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# **The Great Divergence Revisited**

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**Edited by  
Matthias Middell and Philipp Robinson Rössner**



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# 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.<sup>1</sup> 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,<sup>2</sup> others insist on the emergence of a fundamentally new quality emerging over the nineteenth century by using the term "global condition".<sup>3</sup> 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.<sup>4</sup> 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?<sup>5</sup> 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”.<sup>6</sup> 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,<sup>7</sup> 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.<sup>8</sup> 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.<sup>9</sup>

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.<sup>10</sup> Other authors have on the contrary focused upon factors such as the ways in which knowledge has been used, stored, and transferred.<sup>11</sup> 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.<sup>12</sup>

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.<sup>13</sup> 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<sup>14</sup> and multiple modernities,<sup>15</sup> such an openly Eurocentric story became a fundamental challenge.<sup>16</sup> 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.<sup>17</sup> 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<sup>18</sup> – 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,<sup>19</sup> 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.<sup>20</sup> 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,<sup>21</sup> 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<sup>22</sup> 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, 3<sup>rd</sup> 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, NJ 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.<sup>23</sup> 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,<sup>24</sup> the Great Divergence debate in general has not gone very much beyond the Eurasian complex.<sup>25</sup>

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 anthropometric 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.<sup>26</sup> 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”.<sup>27</sup> 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.<sup>28</sup> Others mention the rise of “bourgeois values and dignity”<sup>29</sup> and the eighteenth-century Enlightenment as a revolution in attitude towards more rational thinking about the economy and the market.<sup>30</sup> 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 pre-configured 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,<sup>31</sup> by using economists' linguo, which is not unproblematic.<sup>32</sup> 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).<sup>33</sup> 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”.<sup>34</sup> 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.<sup>35</sup> 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 economic 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,<sup>36</sup> “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. Philippen, *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.<sup>37</sup> 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,<sup>38</sup> scholars have now tended to move away from geo-deterministic approaches.<sup>39</sup> 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<sup>40</sup>) 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<sup>41</sup>? How did other regions contribute to the process we have come to know as the Great Divergence – for instance, the seventeenth- and eighteenth-century German states under the political economy paradigm of Cameralism<sup>42</sup>?

Also, as works by van Zanden<sup>43</sup> and others have shown, some of the European models have their roots in the Middle Ages.<sup>44</sup> 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.<sup>45</sup> 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<sup>46</sup> 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<sup>47</sup>, 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. Lieberman, 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.<sup>48</sup> 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.



# **Decline or Deindustrialization? Notes on the Entangled Histories of Levantine and European Industries in the Late Middle Ages**

**Georg Christ**

## **ABSTRACT**

Die Auffassung von einem Niedergang der spätmittelalterlichen Industrien in Ägypten und Syrien wurde kritisiert und es wurde vorgeschlagen, die beobachteten Entwicklungen als Transformation zu interpretieren. Dieser Beitrag schlägt einen Vergleich vor: Ägypten und Syrien deindustrialisierten sich als Teil einer zentralisierten, bürokratischen Wissensgesellschaft. Das kann mit guten Gründen als erfolgreiche Anpassung gewertet werden, doch die Levante verlor dabei ihre wirtschaftliche Vormachtstellung im industriellen Bereich. Die Juniorpartner in Europa, wohin die Produktion gleichsam ausgelagert wurde, übernahmen zunehmend die Führung. Diese Sichtweise maßt sich nicht an, die bestehenden Erklärungsansätze zu ersetzen, sondern will diese höchstens mit einem Beitrag zur Debatte ergänzen und dabei zum Nachdenken über unsere eigenen postindustriellen Gesellschaften einladen. Indem der Beitrag neben dem Europa-Ostasien-Vergleich auch auf den Vergleich zwischen Westeuropa und der islamisch geprägten Levante im späten Mittelalter eingeht, lässt sich neben relativ selbständigen Entwicklungspfaden vor allem auch die Interaktion und wechselseitige Prägung zwischen verschiedenen Wachstumsmodellen erkennen.

A focus on the so-called Great Divergence risks constructing the European economic and military “ascent” as an independent, *sui generis* and irreversible phenomenon grounded in specific European characteristics. In contrast, by looking at the “Small Divergence”, i.e. the diverging trajectory of economic and in particular industrial development between Western Europe and the Islamic Levant in the late medieval period, we might observe a different image of a mature, retiring, deindustrializing entity that is passing the

baton to a junior partner. This image does not highlight two discrete and separate paths but rather reveals tightly interlaced tracks of development and offers a slightly modified explanation for the “European ascent”.

### **Avant Propos: From Great to Small Divergence**

Several scholars have attempted to explain diverging paths of development between Europe, or the West, and the so-called Rest – the East, or China. Historians such as Kenneth Pomeranz<sup>1</sup> and Roy Bin Wong,<sup>2</sup> as well as social scientists such as Karl August Wittfogel or Charles Murray,<sup>3</sup> and economists have contributed to the debate.<sup>4</sup> Various economists and economic historians have worked on the related topic of the European / Western “miracle”, suggesting a variety of reasons for its development. Charles B. Blankart has highlighted geographically fostered structures of small statehood and federalism.<sup>5</sup> And Regina Grafe<sup>6</sup> (also controversially Niall Ferguson)<sup>7</sup> has emphasized the importance of inner-European competition (one of Ferguson’s “killer apps”) between economic actors, institutional solutions, and small states (cf. also Oliver Volckart’s studies).<sup>8</sup> Although European geography certainly shaped Europe’s history, this does not explain why and how it allowed Europe’s industrial growth to seemingly overtake that of its competitors during the relatively short period between 1350 (with regard to the Middle East) and 1500 or even 1750 (with regard to China and India) to roughly 1850–1900 when the United States started to challenge European hegemony.<sup>9</sup> This happened in the historical context of a very particular, dynamic stage of European unity that combined fragmentary tendencies marked by state formation, multicentric urban growth and competition with unifying elements such as Christianity, the Church, and the umbrella of the fading Roman Empire. These provided a pitched battleground and a common set

- 1 K. Pomeranz, *The Great Divergence: China, Europe and the Making of the Modern World Economy* The Princeton economic history of the Western world, Princeton (N.J.) 2000.
- 2 R. Bin Wong, *China Transformed: Historical Change and the Limits of European Experience* 5<sup>th</sup> print ed., Ithaca 2012; see also his contribution in this volume issue.
- 3 K. A. Wittfogel, *Die orientalische Despotie: eine vergleichende Untersuchung totaler Macht*, Frankfurt a. M. etc. 1977; Ch. Murray, *Human Accomplishment: The Pursuit of Excellence in the Arts and Sciences, 800 B.C. to 1950*, New York 2003.
- 4 They did so with beautiful but varying success, which is, however, not the concern of this essay.
- 5 Ch. B. Blankart, *Föderalismus in Deutschland und Europa* (Neue Studien zur Politischen Ökonomik 1), Baden-Baden 2007.
- 6 R. Grafe, “Was There a Market for Institutions in Early Modern European Trade,” in *Union in Separation: Diasporic Groups and Identities in the Eastern Mediterranean (1100–1800)*, ed. by G. Christ et al., Roma 2015, pp. 593–609, here mainly 595.
- 7 N. Ferguson, *Civilization: the West and the Rest*, London 2011.
- 8 O. Volckart, *No Utopia: Government Without Territorial Monopoly in Medieval Central Europe*, in: *JITE* 158 (2002) no. 2, pp. 325–343, who describes this complicated state convincingly although I do not agree with his conclusions.
- 9 Both in and outside Europe, there are different trajectories according to countries and industrial sectors. Germany’s industrialization, for instance, takes off a bit later perhaps, but it retains a leading position in some sectors for much longer – even today.

of rules, thereby connecting emerging polities within a wider, relatively integrated economic area, through common language, spiritual and legal reference points, and a set of shared, though hotly debated, values and traditions.

Yet, as important as these trans-European connections were, it was the trans-Mediterranean connection that brought Europe in contact with advanced industries also supplied them that superior products and eventually allowed the related knowledge to trickle into Europe. Prasannan Parthasarathi and Giorgio Riello's work entangles the history of cotton production between India and England in the (early) modern period and looks at transfers of technology, know-how, products, and hence market leadership.<sup>10</sup> Parthasarathi emphasized the role of both the environment and the states/governmental bodies in forcing economic change. However, much less research has been done on what one might call the entanglement and trans-Mediterranean connections that catalysed European industrial growth in the late Middle Ages. This "small" or trans-Mediterranean divergence between southern/central Western Europe and the Arabic-Islamic world in the later Middle Ages, however, is crucial for appreciating how European industries entered upon a path leading to a phase – although short-lived and transient – of industrial world leadership in the nineteenth and twentieth centuries.

## Introduction

Although Claude Cahen has argued that the small or Mediterranean divergence was a problem of European ascent rather than Oriental decline, the chronicles do not suggest this reading.<sup>11</sup> Therefore, I will be seeking to contribute not to a history of diverging paths but to a history of entanglement and redistribution which relied on negotiating and rearranging economic resources and roles. This may also, I hope, form a small contribution to a history of economic macro cycles in industrialization and deindustrialization. This study considers how decline and ascent can be entangled and, how the industrial baton was passed from one region to the other.

Economic historians, most notably Eliyahu Ashtor, matching contemporary Mamluk narrative accounts with data from European archives identified an economic but also political decline of the Arabic-Islamic Mediterranean (and indeed the data on demographic decline and the decline of arable land seems extensive enough). More recently, historians have focused on the rather successful adaption of the Mamluk political elite to the new circumstances and insisted on ascertaining a successful transformation and

10 P. Parthasarathi, *Why Europe grew rich and Asia did not: global economic divergence, 1600–1850* (Cambridge 2011), and see also his contribution in this volume; Giorgio Riello and Prasannan Parthasarathi (eds.), *The Spinning World: A Global History of Cotton Textiles, 1200–1850* (Pasold studies in textile history 16), Oxford 2009.

11 C. Cahen, *Quelques mots sur le déclin commercial du monde musulman à la fin du Moyen Âge*, in: *Studies in the Economic History of the Middle East from the Rise of Islam to the Present Day*, ed. by M. A. Cook, Oxford 1970, pp. 359–366; see below note 18 on Shatzmiller's critic of a "misconstrued link."

even successful proto-modern state formation that is seemingly in contradiction with the decline hypothesis.<sup>12</sup>

I do not seek to tackle this problem holistically. It will have to be relegated to another time to argue that the transformation of Mamluk rule was successful insofar as it kept the Mamluks in power. This transformation, however, did not address any of the underlying structural problems triggering the decline and merely helped Egypt's economy to a comfortable retirement. Nevertheless, I would suggest, that the two concepts of transformation and decline are complementary rather than mutually exclusive. Taken together they describe quite accurately a particular state of economic development – a kind of post-industrial age.

Indeed, Western Europe, in this case Venice, did benefit from this decline. Thanks to the strong entanglement between the Mamluk and Southern European economies and the eagerness of Western actors to comply with the needs of the Levantine economies, they were entrusted with the outsourced industrial production. The European “miracle” thus ought to be comprehended as a convergence of *sui generis* European factors and the successful appropriation of the cultural and economic resources of the Levant. First, I will revise some of the main positions regarding this decline and contrast these positions with some of the arguments put forward against them. I will then analyse Veneto-Mamluk trade patterns to highlight elements of this entanglement by focusing on rugs and other fabrics that were traded between Venice and the Mamluk Empire in the early fifteenth century.

### The Decline Hypothesis and its Critics

Before we can set out to explore this hypothesis more closely, one word on the demographic and climatological developments with which Egypt had to grapple may be in order. In the fourteenth century, Egypt was hit hard by the onslaught of plague, which aggravated a situation of slow and rather disadvantageous climate change that had decreased and continued to decrease the resilience of Egyptian irrigation agriculture.<sup>13</sup> Climate change had impacted on the amplitude of Nile flood minima and maxima, with an altered wind regime placing increasing stress on farmland. Subsequent outbreaks of the plague and other epidemics from 1347 onwards reduced the Egyptian population by about a half.<sup>14</sup> Stuart Borsch has demonstrated how the agricultural yield declined sharply.<sup>15</sup>

Within this context of demographic and agricultural decline combined with political turmoil, historians in the 1970s and 1980s, with an interest in economic history, no-

12 See below for references and a more detailed discussion of these positions.

13 G. Christ, *King of the Two Seas: Was There a Mamluk Maritime Policy in the Late 14<sup>th</sup> Century?* Ulrich Haarmann Memorial Lecture, Berlin (forthcoming).

14 S. J. Borsch, *The Black Death in Egypt and England: a Comparative Study*, Austin 2005, p. 15.

15 Borsch, *The Black Death*, p. 54.

ticed an industrial decline. Claude Cahen, in his seminal 1970 essay on the “commercial decline,” argues that there were two reasons why the Islamic world fell behind Europe: it had abandoned Mediterranean navigation and embraced a speculative and consumption-driven economic policy / culture. He opines that the lack natural resources (mining) contributed only in a minor way to this situation. He also rejects the argument that the Islamic mentality contributed to the decline.<sup>16</sup>

In comparative studies based on Levantine sources and extensive research in European archives, Eliyahu Ashtor identifies an industrial decline in the Islamic Middle East. Based on an extensive source base, he argues that Levantine industries fell behind Western industries, which started producing the same products but did it more effectively. He also argues that Mamluk industries were hampered by governmental interference, heavy taxation, and difficulties in harnessing water power, and he convincingly details these developments for various industries such as sugar, paper, glass, soap, and textile.<sup>17</sup>

Maya Shatzmiller challenges the decline hypothesis and more explicitly the link to European ascent, which she calls a “misconstrued link.” While I would agree with the latter, I am not sure about the former.<sup>18</sup> The arguments she musters do not conclusively falsify the decline hypothesis – and were actually put forward in similar forms by the scholars she critiques. She points, for instance, at the fact that imports from Western Europe went hand in hand with exports to Western Europe, which was not denied by either Ashtor or Cahen. Ashtor actually underlined repeatedly that there was a relative change, i.e. in the ratios of import versus export of industrial products.<sup>19</sup> She also rejects the notion that the Islamic states were hostile to trade or that Islamic law might restricted the Islamic world in fact. Cahen had actually argued that favouring of the speculative practices, which would include trade, over production was a problem and he sharply rejected the notion that Islam or Islamic law had anything to do with the decline.<sup>20</sup>

In line with a generally critical view of Ashtor and other scholars of that generation, Francisco Apellániz has suggested a rethink of the economic developments of the late Mamluk Empire as an overall successful transformation from a capitalistic economy dominated by grand merchants to a state-capitalistic economy that was more directed.

16 C. Cahen, *Quelques mots*, pp. 359–366.

17 E. Ashtor, *Levant Trade in the Later Middle Ages*, Princeton (N.J.) 1983, pp. 200–216; id., *A Social and Economic History of the Near East in the Middle Ages*, Berkeley / Los Angeles / London 1976, pp. 305–309; id., *The Economic Decline of the Middle East during the Later Middle Ages – An Outline*, in: *Asian and African Studies: Journal of the Israel Oriental Society* 15 (1981), 253–286. See also a good discussion of his research in Y. Lev, Eliyahu Ashtor (1914–1918) and the History of the Mamlūk Sultanate, in: *Egypt and Syria in the Fatimid, Ayyubid and Mamluk Eras VII. Proceedings of the 16<sup>th</sup>, 17<sup>th</sup> and 18<sup>th</sup> International Colloquium Organized at Ghent University in May 2007, 2008 and 2009*, ed. by U. Vermeulen, et al., (*Orientalia Lovaniensia Analecta* 223), Leuven 2013, pp. 469–494.

18 M. Shatzmiller, *A Misconstrued Link: Europe and the Economic History of Islamic Trade*, in: *Relazioni economiche tra Europa e mondo islamico, secc. XIII–XVIII*, ed. by S. Cavaciocchi, atti della “trentottesima settimana di studi” 1-5 maggio 2006, Fondazione Istituto Internazionale di Storia Economica “F. Datini,” Prato: Serie 2, Atti delle “settimane di studi” e altri convegni (38), Firenze 2007, pp. 237–415.

19 Ashtor, *Economic Decline*, *passim*.

20 Cahen, *Quelques mots*, para 4: “Ni la religion, ni le Droit de l’Islam n’y sont pour rien.” The duplex negatio might be ambiguous at first but it can only really mean: “Neither religion nor law of Islam have anything to do with it.”

However, he does not engage with the specific arguments relating to industrial decline.<sup>21</sup> Similarly, Jo van Steenbergen, rethinking the political history of the Mamluk Sultanate, forms a less pessimistic view of Mamluk decline and suggests that the Mamluk system successfully developed in the fourteenth and fifteenth centuries towards an early modern state, again without specifically addressing the question of industrial decline.<sup>22</sup>

The notion of a decline of industries (or some might prefer the term “manufacture”) might be that controversial because it is connected to a notion of economic or even general decline in the Islamic world. A decline in intellectual vigour of, say, legal reasoning and Hadith science was noted by nineteenth century Orientalists such as Ignác Goldziher. Although scholars seem to be uncomfortable with this hypothesis, I am unaware of a convincing refutation of Goldziher’s core argument.<sup>23</sup>

The possibility of a change in the practice of Islamic law, i.e. in legal reasoning, would have far-reaching consequences as it would unhinge the cardinal point of contention in the debate over the role of Islamic law, which has taken an essentialist turn in the wake of the controversial hypothesis by Timur Kuran. Kuran suggests that Islamic law, in particular inheritance law, formed a restriction on the economic development of the Islamic world. According to him, the rigidity of Islamic inheritance law (*per se*) seriously jeopardized the growth of sizeable companies that could have competed with European firms, which was in turn responsible for the Islamic world falling behind Europe.

The refutation of Kuran’s work by Shatzmiller sets out to prove (based on discrete historical evidence but in a similarly apodictic, essential vein) that Islamic inheritance law was not seriously impeding investment and hence economic growth in fifteenth-century Granada (which is certainly convincing: as it also did not in the Umayyad and Abbasid period or in modern Lebanon).<sup>24</sup> Abdul Azim Islahi points out that Islamic inheritance law in practice could even be advantageous for capital accumulation by protecting estates, bringing it closer to hybrid (Southern) European models of estate management, e.g. the Italian *fraterna*. This was an estate of limited liability for debts incurred by the single heirs or stakeholders and jointly administered between them. It was a common arrangement in the period, e.g. in late medieval Venice.<sup>25</sup> It also remained dominant in the laws of feudal succession until the nineteenth century (giving rise to the famous *Kleinstaaterei* in Germany and regulating the various princes’ share in royal sovereignty

21 F. J. Apellániz Ruiz de Galarreta, *Pouvoir et finance en Méditerranée pré-moderne: Le deuxième état Mamelouk et le commerce des épices (1389–1517)*, Barcelone 2009.

22 K. D’hulster, J. van Steenbergen, and P. Wing, *The Mamlukisation of the Mamluk Sultanate? State Formation and the History of Fifteenth Century Egypt and Syria*, in: *History Compass* (2016, forthcoming); J. van Steenbergen, *Order out of chaos: patronage, conflict, and Mamluk socio-political culture, 1341–1382* (The medieval Mediterranean 65), Boston (MA) 2006.

23 I. Goldziher, *Muhammedanische Studien*, Halle a. S 1889, 2. Teil, pp. 267 seqq.; cf. W. A. Graham, *Traditionalism in Islam: An Essay in Interpretation*, in: *Journal of Interdisciplinary History* 23 3 (1993) 3, pp. 95–522.

