

[Barry Buzan](#), [George Lawson](#)

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## Rethinking Benchmark Dates in International Relations<sup>1</sup>

Barry Buzan (b.g.buzan@lse.ac.uk) and George Lawson (g.lawson@lse.ac.uk)  
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**Barry Buzan** is Emeritus Professor in the Department of International Relations at LSE, a Senior Research Associate at LSE IDEAS, and a Fellow of the British Academy. He was formerly Montague Burton Professor in the IR Department at LSE. Among his books are: with Richard Little, *International Systems in World History* (2000); with Ole Wæver, *Regions and Powers* (2003); *From International to World Society?* (2004); and with Lene Hansen, *The Evolution of International Security Studies* (2009).

**George Lawson** is Lecturer in International Relations at LSE. His research focuses on the interface between International Relations and historical sociology ([www.historical-sociology.org](http://www.historical-sociology.org)), and on processes of radical change, most notably revolutions. He is the author of *Negotiated Revolutions* (2005), *Anatomies of Revolution* (2013), and the editor of *The Global 1989* (2010).

### Keywords

benchmark dates, history, International Relations theory, international system, international society, modernity

### Abstract

International Relations (IR) has an 'orthodox set' of benchmark dates by which much of its research and teaching is organized: 1500, 1648, 1919, 1945 and 1989. This article argues that IR scholars need to question the ways in which these orthodox dates serve as internal and external points of reference, think more critically about how benchmark dates are established, and generate a revised set of benchmark dates that better reflects macro-historical international dynamics. The first part of the article questions the appropriateness of the orthodox set of benchmark dates as ways of framing the discipline's self-understanding. Sections two and three look at what counts as a benchmark date, and why. We systematise benchmark dates drawn from mainstream IR theories (realism, liberalism, constructivism/English School and sociological approaches) and then aggregate their criteria. Part four of the article uses this exercise to construct a revised set of benchmark dates which can widen the discipline's theoretical and historical scope. We outline a way of ranking benchmark dates and suggest a means of assessing recent candidates for benchmark status. Overall, the article delivers two main benefits: first, an improved heuristic by which to think critically about foundational dates in the discipline; second, a revised set of benchmark dates which can help shift IR's centre of gravity away from dynamics of war and peace, and towards a broader range of macro-historical dynamics.

## Introduction

Most research and teaching in International Relations (IR) is implicitly or explicitly organized around five major benchmark dates:

- 1500 – the opening of the sea lanes from Europe to the Americas and the Indian Ocean which created a global scale international system for the first time (e.g. Buzan and Little, 2000: 401-2);
- 1648 – the emergence of modern notions of sovereignty codified in the Treaty of Augsburg and, arguably, institutionalized in the Treaty of Westphalia (e.g. Philpott, 2001: 30, 77; Baylis and Smith, 2001: 54);
- 1919 – the end of World War One as establishing both the main subject matter of IR (dynamics of war and peace; great power relations) and IR as a formal discipline (e.g. Brown and Ainley, 2009: 18-23);
- 1945 – World War Two as marking the shift from a multipolar to bipolar system, establishing a new contest for world power (e.g. Kegley and Wittkopf, 2001; Lundestad, 2005: 1; Oatley, 2007: 27);
- 1989 – the shake-up to notions of sovereignty (by globalization) and polarity (by unipolarity) initiated by the end of the Cold War (e.g. Doyle and Ikenberry, 1997: 1-19; Gilpin, 1987: 3; Russett and Starr, 2004: 5-10).

For the more historically minded (e.g. Reus-Smit, 1999), 1815 also registers, but not on the scale of the 'big five' benchmark dates. In the sub-field of Security Studies, 9/11 now serves as an important benchmark date (Buzan and Hansen, 2009: 226-55).<sup>2</sup>

The function of benchmark dates is to mark important turning points in the character and/or structure of international relations. They are a tool through which history is ordered into distinct, manageable parts. In principle there is nothing wrong with the use of benchmark dates – they are a standard way of simplifying history and fixing attention on particular issue-areas (May, 1975; Green, 1992; Buzan and Little, 2000: 386-406). In practice, however, IR scholars have given little systematic thought to the process by which they choose and institutionalise such foundational dates. They have propagated an 'orthodox set' of benchmark dates without appearing to reflect much on the consequences either of privileging some dates over others, or of leaving important historical dynamics out altogether. For example, it is notable that none of IR's current benchmark dates are located in the 'long 19<sup>th</sup> century', a period that witnessed the emergence and institutionalization of modern international order (Polanyi, 1957; Buzan and Lawson, 2013). More generally, the orthodox set of benchmark dates are serially reproduced in IR research and teaching, despite their often weak role in providing useful shortcuts into wider debates.

Benchmark dates are important for three main reasons: first, because they stand as points of reference for the discipline's self-understanding; second, because they operate as markers for how IR is viewed by other disciplines; and third, because they fix attention on specific events which, in turn, privilege some drivers of change over others. By what they highlight and what they silence, benchmark dates shape how history is understood, funnelling attention towards particular events and processes, while downplaying others (May, 1975). Because history is a contested field of enquiry in which the importance of events and processes is regularly reassessed, choices about benchmark dates will always be subject to critical re-evaluation. However, the choice is less whether or not to use benchmark dates, but whether particular dates are helpful or unhelpful. Benchmark dates are used in every discipline that engages with history as a means of placing boundaries around research and teaching, identifying turning points, and simplifying analysis. In short: benchmark dates are as important as theories – both serve as lenses which foreground some things, while marginalizing others.

Our argument is that the current set of benchmark dates in IR is unhelpful, over-privileging the experience of modern Europe, and focusing the discipline too tightly around wars and their settlements. Our aim is to disrupt current understandings of IR's foundational dates by building on scholarship which shifts IR away from a provincial interest in the history of the modern West (e.g. Tickner and Blaney, 2011). Failing to think sufficiently about either what benchmark dates represent, or how they function in the discipline, reinforces a narrow disciplinary imagination which means that IR is often looking in the wrong places at the wrong things, and missing or marginalising many of the fundamental events that have shaped modern international order. It is worth looking more carefully at how benchmark dates are constructed in IR and how its orthodox set might be improved.

The argument proceeds in three stages. In the first stage, we examine the 'orthodox set' of benchmark dates in IR, questioning how appropriate they are as framing points for the discipline's self-understanding. The second stage takes the form of a two part heuristic exercise which first, identifies criteria for benchmark dates from within the main strands of IR theory; and second, aggregates these criteria into nine tools used by the discipline to orient its research and teaching. This leads onto the third, final, stage of the argument in which we rank benchmark dates according to their global reach and long-term effects, making clear that such a ranking is fluid and subject to on-going reassessment. This stage of the argument includes the addition of new benchmark dates drawn from the 'long 19<sup>th</sup> century' which can turn IR away from its fixation with war and peace, and towards a range of macro-historical dynamics that better define its core agenda. This section also addresses the issue of how to assess recent events where the depth and breadth of changes are not yet clear.

## The 'Orthodox Set'

One is immediately struck by both the presentism and the West-centrism of the 'big five' benchmark dates. Three of IR's primary benchmark dates are clustered in the 20<sup>th</sup> century (1919, 1945, 1989) and are separated by relatively short intervals. The two older dates (1500, 1648) are separated by longer intervals and do not suggest any sustained engagement with world history. It could be that history has accelerated, making big turning points more frequent, so that this compression into the recent past is justified. But IR is notoriously presentist and the list is suspiciously weighted towards both the view that Western history is world history, and to the rise of IR as a self-conscious discipline after World War One. While 1500 is clearly a world historical event, 1648 might better be seen as a local European development. Why are major wars so prominent, and why are some wars favoured over others? Why do the fall of India to Britain in the late 18<sup>th</sup> century and the unification of China into a durable empire in 221 BC not register? And why is there no attention to the 19<sup>th</sup> century 'global transformation' during which many of the most important dynamics in contemporary international relations emerged?

