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Theory, History, and the Global Transformation

Barry Buzan and George Lawson

Abstract

This response concentrates on two sets of issues raised by the contributors to the symposium: the first centers around theory, the second around history. On the former, our argument is that, contrary to some of the comments made by our interlocutors, the book contributes to theoretical debates ranging from issues of causation to those concerning history as a theoretical enterprise. We use these arguments to engage with, and extend, historical questions raised about the book, paying particular attention to the issue of comparisons across time. Our overarching reaction to the symposium is positive – the book has generated an exciting range of responses. This makes us hopeful that *The Global Transformation* will stimulate both intra- and inter-disciplinary conversations, even if these conversations sometimes emerge from frustration with paths not taken and issues not addressed.

Introduction

We would like to thank the participants in this symposium for their careful readings of, and engagements with, *The Global Transformation* (hereafter *TGT*). We found this a highly stimulating set of responses to our book. We accept some of the points made, but will contest others. In what follows, we organize our response around two main themes: theory questions and history questions. The challenge is whether we can transform a set of constructive criticisms into an impetus towards fellow-travelling, focusing as much on what others *might* do as on what we *need* to do. Can we co-opt our commentators into our ambition to make International Relations (IR) think differently about itself and its subject matter by taking the global transformation more seriously?

On theory

To some extent, we have misled our commentators into thinking that *TGT* does not make a contribution to theoretical debates. Several pick up on the following quote (Buzan and Lawson 2015, 9):

Our aim is not to make a novel theoretical argument regarding the causes of the global transformation – that would require a different book. Rather, we use scholarship in economic history, world history and historical sociology to build

a composite picture of the global transformation, focusing on the ways in which its nexus of intertwined dynamics served to drive the development of modern international relations.

What we mean by this passage is that *TGT* is not an exercise in deterministic, let alone monocausal, analysis.¹ The backstory to the project is important here. We began our research by reviewing the not inconsiderable literature on the whys and wherefores of modernity. Our sense was that debates around modernity had become stuck, so much so that different perspectives had taken on the form of theoretical silos. This fracturing meant that scholars inhabiting different sub-fields rarely spoke to each other and, when they did, debate was usually confined to point scoring rather than fruitful exchange.² Rather than contribute to this non-conversation, our aim was to place diverse literatures into direct dialogue with each other. Not only did this seem important in its own right, it also spoke to our desire to demonstrate how contemporary work in history could add meaningfully to theoretical debates about global modernity.

The starting point of the project, therefore, was to eschew what we saw as often false choices between accounts that stressed the emergence of modern capitalism (e.g. Anderson, 1974; Wallerstein, 1983; Brenner, 1985; Rosenberg, 1994) and those that focused on the war-making capacities of states (e.g. Hintze, 1975[1906]); Kennedy, 1989; Tilly, 1990; Morris, 2014). Likewise, we refused to draw a sharp binary between accounts that favored symbolic schemas (e.g. Weber, 2001[1905]); Ruggie, 1993; Gorski, 2003) over those that concentrated on material determinants (Jones, 1981; Goldstone, 2002; Allen, 2009). The more we engaged with the relevant literatures, the more we realized that such choices were somewhere between unhelpful and wrong. The multiple interactions driving global modernity were so complex

¹ Given this, we reject the characterization by Musgrave and Nexon of our account as ‘econocentric’. It is true that we place significant emphasis on industrialization, particularly as this pertains to war-making. But we give equal prominence to rational state-building (e.g. chapter 5) and ideologies of progress (e.g. chapter 4). We also take great care to connect the three master dynamics into a single assemblage – and it is this assemblage rather than any component of it that drives the emergence and development of global modernity.

² Compare, for example, the contributions to Mahoney and Rueschemeyer eds. (2003) with those in Adams, Clemens, and Orloff eds. (2005). Both are explicitly historical sociological, yet neither contains anything more than a passing nod to the other: the former is mostly concerned with historical institutionalism; the latter with cultural approaches. *TGT* is an attempt to generate a meta-approach that, through attention to the *global* dimensions of historical sociology, both sidesteps turf wars and links cognate work taking in place in sociology, history and IR. For more on this move, see Go and Lawson eds. (2016).

that there could be no master key to modernity. Similarly, by talking past each other, many accounts of modernity were failing to see the cumulative build-up of insights into what Pinar Bilgin (this symposium) usefully calls (following Said) the ‘multiple beginnings’ of global modernity, particularly those developed by transnational and global historians. With this in mind, our goal was not to adjudicate between ‘either-or’ positions. Rather, we aimed to develop a synthetic account in which scholarship from IR, various strands of history, and sociology formed part of a single interpretative schema. This resulted in two points of departure: first, the triad of dynamics that we saw as particularly significant to the emergence of global modernity: industrialization, rational state-building, and ideologies of progress; and second, an overt concern with the global (rather than merely European) origins of modernity. As noted by both Alex Anievas and Pinar Bilgin, *TGT* aims to contribute to the formation of a non-Eurocentric historical sociology in which the insights offered by various modes of scholarship are forged into a composite story.