24 M. Shatzmiller, *Economic Performance and Economic Growth in the Early Islamic World*, in: *JESHO* 54 (2011), pp. 132–184.

25 M. Weber, *The history of commercial partnerships in the Middle Ages*, ed. by Lutz Kaelber, Lanham (Md) 2003, III.8, Original: id., *Zur Geschichte der Handelsgesellschaften im Mittelalter nach südeuropäischen Quellen*, Stuttgart 1889.

in France). Kuran's overdrawn conclusion that Islamic law jeopardized early modern "Islamic" growth thus needs revision. Nevertheless, the question of what hindered pre-modern economic growth in the so-called Islamic world remains unanswered. Contrary to what Kuran's critics tend to imply, he certainly knows his sources – e.g., on the urban economy of Istanbul – very well and his findings on economic stagnation and decline in this context are convincing; however problematic his conclusions, he is actually trying to resolve a "real" historical problem.<sup>26</sup>

Goldziher's findings on the mutability of Islamic law provide a clue for the riddle. Albeit unchanging in essence and in its core texts, legal reasoning did change significantly and with it the interpretation and application of the law, which was therefore flexible from place to place and over time. Interestingly, in his highly critical review of Kuran's 2010 monograph Abdul Azim Islahi emphasizes precisely that point. He refutes what he perceives as an attack on Islamic law (i.e. its foundations) by identifying phenomena of decline (with regard to legal reasoning) in Islamic law in that period of "decline".<sup>27</sup> Hence, Islamic law was not the problem but perhaps the problem was, at least partially, how Islamic law dealt with dilemmata arising from new challenges in this period and in certain places.<sup>28</sup>

### Deindustrialization: Egypt's Move towards a Post-Industrial Economy

I suggest that it was not so much industrial decline as it was deindustrialization that befell the Mamluk Empire – that is, an economic transformation to a knowledge-based service economy with high public expenditure and a relatively high level of central state interference. A system dominated by lawyers and specialist bureaucrats. Egypt was a highly sophisticated, mature, "optimized"<sup>29</sup> economy that reacted in a very different way to the challenges of the fourteenth century than did parts of Europe, e.g. England.<sup>30</sup> Levantine industries were technologically very advanced and characterized by a high degree of division of labour. It was, in other words, an optimized, but as a result also highly sensitive, system that was strongly affected by external shocks such as the plague.<sup>31</sup>

26 T. Kuran, *Mahkeme kayıtları ışığında 17. yüzyıl İstanbul unda sosyo-ekonomik yaşam/yayına hazırlayan Timur Kuran = Social and economic life in seventeenth-century Istanbul: glimpses from court records Tarih/history/ Genel yayın 2021–2022; 2104–2105, İstanbul 2010.*

27 A. A. Islahi, Book review: The long divergence: how Islamic law held back the Middle East by Timur Kuran, in: MPRA Paper No. 42146, October 2012, [https://mpra.ub.uni-muenchen.de/42146/1/MPRA\\_paper\\_42146.pdf](https://mpra.ub.uni-muenchen.de/42146/1/MPRA_paper_42146.pdf)

28 Goldziher, *Muhammedanische Studien*, 2. Teil, pp. 267 seqq.

29 Optimized in terms of a high degree of division of labour, urbanization (i.e. clustering of productive resources) and highly effective forms of resource allocation, not least in terms of water in the irrigation agriculture.

30 Cf. J. A. Tainter, *The Collapse of Complex Societies* New studies in archaeology, [11<sup>th</sup> repr.] ed., Cambridge 2004).

31 For the sophistication of Levantine industries at the time, see, for instance, Ashtor, E. Ashtor, *Levantine Sugar Industry in the Later Middle Ages – An Example of Technological Decline*, in: *Israel Oriental Studies* 7 (1977), pp. 226–280, here p. 245; M. Lombard, *Études d'économie médiévale*. Vol. 3.: *Les textiles dans le monde musulman du 7<sup>e</sup> au 12<sup>e</sup> siècle* (EHESS, Centre de recherches historiques. Civilisations et sociétés 61), Paris 1978. It can be somewhat compared to the state of Western finance at the brink of the 2008 crisis: a highly sophisticated, over-

Irrigation agriculture had expanded by almost 50 per cent in the first phase of Mamluk rule, which made the system both highly sophisticated and effective under optimal conditions but also extremely vulnerable.<sup>32</sup> Public expenditure was comparatively high, which again reduced resilience.<sup>33</sup> The Mamluk elite was not a landed gentry but an allogenic meritocratic class of wage earners and public servants, who collectively controlled the agricultural resources without owning them. Stuart Borsch has argued that this considerably reduced the resilience of the Egyptian economy and especially agriculture compared to England with its considerably lower (central) public expenditure and property rights delegated to a relatively broad class of landed nobility and gentry. It should be added though that in Egypt – in the wake of the climate changing since around the year 1000, which put additional strain on irrigation agriculture – such delegation would not have allowed for the further expansion of the system, i.e. the continuous optimization of water usage, which required centrally coordinated and well-financed canal projects. In order to maintain a trajectory of growth, central coordination was needed and in the wake of the partial breakdown of the system, devolution of property (or instead fiscal) rights took place (as will be discussed below).<sup>34</sup>

Therefore, the effect of the outbreak of the plague in the mid-fourteenth century and that of subsequent waves of epidemics seemed to have been that this highly sophisticated and as it were over-optimized agricultural system, which had operated at the very limits of its capacity, was severely hampered by the demographic decline. This occurred not so much because of a decline in demand for agricultural products but because the labour for the upkeep of the irrigation systems was wanting and partly collapsed. Wide stretches of agricultural land were no longer irrigated, they had ceased to be controlled by the Mamluk system and were taken over by Bedouins as pasture land. The remaining population in these collapsing areas moved to the cities, mainly Cairo. This influx of rural population, however, did not help to mitigate the effect of the plague in Cairo with regard to industries. Levantine industries seemed to have been strongly concentrated in main cities and were particularly hard hit by losing roughly half of their workforce. It seems that contrary to European rural immigrants coming, for instance, to Venice, the migrants arriving in Cairo did not have transferable skills which would allow them to become craftsmen.<sup>35</sup> Also, while such skills in home spinning and weaving might have been suitable for the nascent European industries of cheap cloth, the production of high-quality silk products required more specialized skills. Another aggravating factor might have been

optimized system with no redundancies that could be profoundly shaken by a comparatively minor shock, cf. N. Taleb, *The Fourth Quadrant: A Map of the Limits of Statistics*, in: *Edge* (15 September 2008), Phronetic rules.

32 S. J. Borsch, *Environment and Population: The Collapse of Large Irrigation Systems Reconsidered*, in: *Comparative Studies in Society & History* 46 (2004) 3, pp. 451–468; cf. Borsch, *Black Death*, p. 15 seq., 39; Eliyahu Ashtor, *A Social and Economic History of the Near East in the Middle Ages*, Berkeley/Los Angeles/London 1976), p. 316 seqq.

33 H. Rabie, *The Financial System of Egypt A.H. 564–741/A.D. 1161–1341*, London 1972, p. 43 seq., 149.

34 Borsch, *Black Death*, p. 19, 54 and passim.

35 F. Ch. Lane, *Venice*, p. 170; I. M. Lapidus, *Muslim Cities in the Later Middle Ages*, Cambridge (Mass) 1984, p. 83.



that the textile industries were concentrated in coastal cities such as Alexandria, which did not attract as many immigrants as Cairo.

Although some immigrants had scholarly credentials (such as, for instance, the famous Egyptian bureaucrat-scholar Qalqashandî),<sup>36</sup> the rest had only agricultural skills. While the scholars could pursue careers in one of the many institutions of higher learning and the expanding administration, the farmer's set of skills were more difficult to market in the city. Some of the hardy former farm hands might have found employment using their transferable skills as porters, donkey drivers, and similar lowly professions. Others – still considered lowly by the *‘ulamâ* but not entirely unattractive – might have found jobs as hunters, dog handlers, or footmen in the service of an emir. Some others might not have had this chance and joined the ranks of the beggars/*h'arâfîsh*.<sup>37</sup> In view of these options, the farmer-migrant might have found that it was neither easy nor particularly desirable or rewarding to learn the necessary and by no means trivial skills needed for industrial production and then to join more or less exclusive guilds (the highly skilled silk weavers or goldsmiths were considered less desirable crafts). In any case, the costs for skilled industrial labour seemed to have risen and the industrial workforce was apparently not only never restored but continued to decrease over the course of the fifteenth century.<sup>38</sup> The Mamluk chronicles report on the increasing problems afflicting the Levantine economy. In addition to the irregular Nile floods causing famines, the Mamluk lawyer and chronicler Maqrîzî highlights another factor aggravating the situation, thereby turning crisis into catastrophe; rapacious taxations and corrupt public servants that ruin agriculture and industries.<sup>39</sup> This is also echoed by the famous North African thinker Ibn Khaldûn:

*Eventually, the taxes will weigh heavily upon the subjects and overburden them. [...] The result is that the interest of the subjects in cultural enterprises disappears, since when they compare expenditures and taxes with their income and gain and see the little profit they make, they lose all hope [dhahâb al-âmâl]. Therefore, many of them refrain from all cultural activity [lit. civilisation/culture (also, in modern usage: national wealth) declines/diminishes: iantaqis'a 'l-‘umrân]. [...] People scatter everywhere in search of sustenance, to places outside the jurisdiction of their present government.<sup>40</sup>*

36 C. E. Bosworth, al-Qalqashandî, in: Encyclopaedia of Islam, ed. by P. J. Bearman, Th. Bianquis, and C. E. Bosworth, E. van Donzel, W.P. Heinrichs, 2<sup>nd</sup> ed., Leiden, pp. 509a–510b.

37 On the later term *h'arâfîsh*: W. M. Brinner, The Significance of the "H'arâfîsh" and their "sultan, in: JESHO 6 (1963), pp. 190–215; Lapidus, Muslim Cities, p. 84; id., The Grain Economy of Mamluk Egypt, in: JESHO 12 (1969) I, pp. 1–15, here 12 seq.; B. Shoshan, Popular culture in Medieval Cairo, Repr ed., Cambridge etc. 1996, pp. 62 seqq.

38 The wage data is debated: Borsch, Black Death, pp. 103–108, an updated set in id., Subsisting or Succumbing? Falling Wages in the Era of Plague, in: ASK Working Paper 13 (2014) indicates a short term rise but then long-term decline in wages for custodians, doorkeepers, water-carriers and readers which would be expected as the workforce for these professions in Cairo was boosted by rural migrants, cf. M. Shatzmiller, Economic Performance and Economic Growth in the Early Islamic World, in: JESHO 54 (2011), pp. 132–184, here p. 143 pointing at A. Sabra, Poverty and Charity in Medieval Islam: Mamluk Egypt 1250–1517, Cambridge 2000, pp. 121–123.

39 A. Allouche (ed. and transl.), Mamluk economics: A study and translation of al-Maqrîzî's "Ighâthah", Salt Lake City 1994.

40 Ibn Khaldun, Muqaddima translated by F. Rosenthal, vol. 2, pp. 90 seq., 92, 104 (possibly inspired by Hesiod);

One might add to this apt analysis that people also joined the government and its expanding bureaucracy and intricate system of clientele or the government-sponsored academic knowledge economy. Ibn Khaldūn, of course, did so too and thus understandably did not emphasize this delicate matter.

Yet how could it be possible that an economy would allow for such vast bureaucracy and academic pursuit in times of demographic and agricultural decline? In order to understand this aspect of the Mamluk shift towards a post-industrial society, we must consider the Egyptian system of a powerful tax-raising state ruled by an allogenic elite that did not own the land that fed them. This elite could thus in theory not bequeath their positions of power to their offspring. Although this was not always quite true, there was a tendency among Mamluk elite to incorporate their offspring, the *awlād an-nās*, into the civilian, *‘ulamā* elite, which was strongly dominated by lawyers. Inter-marriage with local notables and training as legal scholars were successful strategies for the transmission-cum-transformation of status from Mamluks to their sons. They often tried to buttress this transmission of elite status by providing their offspring with material resources. They successfully withdrew wealth (including state land attributed to them as a fief ([*iqṭāʿ*], thereby turning it into *milk* [private property]) from the fiscal base of the state and additionally protected it by turning it into pious foundations. This was an effective way of transmitting wealth to the next generation while protecting it against the fiscal authorities and the looming threat of reappropriation by the state/emire.<sup>41</sup> It also hedged against offspring squandering their legacy. The “waqfized” assets financed pious institutions that typically included institutions of higher learning with training in Islamic law as the core curriculum.<sup>42</sup> As an important side effect, it prevented the splitting of the estate among the heirs, as stipulated by Islamic law, while creating positions and thus rents for subsequent generations of the family.<sup>43</sup> Konrad Hirschler gives a vivid description of the ensuing sophisticated knowledge economy providing jobs for the civil elite and the literate middle class.<sup>44</sup> This system not only enabled wealthy families to transmit elite status from generation to generation through higher education,<sup>45</sup> but it also created opportunities for meritocratic newcomers, thus adding a degree of social

Arabic: ‘Abd ar-Rahmān Ibn Khaldūn, Muqaddimah Ibn Khaldūn, Beirut 1996, 263. *‘Umrān* is a key concept in Ibn Khaldūn’s thought. The Muqaddima (lit. introduction preceding but also concluding the historical study *kitāb al-cibār*) is really the science of *cumrān basharī*, of human civilization, roughly divided into nomadic (*cumrān badawī*) and urban civilization (*cumrān hād’arī*). It is an affirmative substantivation (of the form typical for adjectives) of the root c-m-r, meaning to live long, to be cultivated, as well as to build.

41 I. Daisuke, The Establishment and Development of al-Dīwān al-Mufrad: Its Background and Implications, in: Mamlūk Studies Review X (2006), pp. 117-140, p. 121; id., Land Tenure, Fiscal Policy and Imperial Policy in Medieval Syro-Egypt, Exeter 2015, 177 seqq., 184 seqq.

42 M. Chamberlain/D. Morgan, Knowledge and Social Practice in Medieval Damascus, 1190–1350 (Cambridge 1994), p. 15.

43 Although in practice cooperation with Mamluks as actual power projectors was crucial for the smooth running of a waqf, Daisuke, Land Tenure, 183, 196 seqq.,

44 K. Hirschler, The written word in the Medieval Arabic lands. A social and cultural history of reading practices, Edinburgh 2012).

45 Chamberlain, pp. 4–7.

mobility to the system.<sup>46</sup> Overall, it might have aggravated the above-mentioned increase in post-plague labour costs.<sup>47</sup>

The lawyer/ Islamic scholar became the dominant figure of the upper-middle and non-Mamluk upper class (*ʿayān*) besides the merchant (and often they were combined in the same person). Due to its close links to the Mamluk establishment (*al-khāṣṣa*), Mamluk urban and imperial culture was accordingly, and arguably, permeated by legalism. Mamluk chronicles commenting on the autocratic rapacity of Mamluk rulers were written by lawyers who were actually quite well represented in the Mamluk system of power. That is why they were interested in power and that is why they dared to write such texts, which are as much indicative of rapacity as of a free “press”/ public opinion that was able to criticize it.

This might have had different effects with regards to deindustrialization. It might have fostered an over-sophistication of legal culture with legal scholars, who abstained from greater innovation and instead annotated text with glosses and the glosses with post-glosses, compiled thematic collections, etc.<sup>48</sup> The strategic alliance between (military) rulers/ power brokers and lawyers/ *ulama*<sup>49</sup> might also have fostered bureaucratic growth, clientele network-driven lobbyism,<sup>50</sup> careerism,<sup>51</sup> heavy taxation, as well as meritocratic permeability, increased importance (and marketing) of higher education,<sup>52</sup> and centralization, or might not.

## Outsourced Industries and Niche Production

The Levantine economy had to adapt to this situation. Adaption was made easier by the massive influx of specie and consumer goods through the international spice trade between India and Europe for which Egypt was an almost obligatory *passage obligé* in the late Middle Ages. The balance of payment surplus allowed Egypt to leave pricy industrial production of cheaper goods for mass consumption to their European partners through the mediation by Venice, its main trading partner.

Nascent industries in Europe thus had a chance to expand. The plague had produced a very significant change within Europe. Some (by far not all) regions and particularly cities of Western Europe were able to rapidly overcome the external demographic shock

46 I. M. Lapidus, *Muslim Cities in the Later Middle Ages*, Cambridge, Mass 1984, pp. 187 seq.

47 M. Shatzmiller, “Economic Performance”, p. 143; cf. A. Sabra, *Poverty and Charity in Medieval Islam: Mamluk Egypt 1250–1517*, Cambridge 2001.

48 I. Goldziher, *Muhammedanische Studien*, Halle a. S. Niemeyer, 1889.

49 Cf. I. M. Lapidus, *Muslim Cities in the Later Middle Ages* (MIT, Joint Center for Urban Studies), Cambridge, Mass. 1984, p. 141 seq..

50 Cf. Winslow Williams Clifford, *State Formation and the Structure of Politics in Mamluk Syro-Egypt, 648–741 A.H./1250–1340 C.E.*, ed. by Stephan Conermann, Göttingen/Bonn 2013, p. 97 seq.

51 *Ibid.*, pp. 225 seqq.

52 Cf. K. Hirschler, *The Written Word in the Medieval Arabic lands. A Social and Cultural History of Reading Practice*, Edinburgh 2012.

of the plague thanks to strong immigration from the countryside. In some cases, this even led to a net growth of cities in the second half of the fourteenth and in the fifteenth century, thereby mitigating the rise of labour costs. Furthermore, in Europe it was easier to substitute human labour with animal power (e.g., from horses, thanks to the invention of the horse collar) or water power, thanks to the availability of fast-flowing water, which, for instance, revolutionized the English cloth industry. With regard to fuel (crucial for glass as well as soap production), Europe had another competitive edge over the Levant.<sup>53</sup>

Industrial power increasingly moved from Southern to Northern Europe. This is at the very heart of the Lopez / Miskimin vs. Cipolla debate over a depression of the late Middle Ages or, in their words, of the Renaissance. The more sophisticated a socioeconomic-political system was, the more it seemed to have suffered. Sicily or Tuscany, on which Lopez and Miskimin focused, were hit hard by the plague. Cipolla's refutation looks at other parts of Europe that took a different trajectory – e.g., England – which were not as “optimized” and thus more resilient than Tuscany or the Middle East. For England or other less “sophisticated” nascent economic areas of Central and Northern Europe – e.g., the textile industries of upper Germany,<sup>54</sup> England,<sup>55</sup> and the Veneto<sup>56</sup> – this was a vital chance. These areas could draw on a rural population surplus with transferable artisanal skills that could replenish, or rather replace, urban crafts diminished by the plague. Thus they triggered a new economic growth and nascent industrialization. These industries could, furthermore, draw on more readily available water power. Labour costs increased, to be sure, but from a very low level and relatively slowly.<sup>57</sup> This potential for growth combined with an increased demand – not least from the Levant – were good conditions for the expansion of European industries.<sup>58</sup>

As a result, the patterns of trade changed over the late Middle Ages. Increasingly, industrial products were imported from Western Europe while raw material, including spices,

53 Ashtor, *Levant Trade*, pp. 200–216; E. M. Carus-Wilson, *The Woolen Industry* (chap. IX), in: *The Cambridge Economic History of Europe*, vol. II, ed. by E. Miller, C. Postan, and M. M. Postan, 2<sup>nd</sup> ed., Cambridge 1987, pp. 613–90, here 669 seq.