There is some discussion of benchmark dates in the discipline, but this is mainly around 1648, and tends to concentrate on the appropriateness (or not) of that date in representing the transition from medieval to modern. As is by now well rehearsed, the 1648 Treaty of Westphalia is usually considered to be the intellectual basis for the discipline, establishing a 'revolution in sovereignty' through the principle of *cuius regio, eius religio*, which is taken to be a 'historical faultline' in the formation of modern international order (Philpott, 2001: 30, 77). Some constructivists see Westphalia as marking a fundamental shift from feudal heteronomy to modern sovereign rule through the emergence of principles of exclusive territoriality, non-intervention and legal equality (Ruggie, 1983: 271-9). Westphalia is also given prominence by realists (e.g. Morgenthau, 1978), English School theorists (e.g. Watson, 1992) and liberal cosmopolitans (e.g. Held *et al.*, 1995). Since the next orthodox benchmark is 1919, 1648 in IR (and only in IR!) stands for the onset of modernity in the form of a system of sovereign territorial states.

Regardless of the cross-paradigmatic hold of Westphalia in the discipline, its centrality to the formation of modern international order is questionable. Most obviously, Westphalia did not fundamentally alter the ground-rules of European international order. Neither sovereignty, non-intervention nor the principle of *cuius regio, eius religio* were mentioned in the Treaty (Osiander, 2001: 266; Carvalho *et al.*, 2011: 740). Rather, Westphalia was part of a long-running contest for the leadership of dynastic European Christianity – its main concerns were to safeguard the internal affairs of the Holy Roman Empire and to reward the victors of the Wars of Religion (France and Sweden) (Osiander, 2001: 266). Westphalia set limits to the idea of sovereignty established at the 1555 Peace of Augsburg, for example by retracting the rights of polities to

choose their own confession. Instead, Westphalia decreed that each territory would retain the religion it held on 1<sup>st</sup> January 1624 (Teschke, 2003: 241; Carvalho *et al.*, 2011: 740). More generally, Westphalia did not lead to the development of sovereignty in a modern sense – European order after 1648 remained a patchwork of marriage, inheritance and hereditary claims rather than constituting a formal states system (Osiander, 2001: 278; Teschke, 2003: 217; Nexon, 2009: 265). As Reus-Smit (1999: 87-154) argues, 1648 was about the transition from a medieval to an absolutist order in Europe, not a modern one. Overall, Westphalia was less a watershed than an affirmation of existing practices, including the centrality of imperial confederation, dynastic order and patrimonial rule (Nexon, 2009: 278-80).

Despite this extensive challenge to the significance of Westphalia as a watershed date, the place of 1648 in the discipline's self-understanding remains strong. This matters not just because it is suspect intellectually, but also because it means that much of IR's research and teaching is fixed around narrow debates (such as sovereignty) and particular regions (such as modern Europe). It saddles IR with an understanding of modernity not shared by other disciplines and leads it to marginalize the later development of the modern nation state that had such a major impact on international relations during the 19<sup>th</sup> century. Indeed, as the discussion around 1648 demonstrates, when a benchmark date represents a specific point-in-time event, there is a tendency for debate to become inward-looking (centring on the precise content of the event which marks that date) rather than outward-looking (using dates as a means to open-up enquiry into macro-historical dynamics).

This blinkered focus is reinforced by other orthodox benchmark dates, particularly 1919, which gravely misrepresents the founding story of IR's establishment as a discipline. 1919 occludes the fact that international thought became increasingly systematized during the last part of the 19<sup>th</sup> century, being taught in some US Political Science departments (such as Columbia) and fuelling major debates in both Europe and the United States (Knutsen, 1997; Schmidt, 1998; Carvalho *et al.*, 2011: 749). Hobson, Angell, Laski, Zimmern, Lenin, Woodrow Wilson *et al.*, were part of a burgeoning discourse which engaged in 19<sup>th</sup> century IR concerns: the rights and wrongs of imperialism, the increasing hold of notions of popular sovereignty and self-determination, the relationship of free trade and protectionism to international conflict, and the capacity of war to be mitigated by international law and intergovernmental institutions (Grant *et al.*, 1916; Hobson, 2012). Standard accounts also tend to omit the closeness of the links between IR, colonial administration and racism (Bell, 2007; Vucetic, 2010; Hobson, 2012), not to mention geopolitics. Indeed, a great deal of IR's intellectual history, and the historical developments that define many of its current concerns, are rooted in 19<sup>th</sup> century preoccupations with the superiority – or otherwise – of white races and Western civilization. IR, therefore, did not spring *de novo* in 1919, but has a longer genealogy formed in

the unprecedented environment of global modernity during the late 19<sup>th</sup> century.

Other orthodox dates are equally suspect. For example, although ‘both academics and policy-makers tend to use 1989 and its surrogate frames (such as Cold War/post-Cold War) as the principal normative, analytical and empirical shorthand for delineating past and present’, there are many parts of the world for which 1989 has little, or uncertain, importance (Lawson, 2010: 1). This benchmark date relates to a series of changes that looked big at the time, but appear less so the further away they get. As we discuss below, 1989 has not passed any test more stringent than the end of bipolarity and it is questionable how significant that shift is in a longer perspective.

Despite their sometimes tenuous historical importance, the orthodox set of benchmark dates have important consequences not just for how IR understands and reproduces itself, but also for how it interacts with and helps to constitute the ‘real world’ that it observes. ‘The myths [of 1648 and 1919] have had a tremendous function in disciplining our thinking about fundamental issues in international politics, “normalising” it as common sense and providing the parameters or outer boundaries within which the disciplinary field is contained’ (Carvalho *et al.*, 2011: 756). This complaint resonates with those who critique much existing IR literature for its weak appreciation of dynamics of imperialism, colonialism, dispossession and expropriation in the formation of modern international order (e.g. Keene, 2002; Suzuki, 2009; Shilliam, 2011). Like the ways in which 1919 delinks IR from its origins in imperialism, racism and geopolitics, other benchmark dates omit the inter-societal configurations which shape macro-historical shifts. Perhaps most notably, the jump from 1648 to 1919 leaves out the inter-societal reconfiguration which, during the ‘long 19<sup>th</sup> century’, both marked the transformation to global modernity and enabled the West to build a hierarchical international order. This period is the central concern for sociology, historical sociology, economic history, world history and law. Its absence from IR’s orthodox set of benchmark dates is both surprising and problematic.

In summary, the ‘big five’ benchmark dates provide few insights into key issue-areas within the discipline. One way of responding to this weakness would be to do without benchmark dates at all. However, as noted above, benchmark dates are indispensable. They play a central role in IR’s self-understanding, operate as signalling devices to other disciplines, and sustain a historical narrative which undergirds how the discipline conducts much of its research and teaching. Another response would be to use the poverty of existing benchmark dates in order to explore issues of temporal heterogeneity within world politics (e.g. Hutchings, 2008). In part, we agree. However, the current use of benchmark dates in IR funnels attention towards a narrowly defined set of issue-areas. If this orthodox set is flawed, it is necessary to find sounder foundations on which to construct alternatives to them.