Approaching global modernity in this way highlights two main theoretical issues: 1) causation; and 2) the relationship between theory and history.

Causation

Several of the contributions to this symposium argue that we lack a ‘theory of change’. That charge is only correct if ‘theory’ is confined to the ‘regularity-deterministic’ accounts that dominate contemporary IR scholarship. Regularity-deterministic accounts see theory as premised on a form of causal analysis that establishes associations between objects that are separated (or at least separable) in space and time (Kurki 2006: 192; Wendt 1998: 105). In these accounts, ‘efficient causation’ acts as a ‘push and pull’ between determinant and regularity: when A (determinant), then B (regularity) (Kurki 2006: 193). If a particular outcome (y) can be traced to a particular cause (x), then the inference is that a set of outcomes (y-type regularities) can be traced to a set of causes (x-type determinants) (Kurki and Suganami, 2012: 403). It is this tradition that Musgrave and Nexon, Braumoeller, and Anievas are working within, even if in the case of Musgrave and Nexon, regularity-determinism is seen as context dependent. These papers are right to note that *TGT* does not work in this tradition. They are also right to note that we did not make our commitments to causation clear. We would like to take the chance to do so here.

Regularity-deterministic accounts of causation rely on a wager about what Andrew Abbott (1988: 170) calls ‘general linear reality’. For Abbott, general linear reality is an assumption that ‘the social world consists of fixed entities (the units of analysis) that have attributes (the variables)’. In this understanding, the interaction of attributes leads to stable patterns, whether these patterns are contextual or transhistorical. Lying behind this wager is the view that social entities are collections of properties that can be disaggregated and the co-variation between their various properties assessed. We wrote *TGT* from a different standpoint. In our understanding, social entities are assemblages that combine in historically discrete ways. As a result, all explanations are ‘case-specific’ in that the processes within which social entities cohere is singular and, therefore, unrepeatable. What we name as social entities – wars, revolutions, depressions, global transformations – are ‘webs of interactions’ rather than collections of properties (McAdam, Tarrow and Tilly 2001, 13; Tilly 2004, 9). They are sequences of events that attain their significance as they are threaded together in and through time. To put this in Abbott’s terms (1988, 179), social entities are ‘closely related bundles’ whose meaning arises from the order and sequence within which their events are knitted together. The focus is not on the disaggregation of entities into discrete properties, but on the relational interconnections that constitute entities in the first place. Our causal explanation relies on generating an ‘intelligible connection’ between ‘closely related bundles’ of historical events (Kurki and Suganami 2012, 404; also see Suganami 2008).

The problem is that all historical events are overdetermined in that there are more causes than outcomes (Adams 2005, 10). As a consequence, all analysis underdetermines the ‘true causal story’ *by necessity* (Little 1995, 53). Indeed, all theoretical work is an act of foregrounding-suppression that simplifies history into hunches about ‘why this and not that’. Theoretical schemas of any kind denote what is significant and what is insignificant about a cluster of historical events. Regularity-deterministic accounts carry out this task by isolating and testing the weight of causal factors that are taken to be particularly significant. Yet such a wager cannot eliminate the effects of the causal factors that lie outside the scope of a particular theory – it simply represses them. In this sense, there can never be theoretical ‘closure’, particularly given that such accounts are, in our view, notably unsuited to examining the *interdependence* of causal processes (Adams 2005, 11-12).

The implication of this argument is significant for how *TGT* unfolds. Rather than approach history as something we can isolate, disassemble and test, we viewed historical dynamics as

temporally specific assemblages. Our first goal was to search for combinations of events that yielded recurrent patterns. Stable accumulations of interactions were constructed into causal narratives that filtered historical complexity into idealized causal pathways (Jackson 2006). These causal narratives were interpretative in that they identified connections that we took to be meaningful. They were also tools of simplification in that they emphasized certain sequences of events and downplayed others. But the causal narratives we constructed were also systematically fashioned and logically coherent (Jackson 2010, 193). This procedure was how we arrived at the tripartite schema of industrialization, rational state-building, and ideologies of progress. Contra Anievas, such an approach bears close similarities to the strategy adopted by Michael Mann (1986), who does not focus on a single cause or combination of causes across world history, but examines how a *particular* constellation of ideological, military, political, and economic dynamics explains *particular* ‘neo-episodic’ spurts. We approached our task similarly, tacking between history and abstraction in order to construct a framework that was ordered and systematic (Tavoy and Timmermans 2014, 123). If we were examining an alternative epoch in world history, the configuration we highlight in *TGT* would have little or no purchase. Lying behind this is an assumption that no single factor or combination of factors can explain world historical development in its entirety.³ All historical change, including the global transformation, is contingent in the sense of arising through unrepeatable nonlinear confluences. As such, distinct times and places require the formulation of distinct causal configurations – something Andrew Phillips (this symposium) demonstrates instructively in its analysis of early modern Asian international society.