54 J. H. Munro, “South German Silver, European Textiles, and Venetian Trade with the Levant and Ottoman Empire, c. 1370 to c. 1720: A non-Mercantilist Approach to the Balance of Payments Problem, in: *Relazioni economiche tra Europa e mondo islamico, secc. XIII–XVIII*, ed. by S. Cavaciocchi, atti della “trentottesima settimana di studi” 1–5 maggio 2006, Fondazione Istituto Internazionale di Storia Economica “F. Datini”, Prato: Serie 2, Atti delle “settimane di studi” e altri convegni (38), Firenze 2007, pp. 907–926, here 914 seq.

55 In fact, here we notice a similar shift from the old cloth-producing centres of Oxford or London towards the North, e.g. the Yorkshire dales, Carus-Wilson, *he Woolen Industry*, pp. 673–682; cf. Borsch, *Black Death*, p. 63..

56 For the history of the industries of the Veneto, see P. Lanaro, *At the Center of the Old World: Trade and Manufacturing in Venice and the Venetian Mainland (1400–1800)*, Centre for Reformation and Renaissance Studies, Toronto 2006; for the silk industry: L. Molà, *The silk industry of Renaissance Venice*, Baltimore 2000, see also B. Lambert, and K. Anne Wilson (eds.), *Europe's Rich Fabric: The Consumption, Commercialisation, and Production of Luxury Textiles in Italy, the Low Countries and Neighbouring Territories (Fourteenth–Sixteenth Centuries)* Abingdon 2015.

57 For England: Borsch, *Black Death*, pp. 104–108, cf. J. E. T. Rogers, *On a Continuous Price of Wheat for 105 Years, from 1380 to 1484*, in: *Journal of the Statistical Society of London* 27 (1864) 1, pp. 70–81, here 72 seq.

58 Cf. also B. J. P. van Bavel and J. Luiten van Zanden, *The Jump-Start of the Holland Economy during the Late Medieval Crisis, c. 1350–c. 1500*, in: *The Economic History Review* 57 (2004) 3, pp. 503–532.

either locally sourced (such as cotton or alkali) or provided through transit trade (spices), were exported. The Mamluk chronicles are fairly outspoken about this: According to Ibn 'Imad al-H'anbalî there were, by the end of the fourteenth century, 14,000 looms in Alexandria still. By 1434, there were only 800 left.<sup>59</sup>

One might first be tempted to identify patterns of colonial trade (finished goods from the motherland vs. raw materials from colonies). Yet this is not the case; such conversion never fully occurred. The table below illustrates that a number of (industrial) products were traded in both directions and that industrial production continued.<sup>60</sup> Levantine industries reinvented themselves on a different (smaller) scale, probably in particular niches, where they could still compete.

This is also suggested by the more nuanced account of the Veneto-Cretan Emmanuele Piloti who described the decline of the Alexandria textile industry in the early fifteenth century. He contrasts the declined state with an even higher, fantastic number of looms in the past (80,000) but he also mentions that some production of high-quality textiles continued on a lower level with exports to the court in Cairo but also to North Africa, Syria, and Turkey.<sup>61</sup>

The papers of the merchant and consul Biagio Dolfin, preserved in the State Archive of Venice, allow us to further appreciate these trade patterns.<sup>62</sup> The table above is based on tax declarations and cargo lists preserved among his papers and supplemented by information drawn from other primary or secondary sources.<sup>63</sup> It provides us with a snapshot tableau of a transitional point in the process transforming the Oriental economy towards a post-industrial economy around 1419.

59 M. Chapoutot-Remadi, *Réflexions sur l'industrie textile dans le monde musulman au Moyen Âge. À propos d'un livre récent*, in: *Annales. Économies Sociétés Civilisations* 35 (1980) 3-4, pp. 504-511, here 506; Subhi Yanni Labîb, "Iskandariyya," in *Encyclopaedia of Islam*, ed. by P. J. Bearman, Th. Bianquis, C. Edmund Bosworth, E. van Donzel, W. P. Heinrichs, 2<sup>nd</sup> ed., Leiden, pp. 132a, here 135a; cf. E. Ashtor, *Levantine Sugar Industry*, 263 seq.

60 This is also confirmed for the time around 1500, B. Arbel, *The Last Decades of Venice's Trade with the Mamluks: Importations into Egypt and Syria*, in: *Mamlûk Studies Review* VIII (2004) 2; cf. E. Ashtor, *A Social and Economic History of the Near East in the Middle Ages*, Berkeley/Los Angeles/London 1976, p. 307.

61 That this was specialized high-quality production is not only suggested by deliveries to the Mamluk court but also the curious remark that there were exports to Syria despite the textile production in Damascus. "Et si labeurent en Alexandrie toile soubtile à grant quantité et de grant valeur; et celles se mande par la voye de mer en Barbarie, en Tunes, en Surie et en Turquie. (...) Par la information que je eulx de personnes pratiques, eulx que anciennement laboroyent en Alexandrie .lxxx.mille ateliers de soye et de lin. Mais à le présent, pource que la terre est déshabitée, se labeure à petite quantité. Ilz y font draps de soye, desquelx besoigne quilz fournissent la court du Cayre; et le demorant le mandent par mer en Barbarie, en Tune, en Surie et en Turquie, nonobstant que à Damasque se labeure draps de soye à grant quantité." Emmanuele qd. Minelli Piloti, *Traité d'Emmanuel Piloti sur le passage en Terre Sainte*, ed. by Pierre-Herman Dopp, Louvain 1958, pp. 89-91.

62 For Biagio Dolfin and his papers, see G. Christ, *Trading Conflicts. Venetian Merchants and Mamluk Officials in Late Medieval Alexandria (The Medieval Mediterranean 93)*, Leiden 2012.

63 Archivio di Stato di Venezia (ASVe), Procuratori di San Marco, Commissarie miste, b. 181, fasc. XIII „carichi". For an introduction to the genre of cargo lists, see B. Arbel, *Les listes de chargement de navires vénitiens (XV<sup>e</sup>-début du XVI<sup>e</sup> siècle): un essai de typologie*, in: *Chemins d'outre-mer. Études d'histoire sur la Méditerranée médiévale offertes à Michel Balard*, ed. by D. Coulon, C. Otten-Froux, P. Pagès, and D. Valérien (Byzantina Sorbonensia 20), Paris 2004, pp. 31-50.

Exports to Europe	Imports into Egypt
Spices	Money, precious metals (mainly gold, copper)
Cotton	Cloth: cotton/fustian, woolen
Potash, cassia fistula, <sup>64</sup> etc.	Wine, oil, honey, nuts, cheese
Soap	Soap
Luxury goods: silks (velvet), jewels, carpets, ironware	Luxury goods: silks (velvet), amber, fur, falcons <sup>65</sup>
Glass	Glass
Slaves	Slaves <sup>66</sup>
Paper (not exported but produced for local consumption)	Paper (certainly for use by Venetian diaspora, probably also for sale)
Sugar (higher quality?)	Sugar (lower quality?) <sup>67</sup>

On the import side, we find pretty much what we expect to find: substantial quanti-

64 Golden shower tree (canafistola).

65 On the falcon trade, see H. A. Shehata, *Traded Animals and Gifts between the Venetians and the Mamluks*, in: *Venetian Rule in the Eastern Mediterranean 1400–1700: Empires, Connectivities and Environments – Festschrift in Honour of Benjamin Arbel*, ed. by G. Christ, Leiden, in preparation.

66 Cf. G. Christ, “Sliding Legalities: Venetian Slave Trade in Alexandria and the Aegean,” in *Slavery and the Slave Trade in the Eastern Mediterranean (c.1000–1500 CE)*, ed. by Ch. Cluse and R. Amitai (*Cultural Encounters in Late Antiquity and the Middle Ages*), Turnhout, forthcoming, pp. 210–229.

67 Ibid., p. 260–262; it seems that the Madeira sugar eventually put strong pressure on Egyptian sugar, perhaps again because of cheaper labour costs (slaves) in Madeira.

68 “A] In Cristo nomine 1419 caricho della nave paron messer L Faglier vienie da Viniexia Modon et Candia (...) Al Arcangeli, Chiaro veluti tela 1 per valor 30 denari grossi meso a so conto (...) Al Zorzi, Francesco veluti tela 1 per valor di denari (...) Al Zorzi, Francesco charicha tela 1 per valor di denari (...) Al Contarini, Carlo savoni sacchi 207 ca. 42, Al Contarini, Carlo rame in foglia ballete piccole 4 (...) Al Contarini, Carlo ambra barili 1 per valor di denari Al Contarini, Carlo duc. 500 g 1 Al Contarini, Carlo duc. 350 g 1 meso a so conto (...) Al Ziorzi, Marco duc. 500 g 1 meso a so conto Al Contarini, Giacomo meli ca. 75 Al Contarini, Giacomo zisalghallo barili 5 Al Contarini, Giacomo chonche sacchi v 813 Al Contarini, Giacomo veludi tela 1 meso a so conto Al Michiel, Polo sete di cavallo bl 5 Al Michiel, Polo zisalghallo car. 4 Al Michiel, Polo veludi tela 1 per valor di duc. 250 meso a so conto Al Bon, Francesco duc. 1400 g 2 Al Bon, Francesco duc. 500 g 1 Al Bon, Francesco duc. 700 g 1 Al Bon, Francesco duc. 650 g 1 Al Bon, Francesco duc. 450 g 1 Al Spera, Maffeo duc. 500 g 1 Al Bembo, Lorenzo qd. Marco duc. 300 g 1 Al Bembo, Lorenzo qd. Marco duc. 500 g 1 Al Bembo, Lorenzo qd. Marco duc. 300 g 1 Al Bembo, Lorenzo qd. Marco duc. 1000 g 1 Al Bembo, Lorenzo qd. Marco duc. 1000 g 1 Al Bembo, Lorenzo qd. Marco duc. 500 g 1 Al Bembo, Lorenzo qd. Marco duc. 300 g 1 Al Bembo, Lorenzo qd. Marco duc. 500 g 1 Al Malerbi, Filippo di tela 1 per valor di duc. 250 Al Malerbi, Filippo di tela 1 per valor di duc. 300 Al Malerbi, Filippo di pani 2 meso a so conto Al Canal, Nadal da duc. 500 g 1 Al Canal, Nadal da duc. 2000 g 4 Al Canal, Nadal da duc. 500 g 1 Al Canal, Nadal da duc. 700 g 1 B Al Donado, Francesco ? duc. 1200 g 2 Al Donado, Francesco duc. 1100 g 2 Al Donado, Francesco duc. 500 g 1 Al Emo, Giacomo duc. 500 g 1 Al Emo, Giacomo duc. 2000 g 4 Al Emo, Giacomo duc. 800 g 1 Al Emo, Giacomo duc. 300 g 1 Al Emo, Giacomo duc. 500 g 1 Al Emo, Giacomo duc. 500 g 1 Al Emo, Giacomo duc. 500 g 1 Al Emo, Giacomo duc. 700 g 1 Al Emo, Giacomo veludi tela 1 quello e di denari di grossi 5 Al Emo, Giacomo pani balle 3 meso a so conto Al Bernardo, Piero duc. 500 g 1 Al Bernardo, Piero duc. 470 g 1 Al Michiel, Angelo duc. 500 g 1 Al Michiel, Angelo duc. 800 g 2 Al Michiel, Angelo oglio in garre 33 meso a so conto Al Gusmeri, Antonio duc. 500 g 1 Al Gusmeri, Antonio duc. 2092 g 4 Al Gusmeri, Antonio duc. 2500 g 5 Al Falier, Alessandro savoni casse 30 meso a conto Al Falier, Luca (che sono in doana) meli ca. 50 Al Falier, Luca (che sono

ties of bullion and wool fabrics; but also luxury goods such as amber, Venetian velvet and other silken textiles,<sup>68</sup> and glass,<sup>69</sup> as well as cheaper textiles such as upper German fustian,<sup>70</sup> Italian cotton,<sup>71</sup> English wools,<sup>72</sup> and soap<sup>73</sup> imported into Egypt. Then there were also the agricultural goods imported into Egypt by the Venetians: oil, honey, cheese, almonds, and wine in considerable quantities.<sup>74</sup> Apparently sugar was also imported, although it was not represented in the cargo lists examined here.<sup>75</sup> We can distinguish roughly two categories that respond to two types or systems of trade. One was the Venetian trade and navigation system of the grand commerce that was based on galleys and cogs; the other was the Veneto-Cretan regional trade system that consisted mainly of shipping agricultural goods. The two systems, however, were deeply interconnected with goods of the grand commerce transported/smuggled by regional ships and some of the foodstuff being re-exported from Egypt towards Yemen and India, and thus they contributed to paying for the Venetian spice trade deficit.<sup>76</sup>

The export side, however, is more surprising. Besides the prominently featured spices and raw materials (cotton, ashes), it also reproduced some of the same industrial products of the import side, although arguably of higher qualities: soap, glass (at least until the late

in doana) pani balle 3 Al Falier, Luca (che sono in doana) tele 106 balle 4 Al Falier, Luca (che sono in doana) istagno in pani p. 10 Al Falier, Luca (che sono in doana) fustani peze 5 Al Falier, Luca (che sono in doana) carte balle 4 mesi a conto de ser Francesco Zorzi perché luy promeso i dity consolazi che fono bx. 3 k. 18 panvolo 1 zircha di suo in nave (...).” Simplified, partial transcript: Al=Alexandria, g=groppo i.e. bullion, duc.=ducats p.=panni, i.e. cloth, ca.=caratelli, bx.=bisanti, k.=karati, cargo list of the ship of Luca Falier, 29 May 1419, ASVe, Procuratori di San Marco, Commissarie miste, b. 181, fasc. XIII “carichi”, f. [9]; cf. also cargo list, galley Dandolo, 11 Oktober 1419, ASVe, Procuratori di San Marco, Commissarie miste, b. 181, fasc. XIII “carichi”, f. [8].

69 Felice de Merlis: Prete e notaio in Venezia ed Ayas 1315–1348, ed. by A. Bondi Sebellico, Indici a cura di A. Mozzato, Fonti per la storia di Venezia. Sez. III–Archivi notarili) Venezia, Comitato per pubblicazione delle fonti della storia di Venezia/Viella, 1973, 1978, 2012 §§ 30 seq., 36, 39 seqq.; D. Jacoby, Raw Materials for the Glass Industries of Venice and the Terraferma, about 1370–about 1460, in: *Journal of Glass Studies* 35 (1993), pp. 65–90; id. Research on the Venetian glass industry in the Middle Ages, in: *Journal of Glass Studies* (1991): 119–121.

70 See above note 69. It is difficult, however, to distinguish wool from fustian, which both seem to be named by the rather unspecific term *panni*.

71 See above note 69, cf. M. Fennell Mazzaoui, *The Italian Cotton Industry in the Later Middle Ages, 1100–1600*, Cambridge 1981; E. Ashtor, *The Venetian Cotton Trade in Syria in the Later Middle Ages*, *Studi medievali* 17 (1976) 2, pp. 675–715; J.-K. Nam, *Le commerce du coton en Méditerranée à la fin du Moyen Âge* (The Medieval Mediterranean, Peoples, Economies and Cultures, 400–1500, 68, Leiden 2007).

72 See above, note 69, cf. Carus-Wilson, *The Woolen Industry*.

73 See for instance, cargo list, galley Dandolo, 11. Oktober 1419, ASVe, Procuratori di San Marco, Commissarie miste, b. 181, fasc. XIII “carichi”, f. [8] cf. note 70; “A In Cristo nomine 1419 caricho della nave paron messer L Faglier vienie da Viniexia Modon et Candia (...) Alessandria: Zorzi, Francesco savoni carateli 28 (...) Contarini, Carlo savoni sachi 207 charateli 42.”

74 See note 69, and cargo List of a griparia patron Chocho di Famagusta, 17.11.1419, ASVe, Procuratori di San Marco 181, Commissarie miste, b. 181, fasc. XIII “carichi”; “Ser Chyareo rezepe in pyu volte +49 butti di oleo +23 charetelletti di mely”; listing honey and cheese, Vasili Zondanillo: cargo list of a griparia, 05.05.1419, ASVe, Procuratori di San Marco, Commissarie miste, b. 181, fasc. “1419”, int. 14, f. [159]; on the wine trade, see Christ, *Trading Conflicts*, pp. 167–174.

75 Ashtor, *Levantine Sugar*, p. 260; cf. M. Ouerfelli, *Le sucre: production, commercialisation et usages dans la Méditerranée médiévale* (The medieval Mediterranean 71), Leiden 2008.

76 S. Y. Labib, *Handelsgeschichte Ägyptens im Spätmittelalter* (1171–1517), Wiesbaden 1965, p. 239.



fifteenth century),<sup>77</sup> high-quality silk fabrics and sugar.<sup>78</sup> Paper was probably no longer exported but was still produced for internal consumption.<sup>79</sup> In addition, there were also some products over which the Levantine industries retained their competitive edge, for instance the production of rugs and oriental metalwares.<sup>80</sup>

In a letter sent from Alexandria to Venice, Biagio Dolfin described some objects he purchased in Alexandria for his wife and which he intended to send to Venice: 3 pieces of velvet; 1 great rough rug; 1, particularly the small multicoloured rug as a door mat; 1 small rug (*tapedo*); and 1 rug in two pieces, possibly for a boat (made the same way as the bigger one for the house).<sup>81</sup> The remark regarding its possible usage on the boat

77 R. E. Mack, *Bazaar to Piazza: Islamic Trade and Italian Art 1300–1600*, Berkeley 2002, p. 8; for the earlier history of glass in the Eastern Mediterranean: E. M. Stern, *Medieval glass from the Athenian Agora* (9<sup>th</sup>–14<sup>th</sup> c.) and some thoughts on glass usage and glass production in the Byzantine Empire, in: *Glass in Byzantium – Production, Usage, Analyses: International Workshop Organized by the Byzantine Archaeology Mainz, 17<sup>th</sup>–18<sup>th</sup> January 2008*, Römisch-Germanisches Zentralmuseum = Glas in Byzanz – Produktion, Verwendung, Analysen: Internationaler Workshop der Byzantinischen Archäologie Mainz, 17.–18. Januar 2008, Römisch-Germanisches Zentralmuseum, ed. by J. Drauschke et al., RGZM-Tagungen 8, Mainz 2010, pp. 107–120; B. Zorn (ed.), *Glass along the silk road from 200 BC to AD 1000. International Conference Within the Scope of the 'Sino-German Project on Cultural Heritage Preservation of the RGZM and the Shaanxi Provincial Institute of Archaeology, December 11<sup>th</sup>–12<sup>th</sup> 2008* (RGZM-Tagungen 9, 2010); B. Borell, *Trade and glass vessels along the Maritime Silk Road*, in: *Glass along the silk road from 200 BC to AD 1000*, *ibid.*, pp. 127–142; L. Biek, Justine Bayley, *Glass and other Vitreous Materials*, in: *World Archaeology* 11 (1979) 1, *Early Chemical Technology*, pp. 1–25.

78 “Mercatores (...) tam vi, tam emptione et contra coram omnimoda voluntate, accipere zucharos in maxima quantitate coacti sunt.” forced sales of Mamluk sugar to Venetians: letter to the Sultan, 03.09.1418, ASVe, Procuratori di San Marco, Commissarie miste, b. 180, fasc. III, f. 6; G. M. Thomas and R. Predelli (eds.), *Diplomatarium veneto-levantinum sive acta et diplomata res venetas graecas atque levantis illustrantia a. 1351–1454*, pars II *Deputazione veneta di storia patria*, Venezia 1899, p. 170.

