and ‘benchmark dates’ in IR, one of the strengths of the present work is that it draws on a wide range of secondary historical scholarship, conceived throughout on a global basis, and synthesises it into an impressively coherent whole. Here I will consider the work initially in terms of the claim that IR has overlooked the importance of this particular historical conjuncture, discuss the key concept of a ‘mode of power,’ and finally ask how Buzan and Lawson’s argument challenges and informs existing understandings of the nature of modernity in international relations.

The claim that IR has neglected the ‘global transformation’ that took place in the long nineteenth century is not entirely unjustified, but even so it ultimately rests on the rather thin ground that the extensive literature that already deals with nineteenth-century international relations in one way or another somehow falls short of a proper interpretation of the period, and offers little more than “scattered prompts” (48).¹ Buzan and Lawson give three reasons why they believe this to be the case. One is that IR is so enraptured by the iconic dates of 1648 and 1919 that the nineteenth century features only as ‘an Absence’; somewhat ironically, they support this proposition by citing a number of important works by IR scholars on the discipline’s own intellectual history that have all already pointed to the nineteenth century as the time when IR really took shape as a field of scholarly enquiry.² The second, perhaps more telling, complaint is that IR theorists have used the nineteenth century merely as a site from which to gather data, inevitably erasing its historical specificity. Again, however, I am not entirely sure that this charge sticks. As the data-gathering pioneers J. David Singer and Melvin Small were well aware, data is made, not found, and even a process as superficially simple as coding observations requires numerous sensitive historical judgements. One might object to the way that Singer and Small, among others, made these judgements as they constructed their datasets for the

¹ Although they cite much of this literature, referencing some fifty separate works -- most of which are monographs -- between pages 55-61, there are nonetheless some surprising omissions, such as Craig Murphy, *International Organization and Industrial Change: Global Governance since 1850* (Cambridge: Polity, 1994).

² For instance, Brian Schmidt, *The Political Discourse of Anarchy: A Disciplinary History of IR* (New York: SUNY Press, 1998); Duncan Bell, *The Idea of Greater Britain: Empire and the Future of World Order, 1860-1900* (Princeton: Princeton University Press, 2007).

'Correlates of War' database, but that does not mean that they were ignorant of the need for historical interpretation in their activities. In fact, they grappled with a number of concerns that suggest a micro-historical focus -- When did Mecklenburg-Schwerin become a member of the international system? -- that are in some ways even more granular than the broad strokes of *The Global Transformation*.

The third accusation that Buzan and Lawson level at IR is that even those scholars who have written on the period in ways that attempt to capture its discontinuous and transformative character have only been "looking at parts of the puzzle," and so "existing accounts tend to miss the whole" (59-60). This strikes me as a remarkably ambitious statement, since it amounts to a claim that in this book Buzan and Lawson have, for the first time, captured 'the whole' of the process of global modernity, from which it is possible to look down upon the selective and partial offerings of previous studies. Having raised the stakes so high, it is disappointing that the book does not really explain how such a claim might be assessed. At no point do Buzan and Lawson attempt to demonstrate the superiority of their specific conjunctural interpretation by putting it up against rival ways of understanding 'global modernity,' many of which themselves make similar claims to be grappling with 'social totalities,' fundamental 'constitutional principles' or globally extended 'patterns of order.'³

The key concept in *The Global Transformation* that might provide the basis for such a large-scale theoretical play-off is the idea that the period witnessed the emergence of a new 'mode of power,' which extends far beyond the obvious and important changes in the distribution of power. With its obvious echo of the notion of a 'mode of production,' this is suggestive of a grand philosophy of international history behind the argument of the book, one that could perhaps stand comparison with equally extensive claims that it was changes in certain key material or ideational factors that drove the process of global modernity, which is often associated in IR scholarship with the influence of Marxist or Weberian social thought. The problem here, however, is that the 'mode of power' idea is not developed in sufficient detail to allow the reader to understand what this potential grand theory actually looks like.

At the beginning of the book the idea of a 'mode of power' is introduced as "the material and ideational relations that are generative of both actors and the ways in which power is exercised" (1, fn. 2). The term is then seldom picked up during the substantial discussions in the empirical core of the volume: it is telling that references in the index to 'modes of power' reveal a concentration at the beginning and end, with none at all between pages 55-274. Then, at the end, we are told that the mode of power represents "the social sources of power that produce political, economic, military and ideological formations" (307). The original (interesting) idea of generating actors seems less prominent here, and indeed it is rather difficult to distinguish this later formulation from the famous IEMP (ideological, economic, military, and political) model developed by Michael Mann, a work that should hardly be novel to most of the IR scholars who will be reading *The Global Transformation*.⁴

³ For example, Justin Rosenberg, *The Empire of Civil Society* (London: Verso, 1994); Christian Reus-Smit, *The Moral Purpose of the State* (Princeton: Princeton University Press, 1999).

⁴ Michael Mann, *The Sources of Social Power: Volume I, A History of Power from the Beginning to AD 1760* (Cambridge: Cambridge University Press, 1986).

