The Great Divergence Debate

Matthias Middell / Philipp Robinson Rössner

ABSTRACT

Die Einleitung dieses Themenheftes, das die seit dem Buch von Kenneth Pomeranz populäre Denkfigur der Great Divergence kritisch betrachtet, rekonstruiert die Debatten um die unterschiedliche ökonomische Performance Westeuropas und Ostasiens nach 1800 als zentrale Auseinandersetzung in der wirtschaftshistorischen Literatur und in allgemeineren globalhistorischen Interpretationen und fragt nach den Möglichkeiten, einen allzu engen Fokus auf den Vergleich zwischen China und England zu überwinden. Die Erweiterung der Debatte auf eine größere Zahl von Fällen bietet ebenso die Möglichkeit zur methodischen Weiterentwicklung wie die Einbeziehung einer Kulturgeschichte des ökonomischen Denkens, die vermeidet, dass in anachronistischer Weise Konzepte des 20. Jahrhunderts auf die Zeit vor 1800 projiziert werden. Damit entsteht, wie auch in den Beiträgen dieses Themenheftes argumentiert wird, die Gelegenheit zu einer reziproken Komparatistik mit einer Fallzahl größer als zwei, die überhaupt erst die Falsifizierbarkeit von Aussagen in diesem Vergleich möglich werden lässt.

The Great Divergence debate has mobilized a lot of scholarly energy and can, without any exaggeration, be called the most central debate in global history today.[1] But is it only one debate with many facets or is it a bundle of discussions targeting different, however related, subjects that interest different groups of global historians? Debate means different things to the humanities and the natural sciences. For the latter, a debate is often a realm for possible breakthrough to which many people contribute by criticising

1 K. Deng, The Great Divergence and Global Studies, in: Konstanze Loeke / Matthias Middell (eds.), The Many Facets of Global Studies, Leipzig 2017 (forthcoming).

or confirming data collected by their predecessors as well as by adding factors possibly overlooked so far in the debate, but overall working collectively at a fundamental shift in the interpretation of a central assumption accepted up to this point. For humanities scholars, they often bring their different paradigmatic points of departure to the debate, which obscures, in some manner, the topic, making it more difficult to describe exactly what the debate is about.

The interesting thing with the Great Divergence debate is that it seems to share both characteristics: a community of scholars not only working competitively but also collectively to solve the originally formulated problem of timing, nature and extent of the 'Great Divergence', and at the same time a larger group relating the topic to different paradigms in global history. Of course, both perspectives have different ontological bases, the one with the idea that there is one historical trajectory and the other with the postulation that there are multiple ones. Comparative strategies, which are central to the Great Divergence, play out very differently when taking the one or the other assumption as a point of departure.

In this way the Great Divergence debate is not only, as its name may suggest, about the reasons for substantial difference in economic performance between Europe and China. First, Kenneth Pomeranz, who popularized the term Great Divergence, insisted right from the beginning of his book that it is not clear what the units of comparison are. Second, the time span to be considered remains uncertain. A third problem concerns the indicators with which one may – or may not – measure the performance. The fourth element is the narrative framing of the findings. And a fifth, by far not the last, dimension is the relationship with current developments and the ability to formulate – on the basis of the achieved consensus on factors leading to the Great Divergence – a prognosis for future development in one world region or the other. To this end, that is to say to understand the debate's different facets, the aim of the workshop held in April 2014 at the Centre for Area Studies of the University of Leipzig in collaboration with the University of Manchester was to bring scholars with different regional expertise and with different approaches to the Great Divergence debate together in order to look at possible convergences in the debate about the Great Divergence.

There is, on the one hand, a specialized debate among economic historians struggling with gross domestic product (GDP) and wage levels, while, on the other hand, historians of early modern times and specialists of the nineteenth and twentieth century fight about the importance of more recent developments in relation to the period approximately between 1500 and 1800. Some distinguish between an archaic and a modern globalization,[2] others insist on the emergence of a fundamentally new quality emerging over the nineteenth century by using the term "global condition".[3] There is no doubt that global

2 C. A. Bayly, 'Archaic' and 'Modern' Globalization in the Eurasian and African Arena, c. 1750–1850, in: A. G. Hopkins (ed.), Globalization in world history, London 2002, pp. 47–73.

3 C. Bright / M. Geyer, The Global Condition 1850–2010, in: D. Northrop (ed.), A Companion to World History, Malden 2012, pp. 285–302.

connectivity can be observed at times prior to 1800 or even 1500. In fact, it dates back millennia. But did it remain, until a certain point in history, a relatively sparse flow of goods and ideas between mainly autonomous "économies mondes" only? In that case, one would have to follow Fernand Braudel, who described the connections in many details without qualifying it as systemic interdependency that only came in a later period.[4] Or do we have to go back to the search for the origins of current inequality in the late fifteenth century, as Immanuel Wallerstein and many other world system theorists have insisted on?[5] What is the importance of early modern expansion and colonialism, which for a very long time was central to the narrative of European superiority as well as to those narratives trying to explain a lack of dynamic development during modern times in other world regions? Some ten years after Pomeranz's book came out it was underlined that it was only at the end of the twentieth century that comparison between the West and China has overcome a situation where "the study of economic history was largely about what happened in American and European history [… while for] historians of China, in the West as well as in East Asia, economic history was largely about what didn't happen. The absence of an industrial revolution struck generations of scholars to be a subject worthy of study".[6] Needless to say, the same applies to a series of other world regions, which, however, have not been subjects of a comparably strong empirical and theoretical interest. "Provincializing Europe" does not mean that all other provinces in global history profit from the same scholarly energy. The particularly strong focus on China has to do with the fascination for current Chinese economic growth – which obviously is a challenge to the traditional narratives that never foresaw a turn in directionality of their explanations – not only with the international opening of Chinese historical scholarship supported by resources from the flourishing economy but also with the recalibration of the internal balance within North American historiography, where a California school looking at the Pacific has gained momentum against Atlanticists from the East Coast.