79 The paper used by Biagio Dolfin and the Venetian community in Alexandria is of Venetian (Veneto) provenance. First, for a while, parallel production and consumption of paper prevailed; A. Michiel remarks that Venetians did not use Arabic paper. Later, this seems to change, cf. F. Babinger, *Papierhandel und Papierbereitung in der Levante*, in: *Aufsätze und Abhandlungen zur Geschichte Südosteuropas und der Levante von Franz Babinger*, ed. by H.-J. Kissling, München 1962–1976; F. Babinger, *Appunti sulle cartiere e sull'importazione di carta nell'impero ottomano specialmente da Venezia*, *ibid.*; letter by Angelo Michiel qd. Luca an Dolfin, Biagio qd. Lorenzo, 21.08.1419, ASVe, Procuratori di San Marco, Commissarie miste, b. 181, fasc. 15, int. d, f. [16]: “E e sia zerto, che fazo quello <ch>e posso, di trovar la verità (...) se volle, ve scrivo, mandeme charta.”; cf. E. Ashtor, *Levantine Sugar Industry*, p. 270.

80 On metalwares see U. Ritzerfeld, *The Language of Power: Transgressing Borders in Luxury Metal Objects of the Lusignan*, in: *Medieval Cyprus. A Place of Cultural Encounter*, ed. by S. Rogge and M. Grünbart, Münster 2015, pp. 277–308; S. Auld, *Renaissance Venice, Islam and Mahmud the Kurd: A Metalworking Enigma*, London 2004; J. W. Allan, *The Influence of the Metalwork of the Arab Mediterranean on that of Medieval Europe*, in: *The Arab Influence in Medieval Europe*, ed. by D. Albertus Agius, Reading 1994, pp. 44–62; *id.*, *Sha'ban, Barquq, and the Decline of the Mamluk Metalworking Industry*, in: *Muqarnas* 2, pp. 85–9; J. de Hond and L. Mols, *A Mamluk Basin for a Sicilian Queen*, in: *The Rijksmuseum Bulletin* 59 (2011) 1, pp. 6–33; for rugs see below.

81 “Anchor la mando ch'io chargado suso la galia Dandola in uno sachio doplo da piper et signado del mio segno charato uno de lira che se lire (abbreviation) 200 a sotel anchor suso la dita tapedo uno in do pezi che sè fato a la sisa (geographical name?) de quello grandio che'ò in chaxa et questo serà ben per la barcha over per quello che le voia arechordandove che queste do chose sia rechomandade al dito patron da cha” Dandolo et anche al nostro ser Zan Moresin et sia? pagado el dito ser Zane et Marcho Dolfin che quando le suo chose se meterà in barcha che li faza meter anche le dite mie si che ve pago se procharar che Pasqaliga abia le dite chose.” Dolfin, Biagio qd. Lorenzo, 05.11.1419, ASVe, Procuratori di San Marco, Comm. miste, b. 181, fasc. 15, int. e, f. [6]; on women as receivers as Oriental luxury goods see also: F. Bianchi and D. Howard, *Life and Death in Damascus: The Material Culture of Venetians in the Syrian Capital in the Mid-fifteenth Century*, in: *Studi Veneziani* 46 (2003), pp. 233–300, here: p. 240.



highlights the character of these goods use of rugs, as highly coveted luxury goods for prominent public display to enhance status and wealth.

The importance of such prestigious high-value Levantine products is also evidenced by an inventory listing several rugs alongside an “Damascene” candlestick base.<sup>82</sup> The prestige of rugs and high-quality “Oriental” silk products is further substantiated by its prominent display in painting (fig. 1).



Fig. 1: G. Bellini, Madonna and Child enthroned, c. 1470, National Gallery, London,<sup>83</sup>

Yet rugs are not the only hint of the existence of a highly specialized luxury goods industry in Egypt that was thriving in the face of general industrial decline. Probably con-

<sup>82</sup> Inventory: “Nobila domina Pasqualiga Dolfin (...) 2 candelieri grandi damaschini, uno zexendello damaschin, (...) 2 carpete grande a varor, (...) 4 pezzi de tapedi grandi, carpeta pizola (...) Giovanni de Buoxis, inventory of the inheritance of Pasqualiga Dolfin, 6 March 1421, ASVe, Procuratori di San Marco, Commissarie miste, b. 180, fasc. XI, f. [3].

<sup>83</sup> [https://upload.wikimedia.org/wikipedia/commons/8/8b/Gentile\\_Bellini\\_Madonna\\_and\\_Child\\_Enthroned\\_late\\_15th\\_century.jpg](https://upload.wikimedia.org/wikipedia/commons/8/8b/Gentile_Bellini_Madonna_and_Child_Enthroned_late_15th_century.jpg), accessed 24.12.2016; H. Lang, The Import of Levantine Goods by Florentine Merchant Bankers: The Adaption of Oriental Rugs in Western Cultures, in: *Union in Separation*, pp. 505-525

nected to the shortage of labour mentioned above, entrepreneurs, such as the Damâminî in Alexandria, in the textile sector gave up the production of cheap wools and began specializing in high-quality silk, which apparently still outrivalled Western copies.<sup>84</sup> There were thus both Western and Levantine velvet, although the Western product clearly emulated and imitated the Levantine product (fig. 2, 3).

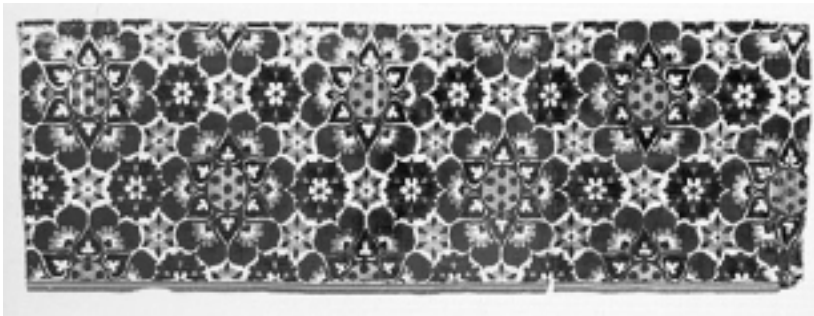


Fig. 2: Velvet fragments with Medici arms, 1440–1500, Florence or Venice<sup>85</sup>

In Alexandria, a state-run textile manufacture that probably produced honorary robes of crucial importance for Mamluk courtly and diplomatic procedure still existed.<sup>86</sup> The Mamluk chronicles give us a good description of the manufacture that eventually shut down under sultan al-Ashraf Barsbay (1422–1438).<sup>87</sup>

Although the micro-historical character of the documentation requires cautious reasoning, Venetian imports of rugs, high-quality silks, soap, glass, and metalware certainly seem to hint at the fact that Mamluk industrial decline was not necessarily linked to a loss of skills or sophistication and was probably not perceived as such in the West. It is indeed very unlikely that Venetians perceived the Levantine industries as declining at all – certainly not qualitatively. They carefully tried to imitate Levantine products, which they perceived as being superior and eminently desirable.

84 Subhi Yanni Labīb, Iskandariyya, in: *Encyclopaedia of Islam*, ed. by P. J. Bearman, Th. Bianquis, and C. E. Bosworth, E. van Donzel, W. P. Heinrichs, 2<sup>nd</sup> ed., Leiden, p. 135a.

85 Silk, metal thread L. 31 in. (78.7 cm), W. 21 in. (53.3 cm), Fletcher Fund, 1946 (46.156.118), <http://www.metmuseum.org/toah/works-of-art/46.156.118>, accessed 24.12.2016

86 On the honorary robe (khil'a) in the Mamluk system, see L. A. Mayer, *Mamluk costume: a survey*, Genève 1952; N. A. Stillmann, *Khil'a*, in: *Encyclopaedia of Islam*, ed. by P. J. Bearman, Thierry Bianquis, and C. E. Bosworth, E. van Donzel, W. P. Heinrichs, 2<sup>nd</sup> ed., Leiden 2013; Bethany J. Walker, "Rethinking Mamluk Textiles, in: *Mamlūk Studies Review* IV (2000), pp. 167–217; M. Springberg-Hinsen, *Die Khil'a: Studien zur Geschichte des geschenkten Gewandes im islamischen Kulturkreis* (MISK 7), Würzburg 2000; W. Diem, *Ehrendes Kleid und ehrendes Wort: Studien zu tašrīf in mamlukischer und vormamlukischer Zeit* (Abhandlungen für die Kunde des Morgenlandes 54.2), Würzburg 2002.

87 M. Müller-Wiener, *Eine Stadtgeschichte Alexandrias von 564/1169 bis in die Mitte des 9./15. Jahrhunderts. Verwaltung und innerstädtische Organisationsformen*, Berlin 1992, pp. 61, 237, 239.

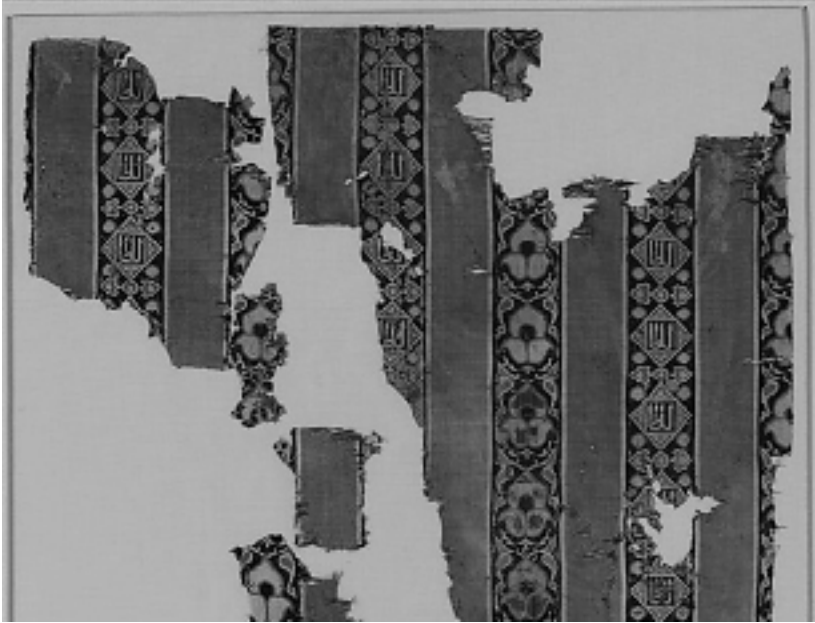


Fig. 3: Mamluk textile fragment, 14<sup>th</sup> century<sup>88</sup>

### Conclusion: Deindustrialization rather than Industrial Decline?

The positions criticizing the notion of Mamluk decline do not succeed in fully dismantling the arguments supporting the decline hypothesis. I would suggest, however, the instinctive is true in its own right. Although Ashtor's theses are factually sound, the notion of decline is misleading. The successful "transformation" did not address any of the underlying structural problems that had triggered decline in the first place. But it did help Egypt's economy to a comfortable early retirement as a post-industrial knowledge and service economy. I do not want to paint a picture of a glorious Mamluk period but I feel that there is something to be gained in modifying the notions of decline or transformation into something closer to a Levantine "autumn of the Middle Ages" might emerge.<sup>89</sup>

88 Accession Number: 1979.462.2, <http://www.metmuseum.org/Collections/search-the-collections/140010356>. The word sultan in the lozenges might hint at usage by sultans or as honorary robes (on which note below) of the highest rank bestowed by the sultan..

89 J. Huizinga, *Herfsttij der middeleeuwen: studie over levens- en gedachtenvormen der veertiende en vijftiende eeuw in Frankrijk en de Nederlanden*, Amsterdam 1997; English with a somewhat misleading title coming closer to decline: id., *The Waning of the Middle Ages. A Study of the Forms of Life, Thought, and Art in France and the Netherlands in the Fourteenth and Fifteenth Centuries* 6. ed. (Harmondsworth: Penguin Books, 1976); Huizinga does not use the term "waning" but "autumn"; he focuses on the saturation of a culture that incubates new forms – a new culture.

Venice could benefit from this development. Thanks to the strong entanglement of the Mamluk and Venetian economies, Venice was the natural pre-industrial partner to take over the production of some goods and the import of others produced in other parts of Europe. Slowly but surely, Western European actors took over production on behalf of the Levantine world. First wool and fustian fabrics, then increasingly also more sophisticated silks were produced in Venice and elsewhere in Italy and Europe. Also paper, glass, soap, and sugar were increasingly produced in Europe and (in the case of the latter) in the new Atlantic colonies of Europe. The European “miracle” was dependent after all; it resembles in some ways the story of the Asian economic ascent of the late twentieth century. European factors of success converged with an opportunity provided by a deindustrializing Levant, which was outsourcing production to Europe.

This is corroborated by the great appreciation for Levantine and Oriental products. Rugs were probably not coveted as exotic, rustic goods but as highly sophisticated products of a world that was seen as culturally superior. And, although, they are not industrial products they emphasize that Venetians at least did not perceive the Levant as a vanishing or declining culture but one from which there was much to learn and to emulate.

# **Entangled Worlds or Cultural Bifurcation? Comments on the Intellectual Origins of the Great Divergence and Modern Economic Growth, c. 1500–2000 AD**

**Philipp Robinson Rössner**

## **ABSTRACT**

Der Autor untersucht die Geschichte ökonomischer Theorien über die Verursachung regionaler Differenzen und ihre Anwendung seit 1500 und fragt nach den Wegen, die solche Theorien genommen haben und nach dem Wechselverhältnis mit den unterschiedlichen Kontexten in Asien und Europa. Damit wird ein Rahmen entfaltet, der es erlaubt, die Dynamik des Verhältnisses von wirtschaftlichen Ideen und Akteuren sichtbar zu machen und weiter zurückzuschauen, wenn es um die Ursprünge der Great Divergence geht. Der Autor kritisiert den häufig sehr engen Blickwinkel, den Ökonomen und Wirtschaftshistoriker nutzen, um globale Unterschiede und wirtschaftliches Wachstum zu erklären. Dies führe dazu, dass die Divergence als eine Anomalie der Zeit ab 1750 gesehen wird. Dabei wird jedoch das Wie mit dem Warum verwechselt und eine unterkomplexe Erklärung der angenommenen Kausalitäten geliefert. Stattdessen plädiert der Beitrag dafür, die Verwurzelung der Unterschiede im Verhältnis der Menschen zur Wirtschaft und in ihrer Wahrnehmung der kosmologischen Ordnung zu sehen. Er sieht das beginnende 16. Jahrhundert als den Zeitpunkt, an dem sich in Europa die Weltwahrnehmung zu ändern begann und sich damit Ideen eines modernen Kapitalismus ausbreiten konnten. An die Stelle einer auf die Vergangenheit gerichteten Zeitwahrnehmung trat nun die Idee einer offenen und steuerbaren Zukunft, woraus Hume und Smith wirtschaftstheoretische Konsequenzen ableiteten. Ideen, die in China prominent waren, wie etwa das Stabilität und Nicht-einmischung verheißende *wu-wei*, wurden in Europa durchaus rezipiert, aber in der bald entstehenden Praxis hochinterventionistischer Wirtschaftspolitiken bald völlig anders interpretiert. Dazu kam es offensichtlich in China aus einer ganzen Reihe von Gründen nicht.

## I

The last four or five centuries have seen one of the most fundamental economic transformations in recorded human history. Between c. 1500 and 2000 AD, Western European per capita gross domestic product (GDP) – the most commonly accepted, yet also inherently ahistorical, measure for cross-sectional and inter-temporal comparisons of economic wealth – grew by a factor of at least 21. The lion's share of this growth fell within the post-1800 period, related to the phenomenon commonly known as industrialization.<sup>1</sup> Between 1820 and 2000 AD, a period for which we have better and slightly more reliable data, world population increased by 500 per cent, whilst world GDP rose by somewhere around 800 per cent. This amounted to a previously unheard-of expansion not only of overall economic wealth – and social inequality – but also human ingenuity and productivity. Whilst *all* world regions have experienced significant increases in income over the past two centuries, notable world-regional differentials have emerged. The highest growth rates are found in northwestern Europe and North America. Between 1800 and 2000 AD, the income gap between the US and the African average widened from a factor of 3 to a factor of 20. Whilst at the dawn of the early modern period (1500 AD) Europe and China may have stood head-to-head in terms of economic wealth and productivity, by the twentieth century a wide gap in wealth had opened, with northwestern Europe emerging and becoming 5 or 6 times as wealthy as China. The pattern has been reversed only fairly recently. These transformations – varyingly known as “industrial revolution,” “Great Divergence,” or sometimes (by scholars dubbed as “Eurocentrist”) “European miracle”<sup>2</sup> – represent intrinsic features of modernity and the rise of the modern world economy.

But why did Europe grow so rich whilst other world regions did not?<sup>3</sup> Historians' narratives of modern economic growth and global economic divergence have rested on variables such as real wages, GDP, or GDP per capita, with the latter two being ahistorical

- 1 During this period, some countries have taken the lead whilst others have fallen back, sometimes catching up again later. Pre-1900 GDP figures presented in A. Maddison, *Monitoring the World Economy 1820–1992*, Paris 1995 (with constantly updated information on the panel data to be found on the Maddison Project website: <http://www.ggd.net/maddison/maddison-project/home.htm>) and discussed, for example, in P. Malanima, *Pre-modern European Economy: One Thousand Years (10<sup>th</sup>–19<sup>th</sup> Centuries)*, Leiden 2009, have been subject to debate (and speculation). They need to be viewed with a pinch of salt: neither GDP nor GDP per capita nor ways of measuring the latter strictly speaking existed before the 1900s; see M. Schmelzer, *The Hegemony of Growth: The OECD and the Making of the Economic Growth Paradigm*, Cambridge 2016 and D. Philipsen, *The Little Big Number: How GDP came to Rule the World and What To Do About It*, Princeton 2015. Derived from a partly circular way of reasoning – mainly by projecting backwards hypothetical per capita GDP figures from c. 1900 thousands of years back in time, usually based upon urbanization figures and some non-quantitative evidence on possible total factor productivity growth rates – the Maddison figures only present the broad historical and dimensions of long-term productivity change according to the most reliable inductive stories we have by historians.
- 2 Eric L. Jones, *The European Miracle: Environments, Economies, and Geopolitics in the History of Europe and Asia*, 3rd ed., Cambridge/New York 2003.
- 3 I have dealt with the problematical nature of such projected backwards “per capita GDP” in a forthcoming contribution to the *Handbook of Transregional Studies*.

propositions because concepts of “the economy” and “GDP” were only fully developed long after 1900 AD.<sup>4</sup> Resource and capital endowment, technology, and, very recently, differential patterns of state involvement in the economy have been the usual suspects for explaining economic modernization and divergence over the past four centuries or so.<sup>5</sup> Much less has been said about culture and ideas. When culture was considered by economists and economic historians, it was often in relation to institutions accounting for variations in national and world-regional economic wealth through social norms and cultural practices.<sup>6</sup> Some scholars have highlighted cultural bifurcation in social norms and kin structure, affecting the nature of contract enforcement and thus possibly causing later divergence between China and “the West.”<sup>7</sup> The cultural inclination attributed to Europeans towards economic and military *competition*, manifested by the market and the tournament, have likewise been cited as crucial factors that set Europe apart from the rest.<sup>8</sup> Others have mentioned the rise of “bourgeois values and dignity”<sup>9</sup> and the eighteenth-century Enlightenment as a revolution in attitude towards more rational thinking about the economy and the market.<sup>10</sup>

Whilst not in any way disregarding such models – apart from the fact that the last-mentioned one (the cultural enlightenment hypothesis<sup>11</sup>) obviously represents a teleology or progress-orientated interpretation of history – the present paper sketches contours of an alternative approach towards, and a critique of, prevailing interpretations of global divergence and modern economic growth. This will take us back at least five hundred years. In

4 Phillipsen, *Little Big Number*; Schmelzer, *Hegemony of Growth*.

5 Landes, *The Wealth and Poverty of Nations*; P. Vries, *State, Economy and the Great Divergence: Great Britain and China, 1680s–1850s*, London 2015; id., *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; K. Pomeranz, *The Great Divergence: China, Europe, and the Making of the Modern World Economy*, Princeton, NJ 2000; R. Bin Wong, *China Transformed: Historical Change and the Limits of European Experience*, Ithaca 1997; J.-L. Rosenthal/R. Bin Wong, *Before and Beyond Divergence. The Politics of Economic Change in China and Europe*, Cambridge 2011; P. Parthasarathi, *Why Europe Grew Rich and Asia Did Not: Global Economic Divergence, 1600–1850*, Cambridge 2011; D. Acemoglu / James Robinson, *Why Nations Fail: The Origins of Power, Prosperity, and Poverty*, New York 2012.