Such an orientation is why we disagree with Anievas than an analysis cannot be both contingent and conjunctural. Take as an illustration the work of William Sewell (2005) on ‘eventfulness’. Sewell argues that all historical events are part of broader chains of events. Chains of events have cascading effects in that they both break and reproduce existing social entities – they are ‘sequences of occurrences that result in the transformation of structures’ (Sewell 2005, 227; also see Mahoney and Schensul 2006).⁴ Because they transform fields of

³ For an alternative view from the standpoint of ‘big history’, see Christian (2004) and Morris (2014).

⁴ There are some overlaps between the approach advocated here and historical institutionalism. In line with historical institutionalism (e.g. Mahoney and Thelen eds. 2010; Fioretos 2011), our book shares an interest in sequence, temporality and context. However,

action, events are theorizable categories. Events such as those we highlight in the book, from the emergence of new information and communication technologies to shifting notions of public and private, illuminate the ways in which moments of temporal heterogeneity morph into a unified field characterized by ‘ruptural unity’ (Steinmetz 2011, 59). Out of contingency (dispersed events) comes a historical conjuncture (a unified causal nexus).

Anievas is right to point out that the three dynamics we highlight are structurally related. But it is wrong to say that this amounts to a structural argument. By structural relationship, we mean that the connections between our three primary dynamics are deeply patterned. This is not the same thing as a structural argument, which is centered on the distinct causal power that emerges from the properties of a particular social entity: capitalism, imperialism, liberalism, and so on. Our emphasis is different – on how it was possible that a ‘near miraculous concatenation of circumstances’ (Gellner 1988, 16), marked by an interrelated revolution of productive, coercive, and ideological forces, emerged in a particular time and a particular place. In *TGT*, our causal narrative unfolded through three steps: first, examining the sequences through which history is ‘evented’; second, assembling these sequences into ‘plot lines’ that are logically coherent and supported by the available evidence; and third, abstracting the configurations that sustain these plot lines into a causal apparatus. Hence, in the book, we focus on three main plot lines: industrialization, rational state-building, and ideologies of progress. We assemble these plot lines into a single configuration – the global transformation – that, we argue, helps to unravel how modern international order emerged and how it has changed over the past two centuries. Such an approach cuts against the grain of orthodox approaches to causation, but it certainly fits within a more pluralistic definition of causal analysis (Cartwright 2004): how and/or why a particular outcome occurred where and when it did. In our understanding, a causal explanation is a logical, systematic account of the sources and emergence of a particular outcome (also see Kurki and Suganami 2012).

Theory and history

Perhaps, as the Introduction to this symposium suggests, the issues at stake here are not just about different approaches to causation, but about different conceptions of theory. The binaries established by some of our interlocutors are revealing in this regard: theory vs.

unlike historical institutionalism, the book makes no specific wager regarding institutions as mechanisms for the translation of actions into outcomes.

narrative; explanation vs. description; structure vs. contingency, etc. Some of the critiques raised by our interlocutors appear to stem from our unwillingness to pin our colors to a single theoretical mast. It is true that we did not work within a single theoretical system. It is equally true that we rejected any master-story of the global transformation, whether rooted in capitalism, imperialism, or the emergence of new belief systems. We did not do ‘Theory’ (with a capital ‘T’), or what Pierre Bourdieu (1998, 774) calls ‘theoretical theory’, a jousting match between theoretical positions in favor of one position or another. Our aim was deliberately synthetic both in terms of how we approached IR theories and in terms of how we conducted inter-disciplinary dialogues. So what was the theoretical contribution of *TGT*?

The distinction made between theory and theorizing in Chris Reus-Smit’s opening essay to this symposium helps to answer this question. Following both Reus-Smit and Richard Swedberg (2014, 16-17), we see theory as ‘a statement about the explanation of a phenomenon’ and theorizing as the ‘process through which theory is produced’. In Swedberg’s (2012: 15) reckoning, theorizing is the act of ‘naming, conceptualizing, constructing typologies, and providing explanations’. *TGT* does all of these things. It names both the period it examines (the global transformation) and stages within this period (‘centred globalism’ and ‘decentred globalism’). As discussed below, it offers new concepts, such as the ‘mode of power’, that elucidate the significance of this period. It generates a number of typologies, for example around forms of capitalist governance (see Buzan and Lawson 2015, ch. 9). And it offers a causal explanation of how the global transformation emerged and unfolded. To us, this constitutes theoretical work. What we sense from some of our interlocutors is an anxiety about what constitutes ‘proper Theory’, one paralleled by the singular way in which causation and, oftentimes, science are understood in the discipline (Jackson 2010). Once it has been formulated, Theory takes on a sense of exteriority – an existence outside history. It is, in effect, ‘frozen’ (Swedberg 2012, 15). That is not our view of theory.