The main messages from this new scholarship – both from East Asia and the US, as well as advanced by the Global Economic History Network built by Patrick O'Brien at London School of Economics and Political Science (LSE), which reaches out to a number of continental European countries – is that China and Europe in the early modern times were, with their well-developed commercial economies, far more similar than it was initially presumed in older European accounts. This establishes a baseline for the debate on the Great Divergence, which now contains many various bifurcations. One of these is about access to resources and opens the debate also up to environmental history. It has been argued, especially by Pomeranz, that the location of coal as the main driver of the change in energy supply was different for China's most prosperous centres of proto-industry, especially the Pearl River Delta, and for the English midlands. A second

4 F. Braudel, Civilisation matérielle, économie et capitalisme, XVe–XVIIIe siècle, 3 vols, Paris 1979.

5 I. Wallerstein, The Modern World-System, Vol. I: Capitalist Agriculture and the Origins of the European World-Economy in the Sixteenth Century, New York/London 1974.

6 P. O'Brien, Ten years after the Great Divergence, 2010 (Reviews in History) online at: http://www.history.ac.uk/reviews/review/1008.

argument used by Pomeranz, and more recently deepened by Sven Beckert,[7] is the access European economies had to resources first from the Americas and later on also from Central Asia and North Africa, providing the textile industries with cotton.

This story, however, is more complicated than the traditional one told from a world system perspective, where there is direct continuation from early modern colonialism to the second wave of colonial dominance over larger parts of the world by European empires in the nineteenth century. But as Patrick O'Brien and others argue, colonial importations generated a very small proportion of GDP in most European countries until the mid-eighteenth century; even in the early decades of the nineteenth century their level remained low.[8] It was only after "connexion matured into integration" that the resource-rich and now independent former settler colonies, the borderlands of European empires (from Eastern Europe and Central Asia to North Africa), and the step-by-step accessible vast zone "located in large part between the Tropics of Cancer and Capricorn" provided support for the rising growth rates in European industries and the massive expansion of world trade.[9]

Both arguments – the location of coal and the accessibility of resources from regions the Europeans expanded into (while the Chinese did not) – strengthen the impression of contingency at work. It helps in understanding a rather short-term advantage of the one over the other, but it also challenges fundamentally older explanations that saw long-term factors at work. It invites counterfactual reflection: what if China would have had the same access to coal and colonies as parts of Europe had? Peer Vries has expressed severe criticism towards such an interpretation and insists on choices societies have made with regard to the arrangement of their political institutions and the respective culture of action.[10] Other authors have on the contrary focused upon factors such as the ways in which knowledge has been used, stored, and transferred.[11] Europe has developed over the course of time an advantage in this respect with its institutions of higher learning, with printing technology and the circulation of more copies, and with a shift from classical (rather theoretically oriented) sciences to practical application.[12]

The centrality of the Great Divergence debate not only goes back to an old discussion since the times of Adam Smith, David Hume, Karl Marx, and Max Weber about the origins, causes, nature, and consequences of global income disparity. With this focus on Western Europe, there was the implicit or even explicit hypothesis of an European exceptionalism that has more or less long-lasting structural foundations, that has found

7 S. Beckert, The Empire of Cotton: A Global History, New York 2014.

8 P. K. O'Brien, Colonies in a Globalizing Economy 1815–1948 (=GEHN Working Paper, 8-04), London 2004.

9 Ibid., pp. 10-16.

10 P. Vries, Are coal and colonies really crucial? Kenneth Pomeranz and the Great Divergence, in: Journal of World History 12 (2001), pp. 401–446.

11 P. K. O'Brien, Historical foundations for a global perspective on the emergence of a western European regime for the discovery, development, and diffusion of useful and reliable knowledge, in: Journal of Global History 8 (2013), pp. 1–24.

12 C. McClellan III, Science since 1750, in: The Cambridge World History, vol. VII/2: Shared Transformations?, ed. by J. R. McNeill / K. Pomeranz, Cambridge 2015, pp. 181-204.

expression and/or has been grounded in a specific cultural attitude towards economic efficiency, and that has led to a sort of economic superiority of a Europe pioneering the transition towards modern industries.[13] When historians' interest shifted from a world history that followed the slogan of the "Rise of the West" to a global history that focuses more on cultural encounters[14] and multiple modernities,[15] such an openly Eurocentric story became a fundamental challenge.[16] A first effect of the debate was – and still is – growing empirical interest in developments outside Europe, a broadening of the geographic scope in research and its reorientation to Asia.[17] This was inspired by the growing academic weight of competencies on both sides of the Pacific – including but not limited to the famous California school in world history writing[18] – as well as by the increasing political attention toward a Pacific future of the world (or at least the US) as proclaimed by Barack Obama on several occasions.

The focus lies on the China-Europe comparison, which has been methodologically and empirically revived since the seminal book on the Great Divergence by Ken Pomeranz,[19] which heralded an ongoing debate between the so-called California school in global history and a counterposition that insists on the long-lasting superiority of Europe over Asia. Prasannan Parthasarathi has recently stressed that China by no means represents the only or even the most obvious starting point for an "East-West" comparison and shifts the focus to Indian textile industries in the early nineteenth century as another case in point.[20] The European comparator – England – however, stays the same in this work. But what about other "Europes", such as the German states, the Netherlands, Scandinavia, or the Iberian Peninsula? What about the region as the crucial factor? In fact, the methodological consequences proposed by Pomeranz,[21] who suggests to first reflect upon the definition of the units to be compared, still represents one of the more obvious gaps in the field and yearns for further contributions to the debate. Notwithstanding, undeniably, the attention given to Asia and the Pacific[22] is not topped by any analysis of an-

13 E. L. Jones, The European Miracle: Environments, Economies and Geopolitics in the History of Europe and Asia, Cambridge 1981, 3rd ed. 2003; D. S. Landes, The Wealth and Poverty of Nations. Why Some are So Rich and Others So Poor, New York 1998.

14 One of the most influential textbooks in this regard: J. H. Bentley / H. F. Ziegler, Traditions & encounters. A global perspective on the past, Boston 2000.