6 For example, D. C. North, *Institutions, Institutional Change, and Economic Performance*, Cambridge 1990; id., *Understanding the Process of Economic Change*, Princeton 2005; id., J. J. Wallis and B. R. Weingast, *Violence and Social Orders: A Conceptual Framework for Interpreting Recorded Human History*, Cambridge 2009, or A. Greif, *Cultural Beliefs and the Organization of Society: A Historical and Theoretical Reflection on Collectivist and Individualist Societies*, *Journal of Political Economy*, 102 (1994) 5, pp. 912–50; id., *Institutions and the Path to the Modern Economy: Lessons from Medieval Trade*, Cambridge 2006, id. and G. Tabellini, *Cultural and Institutional Bifurcation: China and Europe Compared*, in: *American Economic Review*, 100 (May 2010), pp. 135–140.

7 Greif, *Cultural Beliefs and the Organization of Society*, and id. and G. Tabellini, *Cultural and Institutional Bifurcation*.

8 For example, P. T. Hoffman, *Why Did Europe Conquer the World?*, Princeton, NJ 2015; Landes, *Wealth and Poverty of Nations*, and on a different tune, combining military struggle with geography and relative prices for labour (wages) and capital (interest rates), see Rosenthal/Wong, *Before and Beyond Divergence*.

9 D. N. McCloskey, *The Bourgeois Virtues: Ethics for an Age of Commerce*, Chicago, IL 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.

10 For example, J. Mokyr, *The Enlightened Economy: An Economic History of Britain, 1700–1850*, New Haven 2009. A radically different approach to capitalist mentality was formulated by W. Sombart, *Der moderne Kapitalismus: historisch-systematische Darstellung des gesamteuropäischen Wirtschaftslebens von seinen Anfängen bis zur Gegenwart*, Vols. I, II. 4th ed. Munich/Leipzig 1921/1928.

11 Mokyr, *Enlightened Economy*.

fact, the long-term view is crucial when it comes to studying phenomena such as European industrialization, the rise of the modern economy, and also the Great Divergence.<sup>12</sup> Strikingly, such a long-term perspective has been absent from most narratives.<sup>13</sup> This has led to the somewhat foreseeable yet eminently trivial interpretation of both global economic divergence as well as the emergence of modern economic growth after 1750 as some sort of grand world-historical anomaly that needs explanation. But if we look back over the last two millennia, observing that China in the twelfth century may have been as highly developed, in terms of technology and living standards, as Europe was around 1700 or 1800 AD, the mere question “why Europe grew rich whilst Asia did not”<sup>14</sup> becomes trivial. We can give an answer using the words of the first Chinese premier, Zhou Enlai, who, when asked about the consequences of the French Revolution, said that it was “too early to tell.”<sup>15</sup> Modern historians and economists are often stuck within their self-inflicted narrow frameworks of presentism<sup>16</sup> and their short-term perspectives. But historical processes have a “deep history”, a history that is explained as having meaning only when considering trajectories that span several centuries, if not millennia. Thus the “Rise of Europe”<sup>17</sup> may be, world-historically speaking, only a footnote within a deeper human history of several hundreds or thousands of millennia of European culture and economy at the periphery (which may extend to those centuries or millennia yet to come, if the world remains intact).

If we accept, for the time being, the problematic framework traditionally chosen by historians in order to explain global divergence for the past three centuries, it appears as though most of the recent grand narratives are still flawed inasmuch as they provide explanations of *how* this divergence may have been achieved by portraying, and occasionally mistaking, such explanations as actual *causes* of this divergence – the *why*. The latter, obviously, is very different territory. And although historians have correctly emphasized that we should not propel causation in history too far,<sup>18</sup> we may try and look for deeper and more immediate, that is to say non-proximate, explanations of the origins

12 For a long-term view of the great divergence, see, for example, J. M. Hobson, *The Eastern Origins of Western Civilisation*, Cambridge 2004, or U. Menzel, *Die Ordnung der Welt: Imperium oder Hegemonie in der Hierarchie der Staatenwelt*, Berlin 2015; with regard to the European industrial revolution, J. L. van Zanden, *The Long Road to the Industrial Revolution. The European Economy in a Global Perspective, 1000–1800*, Leiden / Boston 2009.

13 Exceptions are to be found in A. G. Frank and B. K. Mills, *The World System: Five Hundred Years or Five Thousand?*, London / New York 1993, A. G. Frank, *ReOrient: Global Economy in the Asian Age*, Berkeley 1998, or Menzel, *Ordnung der Welt*.

14 Parthasarathi, *Why Europe Grew Rich*; Landes, *Wealth and Poverty of Nations*; Jones, *European Miracle*; Pomeranz, *Great Divergence*; Bin Wong, *China Transformed*.

15 I have found this quote in R. Nisbett, *The Geography of Thought. How Asians Think Differently... and Why*, New York et al. 2003, p. 13.

16 F. Hartog, *Regimes of Historicity: Presentism and Experiences of Time*, New York 2015.

17 W. H. McNeill, *The Rise of the West: A History of the Human Community*, Chicago 1963.

18 R. Bin Wong, *Causation*, in U. Rublack (ed.), *A Concise Companion to History*, Oxford 2011, pp. 27–56, and with regard to useful knowledge and discourses about the Great Divergence, see id., Roy Bin Wong, *Useful Knowledge and Economic Change: What Are We Explaining?*, LSE Global Economic History Network Conference, working paper (2004), <http://www.lse.ac.uk/economicHistory/Research/GEHN/Conferences/conference4.aspx> (accessed 17 February 2016).



of the Great Divergence and modern economic growth. Just a few clues will be offered here. Social norms, for instance, far from being causes of or primary origins for global economic divergence rather explain observable variances in developmental patterns over time. They can be important when it comes to *structuring* reality by means of informal institutions. This may even include an immediate future horizon.<sup>19</sup> Therefore, they give us a picture of *how* divergence has unfolded over time. But such cultural norms as social conventions tell us only very little about peoples' *interpretation of the prevailing cosmological order* and how this cosmological order may change through active human intervention. Would this not be much more important when it comes to *explaining*, rather than *monitoring*, processes of divergence? True, North has developed a model of studying humans' cosmological order by linking changes in humans' perception of the cosmological order and the dynamic adjustment, or non-adjustment, of human institutions to such changes.<sup>20</sup> Humans adapt their belief structures (regarding the cosmological order) to prevailing or *changing* material-economic conditions. This may potentially lead to *an improved understanding* of the cosmological order in itself.<sup>21</sup> The post-1660 Newtonian revolution in science and technology seems to be a good example of this.<sup>22</sup> Once again, this only gives us information about improvements in the knowledge of the *how-to-do* but nothing on the deeper meaning and ontology of such processes. What about people's fundamental desires, intentions, and incentives that fill such changes in the "know-how" schedule with meaning? And how do they interact with the economy and the evolution of economic knowledge and reasoning? Can we not expect them – as value judgements – to have been at least as important, if not much more important, than cultural habits and social norms when it comes to explaining processes of global economic divergence and modern economic growth? Cultural norms and social habits, as they are usually framed by institutional economists and historians, rarely explain such alterations, that is to say changes in economic reality after a turning point or "critical juncture" is reached, which means there is no turning back; such "critical junctures" may crucially determine future chances of growth and development for centuries to come.<sup>23</sup>

One may instead argue that such critical junctures require a switch in people's *cosmology*. And such a critical juncture was reached in Europe around 1600 AD through the fundamental alteration in people's moods of thinking about the future (briefly discussed in the next section). A related question is what about the economic *ideas* that underlined, accompanied, and supported processes of growth and development, industrialization and uneven global development? What made Europe rich whilst making others poorer?<sup>24</sup>

19 Greif, Institutions and the Path to the Modern Economy, and Greif/Tabellini, Cultural Bifurcation. The business outlook of merchants and merchant partnerships in medieval and early modern Europe was usually short-lived, extending to one or two years usually.

20 D. C. North, Understanding the Process of Economic Change, Princeton 2005.

21 North, Understanding the Process of Economic Change; D. Lal, Unintended Consequences: The Impact of Factor Endowments, Culture and Politics On Long Run Economic Performance, Cambridge 1998.

22 J. Black, The Power of Knowledge: How Information and Technology Made the Modern World, New Haven 2014.

23 Acemoglu / Robinson, Why Nations Fail.

24 E. S. Reinert, How Rich Countries Got Rich – And Why Poor Countries Stay Poor, New York 2007; Parthasarathi,

What about differences in economic “theories” and political economy regimes, and competing paradigms of what “the state” was and what it should – or should not – do in the economy? These phenomena should be studied within a global and holistic perspective, adopting a deep-time approach as well as a cross-sectional and global perspective on modern economic growth. Consideration should be given to the embeddedness of political economy within the wider realms of culture, including temporal-spatial variations therein. In this way, one would need to seek out new questions and epistemic strategies relating to major processes of global economic divergence over the last few hundred years, as well as narratives and perspectives on one of the most fundamental transformations in recorded human history: Europe’s road to wealth in global perspective. The next section will offer a preliminary strategy for discussing such epistemic changes by drawing on the example of the perception of the future in the European mindspace as it developed after the beginning of the seventeenth century. This may have been one of the possible origins of modern economic growth (1600–2000 AD).<sup>25</sup> Some global implications in terms of possible divergences as well as convergences in economic thought will be dealt with in another section, before a penultimate section provides more detail on early modern German economic discourses on silver and European’s “fear of imports” in a global perspective. The last section provides a conclusion.

## II

*Wir arbeiten für unsere Nachkommen!* (We work for posterity!) – Thus wrote Daniel Gottfried Schreber, professor of economics (cameralism) at the University of Leipzig from 1764 until 1777, in the preface to his translation of Swedish cameralist Anders Berch’s magnum opus *Inledning til Almännas Hushållningen, innefattande Grunden til Politie, Oeconomie och Cameralwetenskaperna* (1747/63).<sup>26</sup> With this brief statement, Schreber, inadvertently, provided a pertinent answer to one of the primordial questions in the modern social sciences: Why did Europe grow so rich over the last half-millennium (especially after 1800 AD) whilst others did not? And why did this process happen so suddenly and rapidly, compared to the thousands of years of zero or little growth that had come before?<sup>27</sup> One possible answer is because the future as a manageable entity had now entered the European economic mindspace.

Why Europe Grew Rich and Asia Did Not.

- 25 This being with the obvious contention that, as a real thing, modern, that is to say Kuznetsian economic growth, did not exist prior to about 1900. A good discussion can be found in P. Vries, *Wirtschaftswachstum*, in: Markus Cerman et al. (eds.), *Wirtschaft und Gesellschaft. Europa 1000–2000* (VGS-Studientexte 2). Innsbruck/Vienna/Bolzano 2011, pp. 76–103. Nevertheless, this should not prevent us from looking for pretexts of this modern paradigm in pre-modern economic discourse.
- 26 Stockholm: Lars Salvius, 1747; Ger. Transl. *Anleitung zur allgemeinen Haushaltung in sich fassend die Grundsätze der Policy-, Oeconomie- und Cameralwissenschaften*, Halle 1763.
- 27 G. Clark, *A Farewell to Alms: A Brief Economic History of the World*, Princeton, NJ 2007 is, perhaps, overly pessimistic on chances of growth before 1800; other historians, such as Jan de Vries, would take issue with this by

Whilst many scholars today would call for an end to growth, acknowledging its ecological shortcomings and psychological rifts,<sup>28</sup> the idea that unlimited economic growth is possible in principle *as well as* desirable is still prevalent. It has dominated public discourses for the best part of the last two centuries. Since the days of Karl Marx and Max Weber, the grand narratives of European economic supremacy have fed on this story.<sup>29</sup> Scholars have linked its rise to the eighteenth-century Enlightenment. All of a sudden, it is said, northern Europeans' lives were subjected to radical acceleration.<sup>30</sup> Their timetables became crowded ever since they discovered the virtues of capitalism, free markets, bourgeois values, and optimal theories of resource allocation, with "optimal" often denoting comparability to the modern neoclassical paradigm,<sup>31</sup> implying that these insights were alien to non-Europeans. But what if this story is wrong? In fact, it seems as though the idea of *infinite economic growth* – an early precursor of modern growth discourses<sup>32</sup> – has a much deeper history. As a cultural code, it is older than the Enlightenment. And it does not have as much to do with the rise of modern capitalism as suggested by many;<sup>33</sup> capitalism had been in Europe since the Renaissance.<sup>34</sup> Rather, it was related to a fundamental cultural and epistemic change that occurred right at the same time when a profoundly new mode of economic thinking gained ground c. 1600 AD.<sup>35</sup> This gives rise to the assumption that these two fundamental epistemic shifts – one cultural, the other economic – were related.<sup>36</sup>

pointing to some period of remarkable economic dynamics in many places within Europe (and elsewhere) even before industrialization. See J. de Vries/A. van der Woude, *The First Modern Economy: Success, Failure, and Perseverance of the Dutch Economy, 1500–1815*, Cambridge 1997 and J. de Vries, *The Industrious Revolution: Consumer Behavior and the Household Economy, 1650 to the Present*, Cambridge 2008; and P. Kriedte, H. Medick/J. Schlumbohm, *Industrialization before Industrialization: Rural Industry in the Genesis of Capitalism*, Cambridge 1981.

28 For example, Philipsen, *Little Big Number*; T. Sedláček/O. Tanzer, *Lilith und die Dämonen des Kapitals. Die Ökonomie auf Freuds Couch*, Munich 2016.

29 Parthasarathi, *Why Europe Grew Rich*, introduction.

30 H. Rosa, *Social Acceleration: A New Theory of Modernity*, New York 2013.

31 For example, Landes, *Wealth and Poverty*; J. Mokyr, *The Enlightened Economy: An Economic History of Britain, 1700–1850*, New Haven 2009, ch. 4.

32 Schmelzer, *Hegemony of Growth*.

33 For example, L. Neal, Introduction, in: id. and Jeffrey G. Williamson (eds.), *The Cambridge History of Capitalism, Vol. I: The Rise of Capitalism: From Ancient Origins to 1848*, Cambridge 2015, pp. 1–23.

34 Sombart, *Der moderne Kapitalismus*.

35 E. S. Reinert/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, pp. 63–86; P. R. Rössner, *New Inroads into Well-Known Territory? On the Virtues of re-discovering Pre-Classical Political Economy*, in: id. (ed.), *Economic Growth and the Origins of Modern Political Economy: Economic Reasons of State, 1500–2000*, London/New York 2016, pp. 3–25; id., *Manufacturing Matters: From Giovanni Botero (c. 1544–1617) to Friedrich List (1789–1846), or: The History of an Old Idea*, in: H. Hagemann, Stephan Seiter/E. Wendler (eds.), *Through Wealth to Freedom*, Milton Park/New York 2016.

36 Research by the present author will connect new research in the economic sciences and the history of economic reasoning (most recently essays in Rössner (ed.), *Economic Reasons of State*) with new findings in the cultural and intellectual history of early modern Europe, especially A. Landwehr, *Geburt der Gegenwart: Eine Geschichte der Zeit im 17. Jahrhundert*, Frankfurt/Main 2014; H. D. Kittsteiner, *Die Stabilisierungsmoderne: Deutschland und Europa 1618–1715*, Munich 2010.

On the one hand, we find the rise of modern, proactive or new economic thinking around 1600 AD,<sup>37</sup> sometimes called “Renaissance economics,”<sup>38</sup> most often being, but ultimately unhelpful, similar or identical to what has been labelled “mercantilism”<sup>39</sup> and “cameralism.” Writers under this paradigm often entertained the idea that *infinite growth was possible in principle*. This was a profoundly modern paradigm of thinking but was no product of the Scottish Enlightenment, as suggested by some historians.<sup>40</sup> This new economic thinking was concerned with the rise of modern markets, manufacturing, and the proactive role that the state should and did play in the process of growth and development.<sup>41</sup> The vocabulary employed in these texts addressed fundamentally new questions of growth, development, and proactive resource management; increasing employment; promoting manufacturing and import substitution; public banks; the abolition of monopolies and rent seeking; exchange rate manipulation; and, most importantly, *future planning* and prospective economic thinking. This was a mode of economic reasoning we look for in vain in any of the pre-1600 European economic texts and genres.<sup>42</sup> From about the same time onwards, we also observe a *cultural* landslide change in Europe: the “discovery of the present.”<sup>43</sup> Until the seventeenth century, most interpretations, cultural visions, and religious discourses had adopted a teleological and backward-orientated vision of time and the future, featuring an “Armageddonist” perspective on human history that essentially determined the cultural outlook on future horizons. The future was either short, bleak, or generally predetermined by the past and God’s chosen order as laid out in the hidden signs of the Bible. Reformers such as Martin Luther or Jean Calvin would often play variations of the “Armageddonist” tune in order to drive home their point: the world as they knew it was deemed – or doomed – to end.<sup>44</sup> In this particular future view, there was not much room for economic *development* – in the sense of conscious human effort and *agency* directed at future betterment beyond the usual common time frame. This was provided by the next weeks and months or the next harvest – perhaps the next one or two years of business in the case of merchants, merchant partnerships, and merchant firms. Historical demographer Arthur E. Imhof sketched an interesting “psychogram” of the early modern mindspace. Time horizons extended into eternity and were soteriologically framed. The *hic et nunc* of the present material world did not carry as much meaning and ontological significance as the Everafter, on which most people’s

37 Essays in Rössner (ed.), *Economic Reasons of State*; E. Reinert/Ph. Rössner, *Cameralism and the German Tradition of Development Economics*.

38 E. S. Reinert, *The Role of the State in Economic Growth*, in: *Journal of Economic Studies* 26 (1999), 4/5 pp. 268–326.

39 Regarding mercantilism, the literature is near endless; see P. R. Rössner, *Heckscher Reloaded? Mercantilism, the State and Europe’s Transition to Industrialization (1600–1900)*, *The Historical Journal*, 58 (2015) 2, pp. 663–683.

40 Mokyr, *Enlightened Economy*, ch. 4; A. Sandmo, *Economics Evolving: A History of Economic Thought*, Princeton 2011.

41 Rössner (ed.), *Economic Reasons of State*, introduction and other essays therein; for the post-1800 period, L. Magnusson, *Nation, State and the Industrial Revolution: The Visible Hand*, London/New York 2009.