This issue speaks to a wider set of concerns about the relationship between theory and history. Much of IR is oriented around an elemental division of labor between theory-building social scientists and (putatively) chronicling historians (Lawson 2012), one that is premised on methods (a focus on secondary sources vs. primary sources); aims (the identification of regularities, determinations and continuities vs. the highlighting of contingency, ambiguity and change); orientation (nomothetic vs. idiographic); sensibility

(parsimony vs. complexity); scope conditions (analytic vs. temporal/spatial); levels of analysis (structure vs. agency), and more (e.g. Elman and Elman eds. 2001). Such a division of labour is, in turn, premised on a mischaracterization of history as an enterprise concerned with ‘narrative’ and ‘description’ (terms used to characterize *TGT* by several of our interlocutors), but not theoretical work.

Even a cursory glance at historical scholarship makes clear that this is not the case. Not only are many historians also theorists (e.g. White 1975; Jenkins 1991; Zimmerman 2010), but theory does not emerge in a vacuum – it arises from somewhere, and that place is history. Just as Hegel’s notion of master and slave was rooted in his reading of the Haitian Revolution (Buck-Morss 2000), so too were the ideas of Dubois (Magubane 2016) and Fanon (Shilliam 2009) based on their encounters with racism and colonialism. Grotius generated his ideas about the laws of the sea from the practices of the Indian Ocean system (Alexandrowicz 1967; Steinberg 2001), just as trade between Britain and India helped to form Adam Smith’s ideas about free trade (Erikson 2016), and the 1870 Franco-Prussian War stimulated new ideas about strategic thinking (Gray 2012, chs. 4 and 5). Theory does not inhabit a realm that is exterior to history. Rather, theories arise historically, formed amid the encounters between theorists and the events they experience and, sometimes, take part in: Marx the revolutionary, Clausewitz the soldier, Freud the analyst (Barkawi and Lawson, 2016). In this understanding, theories are a living archive of events and experiences. We say ‘living’ because theories are not only derived in and from history understood as ‘the past’, they are also recrafted as they encounter new histories. In other words, theories are assessed and reassessed, made and remade through ongoing encounters with history.⁵

If ‘theorizing’ (in Swedberg’s terms) is, perhaps, alive to the co-constitutive relationship between history and theory, Theory is often marked by its abstraction from history. Mainstream IR theory is a good example of how the historical tracks in which Theory is formed are subsequently covered up: realism, liberalism, and the English School are mainly abstractions of 19th century European historical practices. Reinforcement of this point is what we see as the principal contribution of Patricia Owens’ contribution to this symposium. Like Owens, we see IR’s main theoretical positions as rooted in 19th century developments.

⁵ One well-known example of this is the shift within structural realism that took place after the end of the Cold War. Wohlforth (1999) remains the landmark statement.

Indeed, at the beginning of chapter 4 of *TGT*, we relate the rise of social science to specific historical dynamics contained within the global transformation. In later chapters, we discuss how various social policies were forged in reaction to the challenges posed by unruly, often revolutionary, publics. Clearly there is much more to be said about such dynamics (e.g. Owens 2015), but our analysis is complementary rather than contradictory to the position Owens avows: that concepts and categories of thought should always be subject to the denaturalizing impulses of historicization.

Where we disagree with Owens is on the swallowing of all uses of ‘the social’ within a single point of origin: the scaling-up of notions of despotic household governance. Owens usefully tracks this mode of thinking to Comte and his successors: Durkheim in social theory; Bismarck in politics. She also usefully points out that these associations are crucial in a discipline like IR that has both imbibed Durkheimian categories and concerns (Goddard and Nexon 2005), and sees as one of its core concerns practices of counter-insurgency that are often described as ‘armed social work’. However, there was not just one concept – and use – of the social during the 19th century. Alongside those who used the term to domesticate, pacify and discipline subject populations were those who used the social for precisely the opposite purposes: to empower populations, to contest power and to oppose existing systems of governance. Marx, Engels and their successors used social concepts and categories (social class, social change, social movements, social revolutions) to oppose everything that Comte, Durkheim and Bismarck stood for. This was not a ‘partial exception’ to the processes Owens outlines, but a diametrically opposed understanding of how the social emerged, what it meant and how it was put to work.