The main problem is that the distinctively modern ‘mode of power’ supposedly produced by the ‘global transformation’ is considered only in terms of its consequences for relations between the West and the rest, and between the Great Powers (308). It is never juxtaposed against other modes of power in a typology, or even a simple historical comparison. For example, there is remarkably little -- other than what is implicit -- on what the *pre*-modern ‘mode of power’ looked like, and so the unique distinctiveness of the modern world is less obvious than it should be. Nor is there much in the way of theory-building about how modes of power change, in terms of a generalisable argument; we are instead given a conjunctural story that is here organised around industrialisation, rational states, and progressive ideologies, and the reader is left to wonder if this is supposed to be representative of how modes of power always change.

In brief, I regard as unproven the accusation that other historically oriented works on the evolution of international relations during this period are partial glimpses of the larger whole represented by the ‘global transformation.’ Buzan and Lawson’s interpretation is certainly plausible, and in terms of its synthetic reach is original, but it is not obvious that it is definitive of global modernity. Moreover, the chief way in which it might aspire to that latter status -- the theoretical insight of the crucial significance of a new ‘mode of power’ - - is not sufficiently developed to provide the philosophy of history that such an argument would require.

To end, I want to ask where these reflections leave us in terms of thinking about modernity, history, and IR theory. One place to start is to wonder what kind of process the unfolding of ‘global modernity’ in IR looks like. One of Buzan and Lawson’s conclusions is that the ‘global transformation’ is a major disjuncture, but one that is “an uneven, gradual, multilinear one, rather than a ‘big bang’” (320). A further question here, though, is the extent of this disjuncture. If we can extend it to the entire ‘long nineteenth century,’ why not take it even further? Perhaps this should be a three, four (or five?) hundred-year story, rather than a hundred and thirty year one. Moreover, the process of global modernity does not require all its changes to have happened together, as implied by the conjunctural idea, but could rather be understood as a kind of sedimentation, with different periods laying down different elements that together go to make up the modern international system. Perhaps the purpose of this sort of enquiry is not to link everything together in the kind of grand synthesis that Buzan and Lawson give us, but to pick apart the various elements that collectively constitute modernity in IR, to explore their diverse and perhaps unconnected origins. There may be parts of this story that are not driven by the broadly liberal congeries of industrialism, rational states, and enlightened ideologies, but which actually lie in the conflict between this and alternative social forces; parts of the story that are perhaps hybrid or non-Western in origin as well.

It is entirely to Buzan and Lawson’s credit that their book opens up space for these kinds of speculations. Not only does their work directly engage with numerous aspects of the conceptual interface between history and theory, for example in their interesting and insightful discussions of benchmark dates, but more than that, this is simply a big and bold interpretation of what modernity means in international relations. The main problem with the argument, as I read it, is that it is presented more as a definitive statement of fact than an interpretive construction. Thus, at the same time as opening up interesting spaces for conversations about what modernity is and how it matters to IR, there may also be a tendency to close them down because of a certain didacticism about the way those questions are answered here. The question of modernity in IR is, dare I say it, rather too large for a single book, even one with the extensive range of *The Global Transformation*.

Review by Jennifer Mitzen, Ohio State University

In *The Global Transformation*, Barry Buzan and George Lawson synthesize an impressive range of material to argue that what they call the long nineteenth century amounted to a profound transformation in world politics – the birth of global modernity – which continues to shape and affect the system today. But they have another agenda as well. Buzan and Lawson argue that the discipline of International Relations (IR) has problematically ignored this transformation, and a deeper understanding of the nineteenth century will better position IR to understand the twenty-first.

Buzan and Lawson's account is a welcome addition to the growing body of historically oriented IR scholarship, not least in that it provides a much less West-centric understanding of the nineteenth century than IR is accustomed to. Speaking more as a consumer than a producer of international history, I found their historical chapters masterfully done, and while I might quibble here and there I am not in a position to challenge their overall account. What struck me more is their argument about why their historical account is important to the discipline. This is mainly found in Chapter Two and, to a lesser extent, in Chapter Ten, where they lay out the claim that IR is constructed in a way that does not merely downplay the nineteenth century but positively occludes how its dynamics produced the very war system IR so obsessively focuses on today. Indeed, they almost suggest that the discipline needs to do so (e.g., 62-63). To that extent, their book can be read as a critical intervention in the discipline that is aimed at unmasking a problematic silence that constitutes the intellectual status quo.

However, as a reader sympathetic to 'bringing history back in' to IR, I finished the book unconvinced by the way the authors use history to critique the discipline, not because I think it is wrong but because I was not sure precisely what the argument amounted to. Buzan and Lawson do not fully commit to the strong critique of the discipline that they often point to, nor do they lay foundations for the pluralism they end up advocating. I was left with a sense that the authors bring the reader to a critical precipice and then pull sharply back on the reins rather than following through with the jump, in four main ways.