## What is a Benchmark Date?

The big five benchmark dates are embedded (often unreflectively) within existing theoretical approaches in IR. But what exactly are the criteria that underpin them? For 1500, the key point is the expansion in the scale of the international system. Within a few years of this date, European navigators crossed the Atlantic and sailed around Africa in ways that could be replicated. In doing so, they opened the way for a global scale international system. The other four dates are defined by the ending of major wars and their settlements: the Thirty Years War and Westphalia, the First World War and Versailles, World War Two and San Francisco, the Cold War and the end of bipolarity. Looking at major wars and their settlements is a common mode of analysis for thinking about periodization across several schools of IR theory (e.g. Gilpin, 1981; Holsti, 1991; Reus-Smit, 1999; Ikenberry, 2001; Clark, 2005, 2007). But if the ending of major wars is such a major part of how benchmark dates are constructed in IR, why does the discipline give relatively slight attention to Utrecht, 1713, which ended the wars of Louis XIV, and only a little more to 1815 and the end of the French revolutionary and Napoleonic wars? If the answer is that IR does not look beyond its founding as a self-conscious field of study, then Westphalia should not be there either. The fact that IR's benchmark dates are not just about major wars suggests that more than one type of criteria for benchmarking is in play. It also underlines the *ad hoc* quality of the orthodox set.

So what kinds of events and processes should count as benchmark dates? The orthodox set features changes in the scale of the system (1500), changes in the nature of the dominant unit (1648), and changes in the management and/or polarity of international order (1919, 1945, 1989). Since the act of establishing benchmark dates is about declaring some events to be more important than others because they are era-defining, the process of establishing benchmark dates rests on propositions about what constitutes moments of historical change that are particularly significant for IR. The best way to ascertain what should count as a benchmark date in IR is to examine what mainstream theories – realism, liberalism, constructivism, the English School, and sociological approaches – suggest as criteria for identifying significant change in the international system.<sup>3</sup>

Realism presents four ways of thinking about benchmark dates, sometimes explicitly, at other times implicitly. First is a change in the organizing principle of the international system. For neorealists, the only alternative to anarchy is hierarchy, but there is room for debate about way stations between these poles, including hegemony, suzerainty, dominion and empire (Watson, 1992). Second is a change in the status of war from being possible and expected, to being unlikely and unexpected. This change might also include shifts in which war and the balance of power are no longer the defining dynamics of the international system, and/or in which a change in the nature of



military power brings into question the viability of the state, the utility of war and balancing dynamics. The emergence of nuclear weapons is the obvious example of this latter type (e.g. Deudney, 2007). Third is a change in the distribution of power amongst the great powers. This is the central element in neorealist polarity theory; the proposition is that changes matter more as numbers get lower. Fourth is a change in the nature of the dominant unit away from the sovereign territorial state. Realists do not dwell on this possibility because they think it is unlikely. But it is, at least, an implicitly held assumption within the theory. This final issue underpins the importance of 1648 because Westphalia is seen as establishing the principle of sovereignty which underpins the modern international system.

These four candidates fit well with orthodox IR benchmark dates. Realists emphasise continuity in international relations (excepting the distribution of power) and do not expect there to be changes in the system structure, the dominant unit of the system, the salience of the balance of power or the centrality of conflict. The 1648 benchmark date serves realism by emphasising the durability of the sovereign state and anarchy as framing devices. Major wars and their settlements are seen as vehicles through which great powers project their preferred rules and practices onto the international system. Such wars may reflect polarity changes, but unless these are at the small number end, they are not considered to be structural changes. Research on balancing is supported by benchmark dates such as 1945 and 1989, with the system undergoing a shift from multipolarity to bipolarity after 1945, and from bipolarity to unipolarity after 1989.

Liberals, and more broadly those interested in international political economy (IPE), are also interested in changes in the organizing principle of the international system, and many of them have long thought that such changes are underway (e.g. Keohane and Nye, 1977). For liberals, the strict separation between anarchy and hierarchy (or international and domestic) is reduced by heightened levels of interdependence, deeper trade regimes, increasingly powerful mechanisms of global governance, and the durability of security communities. Liberals emphasize shifts in governance through the emergence and spread of international organizations, so developments such as the founding of the League of Nations (1919) and the United Nations (1945) register strongly. Liberals also emphasize major changes in the rules, norms and practices that govern the global political economy. In 1862, for example, the British Companies Act marked a shift to limited liability firms and opened the way to the formation of transnational corporations as a significant new actor in international relations. Along these lines one could also think about 1600 as a symbolic date for the founding of chartered companies by European imperial powers between 1553 and 1670 (Buzan and Little, 2000: 267-8). The great depression which began in 1929 is another possible benchmark date, likewise the major change of rules put in place by Bretton Woods in 1944, the US termination of dollar convertibility in 1971 and the 2008 financial crash.<sup>4</sup>

Looking back further, the repeal of the British Corn Laws in 1846, which opened the way to free trade, could be a benchmark date as could be the first industrial-era depression starting in 1873. The opening up of ocean routes around 1500 is an important IPE benchmark date, transforming the capacity to move people, goods, money and ideas around the world. So too is the period between 1840 and 1870, when the planet was wired for more or less instantaneous communication by telegraph, and huge increases in speed and carrying capacity were instigated by the spread of steamships and railways.

Looking at such a list, it is apparent that there is a less straightforward relationship between liberal/IPE benchmark dates and the 'big five' than there is with realism. The reason for this is obvious – IR's orthodox set of benchmark dates is mainly oriented around political-military events rather than dynamics of interdependence, trade or global governance. Only where there is overlap between these processes and wars (e.g. 1919 and 1945) do liberal/IPE concerns register. The 1500 benchmark date opens up the possibility of other benchmarks defined by predominantly liberal themes, but so far this remains an outlier within the orthodox set.

Constructivists and English School theorists have a number of ways of identifying IR benchmarks. Wendt (1999: 314), for example, posits three cultures of international anarchy – Kantian (friendship), Lockean (rivalry) and Hobbesian (enmity) – and suggests two historical transformations between them: the first from Hobbesian to Lockean in 17<sup>th</sup> century Europe (roughly analogous to 1648); the second from Lockean to Kantian in the decades following 1945, which Wendt locates primarily within the West. Both of these transformations point to regional rather than global benchmark dates.

Reus-Smit (1999), operating within both the English School and constructivism, offers a somewhat different schema, building on Ruggie's (1983) critique of Waltz in order to identify a shift from medieval to modern as a transformation of both ordering principle and dominant unit. Reus-Smit labels this process: 'configurative change'. To this process he adds 'purposive change', defined as change in the 'moral purpose of the state' and consequent shifts in the meaning of sovereignty (as captured by the transformation to modernity). Reus-Smit's scheme generates two candidates for benchmark dates, a *configurative* change from medieval to absolutism, for which he uses the symbolic date 1648, and a *purposive* change from absolutism to modernity, to which he does not give a date, but sees as beginning in the late 18<sup>th</sup> century and becoming dominant by the mid-to-late 19<sup>th</sup> century, roughly 1776-1870.

