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Theory, History, and the Global Transformation
by Barry Buzan and
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The global transformation: more than meets the eye

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Buzan and Lawson's *The Global Transformation* establishes that many of the basic parameters of world politics originated in the 'long 19th century'. Despite finding much to admire in their book, we are concerned that it lacks an explicit theory of change. In its drive to highlight the novelty and exceptionalism of the 19th century, it offers insufficient guidance on two key issues: first, how international relations scholars should situate Buzan and Lawson's 'global transformation' in existing debates over transhistorical processes; and, second, how they should apply lessons from that transformation to understanding emergent trends in the contemporary world. We argue that a more explicit study of causal factors might help account for *why* the 19th century was unusual. We conclude with thoughts about how the field should proceed after *The Global Transformation*. In particular, it points to how concatenating changes could profoundly alter international politics – an approach we term 'Exotic International Relations'. Buzan and Lawson's book therefore serves as a marker for the importance of systematically theorizing how radical potentialities for transformation might rearrange existing structural assemblages in world politics.

The transformations associated with the 'long 19th century' undergird the modern project of social science. Most of the European social-scientific canon assumed that the world was changing, or had changed, in ways that rendered it significantly different from previous eras. Much of that work concerned the how, why, and what of those changes. This preoccupation should not surprise us. As Tilly writes, 'nineteenth-century European observers were not wrong to think that great changes were happening': capitalism 'was undergoing fundamental alterations' and 'European states were entering a new era' (1984, 7, 9).

Buzan and Lawson (2015, 17, 18) remind international relations scholars that they, again to quote Tilly (1984, 1), 'bear the nineteenth century like an incubus'. They argue that, first, many of the parameters of contemporary world politics originated in the 'long 19th century' and, second, these differ – consequentially – from those of prior centuries. Is this change

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'broadly comparable to the shift from hunter-gathering to agriculture'? We certainly agree that both stand as 'unquestionably major developments in world history' (Buzan and Lawson 2015, 17, 18).

But where should we go next? Buzan and Lawson present something of a paradox: if the global transformation of the 'long 19th century' was so radical, then arguably the field should place low value on comparative-historical analysis. That is, their treatment of the 19th century as a profound, following Reus-Smit (2016), 'breakpoint' transformation implies that prior periods offer little to contemporary analysts. But, we argue, *systematically* adopting their 'processual' understanding of change should help spur macro-theorizing about emerging trends with transformative potential comparable to those of the 19th century.

We start by focusing on Buzan and Lawson's *theory of change*. Their position hinges on the broad methodological claim that scholars should adopt a *configurational* approach to international continuity and change. They argue that the 19th-century configuration comprised three elements: industrialization, rational state formation, and ideologies of progress. They offer two additional foci for thinking about variation in international structure: interaction capacity and the 'mode of power'. It strikes us that, in their account, industrialization – and related changes in economic relations – most clearly drives the transformation. Yet they shy away from the larger implications of their account. Consequently, for all its emphasis on avoiding the presentist bias of international relations scholarship, *The Global Transformation* suffers from its own fixations. We conclude with a 'road not taken': the project of 'Exotic International Relations'. The importance of the 19th century consists less in revisiting historical transformations and more on identifying – and contemplating – contemporary revolutionary dispositions.

International change

As Holsti remarks, 'Change, like beauty and good skiing conditions, is in the eye of the beholder' (2004, 12). Reus-Smit's opening essay in this symposium reminds us that identifying 'change' depends on cultural predispositions as refracted through theoretical commitments. Still, Waltz's (1986, 329) claims structure much of our debates: 'Despite all of the changes in boundaries, of social, economic, and political form, of military and economic activity, the substance and style of international politics remains strikingly constant'. Some respond that variation in dominant norms and identities generate variations in the texture of world politics. Others emphasize economic relations: alterations in the mode of production, shifts in the density of economic transactions, and so forth. Still others

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contend that variation in the kinds of units that populate world politics – such as empires, nomadic polities, and national-states – produces distinctive substances and styles of international politics.[1]