First, as someone who also has written on the nineteenth century I might naturally have questioned their premise that IR has "largely forgotten" (46) or been "indifferent" (54) to the nineteenth-century transformation; but in unpacking this claim Buzan and Lawson themselves inadvertently cast substantial doubt on it. They provide ample evidence that the century has not been ignored in Chapter Two (57 ff), where they acknowledge that there was a boom in nineteenth-century studies in the 1990s (60) and discuss the work of several scholars who have dealt at length with aspects of the nineteenth century, such as Jens Bartelson, Rodney Bruce Hall, Martha Finnemore, Jeffrey Legro, Jordan Branch, Ayse Zarakol, Andreas Osiander, John Ikenberry, Dan Deudney, James Mayall, Ed Keene, Shogo Suzuki, Antony Anghie, Robbie Shilliam, Anne Towns, Anne McClintock, and John Hobson. In the historical chapters, they draw on the work of even more IR scholars (e.g., Christian Reus-Smit, Duncan Bell, Srdjan Vucetic). Buzan and Lawson describe IR scholars who engage with the nineteenth century as a "vanguard" (59), but on my reading these numbers reflect more of a movement than a vanguard. Buzan and Lawson also claim that the discipline lacks any nineteenth-century benchmark dates, lamenting their omission as "somewhere between flawed and fraudulent (323)." But that claim seems undermined by the IR scholarship on which they draw, not to mention the fact that the Correlates of War (COW) data set, upon which reams of IR scholarship depends, begins in 1816 (56).

The question is, by what criteria or metrics are Buzan and Lawson judging scholarly neglect? None are offered, but my sense of their bottom line was not so much that the field has ignored the nineteenth century, but rather that IR scholars have missed the sense that the nineteenth century was a profound, cohesive global *transformation*. In short, their claim is that no one has written the synthetic book that they wanted to write. That's fine, but it is a much more modest claim than the charge of disciplinary neglect implies.

Second, and bracketing that concern, even if we accept their claim of disciplinary neglect, the stakes of uncovering it are unclear. On reflection, I think that most IR scholars would be able to list the major changes of the nineteenth century that Buzan and Lawson focus on – industrialization, the rise of the nation state, and ideologies of progress – and few would disagree that they remain somehow important today. If we already know about these changes at the level of practical consciousness, even if we do not all talk about it, what is at stake in uncovering the putative disciplinary silence?

Different silences can have different etiologies and be more or less resistant to treatment. For example, disciplinary silence might reflect nothing more sinister than intellectual path dependency, where problems foregrounded early on were built upon by later scholars. This leaves other problems in the dark, but there is no sense of disciplinary dependence on, or culpability in, the silence. Alternatively, silence might indicate a kind of repression, a positive resistance to bringing certain issues to the fore. In both cases, the scholar's job is to shine a light on and thematize the new problems that come into focus. But in the former case the silence is not necessarily problematic. The latter stance is more political, and implies that change will be difficult because the status quo serves deeply engrained thematic or intellectual interests that will resist movement in ways we may not be consciously aware of.

Buzan and Lawson mostly treat IR as if it simply does not engage the nineteenth century, but at times their rhetoric pushes beyond that reading to imply that IR as a discipline cannot 'think the nineteenth century,' i.e., that for IR to function as an academic discipline requires aspects of that century to be repressed (e.g., 48). This is an intriguing possibility, particularly in light of the discussion of 'scientific racism' in Chapter Four. However, the book does not pursue the possibility systematically, and so I was left still curious as to whether the authors might see their book as political rather than neutral, and, if so, I would have liked to see the authors delve more deeply into why IR cannot think the nineteenth century.

Third, moving to their positive argument about the nineteenth century, I wonder about the status of the century as a rupture. Buzan and Lawson claim (e.g., 5) that the long nineteenth century was so different from what came before that earlier periods, such as the seventeenth century, are practically irrelevant for understanding contemporary international politics. On the other end, changes in world politics since 1919 are somehow not as consequential for thinking about today's political problems and processes as those of the century before.

As with disciplinary neglect, the analytics behind this claim are not clear; and here I am not convinced. For one thing, as Buzan and Lawson acknowledge, ideas such as sovereignty and liberalism, whose nineteenth century instantiations are the core of their argument, were formulated in the seventeenth and eighteenth centuries. If the ideas came from before, why are the politics of those earlier times, which presumably inspired the thinking, not relevant for understanding today? Also, the twentieth century brought enormous political changes, from decolonization and the end of dynasticism to the spread of democracy. True, the seeds of these changes were planted in the nineteenth century, as the authors point out. But why are the nineteenth century seeds more important to the twentieth century than eighteenth century seeds were to the nineteenth?

More generally, it is not clear why the nineteenth century should be treated as the primary progenitor of our world today. Certainly a time traveler from 1750 would find herself more at home in 1850 than a traveler from 1850 would in 1950. This is not mainly because of industrialization or technological change, but because of the vastly different social structures in these different periods. Europe in the 1750s and 1850s was equally dynastic and imperial, for example, whereas by 1950 both dynasties and empires were gone. Indeed, the twentieth century seems to constitute an alternative rupture, and someone who wanted to argue against Buzan and Lawson's argument might say that what we see in the nineteenth century are actually the last gasps of dynasticism more than the birth of modernity. I am not saying that highlighting the nineteenth century is wrong or that this alternative reading is correct. But the burden is on Buzan and Lawson to show that their reading is more correct than other readings, and I did not find the analytical resources in the book to adjudicate between the two interpretations.