42 E. Reinert/Ph. Rössner, *Cameralism and the German Tradition of Development Economics*.

43 Landwehr, *Geburt der Gegenwart*.

44 Ibid., see also P. R. Rössner, *Martin Luther on Commerce and Usury*, London/New York 2015, introduction.

endeavours and efforts were focused.<sup>45</sup> The European textual record after 1600, however, looks different.<sup>46</sup> New literary genres and institutions emerged, such as newspapers or the blank calendar (a personal diary with blank pages for future events). Fire and marine insurance schemes were now developed and turned into productive financial tools. The rank and place of *news* as a category in European discourse and cultural perception changed considerably. Newspapers now grouped together news and information relating to actors, events, and developments, usually political and religious, that were completely unconnected to one another. They did so in a chronological, spatial, as well as social manner (range of actors concerned), creating a fundamentally new “timescape” (Landwehr): time now progressed in an open way, into an open future, driven by multiple versions and visions of people’s contemporaneity.<sup>47</sup> Characteristic of the new landscape of news and information management that emerged after c. 1600 was the proliferation of the mundane. Even “random” things now were classified as “news.” A wider and more open-access *market* for news thus arose. These phenomena represent what one cultural historian has labelled the “Birth of the Present.”<sup>48</sup> They demonstrate a fundamentally changed conception of time as an open-ended flow of events and of risk and chance entering the human equation, and, most importantly, the new vision of the *future as a manageable entity*. This new timescape of the present rested upon the understanding that the future was principally malleable, potentially for the good, by not only human agency and will but, above all, careful management. Can we not expect such fundamental changes in the *cultural* paradigm to have significantly impacted upon – or interacted with – Europeans’ visions of *economy*?

Why is this significant? First, if it is true that industrialization, globalization, and the emergence of the modern economy – all of which have a long pre-history<sup>49</sup> – rested upon a new mode of thinking that predated the Enlightenment, then we may speculate whether the eighteenth-century economic Enlightenment authors such as Adam Smith or David Hume, usually heralded as the intellectual forefathers of economic modernization, are actually the right protagonists in this story.<sup>50</sup> Chances are they were, if anything, marionettes in the play.<sup>51</sup> Secondly, pan-European discourse exhibited considerable and expected national and regional idiosyncratic variation. Only after 1945 did economics attain a clear and canonized shape as an academic discipline, to be homogenized under an epistemic paradigm, which we may term Anglo-Saxon or “neoclassical.” But before the twentieth century, the German and other continental cameralists may be classified as “mainstream,” in the same way as this applies to modern general equilibrium theory

45 A. E. Imhof, *Die verlorenen Welten. Alltagsbewältigung durch unsere Vorfahren*, Munich 1983; id., *Die Lebenszeit. Vom aufgeschobenen Tod und von der Kunst des Lebens*, Munich 1988.

46 Landwehr, *Geburt der Gegenwart* 2014; see also Kittsteiner, *Stabilisierungsmode*.

47 Landwehr, *Geburt der Gegenwart*.

48 Ibid.

49 Van Zanden, *Long Road*.

50 For example, Mokyr, *Enlightened Economy*, ch. 4; Sandmo, *Economics Evolving*.

51 E. Reinert, *Role of the State*; id., *How Rich Countries Got Rich*; E. Reinert / Ph. Rössner, *Cameralism and the German Tradition of Development Economics*.

and neoclassicism.<sup>52</sup> Third, whilst modern scholars have adopted a critical stance towards economic rationality and the often assumed ability to forecast the economic future,<sup>53</sup> no one could deny that it was exactly this assumption or thinking bias<sup>54</sup> of a forecastable future that gave rise to considerable positive economic externalities over the last four centuries of European economic history. This “thinking error” or illusion of being in control (of the future) has considerably stimulated human economic ingenuity. It did so by the proverbial “schemes” and projects, and the rise of insurance and financial mathematics since the seventeenth century. Without doubt, the new cultural-economic programme of post-1600 AD Europe represents a cornerstone of European economic wealth until modern times. To what extent did this programme influence processes such as the Great Divergence? This is another important question of ultimately global significance. We should be clear that this was an open, two-sided or mutual process, with ideas travelling between Europe and Asia, rather than from one direction to another. The Sinophilia of eighteenth-century German economists was proverbial but hardly ever studied in a systematic way.<sup>55</sup> Others have pointed to the possible origins of physiocracy, and therefore modern economic liberalism, in ancient Chinese economic thought.<sup>56</sup> Much more systematic work is needed here: first, by means of a systematic comparison of early modern European with extra-European (for example, Indian or Chinese) economic thought; second, with regard to formulating a possible hypothesis regarding certain developments (and possible divergences) in economic thought as a possible origin point of global economic divergence.

### III

We find two distinct and competing economic paradigms buried within the last thousand years of recorded global history. On the one hand, there was an “Eastern” ideal of

52 For the infinite growth discourses in mid-seventeenth century Sweden, see C. Wennerlind, *The Political Economy of Sweden's Age of Greatness: Johan Risingh and the Hartlib Circle*, in: Rössner (ed.), *Economic Reasons of State*, pp. 156–185, for England, C. Wennerlind, *Money: Hartlibian Political Economy and the New Culture of Credit*, in: P. J. Stern / C. Wennerlind (eds.), *Mercantilism Reimagined: Political Economy in Early Modern Britain and Its Empire*, Oxford 2014, pp. 74–79. For divergent traditions in Germany, see B. Schefold, *Der Nachklang der historischen Schule in Deutschland zwischen dem Ende des zweiten Weltkriegs und dem Anfang der sechziger Jahre*, in: id., K. Acham, and K. W. Nörr (eds.), *Erkenntnisgewinne, Erkenntnisverluste. Kontinuitäten und Diskontinuitäten in der Wirtschafts-, Rechts- und Sozialwissenschaften zwischen den 20er und 50er Jahren*, Stuttgart 1998, pp. 31–70.

53 The economic future, for example, in financial market analysis; see, for example, Sedláček/Tanzer, *Lilith und die Dämonen des Kapitals*; D. Kahneman, *Thinking, Fast and Slow*, New York 2011.

54 Kahneman, *Thinking, Fast and Slow*.

55 See Johanna Menzel, *The Sinophilism of J. H. G. Justi*, in: *Journal of the History Ideas*, 17/3 (1956), pp. 300–310, and some remarks in U. Adam, *The Political Economy of J. H. G. Justi*, Berne 2006.

56 C. Gerlach, *Wu-Wei in Europe. A Study of Eurasian Economic Thought*, Working Papers of the Global Economic History Network (GEHN), 12/05 (2005). Department of Economic History, London School of Economics and Political Science, London; J. M. Hobson, *The Eastern Origins of Western Civilization*, Cambridge 2004, pp. 194–201; J. Daly, *Historians Debate the Rise of the West*, London / New York 2015, p. 129. J. Goody, *The East in the West*, Cambridge 1996.

stability through government non-interference, a discourse sometimes called *wu-wei*.<sup>57</sup> Some would maintain that it became diffused across early modern Europe by means of commerce and cultural transfer, mainly through the early modern Netherlands (Amsterdam was, until the eighteenth century, Europe's door to the world, succeeded by Hamburg and London).<sup>58</sup> Western thinkers and politicians, from Gottfried Wilhelm Leibniz to Jean-Baptiste Colbert, were proverbially Sinophile; Johann Heinrich Gottlob von Justi, Germany's most famous and most prolific Cameralist of all times and most influential economist of the eighteenth century seem to have been significantly influenced by Chinese political and economic thought.<sup>59</sup> Even though we should probably not venture as far as suggesting, in the words of Tan Min, that "Chinese economic thought should be celebrated not only as a glorious achievement in its own right, but also as one of the main intellectual origins of modern political economy,"<sup>60</sup> the parallelism of *wu-wei* and physiocracy are striking and may have represented early foundations of global modern liberal supply-side economics and neoclassical economic thought. During the seventeenth and eighteenth centuries, intercontinental cultural transfer and intellectual cross-fertilization between Europe and China were quite intense. Ideas migrated and mutated over time, following idiosyncratic variation across space(s). Perhaps ancient Chinese thinkers deserve to be called, in Rothbard's words, "the world's first libertarians."<sup>61</sup> The French physiocratic view emphasized action by non-action in the marketplace, mirroring Eastern Confucian propositions of "ruler(s) who reign but do not rule" (*wu-wei*), something which may have considerably influenced government policies towards the economy in early modern China.<sup>62</sup> But there were also liberal influences in eighteenth-century mainstream European economic thought – that is mercantilism and cameralism. A prime example is Johann Wolfgang von Goethe, Germany's well-known poet, civil servant, and economist, who during his career as an individual and civil servant in the services of the Saxon dukes subscribed to four of the current (and competing) economic theories and "schools" of his age: cameralism, physiocracy, Smithian liberalism, and the emerging Older Historical School.<sup>63</sup> Before c. 1945, there was much more variation and compe-

57 See previous note.

58 Gerlach, *Wu-Wei in Europe*.

59 Hobson, *Eastern Origins*, p. 199; Menzel, *Sinophilism of J. H. G. Justi*; Adam, *Political Economy of J. H. G. Justi*. On Justi see also A. Wakefield, *The Disordered Police State: German Cameralism as Science and Practice*, Chicago, IL 2009, and E. S. Reinert (with K. Carpenter), *German Language Economic Bestsellers before 1850*. Also *Introducing Giovanni Botero as a Common Reference Point of Cameralism and Mercantilism*, in: Rössner (ed.), *Economic Reasons of State*, pp. 26–53.

60 T. Min, *The Chinese Origin of Physiocratic Economics*, in: C. Lin, T. Peach and W. Fang (eds.), *The History of Ancient Chinese Economic Thought*, London/New York 2014, pp. 82–97, at p. 97.

61 M. Rothbard, as quoted in Z. Madjid-Sadjadi, *China*, in: V. Barnett (ed.), *The Routledge Handbook of the History of Global Economic Thought*, Milton Park/New York 2015, pp. 294–395, at p. 297. See also Min, *Chinese Origin*, for an overview.

62 See also P. Vries, *Economic Reasons of State in Qing China: A Brief Comparative Overview*, in: Rössner (ed.), *Economic Reason of State*, pp. 214–219, with remarks on economic dogma, as well as Vries, *State, Economy and Great Divergence*, for a comparative overview on Chinese and English practices of state intervention in the economy, c. 1680–1850.

63 Far from being undecided – as the modern interpretation would see it, under the framework of culturally homog-



tition than unity in European and global economic thought, which has only recently moved towards global convergence by European-driven homogenization, in an almost cultural-imperialist manner across the world, apart from heterodox tendencies, which are also inclined to be disaggregative and manifold. The developing classical propositions laid the discursive foundations of the modern post-1800 market economy and, in many ways, the post-1945 international ideological economic order in the West.

But was the evolution of modern economic knowledge and the modern economy really *that* simple? There was another tradition or economic canon, once prominent but nowadays forgotten – a paradigm with a likewise ancient pedigree. We may call it “Renaissance economics” (E. Reinert), “mercantilism,” “Enlightenment economics” (S. Reinert), or “economic reason of state” (Weststejn & Hartman). It was prevalent in the writings of a sixteenth-century Italian Jesuit named Giovanni Botero (c. 1544–1617). But it may have had earlier roots in later medieval Europe, perhaps in the economic policies of the Upper Italian trading republics of Genoa and Venice – exactly these places in which, according to F. Braudel and Werner Sombart, modern capitalism first emerged. This was not a coincidence; the two developments were functionally related. Capitalism emerged in tandem with states and temporal authorities that wanted to be strong and economically interventionist.<sup>64</sup> It placed heavy emphasis on manufacturing with a call for a proactive state in the economic process in order to raise the nation’s wealth. Since the fourteenth-century Renaissance and well into the twenty-first century, this programme of state interventionism continued to make Europe rich.<sup>65</sup> And contrary to modern economic discourse that almost habitually stated that the opposite was best practice (markets always work best when left alone: the “invisible hand argument”), state interventionism (usefully measured by government expenditure as a share in GDP) continuously grew over the last millennium, from modest levels of 5 to 10 per cent for most European “states” around 1500 AD to 50 to 60 per cent of the GDP for the richer and developed nations of today’s world (2014). European economic practice in many ways represented a stark counterpoint to the *wu-wei* (China) or physiocrat (France) model of economy, which tended to emphasize agriculture and non-interference by rulers and governments as true or only sources of economic wealth. In practice, Europeans nearly constantly counteracted such theoretical propositions, even more so after c. 1800 AD, when liberal and “free-trade” ideas gained ground in the public debates whilst states’ economic policies became even more interventionist than pre-1800. This cognitive distortion is one of the striking paradoxes in recent European history.<sup>66</sup> They did so with

enized modern economics – Goethe acknowledged the practical virtues and theoretical insights of each of those competing “schools” in economics. See B. Scheffold, Goethe’s Economics. Between Cameralism and Liberalism, in: Rössner (ed.), *Economic Reasons of State*, pp. 79–100. On heterodox economics, see, for example, the Other Canon ([www.othercanon.org](http://www.othercanon.org)).

64 I am currently developing a book-length *longue durée* narrative on state interventionism and modern European growth, c. 1400–2000 AD.

65 Reinert, *Role of the State*; id., *How Rich Countries Got Rich*.

66 On the other hand, see F. Trentmann, *Free Trade Nation: Commerce, Consumption, and Civil Society in Modern Britain*, Oxford 2008.



manifestly obvious benefit. The first industrialization took place in Europe, not Asia, under considerable and strong support of proactive governments (post-1688 England and post-1800 Germany, Flanders/Belgium, Austria, Italy, Sweden, etc.). Asian governments on the hand were much leaner and fiscally weaker, and much less inclined to interfere in the economy during the early modern period, which may be an important factor in explaining economic divergence.<sup>67</sup>

As Peer Vries has recently noted:

*Qing rulers wanted to be considered “benevolent rulers” who confined themselves to “controlling from afar” and have a policy of wu-wei or rather wu-wei erzhi, literally: “Order and equilibrium will be achieved without the ruler’s intervention.” This principle even became the appropriate description of the ideal Confucian ruler: “One who reigns but does not rule.” In the prevailing political philosophy, government and administration should consist of a small group of people who broadly define the rules, competences and resorts and leave the actual implementation of policies and the details to those locally responsible. Local administrators in turn were supposed to delegate and take local conditions into consideration, which meant that at their level administration and politics were almost indistinguishable. Next to these ideological reasons for having “lean government” there also were practical ones: the realm was simply too big and too populous to be actually and effectively ruled from one central point. Trying to do so, on top of that, would mean that the ruling Manchus had to involve many more non-Manchus in their ruling, something they were not really keen on. Those who already had a job as official normally also were not fond of getting many new colleagues, never mind their “ethnicity.”<sup>68</sup>*

Thus, whilst the Chinese state may not necessarily have been as weak as it has occasionally been portrayed in the literature,<sup>69</sup> it nevertheless acted on a different rationale than European temporal authorities; a rationale, since the dawn of the early modern age, that emphasized the virtues of non-interference with the economy. Moreover, manufacturing – one of the key ingredients to modern European growth since the 1400s<sup>70</sup> – was deliberately neglected and seen as an inferior activity by the early modern Chinese emperors.<sup>71</sup> In both regards, European conditions were strikingly different since the Renaissance. Since the dawn of the early modern period, the emerging fiscal-military states of Europe placed an increasing emphasis on *taxing* their people, often related to the financing of wars, with the share of taxes in total economic activity significantly increasing from c. 1400 to 2000.<sup>72</sup> With this came an increased pressure to refine methods of collecting

67 Vries, *Nation, State and Great Divergence*.

68 Vries, *Economic Reasons of State in Qing China*, p. 210.

69 A point highlighted in Rosenthal / Bin Wong, *Before and Beyond Divergence*, ch. 7.

70 Reinert, *How Rich Countries Got Rich*, ch. 3.

71 Vries, *Economic Reasons of State in Qing China*, p. 208.

72 In some areas, such as the Upper Italian city republics or seventeenth-century England, this share may even have reached levels beyond 10 per cent; eighteenth-century Britain emerged at even higher levels, turning into the most highly taxed economy of its age. C. M. Cipolla, *Before the Industrial Revolution: European Society and Economy, 1000–1700*, New York 1979.

taxes as well as monitoring the tax base, something that is borne out of the increasingly refined sets of customs accounts, port books, national economic-demographic censuses (since mid-sixteenth century in Spain and Saxony), and emerging national trade statistics (since 1696 in England).<sup>73</sup> In this regard, many European states seem to have interacted much more directly and actively with the economy than the Chinese Empire.<sup>74</sup> Similarities to the Chinese pattern may be observed elsewhere. The Mughal emperors in India (post-1526), whilst actively interfering with the economy in certain sectors such as manufacturing and the running of state manufactories, also effectively carried out a more or less *laissez-faire* approach. Taxation levels were moderate, as was economic extortion in general.<sup>75</sup> By the late thirteenth and into the fourteenth century, cities such as Delhi numbered amongst the biggest urban conglomerates within the Muslim world. The sultanate was a sophisticated market economy and levels of taxation extracted from the native peasantry were large enough to keep a prodigious quantity of coin in circulation to make commerce flourish, both regionally as well as in terms of this region's integration into international trade circuits.<sup>76</sup> Whilst Muslim attitudes to commerce and economy are said to have been more relaxed in many ways than Western scholasticism, the same probably applies to Buddhism, which may in fact have taken a far more positive attitude towards business and economic behaviour than is usually acknowledged.<sup>77</sup> Clearly this was a different political economy and cultural perception of the economy, compared to post-1400 "mercantilist" Europe and the European fiscal-military state emerging after the Renaissance.<sup>78</sup> Ancient Indian thought often highlighted the duty of the rulers to promote the well-being of their common wealth through some active interference but mostly through moderate and fair levels of taxation.<sup>79</sup> Confucian economic thought evolved along similar lines – the lower the tax rate, the higher potential total tax yields.

73 For sixteenth-century Spain, see C. M. Cipolla, *Between two Cultures: An Introduction to Economic History*, New York 1991. For eighteenth-century Britain, see the discussion of the history of new state departments, as well as the types of records produced, in P. R. Rössner, *Scottish Trade in the Wake of Union (1700–1760). The Rise of a Warehouse Economy*, Stuttgart 2008, chps. 2 and 3.

74 Vries, *Nation, State and Great Divergence*.

75 T. Raychaudhuri, *The State and the Economy 1: The Mughal Empire*, in: Id. / I. Habib (eds.), *The Cambridge Economic History of India: Volume 1, c.1200–c.1750*, Cambridge 1982, pp. 172–192; P. Parthasarathi, *State Formation and Economic Growth in South Asia, 1600–1800*, in: Rössner (ed.), *Economic Reasons of State*, pp. 189–203.

76 See I. Habib, *Non-Agrarian Production and Urban Economy*, in *Cambridge Economic History of India Vol. I*, pp. 76–92, and S. Digby, *The Currency System*, in *ibid.*, pp. 93–101, and *id.*, *The Maritime Trade of India*, in: *ibid.*, pp. 125–162; Tirthankar Roy, *Capitalism in India in the Very Long Run*, in: L. Neal / J. G. Williamson (eds.), *The Cambridge History of Capitalism, Vol. 1: The Rise of Capitalism: From Ancient Origins to 1848*, Cambridge 2015, pp. 165–192.