15 S. N. Eisenstadt, Multiple Modernities, in: Daedalus 129 (2000) 1, pp. 1–29.

16 We will not go into any detail of the diversified debate about European exceptionalism and Eurocentrism, which was stimulated by Chakrabarty's demand for a provincializing of Europe in 1992 (and since has been often repeated and rephrased).

17 A. G. Frank, ReOrient. Global Economy in the Asian Age, Berkeley 1998.

18 P. Vries, The California School and beyond: how to study the Great Divergence?, in: Journal für Entwicklungspolitik 24 (2008) 4, pp. 6–49.

19 K. Pomeranz, The great divergence. China, Europe, and the making of the modern world, Princeton, N.J 2001.

20 P. Parthasarathi, Why Europe grew rich and Asia did not. Global economic divergence, 1600–1850, Cambridge, New York 2011.

21 K. Pomeranz, The great divergence, chap. 1.

22 K. Pomeranz (ed.), The Pacific in the Age of Early Industrialization, Farnham 2009. The works quoted above about China and India are complemented by the very active Osaka school around Akita Shigeru providing evidence for a special Japanese path towards modernity: Nicholas White / Shigeru Akita (eds.), The international order of Asia in the 1930s and 1950s, Farnham 2010. See the contributions to the Workshop „Globalization from East

other world region over the past two decades.[23] Although cooperation between economic historians of Japan and Africa has resulted in an impressive typology of configurations of production factors such as labour, soil, and capital, which rather supports the idea of multiple pathways to modern constellations than a binary focus on "advanced" and "delayed" economies,[24] the Great Divergence debate in general has not gone very much beyond the Eurasian complex.[25]

The debate is not about the fact that the last four or five centuries have seen one of the most fundamental economic transformations in recorded human history. There is consensus that the growth spurt in European income per capita after 1800 was sufficient enough to set this world region apart from the rest. Over the past 500 years, West European per capita GDP – the most commonly accepted variable for cross-sectional and inter-temporal comparisons of economic wealth – grew by a factor of at least 21. By far, the lion's share of this growth fell in the post-1800 period. It was related to the phenomenon usually known as industrialization. Between 1820 and 2000, world population increased by 500 per cent, whilst world GDP rose by 800 per cent, amounting to a previously unheard-of expansion not only of overall economic wealth (and social inequality) but also human productivity. Whilst *all* world regions experienced significant increases in income over the past two centuries, notable world regional differentials have emerged. The fastest growth rates were witnessed in northwestern Europe and North America. Between 1800 and 2000, the income gap between the US and the African average widened from a factor of 3 to a factor of 20. There are strong claims that at the dawn of the early modern age (1500 AD) Europe and China may have stood head-to-head in terms of per capita GDP. But compared to this situation, a wide gap in wealth opened up by the twentieth century, with northwestern Europe five or six times as wealthy as China. This pattern, however, became reversed only fairly recently with much higher growth rates for China than for Europe between the 1980s and the early 2010s, although starting from a comparatively very low level and in the meantime slowing down substantially.

Asian Perspectives" held in Osaka 15-17 March 2016 at http://akita4.wixsite.com/globalhistoryonline/workshop-march-2016.

23 On Africa see J. Inikori, Africa and the globalization process: western Africa, 1450–1850, in: Journal of Global History 2 (2007) 1, pp. 63-86 and on the role of slavery, respectively its abolition for the development of markets: J. Inikori, The economic impact of the 1807 British abolition of the transatlantic slave trade, in: T. Falola / M. D. Childs (eds), The Changing Worlds of Atlantic Africa: Essay in Honor of Robin Law, Durham 2009, pp. 163-82. From the lacunar evidence that is available, it has been concluded that, in nutritional terms, standards of living in West Africa were not that far from the ones in Europe before 1800: G. Austin / J. Baten / B. van Leeuwen, The biological standard of living in early nineteenth-century West Africa: new anthropometic evidence for northern Ghana and Burkina Faso, in: Economic History Review 65 (2012) 4, pp. 1280-1302. However, the central conclusion from an analysis of West Africa, as presented at the Leipzig workshop in 2014 by Gareth Austin, is that the different pathways various regions took mutually constitute one another. Slave trade has provided other regions and powers with the necessary workforce to run the plantation economy and to circulate silver globally, while at the same time it played a decisive role in the formation of a particular economic model in large parts of Africa.

24 G.Austin / K. Sugihara (eds,), Labour-intensive industrialization in global history, London 2013.

25 K. Pomeranz, Beyond the East-West Binary. Resituating Development Paths in the Eighteenth Century World, in: Journal of Asian Studies 61 (2002), S. 539–590.

One of the results of this situation was Europe's implied superior culture compared to other world regions. The most recent attempt at explaining global income disparities in the longer run comes from Acemoglu and Robinson, who claim that it was inclusive (i.e. superior) economic institutions that were more beneficial for economic growth than others. It was in those world regions that first implemented inclusive (i.e. non-extractive) economic and political institutions (i.e. post-1688 England and then northwestern Europe) where we find the origins of modern economic growth, that being the type of trajectory that would eventually lead to industrialization. Here it was England – and then other European regions – that came first.[26] Some scholars highlight cultural bifurcation in social norms and kin structure, thereby affecting the nature of contract enforcement and later, in turn, processes of divergence between China and "the West".[27] Europeans' cultural inclination to economic and military competition, manifested by the market and the tournament, have likewise been cited as crucial factors setting Europe apart from the rest.[28] Others mention the rise of "bourgeois values and dignity"[29] and the eighteenth-century Enlightenment as a revolution in attitude towards more rational thinking about the economy and the market.[30] The implicit conclusion of these exercises usually is, if not always as explicitly stated as in David Landes' book *Wealth and Poverty of Nations*, that (a) Europe's path to the modern age was the optimum one (best-practice model), and that (b) there existed something like causality, a "law" that somewhat preconfigured this path to modern economic growth. It was thus inevitable for this transition to occur in Europe first.