Buzan and Lawson combine all of these elements. They understand the structural assemblage created by the 19th century as a tripartite 'configuration of industrialization, rational state-building, and ideologies of progress' that came together to produce a variety of effects across politics, economics, culture, and the military (Buzan and Lawson 2015, 21). They highlight two transposable elements – components of international structure that vary across international systems in consequential ways: interaction capacity and the 'mode of power' (Buzan and Lawson 2015, 14–16, 21, 22, 307). The first derives from Buzan's influential corpus of work. The second remains incompletely defined. They speak of 'power configurations' in terms of 'the scale of social orders and their mode of organization' and their relationship to modes of economic production and extraction. They describe the new '*mode of power*' engendered by the 'long 19th century' with reference to the 'configuration of industrialization, rational state-building and ideologies of progress'. They continue: 'the modern mode of power generated a new basis for how power was constituted, organized and expressed, transforming the ways in which interactions in the international sphere took place and were understood'. Much later, they write that they have 'introduced the idea of a *mode of power*—the social sources of power—and argued that this is a deeper concern than issues of power distribution'.

The nebulousness of the 'mode of power' highlights issues with Buzan and Lawson's use of a 'configurational' heuristic for understanding change. They write that, by 'configuration, we mean a set of interlinked events and processes that concatenate in historically specific form'. The 'basic assumption of this approach is that big events do not require big causes. Rather, social transformations arise from the conjunctural intersection of sequences of events and processes that are causally, but contingently, interrelated' (Buzan and Lawson 2015, 1, fn 1). Such claims are commonplace in qualitative historical social science.[2] Some mechanisms, processes, and social formations appear across time and space. But they almost never appear in the same combinations and sequences. Most outcomes of historical interest derive from unique causal chains, yet we can still generalize about mechanisms, processes, and social formations themselves.

[1] These differing emphases refer to and build upon well-rehearsed categories in the social sciences. See Spruyt (1994), Hall (1999), and Nexon (2009).

[2] Compare Max Weber's notion of 'singular causal analysis'.

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We can ask counterfactual questions about the possible outcomes entailed by differences in sequence, the presence or absence of adjacent processes, and so forth. When comparing across cases, we might focus on explaining 'variability among related structures and processes' (Tilly 1995, 1602).

For Buzan and Lawson, configurational analysis operates in two ways. We find clear traces of configurational causal claims in how they *account* for the great transformation of the 'long 19th century'. Here, they correctly focus on sequence and causal interactions among processes. Second, and more important to their story, it helps identify the three key parts of the assemblage that reconfigured global political, economic, and social relations in their great transformation. They then trace the downstream effects of this assemblage on global politics.

The result strikes us as compelling to the extent that we view the 'long 19th century' as *sui generis*. Yet this emphasis on such a 'breakpoint' sits uneasily with Buzan and Lawson's claims to engage what Reus-Smit calls 'processual' and 'evolutionary' theorizing. By exceptionalizing the 19th century, they sidestep important work in theorizing change. This involves *unpacking* aggregate configurations into constituent processes, events, and mechanisms in ways that enable contingent generalizations. Buzan and Lawson do not tease out processes and mechanisms, assign causal weights, identify moments of contingency, and otherwise explore possible and foreclosed 'paths not taken'. They provide a conceptual apparatus for, but no theory of, change.

Instead they tend to favor aggregation in a way that renders analyzing change difficult. Thus, the 'mode of power' stands as a kind of redescription of their tripartite configuration. But it combines a wide variety of elements that are, in principle, analytically distinct and highly variable – both independently and in terms of their combination – even after the long 19th century. They write of *a* relatively static 'mode of power' rather than *modes* of power that configure, reconfigure, and mutate across time and space – even though many constituent aspects of what they call a new 'mode of power' existed (in Europe or elsewhere) before the 'long 19th century' and have, as they imply, continued to shift in the last century.[3]

Ironically, Buzan and Lawson's *narrative* offers a processual theory of change – albeit one they themselves reject. It focuses on economic transformation. Despite its specific causal interdependence on other developments, the industrial revolution stands as an *analytically* distinct process that intersected with, and thus reconfigured, other phenomena to

[3] Compare Mann's (1986) focus on different kinds of power that configure in historically specific ways.

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produce a specific assemblage by the end of the long 19th century. Take one example, their description of the military revolution: '[t]he improvements in [nineteenth-century military] technology … were deeply embedded in wider industrial-technological transformations. The same types of metallurgical, engineering, and design knowledge and skills that were necessary to produce typewriters could also produce machine guns' (Buzan and Lawson 2015, 249).