77 A. K. Dasgupta, *A History of Indian Economic Thought*, London / New York 1993, pp. 13–21.

78 J. Glete, *War and the State in Early Modern Europe: Spain, the Dutch Republic, and Sweden as Fiscal-Military States, 1500–1660*, London / New York 2002; B. Yun Casalilla / P.-K. O'Brien (eds.), *The Rise of Fiscal States: A Global History, 1500–1914*, New York 2012; W. Reinhard, *Geschichte der Staatsgewalt: eine vergleichende Verfassungsgeschichte Europas von den Anfängen bis zur Gegenwart*, Munich 1999; M. L. van Crefeld, *The Rise and Decline of the State*, Cambridge 2000.

79 See, for example, B. Chandrasekaran, *India*, in: Barnett (ed.), *Handbook of the History of Global Economic Thought*, pp. 323–326, at pp. 324–325.

This can be considered supply-side economics not far from the “Laffer Curve.”<sup>80</sup> No German cameralist from the sixteenth to eighteenth century would have taken issue with any of these propositions: tax levels should be moderate and the market should be free, enabling entrepreneurship to flourish under a comparatively liberal political economy regime. Only a healthy and wealthy commercial population would make the state rich and its coffers full.<sup>81</sup> In India, the Mughal emperors may even have had a stronger inclination towards interfering with the economy than usually assumed. But the crucial difference to early modern mercantilist Europe was, in the words of Parthasarathi, that “the Mughal state cannot be said to have had a trade policy per se, save to encourage commerce.”<sup>82</sup> In Europe, on the other hand, import substitution and infant industry protection strategies were applied since the late fifteenth century.<sup>83</sup> Other scholars have noted the positive attitude in Buddhism towards economy and commerce as well as capital accumulation – an attitude more positive perhaps than most medieval European churchmen and sixteenth-century Protestants ever were. In contrast, some historians of economic thought have emphasized the lenient and laissez-faire attitude towards the market revealed in the writings of the later medieval European churchmen such as Bernardino di Siena.<sup>84</sup> Whilst Buddhist scholars knew market and price regulation for some commodities – as did the medieval European scholastics – they also favoured low taxation and generally seem to have exhibited a relatively relaxed attitude towards commerce and profit-making (as did some of the later medieval scholastics, for instance the monks of the mid-sixteenth-century Spanish School of Salamanca, which had been dubbed by some as the first supply-side and laissez-faire “economists”).<sup>85</sup>

Perhaps there was, after all, not so much variance in European-Asian economic *thought*, especially when it came to market regulation, wages, and prices for goods, but rather pronounced differences in terms of *applied economic policy*, especially relating to customs, tariffs, taxes, and trade restrictions, which were used in Europe as means of protecting and promoting infant industries since the late fifteenth century. Thus what people *do* within the economy may often differ from what they *think about* the economy. An interesting question would be why is it that modern capitalism – which according to Braudel and Sombart originated in Renaissance Italy – emerged in those areas of the world that

80 See also Madjid-Sadjadi, China, pp. 297–298, and M. Tao, Confucian Thought on the Free Economy, in Lin, Peach/Fang (eds.), History of Ancient Chinese Economic Thought, pp. 153–165.

81 J. Backhaus, Mercantilism and Cameralism: Two Very Different Variations on the Same Theme, in: Rössner (ed.), Economic Reasons of State, pp. 72–78.

82 Parthasarathi, State Formation and Economic Growth, p. 194.

83 E. Reinert, Role of the State.

84 Dasgupta, History of Indian Economic Thought, On the European Scholastic theologian-economists, see O. I. Langholm, The Legacy of Scholasticism in Economic Thought. Antecedents of Choice and Power, Cambridge 1998; D. Wood, Medieval Economic Thought, Cambridge 2002, esp. 132–152 and R. de Roover, Scholastic Economics: Survival and Lasting Influence from the Sixteenth Century to Adam Smith, in: The Quarterly Journal of Economics 69 (1955), pp. 161–190.

85 Dasgupta, History of Indian Economic Thought, ch. 3.

were perhaps more negatively inclined towards business, profit-making, and trading than China and India?

As usually, one needs to differentiate: as Langholm and others have shown, the medieval European churchmen (scholastics) were not at all opposed to commerce, free markets, and the business of profit-making. They were, in fact, especially during the later period, rather market liberal.<sup>86</sup> Only when business went over the top and profits exceeded the boundaries given by the notion of societal stability and the “common good” would the scholastic churchmen have seen the need to intervene (as do most modern economists, including the market radicals).<sup>87</sup> Perhaps global traditions in economic thought did not differ radically in terms of the basic parameters of economy and commerce. Another important caveat is that there always were – even nowadays – considerable differences between economic thought and economic practice. The early modern European societies provide an excellent example of this. Again the means of interfering with the economy may have been weaker here than in certain parts of contemporary Europe, especially post-1688 England. England, however, would have been the *Sonderweg*, as it had a significantly stronger state apparatus and thus much stronger means of effectively interfering with the economy than most other European states.<sup>88</sup> A good case study is exhibited by early modern Spain. As Regina Grafe has argued, in seventeenth- and eighteenth-century Habsburg Spain mercantilist ideas of domestic industry promotion and protection circulated widely and were actively promoted. The institutional structure of multiethnic and regionalized Spanish society, however, prevented the adoption of an economic policy at the national level that could have effectively promoted Spain’s common wealth in the way the mercantilist writers intended. Grafe argues that a strong tradition of political and institutional fragmentation created a bias towards promoting municipal policies at the expenses of the royal or national interest. This prevented Spain from fully unfolding the “good” mercantilist policies of infant industry protection and import substitution that would make post-1707 England and Scotland rich.<sup>89</sup> A similar example is provided by the Prussian case. As Burkhard Nolte has found, literally all measures aimed at creating a unified and uniform Prussian market under Frederick the Great (r. 1740–1786), using a mercantilist ratio of state-building, ultimately failed, as did attempts at effectively promoting domestic industry.<sup>90</sup> But does this mean that mercantilist ideas were wrong or ineffective? Or did they simply unfold in bad contexts? As Magnusson argues, Prussia managed after the 1830s to undergo one of the fastest transitions towards industrialization ever experienced in history – but only after basic restrictions on individual economic

86 See previous paragraph.

87 P. R. Rössner, Burying Money? The Monetary Origins and Afterlives of Martin Luther’s Reformation, in: *History of Political Economy* 48 (2016) 2, pp. 264–265.

88 W. J. Ashworth, *Customs and Excise. Trade, Production, and Consumption in England 1640–1845*, Oxford 2003; Parthasarathi, *Why Europe Grew Rich and Asia Did Not*, ch. 5; Reinert, *How Rich Countries Got Rich*, ch. 3.

89 For Scotland, see P. R. Rössner, Merchants, Mercantilism, and Economic Development. The Scottish Way, c. 1700–1815, in: *Annales Mercatorum*, 1 (2015) 1, pp. 97–126.

90 B. Nolte, *Zölle und Akzise im friderizianischen Preußen. Intention und Durchsetzung staatlicher Merkantilpolitik*, in M. Isenmann (ed.), *Merkantilismus. Wiederaufnahme einer Debatte*, Stuttgart 2014, pp. 197–221.

activity were lifted with the abolition of feudalism, villeinage, and the manorial system in the wake of the legal reforms of 1806 and 1811. The restrictive legal-institutional system in situ (which was called *Agrarverfassung* in German) – and not mercantilist, or cameralist, ideology – hindered economic entrepreneurship and the incentives of people to productively engage with the market. Many contemporary mercantilist and cameralist writers sought to redress and abolish this problem: arguably, the cameralist writers were as market liberal as the authors of modern “neoclassical” faith. In the early modern European view, markets were “unfree,” not because of the restrictive government policies but because there were restrictions posed on economic entrepreneurship by the unholy agrarian system of the manorial economy. This gave the individual “feudal” landlords the power of arbitrage, extraction, rent-seeking, and other forms of market distortion, at the expense of the common good. Only a strong ruler who would fight back these rent-seeking predators of the native nobility would make the market “free.”<sup>91</sup> Today, this “prince” or strong ruler has been replaced by the numerous regulations detailing how “perfect” markets (such as stock exchanges) should work – rules that are, in terms of numbers and differentiation, much more dirigiste and interventionist than any of the early modern cameralist and mercantilist European states ever were.

This cameralist vision of the “free market” is not at all far from modern concepts of “good” or “perfect” markets.<sup>92</sup> As Stiglitz argues in his most recent book, every government – for the pre-industrial period, we may replace the term “government” with “temporal authority” – has *some* economic policy, unwittingly or deliberate, of interfering with the economy.<sup>93</sup> But in order to create the “free market,” one usually needs a strong and proactive government together with well-designed strategies of intervention.<sup>94</sup> There are no governments that do not interfere with the economy as such; rather, it is the degree of interference – as well as the efficiency or lack of efficiency of economic regulation – that varies across countries and over time, which may explain differences in countries’ macroeconomic performance. The historical record seems to suggest that it was those countries and states with strong and interventionist governments that would, in the long run, witness the highest and most protracted trajectories of economic growth: post-1688 England; the US, Germany, Belgium, France, and some other continental latecomers in the nineteenth century; as well as post-1960 South Korea and China.<sup>95</sup> So, in fact, eco-

91 B. P. Priddat, Kameralismus als paradoxe Konzeption der gleichzeitigen Stärkung von Markt und Staat. Komplexe Theorielagen im deutschen 18. Jahrhundert, in: *Berichte zur Wissenschaftsgeschichte* 31 (2008), pp. 249–63; S. R. Epstein, *Freedom and Growth. The Rise of States and Markets in Europe, 1300–1750*, London 2000.

92 See also B. Harcourt, *The Illusion of Free Markets: Punishment and the Myth of Natural Order*, Cambridge, Ma 2011.

93 J. Stiglitz/B. Greenwald, *Creating a Learning Society: A New Approach to Growth, Development, and Social Progress*, New York 2014.

94 P. R. Rössner, Freie Märkte? Zur Konzeption von Konnektivität, Wettbewerb und Markt im vorklassischen Wirtschaftsdenken und die Lektionen aus der Geschichte, in: *Historische Zeitschrift* 303 (2016), pp. 349–392.

95 H.-J. Chang, *Kicking Away the Ladder: Development Strategy in Historical Perspective*, London 2003; Reinert, *How Rich Countries Got Rich*.

conomic ideas may be expected to make a historical difference, if occasionally unintendedly and within a duration of several centuries.

#### IV

Some of the inherent rationales of the European mercantilist-“economic reason of state” programme of political economy<sup>96</sup> may be explained by the idiosyncratic circumstances of the political, physical, and fiscal geography of early modern Germany – the region where “cameralism” flourished during the early modern period. In order to appreciate the rationale of such political economy, we need to turn our eyes upon the shiny white metal – silver. At the beginning of the early modern period, German silver mines, including, for matters of simplicity and historical sense, the mines in the Austrian Tyrol as well as Bohemian Joachimsthal, produced up to 80 per cent of Europe’s silver output.<sup>97</sup> Between 70 and 100 per cent of this silver was usually exported to southern, northern, and western Europe.<sup>98</sup> A lot of it then travelled around the African coast through the Indian Ocean to China.<sup>99</sup> When the newly discovered South and Central American silver mines began to yield significant quantities after the mid-sixteenth century, European supplies gradually decreased in relation to total world supply.<sup>100</sup> The German mines were crucial to the emerging world trade and global economy. Alongside Japan, which exported considerable quantities of silver to China, Europe was the world’s most important supplier of that metal. The great silver boom (1470–1620), which originated from the Central European mines, and after c. 1550 continued in American mines, coincided with the monetary expansion commonly known as the “Price Revolution.” This may have been a globally shared trajectory considering the fact that systems of monetary circulation and taxation in India and China switched to a silver standard about the same time (post-1470). In China, a silver currency was also reintroduced.<sup>101</sup> Lieberman has identified further “strange parallels” of economic development between India and Southeast Asia and Western Europe during this period.<sup>102</sup> The economic and social consequences of this

96 Rössner (ed.), *Economic Reasons of State*. The term is borrowed from Hartman and Weststejn.

97 E. Westermann, *Zur Silber- und Kupferproduktion Mitteleuropas vom 15. bis zum frühen 17. Jahrhundert*, in: *Der Anschnitt*, 5–6 (1986), pp. 187–211; most recent figures in J. Munro, *The Monetary Origins of the ‘Price Revolution’*, in: D. O. Flynn, A. Giráldez/R. von Glahn (eds.), *Global Connections and Monetary History, 1470–1800*, Aldershot / Burlington 2003, pp. 1–34.

98 P. R. Rössner, *Deflation – Devaluation – Rebellion. Geld im Zeitalter der Reformation*, Stuttgart 2012, chap. II.

99 A. G. Frank, *ReORIENT: Global Economy in the Asian Age*, Berkeley 1998; J. de Vries, *Connecting Europe and Asia: A Quantitative Analysis of the Cape-route Trade, 1497–1797*, in: Flynn, Giráldez/Glahn (eds.), *Global Connections and Monetary History*, pp. 35–106.

100 Munro, *Monetary Origins*, tables and discussion for the most recent comprehensive survey on European production.

101 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.

102 V. Lieberman, *Strange Parallels. Southeast Asia in Global Context, c. 800–1830*, Vol. 2: *Mainland Mirrors: Europe, Japan, China, South Asia, and the Islands*, Cambridge 2009, pp. 1–122; 560–561; 647–648.

increasing connectivity between these two world areas arguably also mark the beginnings of the Great Divergence. As silver was an important economic resource both in Asia, as well as Europe, especially for currency, global competition for this scarce resource increased.

During the early modern period, Germany, as well as many other continental European economies, faced repeated pressures on per capita silver supplies. These were the result of an emerging global trade and payments deficit. Between the early 1500s and the nineteenth century, public discourse repeatedly observed and commented upon a net decrease in German silver stocks. Undoubtedly, it is impossible to test this in quantitative terms because economic data on silver flows and stocks are as speculative as all the other economic data for the period concerning output, prices, interest rate, and employment. Notwithstanding, it probably matters much less whether contemporaries were “correct” in their observation of global silver flows (and chances are that they were), and much more that as a result of shifting silver balances a rich economic discourse arose in the German lands that believed in this silver drain – a discourse that represents the foundations of modern European political economy. This discourse has come to be known by various denominations and idiosyncratic shapes: sometimes as “monetarism” (a slightly unfortunate term because it is ambivalent<sup>103</sup>), “bullionism,” “mercantilism,” “Colbertism” (in France), or “cameralism” (in Germany).<sup>104</sup> Following recent collaborative research efforts, a new terminology is suggested here: “economic reason of state” theory. This economic discourse very often evolved around the question of resource management, and more specifically how to cope with structural shortages and/or a decline in supplies of silver per capita? Because silver was a general purpose means of payments, a shortage of silver usually equalled a structural negative balance of payment (gold after 1500 AD carried much less relevance in trade and the settling of balances and payments).

Around 1500, south-central German, Tyrolean, and Bohemian silver mines exported approximately 16 tons of pure silver per year via Lisbon, Venice, and Antwerp into the Baltic Sea region, Levant, Africa, India, and China.<sup>105</sup> This often equalled the entire annual output of the German mines. Global price differentials in the gold-silver ratio offered the merchants, companies and firms that were engaged in intercontinental trade considerable arbitrage opportunities, as silver fetched an increasing price the further east it went. When brought via Lisbon and the Cape of Good Hope to the Indian Ocean/Southeast Asian realms, its price, measured in terms of units of gold, increased by 50 per cent; it doubled in price when it had finally reached China.<sup>106</sup> German, as well as many

103 Monetarism also refers to a school of thought that sees changes in monetary stocks as a prime mover for changes in economic activity, including prices, incomes, and employment.

104 Rössner, Heckscher Reloaded?, and id., *New Inroads into Well-Known Territory*, for a new approach to rethinking cameralism in a European perspective.

105 Rössner, *Deflation – Devaluation – Rebellion*, ch. II.

106 D. O. Flynn/A. Giráldez, *Cycles of Silver: Global Economic Unity through the mid-18<sup>th</sup> Century*, in: M. A. Denzel (ed.), *From Commercial Communication to Commercial Integration. Middle Ages to 19<sup>th</sup> Century*, Stuttgart 2004, pp. 81–111, at p. 83.



other European, merchants rushed to export an increasing quantity of silver from west to east (c. 1500–1800), where it not only fetched a better price in terms of gold but also in terms of trade, especially measured in spices. The “Price Revolution,” or great expansion of population and total economic activity (per capita GDP stagnated), between 1470 and 1620 was paralleled by American silver imports, which allowed inflation to reign, allowing prices to rise in the face of a decrease of per capita economic resources in the real sector of the economy. Before the American silver arrived after the 1550s, however, the story was different from the usual neo-Malthusian model,<sup>107</sup> which is often used to “explain” the “Price Revolution.” The neo-Malthusian model predicts that a decreasing per capita supply of resources (mainly foodstuffs) leads to inflation in the general price level.<sup>108</sup> Between 1470 and 1530, population did indeed increase, whilst both per capita resources (goods) as well as silver supplies per capita (that is to say the amount of money in circulation per capita of the population) *decreased* in the German lands. But rather than causing inflation at that time, this situation led to *deflation* in the price level, and arguably a depression in terms of wages, living standards, and economic activity, something for which ample evidence has been collected and discussed elsewhere.<sup>109</sup> This suggests that a monetarist model may be a better explanation for some of the more significant macroeconomic trends in Germany in the early Reformation.<sup>110</sup>

The ramifications of this are much broader. Silver was the general means of payment in Germany. The Rhenish florin, or goldgulden, that is to say a gold coin from around 1500 AD usually containing 2.5 grams of pure gold, had by 1500 all but vanished from circulation. It was substituted following the 1480s by a large silver coin minted as the exact equivalent to one florin, or goldgulden. This was the *Groschen so einen Gulden* gilt (literally a “groat equalling one florin”). It attained its nickname of thaler (dollar) after the Bohemian mining town of Joachimsthal, called in contemporary colloquial language simply *Das Thal* (literally “the valley,” today Jáchymov in the Czech Republic), where large quantities of silver were discovered and minted into coin after 1516. The anchor currency, in the German lands the Reichsthaler, or rix-dollar, was now expressed in terms of silver rather than gold content.<sup>111</sup> Literally all coins – from the small change penny (Lat. *denarius*, d.) and heller (half-penny) to groats (groschen) and batzen (middle monetary layer or segment) up to large full-bodied silver coins equivalent to the Rhenish florin/gulden – contained at least *some* silver. Their price on the financial (exchange rate against other coins and currencies) and goods market (i.e., measured in terms of basket

107 W. Abel, *Agrarkrisen und Agrarkonjunktur in Mitteleuropa vom 13. bis zum 19. Jahrhundert*, 3rd ed. [Berlin 1935] Hamburg/Berlin 1978; id., *Agricultural Fluctuations in Europe from the Thirteenth to the Twentieth Centuries*, London 1980.

108 Abel, *Agrarkrisen*.

109 Rössner, *Deflation – Devaluation – Rebellion*, pp. 97–310.

110 *Ibid.*, pp. 166–250.

