

The promise of historical dynamism for the American study of international relations

BEAR BRAUMOELLER

Department of Political Science, The Ohio State University, Columbus, Ohio, USA

E-mail: braumoeller.1@osu.edu

While *The Global Transformation* makes a compelling case for the importance of the oft-ignored 19th century for IR theory, the book also represents an opportunity for an audience for which it was probably not intended: American IR scholars working in the quantitative empirical tradition, whom it should prompt to re-think the question of how the interactions that they seek to understand are conditional on deeper material and ideational dynamics.

As a quantitative, empirically oriented, plausibly rationalist systemic theorist in the American tradition, I should have many reasons to dislike Buzan and Lawson's *The Global Transformation*. The authors explicitly eschew theory (Buzan and Lawson 2015, 9). To the extent that the book does contain a causal story, it's a hot mess: ideologies of progress, industrialization, and rational state-building all slosh around together in a chaotic ferment that somehow separates core from periphery and ushers in modernity. To a scholar familiar with the history of the period,¹ the trends that the book documents will not come as much of a surprise. And, of course, the book contains not a single regression.

In fact, my reaction to the book was quite the opposite, in part because some of the issues I just mentioned are some of the book's greatest strengths. The authors' refreshingly immodest goal is to change how we think about international relations theory by exploring the ideational and material dynamics that comprise the context in which it evolved. The whole of the historical pieces that the book draws together toward that end – a hugely important emergent property, modernity – is considerably greater than the sum of its parts. Although American IR scholars might sniff at the

¹ See especially Hobsbawm (1962, 1999), but also, for example, Kohn (1944), Grenville (2000), Nettle and Robertson (1968), and Polanyi (2001).

idea of describing its emergence rather than coming up with a comprehensive theory (as, indeed, do other contributors to this symposium, even if this is not a view shared by Buzan and Lawson, as their response makes clear), to my mind description is precisely what's called for. Because by definition the entities that combine to produce an emergent property lack its most important characteristics, theorizing about emergent properties is challenging, to say the least, without indulging in flagrant posthockery.² Perhaps most important, the authors' claim that 'little IR scholarship assesses the overall impact of the nineteenth century on either the development of the discipline or the emergence of modern international order' (Buzan and Lawson 2015, 55) can hardly be doubted in light of the mass of evidence that they present. In all, Buzan and Lawson have fired an erudite and lucid shot across the bow of American international relations theory. The warning should be heeded.

My goal in this article is to provide a sketch of how I see *The Global Transformation* contributing both to 'American-school' systemic theory in IR and to more concrete, behavioral studies of state behavior. I will argue that the book prompts us to think not just about the importance of its empirical subject matter for extant theory but about how theories should feed in to one another more generally. Which phenomena create the context within which other phenomena arise? What can we hypothesize, not just about interactions but about changes in the logic of interaction, that might help us to understand more about international relations past, present, and future?

Systemic theory in the American tradition

What I call 'American-school' systemic theory – by which I mean scholarship that focuses on the impact of the structure of the international system on the behavior of states and, sometimes, on the impact of state behavior on the structure of the system as well³ – holds a very unusual place in the American IR literature. It is unusually widely read: the syllabus for most introductory IR seminars at the graduate level has at least 1 week on

² See for example, Bueno de Mesquita (1998), in which the author focuses on modeling an outcome that generally was not considered possible prior to 1989 (Gaddis 1992).

³ See for example, Sprout and Sprout (1956), Kaplan (1957), Knorr and Verba (1961), Rosecrance (1963), Hart (1974), Brenner (1977), Waltz (1979), Organski and Kugler (1980), Gilpin (1981), Modelski (1987), Niou, Ordeshook and Rose (1989), Carlsnaes (1992), Cederman (1997), Jervis (1997), Maoz (2011), Braumoeller (2012). The categorization is approximate, of course. Wendt (1999), an American, has more in common with English-school IR theory, while others (Organski 1967; Gilpin 1981; Modelski 1987) break with American-school tradition to some extent by theorizing about exogenous sources of change in the international system.

systemic theory, and by tradition most syllabi start off with it. There is an impressively long history of works that approach IR from a systemic perspective, and some of the most-cited works in the history of the field come from this tradition. As that fact might suggest, however, they are also among the most criticized works in the field.