Similarly, wealth generated by industrialization abetted the intensification of pre-existing tendencies toward state autonomy. Economic change – first in the form of pre-industrial improvements in manufacturing productivity and then with industrialization – and its reshaping of domestic and intersocietal relations reconfigured existing liberal, racial, nationalist, and socialist precursors to transform them into Buzan and Lawson's 'ideologies of progress'. In their contribution to this symposium, Buzan and Lawson disagree with our assessment. They reaffirm the importance of the causal interaction between their tripartite configuration. But the fact remains that their account suggests a (qualified) econocentric account. This provides an alternative lens on Owens's critique of the ways in which Buzan and Lawson omit 'the social' as a locus of explanatory power. Buzan and Lawson do slight social processes, but they do *not* ignore them; they instead treat 'the social' as an explanandum. Where – and when – they assign causal weight to phenomena, processes of industrialization always emerge as crucial.

Such processual analysis offers a way forward; it also helps to develop their theory of change. For example, Buzan and Lawson might have explored a range of event-based counterfactuals, such as the effects on 'ideologies of progress' if the British had retained control of all their North American colonies or the French Revolution had been quickly suppressed. They might also have explored other counterfactuals, such as if anti-imperial elements of 'ideologies of progress' had scored an early victory and greatly attenuated European imperial expansion. They might also have tackled ginormous ones, such as the absence of an industrial revolution altogether – or the development of an industrial revolution in China, which nearly happened a number of times in the preceding millennium. These all involve high levels of uncertainty and speculation, but they represent what macro-historical accounts *must* do to theorize continuity and change.[4]

[4] Speculative fiction sometimes pursues these. In *The Years of Rice and Salt*, Kim Stanley Robinson imagines a world in which a mutated strain of the bubonic plague wipes out the population of Europe during the medieval period. He traces an outcome not that different from the contemporary world – and thus helps specify the kinds of theoretical assumptions at stake.

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Global transformations before the 19th century

This failure to systematically deploy configurational analysis complicates efforts to assess how the book helps us to theorize change, particularly beyond its specific explanandum. It also undercuts claims about the revolutionary character of the 19th century. Buzan and Lawson downplay discrepant phenomena that challenge their argument that there was a single 'global transformation' linked to a configuration of power in which industrialism was the leading force. Pre-industrial European empires constitute one example. The scale of Spain and Portugal's holdings in the Americas and Asia proved so vast that charting them launched a revolution in cartography with transformative implications for political order (Branch 2011). Britain's colonial expansion into represented a parallel global enlargement of European political order. The depopulation and resettlement of an entire hemisphere represents only, as Phillips argues in his contribution, *part* of the global expansion of European influence before the Industrial Revolution.

This pre-industrial expansion of the European political order deserves more scrutiny. It represents a genuinely global transformation only loosely connected to ideologies of progress or industrialization. In his essay, Phillips establishes how many non-European social orders operated on much larger scales than Buzan and Lawson suggest. And as Phillips (2013, 614) notes elsewhere, 'Westerners gained direct control over core Asian centers of population and production *before* Buzan and Lawson's benchmark date of 1860—that is, in an era when the military spin-offs of the first industrial revolution were either embryonic or non-existent. They did so mainly through a combination of adroit diplomacy and the mobilization of Asian resource portfolios of men, money, and materiel'. The interactions of these 'advanced organic societies' (Goldstone 1998) may pose a greater puzzle to modern scholars than their 19th-century analogues: How *did* globe-spanning relations work when the voyage from London to New South Wales took longer than the Apollo astronauts' voyages to the Moon?

It follows that the problem of counterfactuals proves thornier than Buzan and Lawson suggest. If a global transformation was already underway before the industrial revolution, then we need to ask how this *different* global transformation might have played out. It also suggests some caution about how comprehensive a breakpoint the 19th century represents.

What is to be done?

Buzan and Lawson (2015, 54, 55) hope to change the way we teach and research. They bemoan the absence of the 19th century in international

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relations textbooks, where if 'the nineteenth century is present, it exists mostly as background material'. We hope their book adds to the chorus calling on the discipline to stop talking about Westphalia as the origin of the modern state system and helps introductory courses become more historically literate.

