The challenge of global history

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There is an old and perhaps not very tasteful English proverb which says that 'there is more than one way to skin a cat'. Perhaps its meaning is obvious. In this case the 'cat' a very agile and elusive one – is global history. We can all recognize the value, the importance, even, perhaps, the necessity, of writing global history: first to acknowledge our interdependence as 'national' communities. Second, to recognize the significance over the very *longue durée* of macro-processes like migration, religious conversion, commercial exchange and even climate change. And third, more immediately, to explain (however provisionally) our current situation in a 'globalised' world. But how to study, let alone how to write, global history is not so straightforward. In the second part of this short paper, I consider some of the angles from which it can be approached, but also the shortcomings and difficulties from which each seems to suffer.

My own approach in *After Tamerlane*[1] was more one of *bricolage* than of grand design or theoretical sophistication. It was driven by curiosity about one large but also rather simple question: why did one part of Eurasia came to dominate the rest? Of course that question could hardly be asked without considering two 'supplementary' issues: domination on what terms, and domination for how long? In trying to answer it, I framed a series of general arguments, as well as several assertions, each of them open to challenge, and perhaps refutation. Let us begin begin with the assertions. The book claims that the 'world-island' (Mackinder's phrase) of Eurasia has to be seen as the pivot of world (and global) history, and its decisive arena. This is not a pronouncement that historians of the Americas, sub-Saharan Africa or the Pacific are likely to echo. It is also at odds with the long tradition of studying European expansion into the 'outer world' as the engine of world history. But it is based on the conclusion that in any long view of world history the relations between Western, Middle and Eastern Eurasia – that is, Europe, the Islamic world and East Asia – are the master-key to explaining the distribution of wealth, power and cultural affiliation across the globe as a whole. If this seems far-fetched, consider what difference it made that China had rejected a maritime 'destiny' by 1430; that

[1] John Darwin, *After Tamerlane: the rise and fall of global empires, 1400-2000* (London 2008).

the British took command of the imperial assets of India from the late eighteenth century; that Russia emerged from its Muscovite margins to become the overlord of Inner North Asia between 1550 and 1860.

The second assertion is perhaps just as contentious. The book also claims that empire has been the 'default mode' of political organization for much of recorded history, and should not be treated, as historians often imply, as an aberration or exception in the course of world history. It is the 'norm'. By contrast, the nation-state model is a recent and perhaps only short-lived experiment. That is not to say that empire is desirable: perhaps most of us would prefer to live in the civilized intimacy of the *Kleinstaat* imagined by Jacob Burckhardt.[2] But circumstances have usually favoured the agglomeration of power by ruling groups or states whose reach has extended far beyond the bounds set by language, ethnicity, religion or natural environment, the usual markers of a 'national' political identity. Hence, the struggles for domination in Eurasia are not to be seen as a result of individual perversity (the work of madmen, despots and tyrants), or as the peculiar sin of European imperialists. Instead they reflect the highly unpredictable consequences of unequal commercial, cultural and demographic exchange and the still greater uncertainties of warfare and diplomacy. Set inside the larger frame of the relations between Western, Middle and Eastern Eurasia, is thus a history of empire-building within but also across those 'civilisational' divides. The book said less than it should have about how empire should be defined. That is partly because (in the writer's view) its vital characteristic – the commanding influence (by rule, treaty or force) by one ethnic group (or elite) over several other ethnicities – is what makes it so commonplace. Most of the additional features that historians have lovingly piled up around empire are merely ornamental, that do more to obscure than reveal the fundamentals of power.

What of the arguments? The book advances four general propositions. The first rejects the so-called 'age of discoveries' as the decisive moment of change in Europe's relations with the rest of Eurasia. It argues instead that the drama of Europe's maritime expansion and its internal transformation in an era of state-building were matched by the early modern expansion and renovation of the states of Middle and Eastern Eurasia. Indeed, Ottoman expansion into Europe suggested that reports of the Europeans' global supremacy were at best premature. It was certainly true that Europeans

[2] Jacob Burckhardt, *Weltgeschichtliche Betrachtungen* (Berlin 1905).

displayed greater navigational expertise and adventure than sea-goers in Asia, as well as more general curiosity about other parts of Eurasia than Muslims or Chinese were to show about Europe. But it remained the case through the early modern period up until circa 1750 that all the main civilizations of Eurasia remained on a broadly equal footing judged in terms of their economic autonomy (but not necessarily their long-term capacity); their political independence; and their cultural self-confidence. Europeans might have cruised and traded along the coasts of Asia, travelled far inland and encountered Asian rulers and scholars. But though they were sometimes dismissive of their hosts' mechanical skills and political ethos, very few of them thought that the assertion of European primacy over the Ottomans, Iranians, Mughals, Qing or Tokogawa was remotely likely. And for very good reasons.