The persistent appeal of American-school IR in the face of widespread criticism suggests that, despite their dissatisfaction with it, scholars consider the relationship between structure and agents in international relations to be an important one. Indeed, some have gone to great lengths to build on systemic theory in an attempt to render its predictions more determinate. Neoclassical realists have concluded that structural factors alone are underdetermining of state behavior and have sought to combine them with unit-level attributes in an attempt to produce more concrete insights (e.g. Wohlforth 1993; Schweller 1998; for a good review see Rose 1998). Others have either tried to put systemic theory on more coherent logical footing and/or narrow down more specific, testable hypotheses about state behavior that follow from it (Niou, Ordeshook and Rose 1989; Maoz 2011; Braumoeller 2012).

The reason for the popularity of American-school systemic theory has to do, I think, with the appeal of the basic intuition that drives it: that the structure of the international system is a fundamental component of the context within which international interactions play out. Scholars have argued that the logic of interaction in bipolar systems is fundamentally different than it is in multipolar ones (Waltz 1979); there are important differences between the behavior of states in heterogeneous vs. homogeneous systems (Aron 1966); systems with different patterns of interaction flows reflect the presence of different political communities (Deutsch 1966); and a system's 'culture of anarchy' – Hobbesian, Kantian, or Lockian – pervades much of the behavior within it (Wendt 1999).

What Buzan and Lawson's book compels us to think about is the fact that, much as systemic theory in IR conditions interactions among states, the forces that gave rise to modernity condition systemic theory (Phillips offers a fascinating extension of this point in his contribution to this symposium). In other words, *The Global Transformation* raises an old theoretical issue with immense potential for modern IR theory: the relationship between generative and descriptive structures. Most IR theory is designed to explain interactions among states, between states and non-state actors, and so on. The great promise of systemic theory is that it can provide an understanding of the context within which those interactions take place. As Ruggie (1998) pointed out, however, American-school systemic theory (in particular, Waltz's) lacks a deeper, generative structure that would help us to understand the underlying principles that condition the interactions

among states. In other words, it lacks what Reus-Smit (2016), following Gilpin (1981), refers to as ‘systems change’, or change from one kind of international system to another. Understanding these principles is precisely the goal that Buzan and Lawson set for themselves, and they succeed brilliantly.

Does this mean that American-school systemic theorists need to broaden our horizons – to construct theories that connect the global transformation described by Buzan and Lawson to foreign policy outcomes like treaties and military conflict by way of configurations of power and ideas? I hope not. Frankly, IR theorists already have a formidable task in attempting to boil their subject matter down to something comprehensible without losing sight of its essence in the process. Grappling with the logical implications of existing theories is probably sufficient unto the day. Admittedly, Waltz’s reliance on Durkheim’s concept of dynamic density makes him a fair target for critics who find his conceptualization of the structure of the system unsatisfying. But to my mind *requiring* a generative structure of systemic theory makes no more sense than insisting that evolutionary biologists study the origins of mountain ranges and forests. Insisting that we focus on generative structure privileges a particular, sociological notion of the international system that deserves to be explored rather than taken for granted – or critiqued, as the contribution by Owens to this symposium would have it. Any causal story has to choose a starting point, and criticizing a story that takes structure as given opens us up to wondering where the forces that produced structure came from, and where the forces that produced the forces that produced structure came from, and so on, all the way back to the Big Bang.

While I don’t think that explaining the origins of the structure of the system is a task that we should set for anyone who wants to write about that structure, I *do* think that understanding all of these theories in relationship to one another is a promising direction in which to work. That is because deeper (or more fundamental, or causally prior) phenomena provide the context within which states interact and, in so doing, preconfigure or condition that interaction. In so doing we can see that these theories do not compete, in any meaningful way: rather, more fundamental theories provide the scope conditions within which more specific theories of international interaction are operative.

The geology of IR theory

In short, I would argue that we should take very seriously the idea that theories at one level of analysis condition theories at another, and we should focus our attention on both the promise and the challenge that that

fact represents. Rather than attempting to revisit stale paradigm wars, we should be thinking in a more integrative fashion and asking ourselves how the deeper, elemental forces of ideas, material power, and state structure alter our perspective on the sources of state behavior.

Consider, for example, Herz's (1951, 133, 134) discussion of the special place of political realism in international relations theory:

While the facts observed by Political Realism constitute a kind of geological substratum, the old granite layer under a certain area, and thus determines the fundamental structure and formations of that area, on it there arise a multitude of geological and biological, and even human-social, phenomena which give the substratum now one, now another 'finish.' The granite understructure determines its basic features; but within that framework there are multitudinous possibilities of development.