"Economic supremacy" is usually defined, in the grand neoclassical narratives,[31] by using economists' lingo, which is not unproblematic.[32] The commonly chosen cross-sectional and inter-temporal comparator is per capita GDP, often expressed in terms of 1990 purchasing power parities (PPP; Geary-Khamis $) or some other sort of cross-sectional variable that allows for global comparisons. This, however, rests upon at least two as-

26 D. Acemoglu / J. A. Robinson, Why Nations Fail: The Origins of Power, Prosperity and Poverty, New York 2013.

27 A. Greif, Institutions and the Path to the Modern Economy: Lessons from Medieval Trade, Cambridge 2006; A. Greif / G. Tabellini, Cultural and Institutional Bifurcation: China and Europe Compared, in: American Economic Review, 100 (2010), 135–140.

28 E.g. P. T. Hoffman, Why Did Europe Conquer the World?, Princeton 2015; D. S. Landes, The Wealth and Poverty of Nations: Why Some Are so Rich and Some so Poor, New York 1998.

29 D. N. McCloskey, The Bourgeois Virtues: Ethics for an Age of Commerce, Chicago 2006; id., Bourgeois Dignity: Why Economics Can't Explain the Modern World, Chicago 2010; id., Bourgeois Equality: How Ideas, Not Capital or Institutions, Enriched the World, Chicago 2016.

30 J. Mokyr, The Enlightened Economy: An Economic History of Britain, 1700–1850, New Haven 2009; id., A culture of growth. The origins of the modern economy, Princeton 2016.

31 To which we may count Landes, Wealth and Poverty of Nations, or Jones, European Miracle, or Acemoglu & Robinson's Why Nations Fail.

32 A. Maddison, Monitoring the World Economy, Paris 2003. The "Maddison Project" database is constantly being updated, and Maddison's figures constantly revised, by Maddison's eager and skilled epigones, who strikingly mostly have a Dutch or London School of Economics background. See http://www.ggdc.net/maddison/maddison-project/home.htm. On the questionability of applying modern economic (and thus measuring) parameters to pre-modern economy, see the polemic account in F. Boldizzoni, The Poverty of Clio. Resurrecting Economic History, Princeton 2011.

sumptions that are inherently anachronistic, the first one being that money, markets, and monetary transactions were equally important in either world region at any time during the last millennium. Another inherently questionable proposition lies in the assumption of "the market" (whatever this strange creature may be in reality) being the main or even major clearing agent for transactions, demand and supply of productive factors, and other resources, that is to say goods, services, and other economic transactions across all countries and societies and times compared (i.e. Eurasia).[33] Because only if that were true the cross-regional comparisons of "world GDP" would make sense and be possible and meaningful. Furthermore, historical researchers have failed, as of yet, to come up with a more reliable alternative. The obvious – and commonly chosen – one is real wages, which have been subjected to global and long-term historical comparison by historical economists, thereby reducing them to a common denominator: "grams of silver".[34] The evident problem is now that this comparator for "global" differences in purchasing power only works if all people estimated silver similarly, across all world regions and at all historical times. But anyone vested in the monetary history of medieval and early modern Europe and extra-Europe will immediately remember that there were vast global divergences in people's estimation of silver, with gold-silver ratios varying hugely across world regions over time.[35] In the early modern age, silver was, when measured in gold, twice as valuable in China as in northwestern Europe (and even within Europe there was huge variation in the gold-silver ratio, the commonly accepted dummy variable for the price of silver). Thus someone earning the equivalent of about half the sum of silver (in grams) earned per day in China in comparison to "Europe" actually would have been – following this reductionist economistic logic of the market – theoretically speaking *as well off* as his or her northern European counterpart. Silver wages in Europe simply do not compare well to silver wages in Asia, as degrees of monetization, monetary, and cultural systems of labour, payment, and exchange were different.

The other eminently questionable proposition of exercises – such as Maddison (2003), with the "reconstruction" of historical time series on per capita GDP since the birth year of Christ for the entire world – is buried in the implicit statement that GDP (and per capita GDP) "existed" during the historical time periods and area frameworks chosen for comparison. As new studies have shown,[36] "gross domestic product" as a measurable

33 L. Neal / J. G. Williamson (eds.), The Cambridge History of Capitalism, Vol. I: The Rise of Capitalism: From Ancient Origins to 1848, Cambridge 2015.

34 E.g. Robert Allen.

35 On global differentials in the gold-silver ratio, see D. O. Flynn/A. Giráldez, Arbitrage, China and World Trade in the Early Modern Period, in: Journal of the Economic and Social History of the Orient XXXVIII (1995), pp. 429–428; id./id. 'Born with a "Silver Spoon": the Origin of World Trade in 1571', in: Journal of World History VI (1995), pp. 201–221; id. / id., Conceptualizing Global Economic History: The Role of Silver, in: R Gömmel / M. A. Denzel (eds.), Weltwirtschaft und Wirtschaftsordnung. Festschrift für Jürgen Schneider zum 65. Geburtstag, Stuttgart 2002, pp. 101–114.

36 D. Philipsen, The Little Big Number: How GDP came to Rule the World and What To Do About It, Princeton 2015; M. Schmelzer, The Hegemony of Growth: the OECD and the Making of the Economic Growth Paradigm, Cambridge 2016.

(operational) entity was fully developed after 1900 in Europe. Kent Deng and Patrick O'Brien in a recent article convincingly demonstrate that it is impossible to calculate (or even estimate) GDP for China over different historical epochs.[37] True – contemporaries before 1900 had fairly well-developed concepts of national income, and since the late seventeenth century even developed crypto-statistical ways of "measuring" these. However, as neither the economic nor statistical concepts of pre-1900 income accounting were, strictly speaking, comparable to the commonly agreed upon post-1900 standards of national income accounting – which very much rest upon the Keynesian revolution in economic theory, international comparability, and internationally harmonized measuring schedules – they are unlikely to generate meaningful insights when applied as measuring tools for any period before approximately 1900. Nor do they provide meaningful ways of linking our contemporary economy with European economy in, say, the 1600s or 1700s, or for comparing "China" with "Europe" as based on the same logic of "measurement".