The second proposition is that when this change began to take place, its causes have to be sought not just in the realm of economic capacity (the industrial revolution), but in the interplay between geopolitical, economic and cultural developments and between European and Asian initiatives. The 'great commercialisation' in eighteenth-century Eurasia might have served as a catalyst. But a series of unpredictable geopolitical disturbances in Europe and South Asia between 1755 and 1815 shaped much of the outcome and created a 'new world' in which some European states (Britain and Russia especially) could exert far greater pressure in distant parts of Eurasia than had been faintly conceivable before 1750. What role the 'long Enlightenment' played in this process – and in the 'Great Divergence' more generally has been fiercely and so far inconclusively debated. Did Europeans enjoy an intellectual advantage over other Eurasians by 1750, in terms of their access to reliable and useable knowledge? Perhaps to some extent, yes. But the argument here is that without geopolitical change it would not have been easy to put this into much use as a weapon of Europe's pre-eminence over other cultures in Eurasia.

The third proposition is that the full effects of Europe's 'rise to power' were not to be felt until the mid-nineteenth century when the communications revolution brought by steam and electric and the arrival of a true global economy coincided with the rapidly widening gap between the technological resources of the most advanced Western states and those of Asian states and empires. But even when Europe's relative power was at its greatest, it was not enough to ensure the real domination of Asia. In China, the Ottoman Empire, Iran, in colonial India and above all in Japan, the means of resistance to European primacy were being mobilized just at the time when (to many Europeans) it seemed as if Europe's triumph was almost complete. Just how dependent Europe's Eurasian authority was upon the mutual restraint of Europe's great powers and a tacit doctrine of 'competitive coexistence' between them was revealed by the First World War and its turbulent aftermath.

The fourth proposition follows directly from this. Because Europe's domination was never complete, and its imperial power was (in many places) quite shallow, when its power finally lapsed in the Second World War there was no unified or europeanised world to bequeath to the 'great inheritor', the United States. It was, on the contrary, a world splintered by resistance, nationalism, ideological conflict and wars of succession and then pulled apart by the effects of cold war. And despite globalization (and the end of the Cold War), much of that legacy remains to this day.

Whatever the merits (or otherwise!) of the arguments of the book, it is plain that its method and approach could be challenged from several stand-points. One of the more obvious objections is that global history, and the attempt to explain its central puzzle the 'Great Divergence' between Europe and Asia requires a *systematic* comparison between the states and societies that 'succeeded' in the 'great transformation' and those that 'fell behind'. The historian should not rely upon an impressionistic account of performance and capacity but study in detail the range of key institutions, behaviours and beliefs whose collective divergence might explain the overall outcome. Thus forms of government and their relative efficiencies; the nature of legal regimes and their treatment of property and personhood; social attitudes to knowledge and science and the institutional forms for their collection and diffusion; the nature of class and other social distinctions (including ethnicity), their openness or rigidity; the nature of religious beliefs, not least in relation to science, and the degree of toleration permitted; as well the familiar indicators of economic performance: all require close comparative study to trace why and when their characteristics diverged, and with what wider effects. Once the results of enquiry have been gathered and analysed, we might venture a more confident statement of the causes of divergence, and the drive-motor of world history.

Indeed we might. But we might also blanch at the scale of the task involved. Such an enormous research programme would require many hands. But quite apart from the need to coordinate the various lines of enquiry, systematic comparison would raise a number of difficult methodological issues. For what periods of history, or for what groups of years would comparative study be most fruitful and appropriate? What states or societies should be the targets of investigation? What criteria should we use to decide on both questions? Given the problem that the sources available for most of the key topics vary enormously in different parts of the world, how can we compensate for the inevitable unevenness in the coverage possible? Indeed, at what point might we say that no comparison can be valid where the data is so scanty? Even if we succeed in piling up a huge mound of reliable data, what will it tell us? The vital issue will be: which sphere(s) of activity among those we have examined have exerted a *critical* impact on the historical trajectory of a country or region? But how can we tell? And even if we agree on the critical factors for one part of the world, can we be sure that the same causal relationship holds for all the others as well? Almost certainly not.