Herz's geological analogy helps us, immediately and intuitively, to understand the contribution that he claims for realism: rather than having a direct impact on behavior at surface levels, it conditions the landscape upon which interactions take place. Nearly 50 years later, Ruggie (1998, 152) used a similar analogy to help clarify his discussion of the role of generative structure in systemic theory:

Waltz strives for, but fails fully to achieve, a generative formulation of international political structure In a generative structure, it will be recalled, the deeper structural levels have causal priority, and the structural levels closer to the surface of visible phenomena take effect only within a context that is already 'prestructured' by the deeper levels. For example, we ask of the distribution of capabilities within the international system what difference it makes for the impact of the general organizational effects of the deep structure of anarchy, as mediated by the more specific effects of the institutional framework of sovereignty. That is how the *systemic* effects of changes in the distribution of capabilities are determined. We then go on to ask how these systemic effects condition and constrain outcomes.

The juxtaposition of the two quotes highlights my earlier concern that the process of digging deeper, once begun, has no particular logical end point: it's probably turtles all the way down, from state behavior to structural distributions to sovereignty to anarchy (to biology to physics to, etc.), and there is no particular reason other than habit or disciplinary inertia to condemn a theorist for focusing on some turtles more than others. On the other hand, it does point to the potential utility of thinking of theories in relation to one another and focusing on which outcomes at one level of analysis are contingent on outcomes at other levels – outcomes that change

over time. In so doing, it also brings to the fore the nature of theorizing itself – a recurring theme in this symposium as identified by Reus-Smit in his opening essay.

While it may be unrealistic or even counterproductive to attempt to bridge the gulf between a generative theory of the international system and a theory of foreign policy, therefore, it could be highly productive to reconsider systemic theories, or theories of interstate interaction, in light of the sort of historical dynamics that Buzan and Lawson describe.

In search of deeper answers

While it should be clear that *The Global Transformation* gives welcome empirical accompaniment to theoretical critiques of systemic IR theory that focus on its failure to address generative structures, it might be less obvious that the book holds great promise for a different body of scholarship: the quantitative mainstream American IR literatures on the sources of international conflict, the role and origins of international institutions, the genesis of civil wars, the nature of foreign policy decisionmaking, and so on. By situating the major material and intellectual trends of the 19th century together in broader perspective, Buzan and Lawson make a compelling argument that the past two centuries of international relations can best be understood as having taken place in, and having comprised, a dynamic, evolving international context. As Reus-Smit (2016) and Phillips (2016) claim, Buzan and Lawson consider these changes profound enough to constitute a ‘breakpoint change’, a dramatic transformation to a qualitatively new system. While this is a point that has been made before, it has rarely been made so pointedly or aimed so directly at American studies of international relations. On another level, then, *The Global Transformation* represents a challenge to IR researchers to think about how our understandings of the world might be contingent upon the larger historical context within which they are embedded and to re-think the question of how the interactions that we seek to understand are conditional on deeper material and ideational dynamics.

What kinds of questions might Buzan and Lawson’s arguments raise for empirical, quantitative IR scholars whose focus is the day-to-day interactions of states? A few possibilities come to mind. First, to what extent is the stuff of which the world is made the same stuff that it was made of in 1815? Buzan and Lawson (2015, 55) point out that

[a]lthough mention is often made of the Concert of Europe and changes to the nineteenth-century balance of power, what mainstream IR misses are issues beyond the distribution of power—in other words the

transformation in the mode of power that was fuelled by industrialization, the emergence of rational states, and the novel ideologies associated with historical progress.

This argument strikes directly at the heart of one of the most commonly used metrics in statistical studies of IR: the Correlates of War national material capabilities index. The national material capabilities index measures relative capabilities as a function of a handful of factors, many of them industrial in nature. It does concede some rough discontinuities over time: pig iron production is utilized prior to 1900, for example, while steel production is utilized thereafter, and the scholars who oversee the project make an impressive attempt to generate a consistent measure of energy production given the considerable changes in energy technology over time. Buzan and Lawson's critique is more fundamental, however: it suggests that *how power was understood* changed over time. To the extent that that claim is true, it represents an opportunity to improve the quality of our data on national capabilities.⁴ Less narrowly, it represents an opportunity to improve our understanding of history via the modern metrics in which that history is embodied – to understand the 19th century as something more than 'a neutral site for the testing of theoretical claims' (Buzan and Lawson 2015, 56).