The English School's scheme of 'primary institutions' offers more precision in terms of tracking changes in ideas and their associated practices, norms and rules. Primary institutions are evolved rather than designed, and they are constitutive of both states and international society in that they define the basic character and purpose of any such society. The classical English School focused on five primary institutions – war, international law, the balance of power, great power management and diplomacy (Bull, 1977) – with

sovereignty and territoriality more implicitly also in play, and colonialism in play but not discussed. To this set have been added, *inter alia*, nationalism, human rights, the market and, most recently, environmental stewardship (Buzan, 2004: 240-49). Primary institutions are durable but not fixed, and their rise, evolution and decline can be traced (Holsti, 2004; Buzan, 2004). Nationalism, for example, evolved into a primary institution of international society from the late 18<sup>th</sup> century onwards (Mayall, 1990), while slavery (during the 19<sup>th</sup> century) and colonialism (after World War Two) have declined as primary institutions. It is not always easy to allocate dates to these extended processes of change, but 1870 might stand for the rise of nationalism, 1833 for the demise of slavery and 1945 for the obsolescence of colonialism. Clark (2005, 2007, 2011), also working within the English School, offers the concepts of legitimacy and hegemony as an alternative to primary institutions. Legitimacy, for example, is defined in terms of rightful membership and rightful conduct (Clark, 2005: 2, 9), and Clark (2005: 7, 19-25) sees this as a clearer way than primary institutions to identify significant change in international society. His scheme is hinged to major wars and their settlements – Westphalia, Utrecht, Vienna, Versailles, San Francisco and the ending of the Cold War – and so provides a better fit with the orthodox set of benchmark dates.

Also available within the English School as a candidate for benchmark dates are changes in the membership of international society. The English School's 'expansion' story (Bull and Watson, 1984; Watson, 1992; Buzan and Little, 2010; Reus-Smit, 2011) suggests several possible benchmark dates: the widening of international society from European to Western (with the incorporation of the Americas during the late 18<sup>th</sup> and early 19<sup>th</sup> centuries); the inclusion of non-Western states such as Japan during the late 19<sup>th</sup> century; the breakup of some continental empires after World War One; the universalization of formal membership through anti-imperial struggle and colonial retreat after 1945; and the breakup of the Soviet Union.

Finally, there are several sociological approaches within IR, including Marxism, historical sociology and differentiation theory, which also identify candidates for IR benchmark dates.<sup>5</sup> Many Marxists focus on the 19<sup>th</sup> century as containing the principal shift to modern international relations (e.g. Hobsbawm, 1962; Rosenberg, 1994). This focus on the industrial revolution, and the 19<sup>th</sup> century economic, political and social transformations associated with it, can also be found in other literatures, particularly world history (e.g. Bayly, 2004). Other Marxists, such as Teschke (2003), emphasize earlier dates, placing emphasis on the 1688 Glorious Revolution in England which, it is argued, ushered in a new mode of property relations which worked to unravel absolutist rule and, in turn, enabled the modern international system to emerge. Immanuel Wallerstein goes back still further, seeing 1500 as the transformational point between world empires and world capitalism.

A number of historical sociologists in IR focus on the constitutive role played by revolutions in the making of modern international order, seeing these

as important benchmark dates. Fred Halliday (1999), for example, used revolutions to construct an alternative periodization of modern international order, recalibrating the 16<sup>th</sup> century as a time of political and ideological struggle unleashed by the European Reformations, re-establishing the central optic of the 17<sup>th</sup> century around the upheavals which followed the Dutch Revolt and the English Revolution, re-centring the 18<sup>th</sup> and 19<sup>th</sup> centuries around the Atlantic Revolutions of France, America and Haiti, and understanding the ‘short 20<sup>th</sup> century’ as one in which the primary logic was the challenge – and collapse – of the Bolshevik Revolution and its Third World inheritors. As Halliday and others (e.g. Walt, 1997; Armstrong, 1993; Lawson 2005) show, there is a close relationship between revolutions and international order. Even if the attempts by revolutionary states to overturn existing trade, security and alliance regimes do not fully succeed, there are still a number of instances of revolution (such as Haiti, France, Russia, China, Cuba, Iran and the series of transformations associated with the end of the Cold War) which have had a major impact on international order and which, therefore, stand as candidates for benchmark dates.

Finally, there is differentiation theory (Buzan and Albert, 2010). Differentiation theory sees social structure as distinguished by dominant modes of differentiation: *segmentary* (like units), *stratificatory* (units differentiated by rank or status) and *functional* (differentiation by type of activity). This schema provides a powerful means of surveying macro-historical transformations. For example, it sees the shift from hunter-gatherer to agricultural production as one from segmentary to stratificatory differentiation and the shift from absolutism to modernity as from stratificatory to either functional differentiation (because politics, economy and society are separated into distinct activities) or segmentary differentiation (because within the political sphere, modern states become like units on the basis of sovereign equality).

Where does this survey of possible benchmark dates within IR leave us? The first and most obvious conclusion is that IR theory generates a cornucopia of criteria for benchmark dates, many of which go well beyond the orthodox set. The second conclusion is that, despite their different starting points, there is a substantial degree of overlap among the various approaches in terms of the location of dates deemed to be significant. Looking at the columns in Table 1 suggests a certain amount of coherence in benchmark dates across these strands of theory. The third conclusion is that benchmark dates are necessary to theory building. IR theories mostly centre on differentiating continuity from change. Although they differ in the criteria by which they do this, the process of making such a differentiation rests of the significance of transformation points (i.e. benchmark dates) *by necessity*.

[Insert Table 1 here]

The fourth conclusion is that there are three types of benchmark date operating within IR:

1. Point-in-time events seen as turning points (e.g. 1929, 1989, 2008).
2. Relatively short, sharp, transition periods, often featuring major wars and symbolized by the dates of the treaties that settle them (e.g. 1713, 1815, 1919, 1945).
3. Tipping points for transformative processes that are decades, possibly centuries, in duration (e.g. 1500, 1600, 1648, and the various attempts to capture the 19<sup>th</sup> century global transformation). In this understanding, benchmark dates represent clusters of events which open up enquiry into a range of nested dynamics.

This basic difference in terms of how IR theories approach benchmark dates matters considerably when we come to assess their impact and, in turn, think about how to rank them.

### **Aggregating the Criteria for Benchmark Dates**

At this point we come to a major fork in the analytical road. Since we have argued that the process of establishing benchmark dates is necessary to theory building, it would be possible to form distinct benchmark date schemes for each major strand of IR theory. Doing that, however, can only produce partial, parallel sets of benchmarks, abandoning the attempt to treat IR (or at least mainstream IR) as a whole. We leave that task to others. Instead, we take the second fork: aggregating insights across the range of theories surveyed above. Our method here is to distil the basic principles underlying the dates in Table 1. This is mainly a pragmatic move aimed at generating a synoptic view of the logic underlying benchmark dates for mainstream IR as a whole. The nine criteria for benchmark dates set out below – organizing principle, social organizing principle, interaction capacity, system scale, societal scale, systemic crises, dominant unit, distribution of power, and mode of power – thus represent mainstream IR thinking as it currently stands. Aggregation has its own theoretical justification in that IR theories can best be understood as representing a set of partial truths about international relations. The consonance between the dates in Table 1's columns is not so surprising given that IR theories are in some sense addressing the same 'reality', but looking at it, or constructing it, from different perspectives. Shared, and/or clustered dates across the theories are, therefore, themselves of theoretical interest.

One possible problem with aggregation is that IR theories are divided between those that emphasise material factors and talk in terms of international systems, and those that emphasise social factors and talk in terms of international societies. But there are so many points of contact and overlap between these two traditions that it is difficult to separate them. Even Waltz

talks about ‘socialization’, while many constructivists and English School theorists acknowledge a ‘rump materialism’. With this in mind, our aggregated criteria for identifying IR benchmark dates are as follows.