Still, they misread the implications of their claims for researchers. For those who already take history seriously, *The Global Transformation* preaches to the choir. Indeed, for this audience, the critique feels familiar. Many scholars already study international change; much of their work implicitly or explicitly argues that the periods they investigate displayed stark differences from 'global' modernity; and they generally agree that much of what we take for granted about world politics today is socially and historically contingent.

A more pressing question is whether Buzan and Lawson will enable *others* to reorient their work. Do international relations scholars really need to begin every project by recognizing anew transformations wrought by industrialization, 19th-century European imperialism, and the great ideological struggles they engendered? Do feminists, as the authors suggest, really need to 'remember' the 19th century? Do scholars of international organizations not recognize that their object of analysis came into being within the last 150 years? Probably not. Rather, scholars often omit the 19th century because the questions they pose do not *require* studying it as a point of origin.[5] Anievas (2016) describes this process as 'historical aphasia', but that term suggests that scholars cannot articulate concepts sitting on 'the tip of their tongue'. It might be more useful to describe the omission by 'normal science' of deep genealogical investigations as relegating such material to a 'historical attic', where such questions can be stored until needed but ignored while the quotidian business of theorizing about the contemporary, taken-for-granted world goes on.[6]

When Buzan and Lawson look for processes and mechanisms that operate across historical periods and seek to understand how they configure differently in specific times and places, they contribute much to a rich, ongoing debate. They demonstrate that some prevailing accounts of putatively transhistorical dynamics depend on conditions associated with 'global modernity', not least that arms races and intense security dilemmas depend on time-space compressions induced by rapid technological change, accelerated information flows, and the ability to calculate power beyond

[5] See for example, Nexon (2009) and Spruyt (1994).

[6] From a neo-positivist perspective, the book essentially offers arguments about omitted-variable bias, not a reason to abandon the program. For nothing precludes causal relations from being context dependent; that's one purpose of control variables and interaction terms.

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estimating the population of other polities. However, by neglecting a larger conversation about continuity and change, their book (echoing Anievas) is a lost opportunity. For example, to the extent that realists demonstrate that balance-of-power processes or hegemonic cycles operate trans-historically (cf. Kaufman, Little and Wohlforth 2007), or that constructivists show that dynamics associated with international ideological polarization work similarly across time and space (Owen 2012), then they have *ipso facto* already refuted Buzan and Lawson's (2015, 306) concern that 'mainstream IR has put far too much emphasis on the historical continuity of its basic forms and processes …'.

Indeed, the book's arguments arguably point in the wrong direction for 'historical international relations'. If the 19th century profoundly altered the basic texture of international politics, then this *justifies* a presentist bias for the vast majority of working scholars, most of whom study phenomena that emerged after the 'long 19th century'. It implies that what came before, or happened during, the period matters little to our understanding of contemporary dynamics. Anievas suggests that the era from the 17th century to the early 20th century is a more productive subject than the 'long 19th century'. Perhaps so. For us, no matter how we slice the issue of periodization, insisting on the uniqueness of any particular macro-transformation undermines the analytical value of studying the past.

Exotic international relations

But suppose we accept, *ad arguendo*, the critical claim of the book: that the 19th century engendered such a radical transformation that the (distant) past is no longer prologue. What would that mean for attempts to explain the future? Buzan and Lawson (2015, 82) argue that many recent changes constitute an unfolding of processes unleashed in their tripartite configuration. Thus, they characterize 'planetary observation and surveillance' as 'extensions of steps first taken during the nineteenth century'. Yet, the 21st century is producing similar reflections upon, and reactions to, putative changes as those found in the 19th century. It seems problematic to spend little time on these similarities, particularly for scholars convinced that the 'last round' engendered a breakpoint.

The 'steampunk' esthetic serves as a metaphor for this concern (see Musgrave and Nexon 2013). To oversimplify, steampunk constitutes a mode of speculative fiction that extrapolates from futures envisioned in the science romances of the 19th and early 20th centuries. It filters this 'future imaginary' through contemporary sensibilities. The point, of course, is that we do *not* live, by and large, in the future our ancestors envisioned. Thus,

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while we take tremendous pleasure in the steampunk image that graces the book's cover, it illustrates the problems with implicit attempts to extrapolate the future from the past: dirigibles do not rule the skies today. For all the good reasons to embrace steampunk-style humility, it risks locking us into a limited future imaginary likeliest to distract us from taking radical change seriously.