Fourth, Buzan and Lawson argue that an IR sensitive to the nineteenth century transformation would be better positioned to "take its place as an 'historical social science' (332)." I wanted to agree, but it was unclear what they actually mean by 'social science,' since their approach to at least two important features of social science, conceptualization and generalization, was scientific only in an extremely loose sense.

Their central concept, mode of power, is imprecise. Buzan and Lawson define mode of power solely in relation to a realist understanding of power as the distribution of material capabilities, which overlooks the fact that there is already a large literature in IR on power that has affinities with Buzan and Lawson's account. How does their notion of generative power (e.g., 1, 307) relate to productive power as developed by Michael Barnett and Raymond Duvall?¹ How does their approach to the qualitative dimension of power (e.g., 327) relate to Daniel Deudney's concept of the composition of power? Deudney's work is at least cited (e.g., 55) but not engaged with by the authors in their own interesting discussion of technological change and qualitative arms racing, where it seems particularly relevant (Chapter Eight; cf. Deudney²). Positioning themselves solely against materialist realism is a missed opportunity. Without having a sense of precisely how mode of power relates to other more social accounts of power in the IR literature, it is hard to see what distinctive purchase their concept gives us on the global transformation.

I also wondered, since such claims are not fully developed, about the extent to which the concept is meant to be primarily explanatory. Buzan and Lawson aim to show how the "social transformations arise from the conjunctural intervention of sequences of events and processes that are causally, but contingently, interrelated (1)." They also claim that today's international political dynamics are rooted in the nineteenth-century configuration, so that by understanding that century we are better poised to manage them today (e.g., 318). Taken together, these claims imply that the causal forces producing the system are identical to the dynamics that sustain it, which is not necessarily the case in general. The causal forces that produce a human being, for example, are fundamentally different from those that sustain human life once it gets going. Similarly, the mode of power that caused the global social transformation might be different from the mode of power that

¹ Michael Barnett and Raymond Duvall, "Power in International Politics," *International Organization*, 59, 1 (winter 2005): 39-75.

² Daniel Deudney, "Regrounding Realism: Anarchy, Security, and Changing Material Contexts," *Security Studies*, 10, 1 (Autumn 2000): 1-42.

sustained it. If so, then understanding the contemporary dynamics of system maintenance is what will help us grasp the trajectory of systemic disruption and demise.

Perhaps, though, their claim is ultimately not so much explanatory as much as thickly descriptive. They sometimes refer to the mode of power more as part of an optic (21, 269, 319) rather than as a concept, and this seems to be more consistent with the way the mode of power only loosely tethers their historical narrative. Optics and concepts are different – an optic renders its object more visible and meaningful, while a concept nails down its object’s properties and ideally provides some explanatory leverage. The mode of power is presented as if it is a concept (e.g., 1), a configuration that will help account for things, which makes the optic language appear as a sort of retreat and raises a question about the role that explanatory concepts might play in Buzan and Lawson’s historical social science.

Another ingredient in social science is generalization, and here, too, Buzan and Lawson’s arguments are not clear. Indeed, the methodologically pluralistic Buzan and Lawson at times seemed at war with a more doctrinaire, anti-positivist version of themselves. Their argument that the nineteenth century was a rupture comes close to denying that pre-nineteenth century versions of concepts we rely on in IR, such as international law, the balance of power, or sovereignty, can be relevant for understanding world politics today. Supplement that with their critique of neopositivism (e.g., 319) and quantitative methodologies (e.g., 56, 332) and they seem to reject the scientific enterprise altogether in favor of a purely historical, narrative approach. But they hedge, acknowledging that there are “a range of important continuities between the current era and previous ones” (320; also, 47, 321) and that comparative work across periods can be productive, albeit only if its claims remain “limited and conditional (321).” Indeed, their goal is “to offer a historical interpretation that is of use across IR’s epistemological spectrum (330).” So the book ultimately is a call for methodological pluralism and historical sensitivity. This is a stance with which I agree wholeheartedly, but it is difficult to reconcile with their argument that the nineteenth century was a profound rupture in the fabric of international life. I was thus somewhat surprised to see it as the upshot of their argument.

In sum, I’m very sympathetic to the overall intellectual agenda of this book. Empirically, there is a great deal to like; its horizon-expanding synthesis is a welcome addition to IR scholarship. Analytically, however, the book makes for a more frustrating read. In each of the issues raised above – disciplinary neglect, repression, historical rupture, and social science – Buzan and Lawson suggest or advance a provocative claim only to subsequently retreat to safer ground. The cumulative effect of their approach is deflating. I wish they had been willing pursue even one of their stronger, more extreme positions (suitably backed up, of course), rather than reining themselves in.