### *Organizing principle*

Waltz (1979) opened up deep structural change as a possible system benchmark, but then closed it by arguing that anarchy is, and has been, a universal condition of the international system. Ruggie (1983) challenged the centrality of anarchy to neorealism by opening up the medieval-to-modern transformation as a shift from an organizing principle of heteronomy to one of anarchy. This medieval-to-modern story is somewhat Eurocentric, marginalizing the many classical instances in the non-European world of international systems taking on hierarchic forms (Buzan and Little, 2000). Nevertheless, systems logic clearly allows for deep structural changes. Such changes are likely to be infrequent and, for some neorealists, virtually inconceivable. But when they do occur, they will be extremely significant.

### *Social organizing principles*

All thinking about international society presupposes that an international system exists. For this reason, international society theorists operate with the same, or at least a similar set, of system structures – mainly variations on anarchy. They are also sensitive to the impact of changes in interaction capacity on normative structures. But international society theorists are not just interested in the principles that differentiate units. They are also interested in the normative structures that constitute units and shape their behaviour. As Onuf (2002: 228) astutely observes, for realists ‘sovereignty is the only rule that matters for the constitution of anarchy’. International society theorists see a much richer and more variable picture of social structure. Normative structures vary across space and time. A change in the organizing principle of the system and a change in the normative structure of international society are almost certainly mutually constitutive. But social structure can also change *within* a given system structure, as implied by Wendt’s (1992) dictum: ‘anarchy is what states make of it’ (see also Buzan, Jones and Little, 1993: 244). As noted in the above survey, there are many possibilities for how to conceptualise the normative structure of international society, including: Wendt’s typology of Hobbesian, Lockean and Kantian societies; the English School’s primary institutions; Reus-Smit’s ‘constitutional structures’; Clark’s understanding of legitimacy in terms of the ‘standard of civilization’ that has to be met by those aspiring to membership; Marxist approaches that focus on the dominant mode of production; historical sociological approaches that focus on both macro-historical transformations and the role of revolutions in challenging existing

patterns of international order; and sociological theories that focus on the dominant mode of differentiation.

### *Interaction capacity*

Interaction capacity is about the ability to move people, goods, information, money and military power around the system. A system dominated by agrarian technologies of horses and sailing ships has a much lower interaction capacity than one dominated by industrial technologies: railways, steamships, telecommunications and aircraft. Systems with low interaction capacity are likely to be sub-global. A thin global system was created by relatively advanced sailing ship technologies at the end of the 15<sup>th</sup> century. Since then, the impact of increasing interaction capacity has not primarily been in terms of a shift in the scale of the international system, but about a shift in terms of its density. Steamships, railways, canals and the telegraph massively increased interaction capacity during the 19<sup>th</sup> century, underpinning the creation of an interdependent world economy with a core-periphery structure. During the 20<sup>th</sup> century, radio, aircraft, satellites and the Internet have continued to shrink the planet and increase the density of interactions of all kinds, from politics to pollution, and from sport to finance. This raft of changes in interaction capacity is of high significance to the material and social conditions of the system across several theories, but takes place over the long-term.

### *System scale*

A consequence of rising or falling interaction capacity is increases or decreases in the scale of the international system. This is why 1500 figures in the orthodox set of benchmark dates – it represents the shift to a global scale international order. However, unless humankind begins to inhabit space beyond the planet, no further increases in physical scale are possible. This does not, of course, rule out increasing intensification in terms of the density of interactions.

### *Societal scale*

The scale of international society does not necessarily correlate with the scale of the international system. A variant on this would be that international society can take different forms, or have different layers, within the international system, as during the 19<sup>th</sup> century when a Western international society coexisted alongside a colonial one (Keene, 2002). The result is a different set of benchmark dates for system and society in terms of scale. Whereas one might date the opening of a global international system to circa 1500, a global international society did not come into being until after 1945 with the major

round of decolonization. The English School's narrative of the expansion of international society (Bull and Watson, 1984; Watson, 1992; Buzan, 2010; Buzan and Little, 2010; Reus-Smit, 2011) serves as a general guide to this difference in the scale of international society and international system, providing a novel set of potential benchmark dates.

### *Systemic crises*

We have already noted a tendency within IR, most notably amongst realists, to privilege major wars and their settlements as benchmark dates. Such wars are, of course, crises where the ordering principles of international society fail to contain conflict – or sometimes promote it. But wars are not the only kind of crisis in play. Economic breakdowns such as those of 1873, 1929 and 2008 do not necessarily correlate with wars, but may have a similar scale of effects on norms and practices. Similarly, revolutions in major states are not always correlated with systemic wars, as in 1776 and 1949, although they can be, as in 1789 and 1917. Such revolutions challenge the social structure of international society by creating, as for much of the 20<sup>th</sup> century, configurations of states exhibiting contradictory visions of how international society should be organized.

### *Dominant unit*

Changes in dominant unit are tied to that of organizing principle. Most notably, any change in organizing principle will also embody a change in the dominant unit, as it does for the medieval-to-modern transformation (Ruggie, 1983; Reus-Smit, 1999). That said, it is possible that the organizing principle of anarchy could manifest itself in different types of units. Waltz (1990, 37; see also 1979: 91) himself suggests this with his argument that the structure of anarchy will have the same effect 'whether the system is composed of tribes, nations, oligopolistic firms or street gangs'. In Waltz's scheme, a transformation of dominant unit would count as less significant than a change in organizing principle.

### *Distribution of capabilities*

This is neorealism's most common, but least deep, type of structural change. Waltz concentrates on relative power in terms of the distribution of capabilities, thereby distinguishing great powers from other states. In this perspective, the nature of power does not matter, just its distribution. Changes in the number of great powers are structurally inconsequential above four, but increasingly consequential as the number of great powers shrinks towards two, or more problematically within the theory, one. There is also the hegemonic stability version of this story (e.g. Gilpin, 1981), in which one leading power



takes responsibility for stabilizing the capitalist world economy (Netherlands, Britain, the United States, etc.).

### *Mode of power*

Excluded from Waltz's theory, yet conspicuous in both his work and that of other realists, particularly those who debate the impact of nuclear weapons on the functionality of the state and the utility of war (Waltz, 1981; Herz, 1957), is the dominant 'mode of power'. The debate about the extent to which nuclear weapons have systemic effects opens the door to wider questions about transformations in the mode of power. Nuclear weapons represent a specific, point-in-time transformation. But underpinning them was the shift from agrarian to industrial military power that took place during the 19<sup>th</sup> century. That shift opened up a significant power gap between industrializing and non-industrializing societies. It altered relations between Europe and Asia, changed the criteria for being considered a great power, reoriented the nature and conduct of war, and caused a shift in notions of military rivalry and balance (Buzan and Lawson, 2013). Neorealism assumes that the mode of power is more or less constant. However, at times, differences in power configurations such as those which manifested in the 19<sup>th</sup> century, and which were further realised by the advent of nuclear weapons, matter enormously. Indeed, because they change not just the leading players in a system, but also the ground-rules of the system itself, changes in the mode of power have more significance than changes in the distribution of power.

The explication of these criteria illustrate that most IR theories assume that some types of change are deeper and more significant than others. Most agree, for example, that a change in the organizing principle of the system, or a change in the mode of power, is weightier than a change in the distribution of power. There is also a basic sense that scale matters: some changes are mainly regional (1648), whereas others are global (1500, 1945). However, there is not much in the way of systematic thinking about how to assess issues of depth or breadth. Is a change in the mode of power weightier than a change in the social organizing principle? Might a major change in interaction capacity or scale outweigh a relatively small change in deeper organizing principles? There are no clear answers to these questions from within mainstream IR theories.