These are grave implications regarding the long-term comparison of economic trajectories over time and in a cross-sectional perspective, such as the Great Divergence in income wealth and productivity between Western Europe and "Asia" since the (European) early modern age. Angus Maddison and his cooperators seemingly "confirm" the pattern of a Great Divergence as early as the 1500s. But this reasoning is simply circular because growth rates for European countries and China in these studies were obtained using circumstantial and non-quantitative documentary evidence from the very same periods as well as textbooks and historical works that advocate the story of the "European miracle". These non-quantitative data were, in a second step, "calibrated" so as to link up with modern, post-1900 statistics, thus obtaining the "growth" rates of the European and Chinese economy since the birth of Christ.

The mere implication that some societies / states / "nations" may "fail" in terms of economic and societal development (however defined), whilst others do not, is likewise interesting. It implies that there are "wrong" or suboptimal trajectories in world history as opposed to "good", "right" or "correct" ones, that is to say best-practice paths of development that can be studied, mapped, and then followed by any country or society if only people would choose to follow these good prescriptions. In recent decades, most scholars have become increasingly sceptical of teleological interpretations of history and development. They have instead stressed aspects such as idiosyncrasy (time-space contingency) and path dependency (accumulation of critical steps or events over time until what is sometimes called a "critical juncture" has been reached, making certain outcomes impossible and others inevitable and thus channelling history and development into a certain direction). Many scholars would nowadays probably agree that history should not be read backwards. Rather, societies and their potential to experience or generate economic growth should be interpreted and judged on the basis of their own and idi-

37 K. G. Deng / P. K. O'Brien, China's GDP Per Capita from the Han Dynasty to Communist Times, in: World Economics 17 (2016) 2, pp. 79–123.

osyncratic location(s) in time. Such discussions were fuelled since the appearance of Bin Wong's *China Transformed* (Cornell UP 1997) and Kenneth Pomeranz's *Great Divergence* (Princeton UP, 2001), both who argued that China and Europe shared a crucial set of similar preconditions and constraints in terms of resource endowment until about 1750 or 1800. And whilst the aspect of resource endowment still has some bearing in modern debates,[38] scholars have now tended to move away from geo-deterministic approaches.[39] They have instead focused on aspects such as interaction and connectivity; on people, groups, and diasporas; or on human interactions with the physical and material environment as possible causes for divergent patterns of development.

But the story does not end here. In fact, there is still much room for further discussion and clarification as well as perhaps modification of the story. Whilst our understanding of differential growth trajectories within the modern European industrial economy has progressed, calling attention to the fact that there was a "small divergence" within industrializing Europe, or acknowledging (with Magnusson[40]) that there was a prehistory to this process dating back even beyond the early modern period, needs further elaboration. Moreover, there is much room for further debate in terms of unsolved problems or controversies, especially on questions such as governance, agency, and geographical coverage. The major question is – should we adopt top-down or bottom-up rationales when talking about and explaining economic divergence? Are we talking about markets or individuals, about merchant networks or states? And what is "the state" anyway? Or rather: who is "the state"? Which tension fields, conflicts of interests, and competition for scarce political, social, and economic resources (capability) exist within each world region or society that make the fabric of states[41]? How did other regions contribute to the process we have come to known as the Great Divergence – for instance, the seventeenth- and eighteenth-century German states under the political economy paradigm of Cameralism[42]?

Also, as works by van Zanden[43] and others have shown, some of the European models have their roots in the Middle Ages.[44] Therefore, a starting point for discussions of global

38 J. Baeten (ed.), A History of the Global Economy. 1500 to the Present, Cambridge 2016.

39 But G. Parker, Global Crisis: War, Climate Change and Catastrophe in the Seventeenth Century, New Haven 2013, originally a military historian of early modern Europe, favours a geo-/climatic-deterministic global history approach.

40 L. Magnusson, Nation, State and the Industrial Revolution: The Visible Hand, London / New York 2009.

41 See the major works by W. Reinhard, Geschichte der Staatsgewalt: eine vergleichende Verfassungsgeschichte Europas von den Anfängen bis zur Gegenwart, München 1999; M. Levi van Crefeld, The Rise and Decline of the State, Cambridge 2000.

42 Most recently P. R. Rössner (ed.), Economic Growth and the Origins of Modern Political Economy: Economic Reasons of State, 1500–2000, London / New York 2016; id., Heckscher Reloaded? Mercantilism, the State and Europe's Transition to Industrialization (1600–1900), in: The Historical Journal, 58 (2015) 2, pp. 663–683.

43 E.g. J. Luiten van Zanden, The Long Road to the Industrial Revolution: The European Economy in a Global Perspective, 1000–1800, Boston 2009.

44 See, for a start, E. S. Reinert, The Role of the State in Economic Growth, in: Journal of Economic Studies 26 (1999) 4/5, pp. 268-326; id., How Rich Countries Got Rich – And Why Poor Countries Stay Poor, New York 2007; id. / P. R. Rössner, Cameralism and the German Tradition of Development Economics, in: E. S. Reinert / J. Ghosh / R. Kattel (eds.), Elgar Handbook of Alternative Theories of Economic Development, Cheltenham / Northampton 2016,

economic divergence in, say, 1750 or 1800 may be far too late to yield meaningful bases for comparison; moreover, a focus on England (itself a country with much internal economic diversity) or the Yangtze River Delta may also lead to a somewhat biased picture. Another fundamental – and yet almost completely ignored – methodological vantage point for studying global economic divergence is the realm of economic thought or reasoning about the economy. Sometime after 1500, 1700, or 1800, Europe turned into the richest region on the globe. Hardly any attention has been paid to the *economic ideas* that were important in the process.[45] Which theories and belief systems guided these processes of growth, transformation, and divergence over the last millennium? How did modern economic knowledge and modern political economy become shaped and influenced by "older" ideas? There was obviously a lot of interaction between economic policy and theory guiding that policy since the beginning of the early modern period – often but not ultimately helpfully labelled "Mercantilism" and/or "Cameralism". And to what extent were the differential approaches between West European states and China towards managing the markets and interfering with the economy during the last five hundred years or so informed by different ways or traditions of economic reasoning? – Some have seen Confucianism and non-intervention paradigms as forerunners of eighteenth-century Physiocracy and thus modern economic liberalism. Furthermore, to what extent these differences in economic paradigms caused different policies and different economic outcomes in the long run is something that remains to be tested. The empirical research presented in Vries' recent work seems to suggest that state intervention indeed made a crucial difference[46] as well as that this difference in policy may well have been the outcome of West-East differences in economic reasoning and differing concepts of what the economy was and what "the state" should do to it.