Review by Ayşe Zarakol, University of Cambridge ¹

In 1993, John G. Ruggie argued in his seminal essay “Territoriality and Beyond: Problematizing Modernity in International Relations” ² that “the modern system of states” was in the process of being remade. He also stated: “The long and the short of it is, then, that we are not very good as a discipline at studying the possibility of fundamental discontinuity in the international system; that is, at addressing the question of whether the modern system of states may be yielding in some instances to postmodern forms of configuring political space. We lack even an adequate vocabulary; and what we cannot describe, we cannot explain” (143-4). His solution was to turn to history, and in that particular article, to the transformation period from the medieval to modern. Not many scholars since then have followed Ruggie on this path of turning to history to explain the current transformation.

As a result, IR as a discipline has not had much to say about the transformation that many others believe the global system may be currently undergoing. Other social science disciplines ³ have made serious attempts to locate the present moment in the long durée. IR has in general stayed away from these grand debates about system direction or change (not to mention modernity), focusing either on mid-range (mostly in the U.S.) or meta theory (mostly in Europe) instead. What this implies is that as a discipline we are happy to accept students who are driven to our classrooms with a gut-level unease about the multitude of changes in world politics but have been unwilling to engage with the larger debate among social sciences and humanities as to what may be happening in the world.

In this impressive book, *The Global Transformation: History, Modernity and the Making of International Relations*, Barry Buzan and George Lawson first and foremost challenge this disciplinary dynamic. The book has two major claims. The first argument is that the global international system as we know it today is really a creation of the nineteenth century, which witnessed four comprehensively transformative processes: industrialization and marketization; rational state-building (accompanied by imperialism); the emergence of new ideologies of legitimation, e.g. nationalism, socialism and ‘scientific’ racism; and the destabilization of great power relations due to “rapid technological and social change” (3). By making excellent use of the findings from seminal books on the era from the global history and historical sociology fields—books ⁴ that IR scholars should be but generally are not familiar with—Buzan and Lawson convincingly demonstrate that

¹ This review is based partly on comments I delivered at the London School of Economics on March 18, 2015, on a panel devoted to *Global Transformation*.

² John Gerard Ruggie, “Territoriality and Beyond: Problematizing Modernity in International Relations”, *International Organization* 47.1 (1993): 139-174.

³ See e.g. Immanuel Wallerstein, Randall Collins, Michael Mann, Giorgi Derluguian, and Craig Calhoun, *Does Capitalism Have a Future?* (Oxford: Oxford University Press, 2013), as well as solo books by the same authors; Zygmunt Bauman, *Liquid Modernity* (Cambridge: Polity Press, 2000); Ulrich Beck, *World at Risk* (Cambridge: Polity Press, 2009); David Harvey, *Seventeen Contradictions and the End of Capitalism* (New York: Oxford University Press, 2014); Anthony Giddens, *The Consequences of Modernity* (Cambridge: Polity Press, 2013); Thomas Piketty, *Capital in the Twenty-First Century* (Cambridge, MA: Harvard University Press, 2014), to name just a few examples.

⁴ See e.g. Jürgen Osterhammel, *The Transformation of the World: A Global History of the Nineteenth Century* (Princeton: Princeton University Press, 2014).

many features that seem inherent to the system emerged only in the nineteenth century, and while doing so they also expose why the discipline has been so ill-equipped to deal with systemic change. This brings us to the second major argument in the book, which is that disciplinary IR needs to both recognize the significance of this transformation and reconfigure its identity and agenda accordingly.

In Part III, Chapter 10, by way of concluding, the authors speculate about how the concepts discussed in the book have implications for our thinking about power, security, globalizations, etc. They also discuss how the discussion may illuminate the thinking of existing IR schools on matters such as security, making helpful suggestions such as “Realists need to think more about the mode of power...Doing so will give them a richer and more productive view of the concept they take to be their core concern” (327). As a reader, I appreciated this effort, but I am much less sanguine about the possibility that the card-carrying members of these schools—to the extent they still exist—would be willing to engage with the claims in this book. Ahistorical approaches in IR, for all their claims about universality and generalizability, are ironically very much a product of their time. This is precisely why the discipline has, for the most part, found it difficult to explain systemic change or identify future trends: most of our theories rest on historically contingent assumptions about agency and statehood. They have to be overhauled, not simply modified.

Also in Part III, the authors use their analysis of the earlier global transformation to evaluate the present moment. They suggest that the world may be moving into the third stage of global modernity, which they call decentred globalism, following two stages of centered globalism, i.e. the Western colonial international society [late eighteenth century to 1945] and the Western global international society [post World War II to the present] (273). For example, they predict that “the core will become both bigger...and less Western” (275). They also suggest that ideological division will decrease, and convergence around shared primary institutions will increase (281). There are two observations I can make about these predictions. On the one hand, while sympathetic to the analysis, I am somewhat skeptical about the predictions of the decrease of ideological divisions. It seems to me that precisely because of the history discussed in this book, many countries in the world have incentives to manufacture ideological divisions with the West as a way of anchoring their national identities as authentic,⁵ as demonstrated, for example, by the gay-rights situation in present day Russia. On the other hand, grounding observations in historical trends in the manner of Buzan and Lawson seems to me to be the only sound way to go about making future predictions about the international system while avoiding the very common mistake of wishful thinking. Quibbles aside, Buzan and Lawson give us a very plausible narrative about the trajectory of the international system that is based in historical material data and ideational analysis. For this they are to be commended.