Despite this point of disagreement, we see the issues raised by Owens as complementary to our analysis in three main ways. First, the discipline needs to be much more aware of how its main approaches are abstractions from 19th century Western history. If IR had started in some other place and some other time, it would look quite different than it does now (Acharya and Buzan 2010). Second, all work, including that engaged closely with history, makes choices about what to foreground and what to suppress: to paraphrase Robert Cox, history is always for someone and for some purpose. Given this, it is surprising that none of our interlocutors question the triad that we see as underpinning global modernity. Perhaps because of their sense that *TGT* simply parsed the available scholarship into an uncontroversial assemblage, our critics do not question the act of interpretative adjudication through which we selected

some dynamics and downplayed others. Yet our choices of industrialization, rational state-building and ideologies of progress are exactly that: choices. Clearly we think that our choices are sound. But equally clearly, our claims are built upon acts of selection – there is no ‘total history’ any more than there can be a ‘total theory’. The act of interpreting which dynamics to highlight in a causal narrative such as that in *TGT* is a necessarily *theoretical* one.

The final issue prompted by Owens, as well as the essay by Chris Reus-Smit, is the crucial role played by concepts in historical work. As Bourdieu puts it, ‘concepts should always be picked up with historical tweezers’ (in Steinmetz 2011, 59). In *TGT*, conceptual work took a range of forms, including the division of modern world history into three periods (‘Western-colonial’, ‘Western-global’, and ‘decentered globalism’), the pooling of diverse ideational schemas into a single thread, and the formulation of the ‘mode of power’, which was used to signify foundational shifts in the ways in which power is expressed and conceived. We see the latter as a useful shorthand for thinking about the ways in which social relations are ordered. During the global transformation, the interactions that structured social life became mediated through the configuration of industrialization, rational statehood and ideologies of progress. As illustrated by terms ranging from the ‘standard of civilization’ to ‘free trade’, this assemblage amounted to a new grammar through which peoples, polities and institutions related to each other. Musgrave and Nexon are right to ask for more than the ‘nebulous’ formulation we offer of this concept in *TGT* – in this sense, our use of mode of power is typical of what Robert Merton (1984, 267) called a ‘proto-concept’: an ‘early, largely unexplicated idea’ rather than a fully formed construction. If the mode of power is to be more than a proto-concept, we need to differentiate changes *in* and *of* the mode of power, and to ground these in a broader historical framework.

To summarize: rather than take up the standard notion in IR of an elemental division of labor between theory and history, our working assumption mirrors the formulation offered by Chris Reus-Smit: theory and history are necessarily co-implicated. Indeed, we see our book as simultaneously descriptive, heuristic, analytical, interpretative, explanatory, synthetic and theoretical. We could have done more to make this rather ambitious claim more explicit. But it is certainly there implicitly in the sense that the theoretical and historical components of the book are not separated through the artifice of a ‘theory chapter’ that is surgically removed from a set of ‘case studies’ only for the two to be later sutured back together. There was no

explicit statement of theory construction, let alone an attempt at formal verification against ‘the historical record’. Rather, in *TGT*, theory and history are mutually reinforcing components of the causal story that we tell.

On History

If there is disagreement over how *TGT* approached issues of causation and the relationship between theory and history, there is surely no such disagreement over the idea that effective theorization is impossible if what we have to work with are faulty historical materials. Because history is the only ‘data’ we have, it is vital that we give a ‘good enough’ account of it. Because all theoretical claims rely on assumptions about history, all theorists are also, up to a point, historians. It does not follow, though, that theorists make good historians. Over recent years, IR has been castigated for the parochial nature of its historical imagination (Buzan and Little 2001; Bell 2007; Shilliam ed. 2011; Hobson 2012; Tickner and Blaney eds. 2012; Vitalis 2015). *TGT* aims to contribute to an expansion of this imagination, helping the theoretical parts of the discipline become as ‘worldly’ as the history it confronts.⁶

The problem is that much of IR tends to ignore the global transformation, willfully or otherwise. The authors of Musgrave and Nexon claim that IR scholars only neglect the 19th century because ‘the questions they pose do not require studying it as a point of origin’. This is not our sense. Our view, backed up by the research we conducted on reading lists, textbooks and journal articles makes us think that many, and perhaps an increasing number, of scholars and students are likely to hold only the most superficial knowledge of the importance of global modernity in shaping contemporary world politics. We therefore needed *TGT* to expose the costs of this neglect in a way that made people take notice. Our contention is that, if we can make people *realize* the importance of global modernity by relating it to the terms in which they do their work, then, and only then, will they *see* it. Did we succeed?

⁶ We are aware here that we are overstating our case. Realists have conducted an extensive debate into the operation of the balance of power across world history (e.g. Kaufman, Little and Wohlforth eds. 2007), as have constructivists on the origins of the modern state (e.g. Ruggie 1993; Philpott 2001; Reus-Smit 2013). There are many insightful mainstream studies of far off places and times (e.g. Tin-bor Hui 2005; Eckstein 2009). However, our contention is that these are exceptions rather than the norm. In our view, most analysis in mainstream IR theory is both Western-centric and presentist, by which we mean predominantly focused on the 20th century Anglo-American world.