### **Revising Benchmark Dates**

The final step in our argument is to build on the nine criteria identified in the previous section in order to find benchmark dates that: a) better represent what is important to IR as a whole rather than just specific theoretical traditions within it; and b) establish a more productive agenda for the discipline than that

envisaged by the orthodox set. We are conscious that even with the systematization and aggregation proposed above, there is still considerable leeway for argument about how to assess benchmark dates. We are also conscious that we are addressing IR as a whole, and that it is worth trying to identify clusters of significant dates across a range of theories. One constraint is that both clustering and significance are easier to see the further back they are in time. Understanding the significance of 1500 or 1648 is a lot easier than evaluating 1989, 2001 or 2008. We also take into account the extent to which events are global or regional in scale, and how major or minor their effects are. At the same time, we need to keep in mind both the three types of benchmark date (point in time events, short transition periods, long-term transformations) and the nine forms of change noted above. This allows us to organize benchmark dates into *primary*, *secondary* and *tertiary* categories.

*Primary benchmarks* are clusters of events that signify major processes of macro-historical transformation. They: a) stand as demarcation points for examining a range of transformational processes; and b) act as markers for a concatenation of interlinked – or nested – events. Primary benchmark dates display a substantial cluster of significant changes of which either at least one must be deep, or else several must be of substantial weight. They must also carry global significance. Clusters of events will not, of course, all fall in the same year. As such, following the precedent set by 1648, we try to find a median, or tipping point, date that represents a useful segue into these dynamics. Where possible, and against existing practice, we favour neutral years in which no specific event of significance took place. This loses the drama of ‘big date’ events, but avoids the distraction of inward-looking, scholastic controversies about the significance or otherwise of particular events, such as those that have plagued discussions of 1648. It also reduces the association of benchmark dates with certain normative positions – for example, 1989 is celebrated in some countries (particularly those in Eastern and Central Europe), but has mostly negative connotations in others (as for some in Russia and China). Most importantly, such a strategy opens up IR to a range of macro-historical processes which are otherwise submerged or overlooked. For example, we make the case below for seeing 1860 as a primary benchmark because it serves as an illustrative date for a range of nested processes: state rationalization, industrialization, technological change, shifting modes of warfare, ideological transformation, and so on. A neutral date like that is far better than say, 1848 or 1870, each of which would privilege a particular view of a much wider transformation. Although major wars and their settlements remain important to some benchmark dates in IR, this new understanding sees primary benchmark dates as interlinked configurations of social processes. This way of thinking carries with it the promise of realigning research and teaching in IR around broader configurations of macro-historical change rather than the punctuation marks which often neglect, disguise or occlude these dynamics.

*Secondary benchmark dates* display a lesser cluster of significant changes without any being both deep and global. They might be more local in influence, or if global in consequence, less significant than primary benchmarks. *Tertiary benchmark dates* are mainly point-in-time events which are not significantly clustered with other dynamics or are local/regional rather than global. Introducing ranking in this way raises an ambiguity about whether primary, secondary and tertiary dates are distinct categories or somehow nested. All, or very nearly all, point in time dates are by themselves tertiary, and most of these will be scooped up into primary or secondary clusters. Primary and secondary benchmark dates are made up of different scales, intensities and depths of clustering. Secondary clusters might get incorporated into primary ones or might stand separately in their own right. This nesting quality differentiates benchmark dates from chronological attempts at periodization. Periodization is a linear form of benchmarking which assumes that history can be divided into a sequence of eras generally assumed to be of more or less equal significance. By ranking benchmark dates, we want to emphasise the potential for nesting and clustering, and thereby open up IR to a subtler way of thinking about eras.

It is important to note that benchmark dates can move between these layers as their association with clusters of processes becomes either more or less pronounced. This is particularly likely when examining dates close to the present day. For example, events such as the 2008 financial crisis or 9/11, although appearing to be relatively confined to certain regions of the world, may in retrospect end up as part of a broader configuration which marks the diffusion and re-centring of global power. Equally, other possible benchmark dates, such as 1815, may decline in influence as their value as entry points into macro-historical dynamics is taken by alternative vantage points, such as 1860. In this way, benchmark dates should be seen as fluid rather than permanent, part of on-going discussions rather than performing any kind of fixed, disciplining function. Benchmark dates, like all historical analyses, are open to debate and reassessment in the light of how subsequent developments affect appreciation of their significance.

We confine this preliminary ordering of benchmark dates to modern history. If we were to extend the exercise further back, we would have to consider earlier expansions of international systems, the Axial age of world religions, the discovery of agriculture, the rise of cities and the more complex cultures that went with them, and so on. We would also run into the problem that there was no global international system before 1500, which would require dealing with several sub-global assessments in parallel. We think that benchmarking exercises could and should be done for pre-1500 international systems, and anticipate that their relevance will increase as a more globally based international order displaces the transitional period of Western hegemony associated with the past two centuries or so.

## Primary benchmark dates

There are three clusters of modern changes that meet the criteria for primary benchmark dates in IR, all of which are of global reach and have deep significance. The neutral years that stand as gateways into these broader configurations are: 1500, 1860 and 1942.

### *1500*

Here the main transformative event is the rapid opening up of the sea-lanes from Europe to Southern Africa, the Indian Ocean, the Americas, and across the Pacific between 1487 and 1522. As just noted, this expansion of the international system to planetary scale changed the nature of IR. It precipitated the death of most of the native populations and civilizations of the Americas, inaugurated a great transfer of flora and fauna and diseases among Eurasia, Africa and the Americas, opened the Indian Ocean trading system to the dominance of European gunboats, and led to a thin but significant global economy in commodities ranging from silks and silver to spices and slaves. For some scholarship, this marks the beginning of world capitalism (e.g. Wallerstein, 1974). The impact of this cluster of macro-historical processes around 1500 was limited by the scope of agrarian technology, but it paved the way for the huge intensification of the global economy, and the mass human migrations that accompanied it, during later periods. This form of change is generally now occluded in IR theory because a change in the physical scale of the international system has no immediate contemporary relevance. The transformation to global scale was a one-off process, one which is clearly both deep and, by definition, of global significance.

### *1860*

1860 represents the biggest, deepest, widest cluster of transformative processes in the last 500 years. 1860 is nested within a configuration of changes that flowed throughout the 19<sup>th</sup> century, but accelerated and became dominant between 1840 and 1870. The deepest change was that from an agrarian, absolutist form of society to an industrial, mass society – in short, to modernity. A configuration of interlinked changes gave rise to a new mode of production, new class structures, new ideas, new modes of organization, and vastly increased interaction capacity. Taken together, these generated a new mode of power.

In terms of ideas, by 1860, nationalism, socialism, liberalism and ideas of permanent human progress had profoundly changed the social organizing principles of international society. Darwinism (1859) had just hit. The nation state was in the midst of replacing absolutist composite states – the unification of Germany (1870) both displayed the triumph of the national principle and created a new industrial power that was to destabilize the balance of power for the next eight decades. By 1860, industrial capitalism was well advanced in

replacing agrarian forms of political economy and was in the midst of creating a more integrated, but sharply unequal, global economy. It was also beginning to push global CO2 emissions and population growth into the geometric curves familiar today. Industrialization further revolutionised transportation (railways, steamships, the Suez Canal) and communication (the telegraph), which, in turn, engendered major increases in both trade and migration. By 1860, industrialization was also beginning to change the face of arms racing (the French ironclad *Gloire* was launched in 1859) and war (as was made apparent during the US Civil War of the early 1860s). It had already introduced the huge inequality of military power between core and periphery demonstrated by Britain's easy defeat of China in the 1840s. On the other side of this dynamic, the Indian Revolt of 1857 was the forerunner to later resistances to Western imperialism.