All in all, *The Global Transformation* is an excellent, timely, and much needed contribution to the discipline of IR, and I hope it makes its way to every IR scholar’s bookshelf and every IR student’s syllabus.

⁵ For a more extensive discussion of this claim, see Ayşe Zarakol, “What made the modern world hang together: socialisation or stigmatisation?” *International Theory* 6.2 (2014): 311-332.

Author's Response by Barry Buzan and George Lawson, London School of Economics and Political Science

International Relations, Power, and History

Introduction

We would like to start by thanking the three contributors to this forum for reading our book carefully and engaging with it in a spirit of constructive criticism. In what follows we concede some points, contest others, and give reasons for paths either taken or not taken. We organise our remarks around four main themes: International Relations (IR) and the nineteenth century, the concept of the 'mode of power', the relationship between IR and history, and how to think about IR as a 'historical social science'.

International Relations and the Nineteenth Century

Both Eddie Keene and Jennifer Mitzen argue that, contra our argument, the discipline of International Relations already looks in some depth and detail at nineteenth-century developments. It is not the case, as Mitzen states, that we claim that all IR ignores the nineteenth century. Our book engages with, and cites, the list of people she provides who already work on aspects of the nineteenth century. The crucial issue is whether this literature addresses diverse aspects of that century or thinks about it as a whole. This matters because, without taking this wider optic, the nineteenth century does not register in IR as a *global* transformation. Keene is therefore closer to the mark in seeing our main claim as pointing to the partial ways in which existing accounts have engaged with the nineteenth century. A fairly substantial part of the IR discipline clearly knows that the nineteenth century was important to *some degree*, but does not use this insight to generate accounts of how and why the century was important *as a whole*.

It does not follow, as Keene avers, that we claim to have "for the first time, captured 'the whole' of the process of global modernity." Such a claim would be foolish, as would be seeing the book as a 'definitive statement of fact.' Rather, we see it, as Keene suggests we should, as 'an interpretative construction.' Our claim is that our 'interpretative construction' presents for the first time what Keene calls a 'broad brushstroke' account of global modernity as it affects, and is affected by, IR as a whole. Our book is meant to underline the extent to which virtually every issue in IR either has its roots in, or was transformed by, nineteenth-century developments. This, as far as we know, is an original argument. With very few exceptions, IR scholarship has not seen modernity as the starting point for its collective endeavour. This is remarkable when considered next to the histories and interests of IR's fellow social sciences. But it is the case. Our book explores why this is so and what it would mean for IR to begin with and from an account of global modernity.

Keene is right that we do not test our 'interpretative construction' of the global transformation against alternative explanations in a systematic way. Rather, *The Global Transformation* develops a configurational account of global modernity in which the interactions between three dynamics – industrialization, rational statehood, and ideologies of progress – generate the global transformation through a contingent, historically unprecedented concatenation. This interpretation makes a contribution to existing historical sociologies of modernity by emphasizing its global origins as well as global outcomes. It critiques Marxist accounts by stressing the constitutive impact of ideational schemas in generating global modernity. It stands against constructivist work by stressing the ways in which ideational schemas interlaced with material forces ranging

from imperialism to technological developments. It challenges realist accounts by arguing that transformations in the sources of power are more important than issues around the distribution of power (more on this below). It situates itself against liberal approaches by stressing the contingent, multidirectional, interactive quality of modern international order. Although we set out these contributions in the final chapters of our book, Keene is right that we do not make them the front-and-centre of our argument. Perhaps we should have done so. Either way, we are happy to reiterate them now.

Mode of power

Both Keene and Mitzen argue that one of our key concepts, the ‘mode of power’, is insufficiently developed. We did not, and do not, intend mode of power to be what Keene calls a “grand philosophy of international history.” In Chapter 8, we differentiate between modes of power as these pertain to the pace of military innovation and the emergence of new technological, ideational, and organizational forms. We show how this feature of the modern mode of power destabilized great power relations (permanently), and underpinned a global core-periphery structure (durably but not permanently). That said, we do think that Keene and Mitzen are right to state that we neither elaborate the concept of the mode of power sufficiently, nor explore its analytical utility in carrying out macro-historical comparisons. This is very much on our radar as something to explore in future work.