As noted, *TGT* required prioritizing some themes rather than others, some times rather than others, and some events rather than others. We are happy to defend our choices, but with the proviso that we are open-minded about exploring the themes, times and events that others think are poorly served in the book. We see our choices as vulnerable to two main lines of critique: the first exposes what was left out of the period under consideration; the second questions the book's temporal focus by arguing that the changes in question had important antecedents. Our critics take aim from both directions, and they make some very good points. The question is not so much whether their points are valid, but whether they complement or contest our account and, if the latter, to what extent they do so.

On the former, we do not think that any serious test can be conducted on the basis of whether the book contains gaps – by their very nature, books leave out far more than they contain. This means that any gaps highlighted by our critics need to be significant enough that they either challenge the main contours of our account or are so important that the narrative cannot succeed without them. Here, one issue raised by our interlocutors seems particularly important – the voices of ‘others’ discussed by Pinar Bilgin. Bilgin is right to push us harder on Eurocentrism, going beyond an account of ‘multiple beginnings’ towards incorporating ‘others’ conception of the international. Although one of our central concerns in *TGT* was to make clear the co-constitutive histories that tied together core and periphery, we did not do much to interrogate the ways in which the voices of those in the periphery challenged our core narrative. As Bilgin makes clear, the goal here is not the validation of ‘others’ conceptions of the international as if they were isolated, ‘authentic’ voices. *TGT* is thoroughly, and in our view rightly, relational in its stressing of the entangled histories that generated global modernity. We look forward to seeing how the project instigated by Bilgin challenges the explanation we offer in the book, not least because it deepens what we see as a potentially productive inter-disciplinary conversation between IR scholars and those working in global history (e.g. Bayly 2004; Rosenberg ed. 2012; Iriye ed. 2014; Osterhammel 2014). In this conversation, themes of connection, entanglement and relationality take centre stage.

On the latter, Musgrave and Nexon, Anievas, and Phillips all stress the considerable antecedents to the 19th century global transformation. To be clear, our guiding motivation was to show that, during the 19th century, a cluster of deep changes occurred together, on a global scale, and within a relatively short span of time. As we note in the book, all of these changes had deep roots, but our focus on clustering rather than roots stemmed from the ways

in which, during the 19th century, these changes not only flowered, but began to have major consequences on a *global* scale. This means that there is no conflict between our account and that offered by Andrew Phillips on the ways in which early modern regional orders acted both as precursors to the 19th century global transformation and as filters through which it was globally refracted. Phillips stresses the importance of an early-modern ‘Eurasian Transformation’ based on developments in world trade, the consolidation of absolutist states, and a civilizing process of containing warrior classes. We welcome this exercise in periodization and regional integration – his work on this subject is extremely exciting (Phillips and Sharman 2015). Such work is likely, in our view, to provide sturdier foundations through which to assess the ways in which earlier formations interacted with 19th century developments to form the main contours of contemporary world politics.

If the contribution by Phillips is potentially complementary to *TGT*, that is not the case with the argument put forward by Anievas that understandings of ‘modern’ international order should be extended to a period running from roughly the mid-17th century to the early 20th century. In our previous work, we argued that such a periodization over-privileges the experience of modern Europe and focuses the discipline too tightly around wars and their settlements (Buzan and Lawson 2014). More importantly for the purposes of this symposium, what lies behind this periodization is the view that the dynamics we highlight in *TGT* can be traced back to earlier times. As we make clear in the book, they (nearly always) can. The key point is that, during the 19th century, even well established ideas and practices were contested, abandoned or reformulated. Racism, which Anievas uses as an illustration of his argument, is a good example. Although racism has deep roots, our view (which we discuss at length in chapter 4 of *TGT*) is that *only* during the 19th century did a powerful conjuncture emerge between racism and the authority of a ‘scientific’ validation that linked the legitimization of discrimination to biological (and therefore fixed) rather than cultural (and therefore malleable) markers. This was an important change. Cultural differentiation pointed towards a civilizing mission in which higher cultures should help lower cultures to ‘improve’. Biological differentiation, in contrast, pointed towards displacement through direct occupation, selective breeding (eugenics) or outright extermination. As Duncan Bell (2013, 1) notes: ‘for the opening few decades of the [20th] century, race was widely and explicitly considered a fundamental ontological unit of politics, perhaps the most fundamental unit of all’. Fascism was the ghastly apex of this; not an outlier to modernity, but one of its primary contours.