The essays in this thematic issue are a selection from what was presented at a workshop in Leipzig that took inspiration from the collaboration in the Global Economic History Network set up by Patrick O'Brien and his colleagues at LSE with which Leipzig has a joint global studies master's programme, as well as the group of world historians active at the University of Manchester History Division. Roy Bin Wong opened the workshop with the Centre for Area Studies' annual lecture on 11 April 2014 and the following two days were devoted to case studies (ranging from Gareth Austin's and Geert Castryck's Africa to Laurence Brown's networks of migrant labour and commodity chains and Markus Denzel's history of book keeping and payment formats since the fourteenth century) and conceptual discussion (opening to the debate on the Anthropocene by Prasannan Parthasarathi, and the role of the state in modern histories by Bin Wong). We have chosen the following four articles for this issue not because they were the only interesting

pp. 63-86; P. Vries, Governing Growth: A Comparative Analysis of the Role of the State in the Rise of the West, in: Journal of World History 13 (2002) 1, pp. 67–138; id., State, Economy and the Great Divergence: Great Britain and China, 1680s–1850s, London 2015.

45 Rössner (ed.), Economic Growth and the Origins of Modern Political Economy.

46 Vries, Nation, State and Great Divergence, passim.

ones but because they articulate the wish to transcend the state of the debate in various directions.

Georg Christ, in his essay on the late Middle Ages as the point of departure for an entangled history of the Levantine and the European industries, is cautious of approaching European divergence as a sui generis phenomenon, and alternatively argues that the "decline" of the Islamic Levant was precisely what produced a fertile ground for the Europeans to take over. For him, the historic circumstances that have led to deindustrialization of the Levant are what allowed this knowledge to "trickle into Europe". This "small divergence", often underrepresented in the current literature on the Great Divergence between Western Europe and East Asia, allows us to better understand how parts of Europe got to the position from where they started the global adventure.

Christ begins his analysis by covering the debate on whether such decline has indeed taken place. In his literature review, he contrasts the contested factors that have been attributed to the decline with a similarly long list of counterarguments and refutations. One of such factors, for example, concerns Islamic law and in particular inheritance law, which has been argued by some historians to have been an impediment to the economic growth. However, pointing to similar practices elsewhere, it cannot be considered as a hindrance; oppositely, perhaps, as other scholars argue, it can be seen as an advantage. In general terms, he underscores that Islamic law itself cannot be the problem, but rather how it is the way it is applied or not applied.

Christ carries on to describe the deindustrialization that the Mamluk Empire witnessed. He asserts that it was a highly sophisticated economy with high public expenditures, but due to the fact that the elites were an "allogenic meritocratic class of wage earners and public servants, who collectively controlled agricultural resources without owning them", they reduced the resilience of the system, as opposed to England with lower and centralized public expenditure and property ownership by the elites. For Christ, the turning point for the decline is to be found in climatic change and the plague that broke out in the mid-fourteenth century. As a result of these devastating catastrophes, Egypt lost about half of its population, which in turn produced a massive shortage of labour, thereby creating a tremendous negative impact on the irrigation systems in place. Those, due to the lack of maintenance and the inability of the elites to secure the land, were taken over by the Bedouins as pasture land. A large percentage of the remaining rural population then moved to the cities, where they faced trouble attaining new employment due to a lack of skills (such as weaving). Learning new crafts was not encouraged or perceived to be rewarding enough. In these economic circumstances, skilled industrial labour costs increased, but were never restored.

Christ questions how this could possibly happen and finds an explanation in the rationale of the ruling elites and upper classes, who were strategically allied with lawyers, thus catalyzing "bureaucratic growth, clientele network driven lobbyism, careerism, heavy taxation, as well as meritocratic permeability, increased importance of higher education and centralisation". Indeed, this decline in industrial production is what then permitted European industries to take over as they were able to recover from the plague much quicker and address industrial demands. Venice in particular collected the highest benefits due to its proximity and magnitude of trade relations. However, because Egypt was an obligatory passage for the trade routes of the time, its economy could stay afloat by importing goods; however, opposed to common assumption, it did not exhibit patterns of "colonial trade", whereby raw materials would be exported and finished goods imported. In this case, Christ presents an account of exports and imports based on primary sources that disproves such an idea. On the contrary, it shows that Egypt managed to maintain a niche production of certain luxury items by restructuring them into smaller scale productions, such as silks and rugs, as well as a monopoly on metal wares. These findings indicate that the economy was not subjugated to West European export but retained spheres of influence in terms of trade. These goods were desirable and, even though often imitated by the European counterparts, were in demand due to their superior quality and as indicators of prestige. Another demonstration of the economy's endurance, rather than decline, is the way Egypt was able to re-export the imports it acquired to Yemen and India, which helped to balance out the Venetian trade deficit.

By showing that when facing drastic circumstances the Mamluk Empire, although giving up a lot of its industry, especially production of cheap goods, specialized in certain high quality goods, Christ affirms that this counters the idea of a loss of skill or sophisticated techniques. What has been called decline is therefore rather a transformation under the pressure of interaction with neighbouring economies – that is to say, a theoretical model for explaining interdependent growth that might inspire us to analyse the configurations in the Great Divergence such as Gareth Austin, for example, argues with respect to sub-Saharan Africa.

Philipp Robinson Rössner approaches the topic of Great Divergence from another angle by venturing into exploring the nexus between the history of economic thought and the regional differences in its application since 1500 while examining the ways in which ideas about economic growth have travelled to and interacted with various contexts in both Europe and Asia. This approach allows for a framework to be developed that encapsulates the dynamism of ideas and actors. It changes the conventional understanding of the origins of the Great Divergence and prompts us to look further back with more meticulous reflections on the matter.