One issue that is also on our radar for future work is a follow-up to Keene’s question about the relationship between our concept of the mode of power and existing schemas such as Michael Mann’s IEMP model.¹ Mann uses the IEMP model as a means of assessing leaps in power across the whole of recorded human history. When Mann finds a developmental ‘spurt,’ he ‘stops to take a look,’ expecting to see a change in one or more of the four sources of social power he identifies. Clearly our approach bears some similarities to such a strategy. But it is also different in three main ways. First, our temporal and analytical scope is much more limited than Mann’s, being restricted both to the last two centuries and to developments in *international* order. Second, we do not seek to explain world historical change through developments in particular parts of our tripartite configuration but via their intersection. In other words, a shift in the mode of power is something that requires not just a change in one domain of social life, but in all of them simultaneously. It is the concatenation itself that is productive of macro-historical transformations. Third, we build our account on changes specific to that period rather than on a transhistorical framing. This point relates to our general view on historical social science, on which more in the last section.

History and International Relations

Perhaps the main issue raised by our interlocutors concerns the relationship between history and IR. Keene and Mitzen question the timeframe of the global transformation. Keene wants to push back our story to earlier periods, seeing these as a “kind of sedimentation” in which different periods lay down “different elements that make up the modern international system.” We find this idea attractive; it is certainly not one that contradicts our account. Clearly, all three of the master dynamics we highlight have long lineages. However, it was only during the nineteenth century that these developments combined in a way that produced intense consequences on a global scale. The conjuncture we highlight was in place from the early-middle decades of the nineteenth century. From then on, it generated a range of consequences on a planetary

¹ Michael Mann, *The Sources of Social Power, Vol. 1* (Cambridge: Cambridge University Press, 1986).

scale. Our interest is in both identifying the emergence of this conjuncture and explaining its consequences. In this way, we hope to show the deep debt that contemporary international relations owes to the global transformation.

We agree up to a point with Mitzen's assertion that twentieth-century changes may be just as great as those of the nineteenth century. Our book divides the global transformation into two main stages: a Western-colonial period that lasted from the late eighteenth century to the immediate aftermath of World War Two; and a Western-global stage that lasted from 1945 until fairly recently. The key point is that this shift happened within the global transformation. The changes that took place during in the twentieth century can nearly all be understood as the working out of dynamics that were forged in the crucible of the long nineteenth century. The second half of the twentieth century therefore belongs to the nineteenth century far more than the nineteenth century belongs to the eighteenth century. In our view, future historians are likely to look back on the nineteenth, twentieth and twenty-first centuries as a single transformative period, examining the ways in which global modernity reconfigured social relations on a planetary scale.

Contra Mitzen, we are therefore confident that the principal strands of the global transformation assembled during the nineteenth century. We are also confident that the basic ideational, organizational and technological framings of contemporary international relations are downstream iterations of these assemblages. We are less confident about being able to predict further such changes. We spend some time in the book outlining ways of assessing the form, character and scale of macro-historical transformations. But the question as to whether the world is currently undergoing a new transformation involves the realm of futurology. Human affairs are the quintessential open system. And there are many contemporary trends that could be moving human history into a new developmental phase. The digital revolution is, on some accounts, hurtling towards forms of artificial intelligence that might well outpace human capacities. Forms of genetic engineering may make the 'Mark 1' human being inferior and, in time, obsolete. Either development would in one sense be a further iteration of modern technology. But each would also mark a change in the basic structure of human interaction so profound as to constitute another global transformation. On this, only time will tell.

This brings us to Ayşe Zarakol's concern that we are wrong in thinking that a core feature of contemporary world politics, which we describe as 'decentred globalism,' is contained within a relatively narrow ideological bandwidth. We see decentred globalism as distinct from previous stages of the global transformation in that, although global interactions are intensely interdependent (hence 'globalism'), the mode of power that initially permitted a handful of polities to dominate large parts of the planet is now dispersing (hence 'decentred'). In Zarakol's interpretation, the decentred part of this adage is more important than we suggest. Her argument is that world politics is a realm in which rising powers will seek to differentiate themselves against the West – anti-Westernism is a potent force in world politics. We agree. And Zarakol is also right to stress the many examples of how such distinctions act as points of differentiation and tension – the mantra of 'Chinese characteristics' is one obvious illustration of this dynamic.

However, our argument is not that distinct ideological commitments are unimportant to contemporary world politics. Rather, our argument rests on the fact that *all* of the world's great powers share a commitment to the maintenance of global capitalism. This is a substantial shift from twentieth century international order, which can crudely be characterised as one defined by 'capitalism or not.' Strong ideological differences will continue to generate wide disparities in how capitalist polities are governed. But the general commitment to global capitalism shared by virtually all of the world's polities, along with the irrationality of great power war in a

nuclear world, means that the type of systemic conflict that marked the twentieth century is very unlikely to return. That shift, perhaps best defined as one from geopolitics to geoeconomics, is a major change in world affairs.