To put this another way: what if the real change wrought by the 'long 19th century' is not in the rate of change, but in the rate of *acceleration* of change? Consider the comparative growth rates of 19th-century Britain and contemporary China. As we argued in response to Buzan and Lawson (2013), 'what is now happening to China is in some ways [the] 19th century in one country—but accelerated to rates unimaginable in, say, 1860. At 1% annual growth rates, a country doubles its GDP about every seventy years; at 10%, it doubles its GDP about every seven. In these terms, the 19th century looks much more like prior periods … than it does like the present' (Musgrave and Nexon 2013, 638). Claims that the 19th century constituted a one-off event overlook the fact that billions of people are just now entering the level of development associated with industrial standards of living. To imagine that a world in which most people live above subsistence levels, instead of just a few, will be just like the past, only more so, would be (in essence) to celebrate that many more people will soon travel on Zeppelins.

These reflections point toward the need to theorize about the deep generative forces that constitute global politics, a point raised by Braumoeller (2016). The main lesson of 'the long 19th century' might involve recognizing extant potentialities for radical transformation. We call this 'path not taken' by Buzan and Lawson the project of Exotic International Relations. Instead of projecting a largely linear future, the lesson of 'the long 19th century' might involve embracing existing potentialities for radical transformation. If the pace of economic change is exponential, not linear, it follows that linear extrapolations will consistently mislead us (Jervis 1997, 29). If novel configurations of processes – some already existing, others just emerging – can restructure world politics, then we need to take the possibility they may do so seriously.

Some scholars have called for broadening the scope of international relations to include topics often viewed as beyond the boundaries of acceptable thinking (e.g. Carpenter 2016). We join these arguments and state their implications more concretely. IR scholars should ask – and offer tentative, if provisional and likely wrong answers to – questions like

- Does the 'resurgence' of religion merely represent how 'very old ideational schema have withstood the challenges of modernity' (Buzan and Lawson

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2015, 317) or presage radical new possibilities akin to the role that 'ideologies of progress' played in the past?

- How would world politics operate *without* major divergences in per-capita GDP? Could international order accommodate a materially egalitarian world? Or would global status hierarchies – and rivalries – instead be produced through other mechanisms?
- If some process (global climate change, secular stagnation) reversed the presumption of perpetual economic growth embedded in the current international order, will states be able to bargain in a negative-sum world more or less successfully than they have in a positive-sum world?
- Does the digital age, and the resultant 'revolution' in information and communications technologies, presage a transformation as profound as the industrial revolution?
- Could global society adapt to a species-threatening event (an asteroid strike or a pandemic)? And would those adaptations prove transformative or ephemeral?
- How would international society adapt to the emergence of non-human (artificial) intelligences?

None of these questions can be answered definitively. But we can offer theoretically and empirically informed analyses – and perhaps narrow down the plethora of answers that others already offer to the Pentagon, Zhongnanhai, and other actors.

Scholars in other disciplines productively engage in forms of extreme theorizing, from biologists considering models of 'exobiology' (ways in which life might emerge in habitats other than the Earth's surface) to physicists theorizing the properties of 'exotic' particles and undetectable forces. Some readers may mutter that these sorts of speculations belong to the realm of science fiction. Maybe so, and the 'Future of War' project at The Atlantic Council has embraced that implication squarely. But this is simply to underscore Buzan and Lawson's point that we *already* live in a world that is science fictional – at least from the perspective of Machiavelli, Thucydides, or any other of the pre-industrial figures often invoked as forerunners to contemporary IR theory. It is not too big a stretch to imagine that Buzan and Lawson could inspire work that charts post-industrial, post-Eurocentric, or even post-anthropocene worlds.

Conclusion and discussion

Although many IR scholars recognize the individual transformations that took place during the 19th century, Buzan and Lawson's great contribution is to provide a persuasive account of the ways in which these

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individual changes became something more profound. Having established that fact, however, they leave the reader with an ambiguous sense of what the 19th century means for readers living in the 21st century. We argue that the field should engage in a more systematic approach to studying the rates and processes of change – not just by studying changes in history, as Phillips and other contributors to this volume suggest, but through imagining the implications of similar global transformations that could afflict the contemporary world. As Buzan and Lawson remind us, processes of change can concatenate and configure in radical ways indeed.

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