Rössner begins by offering a critique on the way economists and economic historians maintain a very narrowed view on culture as a factor in explaining global divergence and economic growth. These "self-inflicted, narrow frameworks of presentism and short-term perspectives" have led such scholars to see the divergence as an anomaly of the 1750s. This vision, he argues, is inherently flawed in terms of the assumed causality – mistaking the how with the why. Instead, Rössner suggests that looking into how people interact with the economy as well as analysing the change in their perception of the cosmological order, along with it the economic knowledge and reasoning, are bound to provide a much more enriching and deeper understanding of the origins of the Great Divergence in economic thought.

The author takes the beginning of the sixteenth century as a focal point for the change of the European mindscape, which in essence permitted the ideas of modern capitalism to flourish. Since prior to 1600s the common perception of time was oriented to the past, the future was seen as dark and full of fearful uncertainty as well as predetermined by God. Within this vision, there was no room for agency. This changed around 1600, with the proliferation of newspapers and blank calendars. In this new vision, the future became open and manageable and with it the economic activities too. This is important, Rössner argues, as it takes us further back than Hume and Smith, suggesting that they just codified the ideas that had been around long before their time. It further questions the predominant perception of pan-European discourse and the need to view differences in economic thought and practices within various European lands. Finally, it brings up the necessity to examine the travelling of ideas as a two-sided (rather than one-sided) process between Europe and Asia.

The idea of *wu-wei*, which implies stability and non-interference, stemming from Eastern thought and having entered European ports around 1600, was diffused via commerce and processes of cultural transfer. Rössner refers to multiple Western thinkers as Sinophiles, who were to large extent influenced by Chinese economic thought. While the idea may have been similar, he demonstrates how its practice has differed not only across but also within the continents. This sheds light on the cognitive dissonance between the predominant idea of the "invisible hand" guiding the market with the reality of highly interventionist practices by many European authorities that in turn constructed the capitalistic system as we have come to know it today. The obvious rationale for this was the creation of strong states and militaries within the competitive environment. This did not happen in China, for example, due to the vastness of the empire, conviction to keep the non-Manchus out of the ruling positions, as well as their negative perception of manufacturing.

Drawing on the historically informed economic practices in India, China, Italy, Spain, England, and Germany, Rössner supports his claim of how economic ideas were implemented within their specific contexts and how various factors, beyond mere ideas, played a crucial role in economic policies and governance. Such an example is the abolition of the feudal system and the need to develop strong governments in the context of European fragmentation and this, as a result, may correlate with their greater economic growth in the future. To explore how Europeans have developed more interventionist economic state policies, Rössner examines the role of silver balance and the way it was handled by the early mercantilists. This journey begins in the early modern period, focusing on Germany as one of the largest exporters of silver and the consequences it had to face and bare as a result of misbalances. With the "Price Revolution" and the increased scarcity of the resources, German merchants seized the opportunity to make profits by draining silver out of the state, which in turn put pressure on the state to find a solution.

Facing this pressure, thinkers like Luther expressed their strong sentiments against foreign trade, blaming it for taking the wealth away from the nation. Consequently, this sentiment grew into the "fear of goods", referring to overpriced foreign imports such as

Italian clothing. This was a prelude to the continuous development of the interventionist economic state policies. Although at this point in time they were merely reacting to the troubling circumstances, in the future they would evolve into proactive state strategies. This example demonstrates, in the words of Hörnigk, a small divergence within Europe in the middle of sixteenth century, an economic divergence that is recorded much earlier than conventionally assumed.

Roy Bin Wong instead focuses on the current debates on the Great Divergence and the difficulties to formulate a convincing comparative strategy that transcends the simple opposition of two very different cases[47], namely the lack of an appropriate framework, which in his eyes ought to ensure falsifiable propositions. Neither measuring with a Eurocentric yardstick nor equating the British experience with the entirety of Western Europe is helpful in this context. As shown in his previous work, together with Rosenthal, there is, however, the possibility to make falsifiable claims, for example by comparing the organization of long-distance trade and examining how this has been handled by Chinese versus European institutions.

He highlights the main differences between China and Europe, whereby the former was a unitary state and the latter experienced political fragmentation. Such fragmentation entailed war, which, as a result, entailed the need for capital and labour in the cities and different, more urgent and necessary in these circumstances, demand for technology. Opposed to Pomeranz, he asserts that these are the origins of modern economic growth rather than modern world economy. In the case of Britain and textile production, if it were not for the cotton industries, the historical preconditions would not have set the stage for Western Europe to be the likely location for the Industrial Revolution to take off. He continues by exploring the cotton industry in Britain, recognizing that British relations with Asia and North America indeed matter in the foundation of modern world economy, again pointing to the need for the British to compete with cheap Indian labour and therefore create its rigorous mercantilist vision. He further uncovers the relation between the role of the state in economic exchange prior to and after industrialization. In doing so, he claims that mercantilist state policies yielded positive effects on economic growth, but that these were not intended consequences. Other parts of the world witnessing the beginning of the Industrial Revolution had a different outlook on desirable types of knowledge and ways of managing problems. In China, the agenda was not to compete but to expand output and counter inequalities that were the results of harvest fluctuations.

In terms of analysis, he stresses the need to distinguish between the Great Divergence being a specific phenomenon and a general economic transformation. This relates to the historically coinciding processes of state building and growth, which should be interpreted separately. Otherwise, we fall into a trap of evaluating why China did not become

47 On the problem of comparison with a very small number of cases see already S. Lieberson, Small n's and big conclusions. An examination of the reasoning in comparative studies based on a small number of cases, in: Social Forces 70 (1991) 2, pp. 307–320.