What does this mean for IR? Although Zarakol is mainly sympathetic to how we conceive the relationship between history and IR, she argues that we are reformist rather than revolutionary in the implications we draw for the discipline. For Zarakol, IR theories should be “overhauled, not simply modified.” We are less convinced than Zarakol that IR theory is a completely lost enterprise. We certainly agree with her that it is too dominated by Western histories and systems of thought. IR would benefit greatly from establishing a more ‘global’ field of enquiry. Our book is meant to help in that regard by demonstrating the relational, global character of modern international order. One of the issues that has plagued IR since its inception has been its failure to contribute significantly to debates across the social sciences. Our book is an attempt to synthesize existing work in IR with that in cognate fields so that such trans-disciplinary conversations can happen more easily. It is here that we see the rudiments of a global IR, one that is simultaneously particular and general in aspiration. It may take some time for the IR super-tanker to turn. But we hope that our book can at least give it a nudge in that direction.

A Historical Social Science

The final issue raised by the forum participants concerns the relationship between history and social science. Most notably, Mitzen takes us to task for failing to outline the contours of a convincing “historical social science.” Here, we detect some major points of disagreement. We do not see a hard-and-fast distinction between history (which Mitzen describes variously as ‘thick description’ and ‘narrative’) and social science (which she sees as necessarily “explanatory”).² On the one hand, systems of thought are derived in and from concrete historical practices: Karl Marx the revolutionary, Carl von Clausewitz the soldier, Sigmund Freud the analyst. On the other hand, doing historical work, like theoretical enquiry, is an act of occlusion. Most histories are causal narratives, by which we mean structured stories that shape particular sequences of events into intelligible plots. These narratives are necessarily interpretative exercises. In this sense, theory emerges from history, while history itself is a theoretical enterprise.

Although this point may seem obvious, it involves a considerable reorientation of much contemporary IR away from a view of social science as being equivalent to mathematics or physics and towards historical sciences such as biology or geology. Although biology and geology work within broad overarching paradigms – natural selection and plate tectonics, respectively – it is only through analysis in which processes are traced, patterns deduced, and taxonomies constructed that knowledge is seen to accumulate. Historical sciences knot together causes, contexts, and nonlinear interactions into wider plotlines that are logically consistent. These plotlines, in turn, act as a means for generating contextually-oriented interpretations.³ The result is accounts, like ours, that assemble historical events into meaningful causal sequences. It is this view of historical social science with its reliance on context, nonlinear confluences, and meaningful causal sequences that animates *The Global Transformation*. In this sense, our argument should be read as both constitutive and explanatory.

² George Lawson, “The Eternal Divide? History and International Relations”, *European Journal of International Relations* 18(2) (2012): 203-26.

³ Richard Ned Lebow, *Forbidden Fruit* (Princeton, NJ: Princeton University Press, 2010).

It is constitutive in that it provides a generative account of how global modernity was produced through the intersection of three macro-processes. It is explanatory in that it offers an account of how these interactions produced a new basis for how power was conceived and practiced, and how international order was structured. This is historical social science as we understand the term.

There are further disagreements in interpretation between Mitzen and ourselves that follow from this basic difference in sensibility. First, we do not think that concepts ‘nail down’ an ‘object’s properties.’ On the contrary, we see concepts as open-ended formations that are constitutive rather than categorical – they change in meaning and significance as they are put to work across time and place. Second, we are not hostile to positivist methods as long as they are part of a suite of approaches to the study of IR. What we reject is the claim, one that is widespread in North American IR, that positive methods are the only legitimate form of knowledge production. Finally, we do not see generalizability as a necessary condition of social scientific work – such a view would see Max Weber, Charles Tilly and many other prominent social scientists de-credentialed in a way that we find difficult to accept. The central claim of our book is that global modernity arose through a particular configuration of industrialization, rational statehood, and ideologies of progress. This configuration was contingently formed in that these dynamics assembled in historically specific form. There was no necessary reason for the global transformation to emerge why, when, and how it did so. Rather, global modernity was a *particular* historical crystallization rather than a *necessary* set of conditions.

What does this mean for the ‘politics of history’? Mitzen argues convincingly that we need to explore this issue more deeply. We agree with her that the relative silencing of the nineteenth century in IR, at least in terms of assessing its full-spectrum influence, matters deeply. It matters because it reinforces the lamentable habit of the discipline to periodise international history in terms of big wars and their outcomes, and thereby leads it to ignore other types of macro-historical transformations.⁴ It also matters because it allows IR to tell a ‘noble’ story about itself as being a post-First World War creation aimed at solving the problem of war and peace, thereby ignoring its darker roots in racism and imperialism.⁵ The ‘don’t look back’ habit of IR underpins a Western-centric blindness to the ongoing consequences of the huge inequalities that opened up during the nineteenth century. These consequences range from the general hold of anti-racist and anti-colonial rhetoric in contemporary IR to specific issues such as ongoing tensions in Northeast Asia and outright conflict in the Middle East. This is serious stuff and Mitzen is right to point to the implications of our argument for ‘forgotten histories,’ whether these histories have been forgotten deliberately or otherwise. As with many of the other excellent provocations raised by our three respondents, we hope to come back to this issue in future work.

⁴ Barry Buzan and George Lawson, ‘Rethinking Benchmark Dates in International Relations’, *European Journal of International Relations* 20(2) (2014): 437-62.

⁵ Robert Vitalis, *White World Order, Black Power Politics* (Ithaca: Cornell University Press, 2015).