If we are digging in our heels, therefore, when it comes to making the already ‘long’ 19th century even longer, we do agree that discussion about the precursors to the global transformation is valuable. We would have liked to dig more deeply into earlier industrial and military revolutions, and tracked further the origins of scientific thinking and ideas of progress. We did not do so in part for the reasons familiar to any author: constraints on word length and the maintenance of sound mental health. In more substantive mode, we see many of the issues raised by our interlocutors as a rejection of the claim made by Musgrave and Nexon that we have made it harder to carry out forms of historical comparison. To the contrary, both Anievas and Phillips question the place of the 19th century as a macro-historical watershed, while Owens argues that we are insufficiently attuned to the particularities of the century. This hardly speaks to a lack of comparative historical debate. Moreover, our intervention – and several others in this symposium – add to a range of moves already underway in the discipline: the recovery of histories of the various international societies into which global modernity was incorporated (Keene 2002; Little 2005; Zarakol 2011; Suzuki, Zhang and Quirk eds. 2013; Phillips and Sharman 2015); the desire to make IR more explicitly ‘global’ (Acharya 2014); attempts to ‘de-colonize’ the international imagination (Shilliam 2016), and more.

All in all, there is considerable scope for a series of projects that both flesh out our explanation of how global modernity emerged, and that sets this story within a wider set of global and regional contexts. Take as an example the contention by Musgrave and Nexon that the rate of growth in contemporary China is unprecedented, meaning that growth in the 19th century is more like that witnessed in previous centuries than the present day. We think that Chinese growth fits well within the account we present in *TGT*. Many periods of world history have seen remarkable, but *temporary*, ‘efflorescences’: Ming China, the Italian Renaissance, or the Dutch Golden Age stand out in this regard (Goldstone 2002). But only during the 19th century did the world witness *sustained* growth free from Malthusian constraints – what we now know as modern economic growth. After several centuries in which the volume of world trade increased by an annual average of less than 1%, trade rose by over 4% in the half century after 1820 (Osterhammel 2014, 726). And it was not just the volume of trade that was increasing at such a rapid rate – its value was also soaring. Between 1850 and 1913, the value of world trade (at constant prices) increased tenfold (Osterhammel 2014, 726). In this sense, as remarkable as the recent development of China has been, it sits

downstream from dynamics unleashed during the 19th century both in terms of its causal configuration and its regional precursors, most notably Meiji Japan and the post-War Asian Tigers. In the temporality of the global transformation, polities in Asia are not early adopters but relative latecomers, which had to overcome both predation from outside powers and domestic sources of resistance to the global transformation. China's rise is a rapidly compressed, deeply intensified version of previous adaptations to the global transformation.⁷

All of this points towards the debates and projects that we hope *TGT* will trigger. Any work that stresses the importance of a particular period, a particular causal configuration, or a particular conjuncture in world history necessarily challenges prevailing wisdoms and periodizations. After all, both we and several of the contributors to this symposium agree that Westphalia is not a particularly useful benchmark date for thinking about the emergence of modern international order (Buzan and Lawson 2014; also see Nexon 2009). But that has not stopped a (mostly) productive debate taking place exactly around this issue. As we noted in the first section of this paper, any historical-theoretical work is an act of foregrounding-suppression. Engagement with *TGT*, therefore, can take many different forms: contestation of its explanatory schema (Musgrave and Nexon, Anievas); the highlighting of themes either captured poorly or not at all by the book (Owens); or the extension of its argument to other times, places and theoretical traditions (Braumoeller, Phillips, Bilgin). Far more closing down discussion, our hope is that *TGT* will help to open up a space for these debates – and others – to take place.

Consequences

The Global Transformation, therefore, is a book premised on an argument about how modern international order emerged and the consequences of this order for both contemporary international relations (as lived and practiced) and International Relations (as studied and understood). Our contention, which some, but by no means all, of our interlocutors share, is

⁷ It is also what is expected from the analytic of uneven and combined development (UCD) that we make use of in *TGT*. UCD argues that 'late' developers are not carbon copies of original adopters, but develop their own distinctive characteristics. The increasingly intensified interactions between differently located social orders in the contemporary world produce not convergence, but (often unstable) amalgams of new and old. These 'contradictory fusions' make clear that historical development is not linear or sequential, but jumbled and, as the experience of contemporary China aptly demonstrates, compressed. For more on this, see Rosenberg (2010, 2016).

that the 19th century global transformation is a macro-historical conjuncture of world historical proportions. This is a claim central to the formation and operation of the modern academy. But not IR. With this in mind, our aim was to direct our intervention at an IR audience with the hope that, whatever their theoretical persuasions and areas of interest, IR scholars and students would be forced to confront the principal arguments of the book. As Braumoeller puts it, *TGT* attempts to establish the ‘generative structure’ that conditions international interactions, providing a macro-level schemata that can serve as the ‘scope conditions’ for the reconsideration of a range of debates, from changing notions of sovereignty to the role of revolutions in shaping international order.⁸ *TGT* offers a theory of emergence alongside an account of how modern international order developed and, in the contemporary world, is changing.