Britain. In his critique of Peer Vries' recent book, he underlines that focusing only on central revenues leads to misinterpretations of the institutional settings in China compared to England. The inner workings and rationales of the two states are not that simple to be compared. Central and local governments in the two respective states, together with their revenues and expenditures, do not follow the same structure of financial flow and distribution. The Chinese central government, due to the vastness of the empire, delegated a large fraction of its financial management to local authorities, which meant that a lot of the revenues never reached the centre. These local authorities, in contrast to the British, did not compete; nor did they have the same power hierarchies. Similar mistakes are made when assessing the welfare policies by acknowledging the funds for the poor in Britain, which were not provided by the central government but were sponsored by wealthy members of society in exchange for privileges and power, while rejecting the Qing granaries as a welfare state–oriented policy, which was also supported by local notables. Bin Wong also points to Vries' disregard for the importance of water control, as it was crucial for water paddy rice agriculture and commerce. Contrary to Vries, Bin Wong asserts that the scale and importance cannot be compared to that in Europe as well as the fact that these projects were funded. Finally, the lack of an informed statement about customary fees, which were also never collected by the central government but instead remained on the local level, further impairs the comparison between the two states.

All the above-mentioned factors are undeniably important but extremely difficult to compare. This makes Vries' conclusion in Bin Wong's eyes highly problematic since it systematically measures the Chinese case against what the British state has done. The article shows the outright difference between the comparative strategies used in the debate about the Great Divergence, the one measuring against the yardstick of the seemingly most successful case (Britain with its early entrance to the Industrial Revolution), while the other carefully reconstructing the different functioning of a society that follows its own historically grown logics. It becomes clear that the Great Divergence debate allows, so far, for two completely different framings to emerge, the one searching for the Great Divergence within one modernity and the other looking for the Great Divergence – and the many small ones – as part of a set of multiple modernities.

Finally, Eric Vanhaute locates the debate within the broader context of world history writing and praises the Great Divergence debate as not only the most important debate in recent global history but perhaps in social sciences at large. The intrinsic value of the debate is in its challenging nature, which fosters new approaches, data, and knowledge. These dynamics expand and break out of their own limits to continuously stimulate the (re)interpretations of global capitalism. He highlights the central points in the chronology of the debate, focusing on the three existing models and accompanying types of analytical frameworks. As an ardent proponent of the multilayer systems approach, Vanhaute advocates its utility. In doing so, he presents its core methodological assumptions and their analytical value in transcending narrow, reductionist research strategies towards a more holistic and inclusive framework that combines both top-down and bottom-up perspectives.

He begins by tracing the evolution of the debate with its key thinkers such as Marx, Weber, and Williams, who gave rise and propelled the idea of Western superiority, placing culture at the heart of this explanation. This vision assumed the development of the rationalization of thought to evolve into actions and institutions of the Western world, whereby industrialization was perceived as a natural outcome. This type of research remains within a Eurocentric framework and narrow in scope. He credits the California school of thought for sparking the debate and bringing the issue to the forefront of academia and popular discourse since the 1990s. By challenging the conventional explanations of the Western economic success, scholars present new evidence of striking similarities in development between China and the West until the early nineteenth century. This calls into question the basic assumption of Europe's gradual ascent starting from before 1800 as well as of the inevitability of the Industrial Revolution. Instead, the new interpretations demonstrate that the revolution was neither foreseen nor planned, but was rather a contingent process that had both exogenous and endogenous roots. These interpretations hold promise of providing an in-depth insight into the multi-layered historical dynamics at play, whereby it is the contacts with the outside world that had a crucial effect on multiple levels. Primarily, it is the capitalistic system that penetrated other parts of the globe that served the core, together with its production of an extensive knowledge systems via accumulation, which were then used to develop strong states and military presence.

Vanhaute proceeds with his reflections on methodology, charting and critically evaluating the three predominant types, namely reciprocal comparative analysis, whereby individual cases are examined but are taken at equal value; networks analysis or otherwise a translocal or transnational analysis, which examines the interconnectedness between societies and systems; and lastly systems analysis. Pointing to the limitations of the first approach – for instance in its choice of measures such as GDP, or dichotomous variables, or the risk of essentializing aspects of national history – Vanhaute builds a case for the systems approach. Not completely disregarding the networks approach, he portrays it as an improvement of the first one, a sort of a stepping stone towards the most recent development of thought on this matter.

Dedicating an extensive part of the work to the systems perspective, the author explains how this perspective "does not narrow down the lens to macro boundaries, it aims to understand how the different scales or frames of time and space within the system tie together, forming a multitude of 'worlds'". There "worlds" are not constant or absolute, but are dynamic, interactive, overlapping, existing, and (re)producing within the specifics of time and space. Vanhaute's systems approach entails moving away from the conventional uses of the term "globalization" as a tool to legitimize neoliberal "modernization", away from the reductionist terms, binary units, and short-sighted history. By deploying the notion of frontiers, Vanhaute demonstrates their importance in historical processes, not only as sites where changes occur as a single event but as a continuous, dialectic processes of homogenization and heterogenization.

In his epistemological reflections, the author pays particular attention to four points. Firstly, he advises us to rethink spatiality, that is to say to not take regions simply as given but rather as the product of the Great Divergence.[48] Secondly, he negates the idea of capitalism being an invention of eighteenth-century England, and rather that of a much longer history. Thirdly, Vanhaute addresses the need to explore the complexity of the debate, which should not be reduced to a few indicators measuring superiority or inferiority. Lastly, he reasserts the intellectually stimulating streams of thought that the debate has yielded and its wide implications for knowledge production in various fields where the debate's potential is not necessarily already recognized.

Global history, due to its stage of adolescence as a research field, has a lot to gain from struggling with the many facets of the Great Divergence debate. The endeavour to deconstruct histories from around the world in all their complexity and across spatial scales in a holistic, interdisciplinary manner will profit a lot from a debate that is more than a discussion among specialized economic historians.

48 Unfortunately, the contribution by Werner Scheltjens (Leipzig) on "Between the Delta and the Deep Blue Sea: Reconfiguring Divergence from a Maritime Perspective" to the workshop on the relationship between river deltas as gates between the open sea as the space for transportation and the territorialized hinterland as the space of production and consumption with the emergence of very specialized services was not available for publication in this issue but has clearly demonstrated that spatial categories such as regions, nations, and national economies are anachronistic when it comes to the early modern times. So far, the debate on spatial formats and the Great Divergence has not yet really started but there are many hints at the network character of some of the involved economies while others are rather territorialized.