A second approach is to discard systematic comparison in favour of 'big history': the macro-processes which, it seems plausible to think, have shaped global history over the longue durée. There are plenty of candidates. Among those that have been most favoured by historians are the 'gunpowder revolution', the 'military revolution', the Enlightenment, the Industrial Revolution, the 'high imperialism' of the late nineteenth century, the 'world revolutions' of the twentieth, and decolonization. It is perfectly true that each of these would make a fascinating chapter in a textbook account of global history since 1400. But questions quickly arise. What principle of selection will help us to choose which macro-processes to study? After all, there are plenty of others that might deserve our attention. Should we not also consider the demographic revolutions that have changed the balance between different parts of the world? And what of the agricultural revolution that transformed British and European agriculture, and eventually much of the world's? Jan de Vries'[3] 'Industrious Revolution', and the atomic revolution of 1945 could make a strong case for inclusion. So could the phenomenon of religious conversion and the missionary movements – Christian, Muslim and other – that have changed the cultures of continents. What about climate change and the effects of disease, the uneasy relations between plagues and peoples so brilliantly evoked by William McNeill. Even if we could agree upon the full list of macro-processes that have structured world history, what chance would we have of deciding on their relative importance? Or would we have to fall back on a

[3] Jan de Vries, *The Industrious Revolution: consumer behavior and the household economy, 1650 to present* (New York 2008).

lame consensus that they were all 'significant'? And are we to treat them as distinct and separate phenomena? If not (and it would be implausible), how should they be related? Are we anywhere near forging the methodologies needed to find the connections and measure the 'weight' of outwardly impressive 'revolutions' – some of which (like the 'military revolution') have been wildly exaggerated?

The third 'grand strategy' to which global historians have been attracted is to search for more 'manageable' units through which to trace the exchanges and reciprocities that we broadly agree form the agenda of global history. There have been two main techniques. The first is regional. Perhaps influenced originally by the example of Braudel, the most popular 'regions' have been maritime. Rather than states or empires, it is (in the early modern world at least) maritime connections that bind peoples together. Thus we have the 'Atlantic world' to replace the 'old story' of Europe's colonization of the Americas. There is an 'Indian Ocean world' in which the commercial and cultural centrality of India replaces the familiar Eurocentric version the heroic myth of Vasco da Gama. There is the vast Indo-Islamic panorama conjured up in Andre Wink's *al-Hind*,[4] and the Southeast Asian world so brilliantly realized in Anthony Reid's *The Lands Below the Wind*.[5] The 'Black Atlantic' evokes the human and cultural traffic that bound Africa to the Americas after 1500: a traffic that was wider and deeper than the trades controlled by European merchants. The great attraction of this 'thalassology' is in the way that it alerts us to the intensity of exchange and reciprocity or 'connectivity' between locations whose links were obscured, submerged or destroyed by the geographical template of European colonization and the shadow of a Eurocentric historiography. Perhaps its greatest pioneer was Jacob van Leur. But we need to be aware of the limitations and dangers that (as always) lie in wait for the most seductive kinds of enquiry. As the Canadian historian Ian Steele pointed out, in a scathing assessment of the 'Atlantic world' history championed by Bernard Bailyn (merely U.S.history writ large, said Steele), such maritime 'worlds' may serve to conceal the most important sources of political and cultural identity. 'Atlantic world' history that ignores the grip of the European empires is merely a flattering fiction.

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[4] Andre Wink, *Al-Hind: The making of the Indo-Islamic World,* 3 Vols. (Leiden 1990-2004).

[5] Anthony Reid, *Southeast Asia in the age of commerce, 1450-1680: Volume one: The Lands below the Winds* (New Haven, CT 1988)

It would be possible to say more than there is space for here about another kind of history, the history of commercial transactions in which the exchange of foodstuffs, luxuries, fabrics, warhorses and elephants (among many others) carried with them powerful cultural 'messages' and exerted subtle but sometimes profound effects on both partners involved. This kind of history can enormously deepen our knowledge of the kinds of exchanges with which global historians are especially concerned. It can challenge the 'big picture' with which we try to make sense of the global past, but perhaps not replace it.

To what kind of conclusion can we come to at last? It is, perhaps, that for some time (perhaps a long time) to come, anything resembling a comprehensive global history will continue to elude us. We will need to pursue the kinds of detailed inquiries that have been outlined above. We will need to think hard about the methodologies needed to relate the various spheres of human activity that we want to compare, and assess their importance (at various times) for preventing, limiting or promoting the 'Great Divergence' – the core problem round which (as Kenneth Pomeranz rightly insists) global history must turn. Yet for the time being, at least, we still need a 'big picture' to make sense of our world: if historians don't provide one, others will certainly do so. But we would be wise to acknowledge that any 'big picture' will be a provisional, impressionistic and personal essay, subject to sceptical scrutiny, and to be quickly replaced if a better model arrives. Meanwhile, before we make plans for the cat, we'll have to catch it first.