TGT is thus an ambitious book, and it is fair to ask what difference it should make to the discipline if people read it and took seriously its message about how dynamics in the 19th century established much of the framework for contemporary world politics. In other words, what does it enable IR to see that it cannot see now, and what difference does it make to how IR theory is used and understood?

Our main answer is the forging of a link between the 19th, 20th and 21st centuries that is so strong that the former is seen as an inescapable starting point for understanding the latter. This is the main work of chapters 3-9 of *TGT*, which show in detail how most of the main features of the modern world, from modern statehood to industrial arms racing, and from the definition of great powers to the core-periphery structure of the global political economy, originated in the 19th century and unfolded into the world we now inhabit. Our view was that existing mainstream theories provided far too narrow a view of these dynamics.⁹ Without a secure historical antenna, mainstream IR theories disguise their partiality, either pushing into the background or ignoring legacies from the 19th century that still exert a powerful influence

⁸ We are pleased that Braumoeller picks up on this potential by pointing to the ways in which our formulation could contribute to debates around the Correlates of War (CoW) project – certainly CoW would benefit from conceptualization of the ways in which imperialism and its legacies have been at the forefront of many international conflicts over the past two centuries.

⁹ The partial exception here is the English School, which despite its early Eurocentrism, is a rich resource for this kind of enquiry (e.g. Bull and Watson eds. 1984; Mayall 1990; Keene 2002; Clark 2007).

in contemporary international relations. As the debates about the rise of China, the decline of the US and the conjuncture of these two in a power transition crisis suggest, much of the discipline prefers to think in terms of polarity rather than core-periphery relations. If our analysis is correct, and the dynamics that emerged in the 19th century are pushing towards a world without superpowers, then IR's weak historical grounding is diverting its gaze away from the main power-political story of our time. At the same time, IR as a whole still underplays the ongoing impact of racism and anti-colonialism in debates ranging from nuclear non-proliferation regimes to the rise of China. The temporal provincialism of much mainstream IR greatly restricts what it sees when it encounters the contemporary world. Our approach offers a sharply different view, one which, contra Musgrave and Nexon, we see as directly opposed to IR's presentist tendencies.

As well as challenging IR's presentism, we also hope to challenge existing accounts of historical change. We do not see our account as fitting easily within the schema suggested by Chris Reus-Smit (this symposium). To borrow Gilpin's terminology, we chart a shift from systems change (from a 'polycentric world with no dominant centre' to a 'core-periphery' order in which the centre of gravity resided in the West) to systemic change (from 'centred globalism' to 'decentred globalism'). This dual movement was a change first *of* the system and then *in* the system. Contra both Reus-Smit and Phillips, our account combines elements of evolutionary, processual and breakpoint thinking. It is evolutionary in that the global transformation was generated by a scale-shift of dynamics within the system (such as the global incorporation of regional trading systems and coercive interactions examined by Phillips). It is processual in that it was an assemblage of events that concatenated in a historically specific configuration. And it is breakpoint in that it engendered a period of dramatic instability. Yet we make no stark contrast between the 'dynamism' of the global transformation and a 'static' agrarian era (as suggested by Phillips). As we make clear in the book, modernity was not a year zero, a big bang or a moment of 'all change'. Rather, our argument is that, during the 19th century, a concatenation of dynamics combined to produce a major transformation in how social orders were organized and conceived, and in how polities and peoples related to each other. These changes combined to generate a new mode of power that, in turn, reconfigured the foundations of international order. Rather, therefore, than be boxed into any single category, we prefer to see our analysis as offering something distinct – a configurational account of historical change.

What does this understanding of change mean for how we might conceptualize IR in the future – a task Musgrave and Nexon nicely label: ‘Exotic International Relations’? In *TGT*, we developed a set of projections made on an ‘other things being equal’ basis, acknowledging that other things never are actually equal. As Musgrave and Nexon note, there is no shortage of potential game-changers out there, some of which would require the drafting of an obituary for IR as a discipline. If, for example, Ray Kurzweil’s (2005) predictions about the singularity prove to be correct, then forms of artificial intelligence could surpass human capabilities in ways that render all current conditions superfluous. IR would become a historical artifact that only recognizes its existence shortly before it becomes extinct. The use of speculative fiction and similar forms of analysis strike us as a potentially rich vein of enquiry, one that requires much needed ‘out of the box’ thinking. Of course, there is no thinking that is entirely free from history – even when it comes to speculative fiction. But this, like many of the other points noted by our interlocutors, represents a stimulating opportunity for further research. Indeed, most of the engagements offered by our critics suggest to us that, if *TGT* is successful in changing the ways in which IR thinks about global modernity, it will help to open the way for contributions along the lines suggested by the contributors to this symposium.

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