Similarly, the authors provide a compelling account of the growth of interaction capacity – both physical and social – throughout the 19th century (Buzan and Lawson 2015, Ch. 3). Given that international interactions require both the *opportunity* to interact and the *willingness* to do so (Starr 1978), accounting for interaction capacity is a crucial and often-neglected component of modeling state behavior, whether our focus is pairs of states or (as is increasingly the case) networks. While early attempts to deal with this problem utilized a fairly arbitrary loss-of-strength gradient (Boulding 1962; Bueno de Mesquita 1981), at present most scholars follow the simple rule of thumb laid out by Maoz and Russett (1993): pairs of states are 'politically relevant' if they are contiguous or if one of them is a major power. Only Lemke (1995) has sought to improve on this measure of political relevance by using historical sources, and he only did so within a very narrow spatial and temporal domain. An improved measure of interaction capacity would be a fascinating study in its own right, and to the extent that present approximations of political relevance overstate interaction capacity, an improved measure would almost certainly uncover larger and more robust relationships between variables of interest to students of IR.

⁴ That said, it is worth noting that Schweller (1998) sets out to create such an index based on a more nuanced reading of history. In the end, he found such a high correlation between his index and the existing one that he abandoned the exercise.

Digging still deeper, we might find that relationships of interest to us vary over time in a manner consistent with the unfolding of modernity. In my own work, for example, I find that the Great Powers were consistently able to alter the ideological and material balance of the international system – *except* in 19th century Europe, where the flowering of precisely the ideologies of progress described by Buzan and Lawson drove large changes in political ideology on the Continent that could not be accounted for by the activities of the Great Powers. Conversely, when I used a moving sum of averages test to explore the question of whether the Great Powers' responsiveness to systemic incentives or ability to affect systemic outcomes had changed significantly across the 19th century, as Craig and George (1983) argue they had, to my surprise I found no evidence to that effect. While recent advances in political methodology by Wawro and Katznelson (2014) make it easier to explore the impact of historical context on outcomes in international relations, thoughtful scholars have been making this point for quite some time (e.g. Goertz 1994). What Buzan and Lawson add is a coherent and powerful account of the sorts of historical forces that should be taken into account when thinking about the context of international relations.

In short, the critique by Swedberg cited in the opening contribution to this symposium – that 'theorizing has been reduced to choosing between pre-existing theories, as one chooses cereal in a supermarket' – applies equally to the selection of pre-existing data sets and methodologies. Buzan and Lawson's book makes a compelling case that we must do better.

Conclusion

As the other contributors to this symposium have pointed out, Buzan and Lawson's contribution to IR theory and to the historical sociology of the 19th century is considerable. While echoing these points, I have also argued that their work has great (and probably underappreciated) potential to enrich the American, and especially quantitative, study of IR by focusing researchers' attention on the 'deeper' phenomena that provide the context within which international interactions take place. I don't know how many members of (say) the Peace Science Society will heed this call to incorporate the rich, dynamic history described by Buzan and Lawson into their work, but I am certain that, to the extent that they do, our understanding of IR will be very much improved as a result.

Acknowledgements

The author is grateful to George Lawson, the editors of *International Theory*, and three anonymous reviewers for thoughtful and constructive feedback.