This period also marks the beginning and take-off of 'liberal ascendancy' (Ikenberry, 2009). By 1860, economics was acquiring some autonomy from politics and the global market was beginning to overwhelm local circuits of exchange and production. At the same time, modern multilateral diplomacy began to replace dynastic diplomacy. In 1865, for example, the International Telecommunications Union became the first standing intergovernmental organization, symbolising the emergence of permanent institutions of global governance. The British Companies Act of 1862 has already been mentioned. During this period, Western international society began to include some non-Western nations, such as Japan, on the basis of a liberal 'standard of civilization' (Suzuki, 2009). At the same time, stratificatory differentiation both within and between states gave way to functional differentiation. In short, 1860 serves as a useful shorthand to mark the coming into being of the modern world of rational-bureaucratic states, industrial societies, permanent technological change and global markets familiar to the 20<sup>th</sup> and 21<sup>st</sup> centuries.

This transformation does not loom large if viewed through the Waltzian lenses of organizing principle and distribution of capabilities. But it is a major transformation point for other tools of assessment, including social organizing principles, interaction capacity and mode of power.

## 1942

1942 represents a third primary benchmark date, symbolizing the period between 1929 and 1949 during which a major financial and trading crisis paved the way to World War Two via breakdowns in liberal economic and political practices, and ideational changes including the rise of fascism in three great powers (Germany, Italy and Japan). By 1942, all the great powers were engaged in a systemic war whose outcome hung in the balance. The 1929 crash was followed by Japan's invasion of Manchuria in 1931 and the expansionism of Nazi Germany, which were the forerunners of systemic conflict. During the same period, nuclear weapons changed the utility of war by blurring the distinction between victory and defeat, and by opening the

possibility of human species suicide. Clustered around this date are several other substantial transformations: a polarity shift from multipolar to bipolar; the demise of colonialism and racism as institutions of international society; and a major increase in the membership and scale of international society through extensive decolonization. The US succeeded Britain as hegemon of the liberal order, and helped to extend the scope of liberal international order through institutions such as the UN and the introduction of new financial and trade regimes. The 1949 revolution in China added a new revolutionary power to the system, with considerable long-term effects. In terms of interaction capacity, civil aviation took off in a big way on the back of aircraft technology developed during the war, quickly replacing shipping as the main form of international transport for human beings and light freight.

In the 1942 cluster, therefore, sit changes in the distribution and mode of power, systemic crises (both military-political and economic), shifts in both social organizing principles and the scale of international society, and a lesser change in interaction capacity.

### Secondary benchmark dates

Secondary benchmarks represent significant clusters but are not associated with deep change and do not necessarily carry global significance. We highlight three: 1648, 1800 and 1916.

#### 1648

We reluctantly stick with 1648 as a benchmark date because, whatever the ins and outs of the Treaties of Westphalia themselves, this date is a reasonable tipping point in the emergence of the absolutist sovereign state. This process stretched from the late 15<sup>th</sup> century (see Buzan and Little, 2000: 401-2) through the Glorious Revolution in England in 1688, to the Treaty of Utrecht in 1713, which consolidated the balance of power as an institution of European international society. 1648 thus ticks two big Waltzian boxes: change in the organizing principle and change in the nature of the dominant unit. It also involves big changes in the social organizing principle and a major crisis (the 30 Years War). For many in IR, this transformation marks the beginning of 'modernity' in the form of the sovereign territorial state as the defining unit of analysis (as opposed to the conglomerate empire). And, as Clark (2005: 51-70; more tentatively Holsti, 1991: 39) argues, Westphalia also marked the coming into being of a self-conscious international society of states in Europe.

However, there are two major reasons why 1648 is a secondary rather than a primary benchmark date. The first is that it is a largely European event, therefore lacking the global credentials for primary benchmark status. Only those in thrall to the idea that European history *is* world history could see 1648 as globally significant. Second, Westphalia does not mark the transition to modernity. It is true that Westphalia can stand for the idea of the territorial state

and, more arguably, the idea of sovereignty (but *not* sovereign equality) as a defining practice of international relations. What eventually spread around the world, however, was not the absolutist state associated with Westphalia, but the rational-bureaucratic state which emerged during the 19<sup>th</sup> century (for which 1860 serves as a superior segue).

### 1800

1800 serves as a symbolic date for the cluster of changes stretching from the American revolution (1776) to the Treaty of Vienna (1815). This cluster, usually represented by 1815, already receives some attention in IR. Ikenberry (2001) sees the end of the Napoleonic Wars as an important forerunner for settlements that bind states together in cooperative institutional frameworks and lock-in stable forms of order. Much realist work uses the 1815-1848 period as a test case for balance of power theory (e.g. Schroeder, 1994). And both democratic peace theory and the Correlates of War databases tend to use 1815-16 as the starting point for their coding. Few IR frameworks pay much attention to the Atlantic revolutions of France, Haiti and America. Yet these revolutions established the contours of debates about social organizing principles which were to resound loudly over the subsequent two centuries: the extension of market relations and the emergence of resistances to them; the rise of nationalism and republicanism as ideological forces; and (particularly in the case of Haiti) the role of racism in sustaining global hierarchies. Likewise, little scholarly attention is paid to the Latin American Wars of Independence during the early part of the 19<sup>th</sup> century. Yet these wars were important to the formation of modern international society in two ways: they led to an expansion of international society from European to Western; and, like the Atlantic Revolutions, they marked the rise of new ideologies such as republicanism and nationalism that proved disruptive to international order over the next two centuries.

Yet although 1800 represents a significant cluster, it does not have the global consequences required to be a primary benchmark date. International society still remained largely defined by states within the European cultural tradition. There was no significant change in polarity – the US remained peripheral to world politics and was not a great power for some time to come. The Treaty of Vienna was a conservative peace which re-imposed the absolutist *status quo ante*, meaning no basic change in the dominant unit. Although ideologies like nationalism and republicanism were beginning to disrupt international order, these were not yet altering the primary institutions of international society. At the same time, more radical ideas, such as socialism and free-trade liberalism, were not yet transforming the fundamental practices of the international system. Similarly, there are no global changes in interaction capacity associated with this date and only marginal shifts in the mode of power (such as the emergence of the *levée en masse*).

## 1916

1916 serves as a shorthand for a cluster of changes stretching from the fall of the Qing Empire in China in 1911 through to the end of the First World War. It embodies two major crises: World War One itself, and the Russian revolution. During this period, the Ottoman and Austro-Hungarian Empires also collapsed, leading to another expansion in the membership of international society. World War One was important as the first systemic industrial conflict and as the culmination of dynamics which had festered during the latter part of the 19<sup>th</sup> century. The war accelerated Britain's decline and heralded, but does not represent, a power shift from Europe to North America. In retrospect, the war made only marginal changes in the distribution of power, and almost none in social organizing principles. Even the League of Nations was only an extension, albeit an ambitious one, of developments in standing IGOs started several decades earlier. Although there were some changes in the mode of power (aircraft, submarines), these reflected earlier technological breakthroughs. Arguably the Russian revolution was the most important event in this cluster for understanding the configurational quality of macro-historical dynamics around this time. By embedding a new social organizing principle within a powerful state, it influenced the ideological make-up and conflictual alignment of the international system for the following seven decades.