References

- Aron, Raymond. 1966. *Peace & War: A Theory of International Relations*. New York: Praeger.
- Boulding, Kenneth. 1962. *Conflict and Defense: A General Theory*. New York: Harper & Row.
- Braumoeller, Bear F. 2012. *The Great Powers and the International System: Systemic Theory in Empirical Perspective*. Cambridge: Cambridge University Press.
- Bremer, Stuart. 1977. *Simulated Worlds: A Computer Model of National Decision-Making*. Princeton, NJ: Princeton University Press.
- Bueno de Mesquita, Bruce. 1981. *The War Trap*. New Haven, CT: Yale University Press.
- Bueno de Mesquita, Bruce. 1998. "The End of the Cold War: Predicting an Emergent Property." *Journal of Conflict Resolution* 42(2):131–55.
- Buzan, Barry, and George Lawson. 2015. *The Global Transformation: History, Modernity and the Making of International Relations*. New York: Cambridge University Press.
- Carlsnaes, Walter. 1992. "The Agency-Structure Problem in Foreign Policy Analysis." *International Studies Quarterly* 36(3):245–70.
- Cederman, Lars-Erik. 1997. *Emergent Actors in World Politics*. Princeton, NJ: Princeton University Press.
- Craig, Gordon A., and Alexander L. George. 1983. *Force and Statecraft: Diplomatic Problems of Our Time*. New York: Oxford University Press.
- Deutsch, Karl W. 1966. *The Nerves of Government: Models of Political Communication and Control*. New York: The Free Press.
- Gaddis, John Lewis. 1992. "International Relations Theory and the End of the Cold War." *International Security* 17(3):5–58.
- Gilpin, Robert. 1981. *War and Change in World Politics*. Cambridge: Cambridge University Press.
- Goertz, Gary. 1994. *Contexts of International Politics*. Cambridge: Cambridge University Press.
- Grenville, J.A. S. 2000. *Europe Reshaped 1848-1878*. Malden, MA: Blackwell Classic Histories of Europe.
- Hart, Jeffrey. 1974. "Symmetry and Polarization in the European International System, 1870-1879: A Methodological Study." *Journal of Peace Research* 11(3):229–44.
- Herz, John H. 1951. *Political Realism and Political Idealism: A Study in Theories and Realities*. Chicago, IL: University of Chicago Press.
- Hobsbawm, Eric. 1962. *The Age of Revolution, 1789-1848*. New York: Vintage Books.
- Hobsbawm, Eric. 1999. *Industry and Empire: The Birth of the Industrial Revolution*. New York: The New Press.
- Jervis, Robert. 1997. *System Effects: Complexity in Political and Social Life*. Princeton, NJ: Princeton University Press.
- Kaplan, Morton. 1957. *System and Process in International Politics*. New York: John Wiley and Sons.
- Knorr, Klaus, and Sidney Verba, eds. 1961. *The International System: Theoretical Essays*. Princeton, NJ: Princeton University Press.
- Kohn, Hans. 1944. *The Idea of Nationalism: A Study in its Origins and Background*. New York: Macmillan.
- Lemke, Douglas. 1995. "The Tyranny of Distance: Redefining Relevant Dyads." *International Interactions* 21(1):23–38.
- Maoz, Zeev. 2011. *Networks of Nations: The Evolution, Structure, and Impact of International Networks, 1816-2001*. New York: Cambridge University Press.
- Maoz, Zeev, and Bruce Russett. 1993. "Normative and Structural Causes of Democratic Peace, 1946-1986." *American Political Science Review* 87(3):624–38.
- Modelski, George. 1987. *Long Cycles in World Politics*. Boulder: Lynne Rienner.

- Nettl, J. P., and Roland Robertson. 1968. *International Systems and the Modernization of Societies: The Formation of National Goals and Attitudes*. New York: Basic Books.
- Niou, Emerson M. S., Peter C. Ordeshook, and Gregory F. Rose. 1989. *The Balance of Power: Stability in International Systems*. Cambridge: Cambridge University Press.
- Organski, A. F. K. 1967. *The Stages of Political Development*. New York: Alfred A. Knopf.
- Organski, A. F. K., and Jacek Kugler. 1980. *The War Ledger*. Chicago, IL: University of Chicago Press.
- Phillips, Andrew. 2016. "The Global Transformation, Multiple Early Modernities and International Systems Change." *International Theory* 8(3):481–91.
- Polanyi, Karl. 2001. *The Great Transformation: The Political and Economic Origins of Our Time*. Boston, MA: Beacon Press.
- Reus-Smit, Christian. 2016. "Theory, History, and Great Transformations." *International Theory* 8(3):422–35.
- Rose, Gideon. 1998. "Neoclassical Realism and Theories of Foreign Policy." *World Politics* 51(1):144–72.
- Rosecrance, Richard N. 1963. *Action and Reaction in World Politics*. Boston, MA: Little, Brown and Co.
- Ruggie, John Gerard. 1998. *Constructing the World Polity: Essays on International Institutionalization*. London and New York: Routledge.
- Schweller, Randall L. 1998. *Deadly Imbalances: Tripolarity and Hitler's Strategy of World Conquest*. New York: Columbia University Press.
- Sprout, Harold, and Margaret Sprout. 1956. *Man-Milieu Relationship Hypotheses in the Context of International Politics*. Princeton, NJ: Princeton Center of International Studies.
- Starr, Harvey. 1978. "'Opportunity' and 'Willingness' as Ordering Concepts in the Study of War." *International Interactions* 4(4):363–87.
- Waltz, Kenneth N. 1979. *Theory of International Politics*. New York: Random House.
- Wawro, Gregory J., and Ira Katznelson. 2014. "Designing Historical Social Scientific Inquiry: How Parameter Heterogeneity can Bridge the Methodological Divide Between Quantitative and Qualitative Approaches." *American Journal of Political Science* 58 (2):526–46.
- Wendt, Alexander. 1999. *Social Theory of International Politics*. Cambridge: Cambridge University Press.
- Wohlforth, William Curti. 1993. *The Elusive Balance: Power and Perception During the Cold War*. Ithaca, NY: Cornell University Press.