### Tertiary benchmark dates

Tertiary benchmarks are mainly point-in-time events which either do not have a supporting cluster, or are mainly local/regional. We also propose that this classification be used for recent dates that appear to be significant, but for which there is insufficient distance by which to assess their depth and breadth. Some constraint is necessary to avoid premature elevations of recent events to benchmark date status, as is the case with 1989 and 2001. Perhaps something like a 'thirty-year rule' might be applied before moving candidates out of tertiary status. We highlight one older tertiary benchmark date, 1905, and several recent events that mostly fall within the thirty-year rule.

## 1905

The Japanese defeat of Tsarist Russia in 1905 meant that the country became recognized as the first non-white great power. This was a stirring event at the time, providing a challenge to Western notions of cultural and racial superiority. In retrospect, it stands as the first major move in what is currently thought of as 'the rise of the rest', when non-Western societies harnessed industrialization, modern state building and ideologies of progress in indigenous configurations (Zakaria, 2008). The potentially greater significance of 1905 is that it marks the end of a period in which the West established the contours of a core-periphery international order. The inter-societal formations which enabled the emergence and institutionalization of this system also



contained, paradoxically, the seeds for the rise of such non-Western powers. As a result, the specific rise of Japan, as captured in its defeat of Russia, portended the general decline of the West. Or to put this another way, the debate about global (as opposed to merely European/Western) power shifts that occupies many commentators in the contemporary world has its origins in late 19<sup>th</sup> century and early 20<sup>th</sup> century dynamics. 1905 represents a segue into this wider dynamic.

### *1978-2008*

1989 has already become embedded as a benchmark date for IR, marking a significant change in the distribution of power and a minor expansion of international society, as well as the end of a major ideological struggle over the organization of modern social orders. In terms of changing the distribution of power and, perhaps, serving notice of the end of this ideological confrontation, China's opening-up in 1978 looks at least as significant as 1989. And polarity shifts are the least important form of system structure change. The on-going transformation of interaction capacity suggests 1991 and the release of the World Wide Web as a landmark perhaps comparable with the emergence of the telegraph. The consolidation and expansion of the European Union from 1993 might also be important.

Into this period fits the 2001 terrorist attacks on the US, with their major impact on security practices, and the 2008 economic crisis. It is too early to tell what the impact of the 2008 crisis is, although there is a developing sense that it is a major global event. As things stand, the events of 2008 and since have primarily impacted on Europe and the United States. However, the ripple effects of the crisis are potentially substantial and it may be that, over time, these events form part of a broader configuration in which neoliberalism becomes reduced from an orthodoxy into one amongst a range of capitalist assemblages (Sassen, 2010). At the same time, it is possible to see the financial crisis as, like the Japanese defeat of Russia in 1905, part of a broader range of processes that mark the relative decline of Western power. As such, 2008 serves to make the point that benchmark dates can shift in significance depending both on optic (long-term vs. short-term) and context (regional vs. global). Finally, this period is also associated with heightened awareness of climate change. The rise of environmental concerns might, in a longer perspective, constitute a tenth major form of change in international relations.

***Table 2 – A Summary of Proposed IR Benchmarks***

<b>Primary Benchmarks</b>	<b>Secondary Benchmarks</b>	<b>Tertiary Benchmarks</b>
1500		
	1648	

1860	1800	
	1916	1905
1942		
		1978-2008

While the process of assessing benchmark dates is always open to new developments and changes in historical understanding, recent events are, as noted above, particularly difficult to evaluate. Table 2 could, therefore, look quite different in a longer-term perspective. For example, a macro-historical perspective might entail seeing World Wars One and Two as a single event – the civil war of modernity – running from 1911 to 1949, with a tipping point at 1931. That would be more in line with other three-to-four decade clusters like those around 1500, 1800 and 1860. Perhaps such a longer-term lens would also incorporate the Cold War as a second round of the civil war of modernity, entailing a cluster from 1911 to 1989, with a tipping point at 1940. Thinking along these lines suggests a different approach to ranking benchmark dates. Rather than assessing their relative global significance and the depth of their nested changes, the aim would be to place dates within a cluster that represents a specific macro-historical configuration, and whose significance is defined in relation to their roles within that configuration.

## Conclusions

This paper is intended to open rather than close a debate. Our suggested schema for benchmark dates should be seen as a provocation rather than as a cast-in-stone proposition. Our view is that, whatever its faults, this schema represents a more incisive way of opening-up international relations as a field of enquiry than IR's orthodox set of benchmark dates. By bringing in criteria from a range of mainstream IR theories, and making them explicit, it moves the process of allocating benchmark dates beyond the realist criteria that dominate the orthodox set. It questions, but does not dismiss, the importance of major wars as benchmark dates, while at the same time elevating the status of other transformative processes, particularly those associated with the 'long 19<sup>th</sup> century'. It emphasizes clusters of processes more than point-in-time events. And more than anything else, the paper invites IR scholars to think about, reflect upon and problematize the constitutive practice of conferring benchmark status on particular dates. The discipline does this all too easily, most recently in relation to 1989 (Lawson 2010).

Our argument suggests five main conclusions:

- First, benchmark dates in IR should be ranked in order of importance: primary benchmark dates feature a combination of deep change, extensive clustering and global impact; secondary dates are tipping points around

which a set of changes cluster, but do not necessarily carry global importance; tertiary dates are point-in-time events which are usually local/regional rather than global in consequence.

- Second, over time, benchmark dates are likely to move up and down, or in and out of, any given ranking. Reassessment is a necessary part of the process. That said, more awareness of the criteria outlined above for allocating such status would enable better judgments, better critiques and better conversations between theoretical perspectives. IR scholars and students will continue to debate the importance of particular benchmark dates. But they should be more critical about this exercise and conduct it in a more systematic way.
- Third, major wars should no longer be seen as the main or automatic defining feature of IR benchmark dates. Some have that status; others do not. Economic, technological and ideational developments should be given more attention, even when they are not associated with major wars.
- Fourth, IR should pay more attention to benchmark dates rooted in and around the 19<sup>th</sup> century 'global transformation', exploring the ways in which the configuration of industrialization, rational state-building and ideologies of progress transformed key processes in the international sphere.
- Fifth, there needs to be a greater differentiation between regionally and globally specific benchmark dates, and within this an understanding that Europe is also a region. Before 1500 there is no global level to speak of, so pushing this kind of analysis deeper into history would require altering the scales of analysis.

If nothing else, this exercise makes clear that IR's orthodox set of benchmark dates is flawed. The orthodox set emphasizes the distribution of power without focusing sufficiently either on the changing mode of power or on the social, economic and political transformations which underpin such changes. And it emphasizes wars without looking closely enough at the social developments that give rise to them. IR's orthodox benchmark dates narrow IR's engagement with both history and the adjacent social sciences, and reinforce a parochial set of concerns. A rearticulation of IR around a more carefully considered and open set of benchmark dates would generate both a more acute historical antenna and a more deeply formed contemporary agenda. It would also put IR in a stronger position to exchange ideas with neighbouring disciplines in the social sciences and history.

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<sup>2</sup> We do not look at subfield specific benchmark dates in this article, although this could be an interesting area for further exploration.

<sup>3</sup> Our focus is on mainstream theories because our aim is to establish the principal ways in which IR organizes its research and teaching, and presents itself to other disciplines. Because *most* IR scholars use these theories, such a survey also illustrates what events and processes are considered significant by *most* of the discipline.

<sup>4</sup> An alternative to 2008 might be 2011, when the OECD economies became less than half of the world total. Thanks to Yves Tiberghien for this point.

<sup>5</sup> Feminist work might be included here, but does not make an explicit feature of benchmark dates. Some feminist analysis fits within dates already in play: the gendered construction of the Westphalian state, and the improvements in women's rights associated with the First and Second World Wars. Someone should do an article on this.