111 B. Sprenger, *Das Geld der Deutschen. Geldgeschichte Deutschlands von den Anfängen bis zur Gegenwart*, 3rd ed., Paderborn et al. 2003, ch. 7; M. North, *Das Geld und seine Geschichte. Vom Mittelalter bis zur Gegenwart*, Munich 1994, pp. 71–86.



of consumables one particular coin would buy) was determined by the coins' intrinsic value, that is to say their silver content.<sup>112</sup>

The problems faced by Europeans in the light of a structurally negative balance of silver/ payments may be framed using the simple Fisher equation, which tells us that monetary mass, defined by the product of the amount of money in circulation multiplied by its velocity, must equal total economic activity (or transaction volume), measured in monetary terms, multiplied by the price level (which is a dimensionless variable).<sup>113</sup> Because the non-monetized and non-market sector within the economies of sixteenth-century Europe may have been substantial, using the Fisher equation poses a certain epistemic risk to the historian. Nevertheless, the Fisher equation illustrates some of the broader and general implications of silver mining trends and possible economic consequences. With per capita silver money supply declining in per capita terms in the German lands (c. 1490–1540) and a declining velocity<sup>114</sup> vis-à-vis an increasing total population, prices in the market economy may be expected to decline. Indeed, available grain price data show deflation between 1490 and 1530 as well as around 1620 (the period immediately preceding the Thirty Years' War and the "Kipper and Wipper" hyperinflation from 1619 to 1623) and, again, during the years around 1660 to 1680 (wars against the Turks, sometimes nicknamed as the "second Kipper inflation"). These times were phases of monetary contraction, occasionally with bad harvests, bad sales for artisans and merchants, stagnant real wages, and decreases in public expenditure – times we would associate with the modern notion of depression.<sup>115</sup> Silver resources became scarcer, whilst outflows to the Baltic Sea region and Asia were pronounced. Curiously, these were also times when European discourse produced some of the finest works regarding economic analysis.

"One cannot deny that buying and selling are necessary," Luther stated in his great economic treatise of 1524: *Von Kauffshandlung vnd Wucher* (On Commerce and Usury)<sup>116</sup>. Additionally, he declared, "Lest every member of society be entirely self-sufficient, commerce is necessary." But Luther found the emerging global trades of his day disturbing. He called them *ausländische kauffs handel* (literally "foreign trade"), "which bring from Calicut, India, and such places, wares such as costly silks, gold-work and spices, which minister only to luxury and serve no useful purpose, and which drain away the wealth of land and people."<sup>117</sup> This concept of superfluous luxury is something Luther shared

112 Rössner, Deflation – Devaluation – Rebellion, ch. III.

113 Mathematically this can be written, in its simplest form, as  $MV=PT$ .

114 See Rössner, Deflation – Devaluation – Rebellion.

115 For example, G. Parker, *Global Crisis: War, Climate Change and Catastrophe in the Seventeenth Century*, New Haven 2013; J. de Vries, *Economy of Europe in Age of Crisis*, Cambridge 1976; P. R. Rössner, *The Crisis of the Reformation (1517): Monetary and Economic Dimensions of a Change in Paradigm*, in the Proceedings of the International Conference / Settimane di Studio of the Istituto Datini in Prato/Florence (2016), pp. 259–285 (in print).

116 The American translators of Luther's works chose *On Trading and Usury*, which is the commonly known title of the pamphlet in the English-speaking world. This is a rather unfortunate translation. A new translated and commented edition of Luther's *Von Kauffshandlung vnd Wucher* (1524) is to be found in Rössner, *Martin Luther on Commerce and Usury*.

117 Rössner, *Martin Luther on Commerce and Usury*, p. 176.

with the early English, Italian, and German mercantilists and cameralist authors up until the seventeenth century. Thereafter, later mercantilists developed a more relaxed outlook on the virtues of luxury imports, acknowledging that particular (bilateral) trade balances may be negative and that luxury goods allowed into the country – as long as they could not be produced domestically and as long as the *aggregate* or overall balance of trade remained positive, that is to say as long as there were other branches of the domestic economy that counterbalanced luxury imports – could cause a positive or net-influx of bullion. Luther went on:

*We have to throw our gold and silver into foreign lands and make the whole world rich while we ourselves remain beggars. England would have less gold if Germany let it keep its cloth, and the king of Portugal, too, would have less if we let him keep his spices. Calculate yourself how much gold is taken out of Germany, without need or reason, from a single Frankfurt fair, and you will wonder how it happens that there is a heller left in German lands. Frankfurt is the gold and silver sink through which everything that springs and grows, is minted or coined here, flows out of Germany. If that hole were stopped up we should not now have to listen to the complaint that there are debts everywhere and no money; that all lands and cities are burdened with charges and ruined with interest payments.*<sup>118</sup>

Alongside Wall Street and Canary Wharf, the Frankfurt Börsenviertel still ranks amongst the top financial places of the world, much more so now than during Luther's times. It is the very last sentence that is much less frequently cited in the literature than the others. Luther seemed to hint here at a situation of monetary contraction, that is to say an economic depression, which would explain why interest rates (if his observations were correct) were increased in the first two decades of the sixteenth century. Luther made similar remarks in his *Address to the Christian Nobility* (1520). In the same year that his *Kauffshandlung* appeared in print, a small book was published by popular preacher Johann Eberlin von Günzburg entitled *Mich wundert, dass kein Gelt ihm land ist. Ein schimpfflich doch vnschedlich gesprech dreyer landtfarer vber yetz gemelten tyttel* (I Wonder Why there is so Little Money in Germany, 1524). Ulrich von Hutten and the Imperial Knights (*Reichsritterschaft*), highly educated but impoverished robber barons (*Raubritter*) who played an important role in the early Reformation public discourse, sung a similar tune. In his *Vadiscus* dialogue (1519/20) of the "Romans" (meaning the Curia/Papal Court), von Hutten reported how Rome devised new means of "taking away" money from the Germans day after day. It was three things in particular, von Hutten wrote, that anyone returning from Rome would bring: "bad conscience, an upset stomach, an empty purse." And there were three things everyone at Rome desired: "short Mass, good coins, having a good time."<sup>119</sup> Here von Hutten hinted at what has become colloquially

118 Rössner, Martin Luther on Commerce and Usury, p. 176.

119 R. Bentzinger (ed.), *Die Wahrheit muß ans Licht! Dialoge aus der Zeit der Reformation*, Leipzig 1982, p. 46, 52, 72–74.

known as Gresham's law (spontaneous debasement). If coins of differing amounts of precious metal circulate alongside each other at the same denomination level (for example, groschen or batzen) and same face value, rational actors will separate out the good money and replace it with bad money in domestic circulation. The bad money remained in Germany according to von Hutten. Recent empirical research has demonstrated that this caused significant imbalances and asymmetries in payment and social relations, contributing to popular unrest and revolts, especially during the many peasant wars that shook the German lands between the 1450s and the mid-1520s.<sup>120</sup>

Because luxurious clothes usually had to be imported, often from Italy, as Luther himself pointed out in his piece *Italian Clothing is Better Than German* (1538), possible multiplying effects of the spending on domestic manufactures for consumption were foregone.<sup>121</sup> The debates of the Imperial Diet in Nuremberg in 1522 recognized the importance of the export of (good) money, as did the Imperial Knights in 1523 in their complaints.<sup>122</sup> In this apparent "fear of goods" (Heckscher), Luther and his contemporaries thus followed a rather common bullionist stance, which had developed in Europe since the fourteenth century.<sup>123</sup> But Luther also made an interesting reference here to the question of development by using England for comparison. England had achieved an improvement in its export position regarding manufactures only very late (after the 1470s), and partly due to a new economic policy or outlook, manifested in Henry VII's customs reform in 1485.<sup>124</sup> By the early sixteenth century, the process of substituting manufactured woollen cloth for raw wool exports was already in full swing. It arguably laid the foundations for later export-led growth in the English (and Scottish) textile industries, not only within the woollen sector but also in the linen and more importantly the cotton industry of the late seventeenth and eighteenth centuries.<sup>125</sup> Luther may have been aware here, if implicitly, that it would be desirable to have a native cloth industry with a positive net contribution to exports. But in the present example, he did not distinguish between spice imports from Portugal (something which did Portugal no great service, as spices were primary/unprocessed goods with little potential for adding value), and cloth imports from England (which were manufactured and thus carried a higher share of value added in the final price and thus *could* have made a positive contribution to sustained and balanced growth and development in England<sup>126</sup>). Philip Wilhelm von Hörnigk (1640–1714), who seemed to have developed the idea of relative underdevelop-

120 Rössner, Deflation – Devaluation – Rebellion.

121 Luther's Works, ed. H. Lehmann, Vol. 54, Table Talk, ed. T. G. Tappert 1967, Nr. 3956, p. 298.

122 G. v. Schmoller, Zur Geschichte der national-ökonomischen Ansichten in Deutschland während der Reformations-Periode, Zeitschrift für Gesamte Staatswissenschaft 16 (1860), pp. 461–716, at pp. 635–638.

123 Wood, Medieval Economic Thought, pp. 125–131.

124 Reinert, How Rich Countries Got Rich, ch. 3.

125 Parthasarathi, Why Europe Grew Rich, ch. 5.

126 This is because the elasticity of demand for manufactured goods is higher than for primary goods (such as food or raw material inputs). Therefore, if incomes increase, manufactures will attain a share in increased demand that is proportionally larger than demand for foodstuffs and primary goods. Thus manufactured exports have a higher growth potential than primary exports.

ment in more detail in his *Oesterreich über alles, wann es nur will* (Austria Over All, If She Only Will, 1684), picked up on this, writing what would become one of Europe's best-selling economics books prior to Adam Smith's *Wealth of Nations* (1776). In this classic pamphlet, Hörnigk identified a "small divergence" within Europe following the late Middle Ages. Whilst beginning with the now age-old lament about the loss of specie and foreign exchange of luxury imports, especially to France,<sup>127</sup> he said that in about a hundred years or so France, England, and the Netherlands would have stood at about the same level of wealth as the German lands. But after that date, Germany had lost out compared to the other three. This would place the reference point for Hörnigk's small divergence somewhere in the middle of the sixteenth century.

Hörnigk's observation ties in with modern research on European urbanization and real wages. In the longer run, throughout the sixteenth, seventeenth, and eighteenth centuries, the German lands became progressively less developed than the Atlantic fringe and the Italian sea ports. The latter had been the richest and economically most advanced region of Europe in the Middle Ages. Contemporary discourse and economic legislation picked up on this continuously, leading to a deep history of European understanding and theories of state intervention in the economy that would make Europe grow rich eventually, certainly in the post-1800 period, but with an important pretext from the period between the 1400s and 1800s.<sup>128</sup> The German *Reichspolizeyordnung* (Imperial Economic Policy Ordinance) of 1548 issued a general export ban on raw wool, as well as the admonition to "wear only domestically manufactured cloth." The Imperial Resolution of 1555 sounded similar.<sup>129</sup> To what extent such laws and edicts could be enforced effectively is another matter; the evidence suggests that they were largely unenforceable. What matters, however, is that people had *thought* and would continue to think – in Germany's case well into the late nineteenth century – along these lines since very early times. Luther and the contemporaries, in the wake of the imperial reform movement (*Reichsreform*), which included a whole range of economic policy matters such as currency, cloth production, trade policy, and restrictions to be coordinated or regulated on the imperial level, entertained the idea of *relative wealth* fluctuating both over time (economic growth) as well as varying across a spectrum of countries (relative economic development). They also entertained the notion (Luther less so) that the state could, and should, do something to alter the process. Luther and his contemporaries were first-hand witnesses of the beginnings of Europe's "small divergence" in the early modern age; they were the first to develop processes meant to relieve this state of underdevelop-

127 Philipp Wilhelm von Hörnigk, *Österreich ueber alles wann es nur will*, Nuremberg 1684, pp. 18–19. See the new edition: Philipp Robinson Rössner (ed.), *Austria SUPREME (if It So Wishes): A Strategy for European Economic Supremacy* (1684), transl. Keith Tribe. With a book-length introduction (c. 100 pp) by P. R. Rössner, London / New York, expected 2016).

128 Reinert, *How Rich Countries Got Rich; Role of the State*.

129 Schmoller, *Zur Geschichte der national-ökonomischen Ansichten*, p. 650s; F. Blauch, *Die Reichsmonopolgesetzgebung im Zeitalter Karls V. Ihre ordnungspolitische Problematik*, Stuttgart 1967, pp. 17–37; F. Blauch, *Die Wirtschaftspolitik des Reichstags im Heiligen Römischen Reich. Ein Beitrag zur Problemgeschichte wirtschaftlichen Gestaltens*, Stuttgart 1970, pp. 135–153.

ment. It is a well-known fact that cameralists – especially Johann Heinrich Gottlob Justi (1717–1771) to the nineteenth-century German Historical School in Economics from Friedrich List (1789–1846) to Gustav Schmoller (1838–1917) and Wilhelm Roscher (1817–1894), developed a fine set of axioms and full theory about economic development within or starting out from a context of underdevelopment.<sup>130</sup> They need no further discussion here as enough has been said on this.<sup>131</sup> But it is important to know that these discourses of the nineteenth century – most famously laid out in Friedrich List’s *National System of Political Economy* (1841) – had a “deep history” that dates back to the Renaissance. Clearly, development early on was on the people’s mind and more specifically it was related to a changing monetary supply (silver) and possible strategies of alleviating or solving the “silver” problem. European economic reasoning firmly rested upon this “hunger” for silver. Were its aggressive mercantilist policies based upon this rationale as well? And were these also causes for global economic divergence?

## V

This takes us back, by way of a conclusion, to the more global dimension. Following the sixteenth century, central European economies suffered from a pressure on silver balances. A lot of the German silver went to Asia and ultimately India and China, but also into the Baltic Sea region and the Levant. Mercantilist or bullionist discourse was essentially concerned with managing the balance of payment. It was, from its axiomatic base, essentially a reactive rather than proactive political economy, a reaction to ongoing processes (balance of payment constraints, sometimes underdevelopment vis-à-vis others that fared better) and an increase in the global connectivity and imports from Asia. Mercantilism could – and would – later on be turned into a proactive strategy by actively encouraging the development of export-orientated industries, protectionist measures, and strategies aimed at increasing knowledge transfer, innovation, and the adding of value to the domestic economy. Mercantilist policy apparently did just that for eighteenth-century England,<sup>132</sup> but it is frequently overlooked that the origins of mercantilism are much earlier, emerging from a distinct “fear of goods” that was essentially a fear of silver flowing out of the country. German economic authors since the times of Luther had every reason to be fearful of that: too much of an outflow of silver created too many problems, both economic as well as social. China and Europe developed distinct systems of political economy. The Europeans were more mercantilist and bellicose, emphasizing international trade (which could be a source of fiscal revenue) and political rivalry as a means to get rich, whilst the Chinese economic discourse placed more emphasis on

130 Reinert, *How Rich Countries Got Rich*.

131 Ibid.; J. Burkhardt and B. Priddat, *Geschichte der Ökonomie*, Frankfurt-on-the-Main 2009; K. Tribe, *Strategies of Economic Order: German Economic Discourse, 1750–1950*, Cambridge 1995, introduction.

132 For example, Chang, *Kicking Away the Ladder*; Parthasarathi, *Why Europe Grew Rich*; Vries, *Nation, State and Great Divergence*.

domestic stability, grain storage, and stable agrarian development. Europe's "silver problem" since the age of Martin Luther may have played a role in these global variances of political economy.

Thus whilst in many ways European and Asian economic thought may have shown similarity over the past millennium, it was in the realm of economic policy where divergence becomes most notable, and this divergence may, on the European side, have been informed or triggered by a notorious fear – the fear of a silver outflow but also different ways of thinking about the state and the economy. The silver fear gave rise to useful and beneficial policies of import substitution, infant industry protection, and the promotion of value-added activities across the entire economic spectrum. These policies would, perhaps, make Europe grow rich, whilst Asia did not. Perhaps these policies were derived from a new cultural perception of the future as a manageable entity which seems to have engrained itself firmly into the European mind since the 1600s.

Therefore it is beneficial to look at ideas in the same way as politics, institutions, geography, and resource endowment when it comes to explaining processes of global economic divergence over the last 500 years.<sup>133</sup> Apart from economic ideas and theories, different contexts have also mattered considerably. Future research may therefore want to address the following questions. (1) Which role did ideas play in the process of economic growth and global economic divergence over the last millennium? (2) To what extent did theories of economic growth and development overlap or diverge? (3) Why did they often notably diverge from economic practice? To what extent can modern economic thought be ascribed to "cognitive dissonance" (Boldizzoni)?<sup>134</sup> (4) Eastern and Western economic paradigms seem worlds apart and yet, were they really? What about other Chinese political-economic and legal-philosophical paradigms, such as legalism (which emphasized the need for states to support manufacturing and industrial production)? What about learning processes, travelling ideas, and cultural transfer between East and West?

133 Parthasarathi, *Why Europe Grew Rich and Asia Did Not*.

134 F. Boldizzoni, *The Domestication of the Economic Mind: A Response to the Critics*, in: *Investigaciones de Historia Económica – Economic History Research* 9 (2013), pp. 71–74.

# **Divergence Displaced: Patterns of Economic and Political Change in Early Modern and Modern Global History**

**R. Bin Wong**

## **ABSTRACT**

Der Autor erörtert die laufenden Debatten zur Great Divergence und die Schwierigkeiten, eine überzeugende komparatistische Strategie zu formulieren, die die einfache Opposition von zwei Fällen überwindet und das Problem der Falsifizierbarkeit adressiert. Weder sei es weiterführend, alles an einer europäischen Entwicklung zu messen, noch die englische Erfahrung mit der Gesamtheit Westeuropas in eins zu setzen. Um falsifizierbare Aussagen treffen zu können, gelte es etwa die Organisation des Fernhandels und seine Institutionalisierung in China und Europa zu vergleichen. Als zentrale Differenz arbeitet der Autor heraus, dass China ein einheitlicher Staat war, Europa dagegen politische Fragmentierung erlebte, die wiederum Krieg und den daraus folgenden Bedarf an Kapital und Arbeit in den Städten sowie eine dringendere Nachfrage nach Technologien nach sich zog. Im Unterschied zu Pomeranz sieht er hier die Ursprünge modernen Wirtschaftswachstums. Allerdings spielte für die britische Baumwollindustrie die Beziehungen nach Asien und Nordamerika eine wichtige Rolle, da die britische Industrie mit den niedrigen Arbeitskosten in Indien konkurrieren musste, woraus sich der rigorose Merkantilismus ergab, der positive, wenn auch unintendierte Effekte für das Wirtschaftswachstum hatte. Allerdings zeigt der Vergleich, dass andere Weltregionen vor anderen Problemen standen und mit anderen Kombinationen von Wissen und Problemlösungen reagierten. Dies führt zur Unterscheidung zwischen der Great Divergence als einem spezifischen Phänomen und einer allgemeinen Transformation, womit vermieden werden könne, in die Falle der Frage zu tappen, warum China nicht Großbritannien geworden sei.

## The Great Divergence Debate and the Problem of Non-Falsifiability

Europe's early modern era formation of states and modern era development of an industrial capitalist economy have long been recognized as processes substantially shaping the contours and content of global history since 1500, both in terms of what they represented as well as what they caused. A half century ago modern world history still followed the basic templates which Marx, Weber, and other major intellectuals of the late nineteenth and early twentieth centuries cut and left for others to fill in. Europe was the center of political and economic dynamism which spread to other world regions, both as a force to incorporate others into a European centered political and economic system and as a model of wealth and power from which others sought to learn. In the intervening half century since the mid-1960s, global history has emerged as a field producing and integrating knowledge on non-European world regions and viewing their connections to the West from multiple perspectives. Along the way many scholars have advocated forsaking the conventional understandings of an earlier era but without working very explicitly and effectively on a template according to which future research could be designed and constructed, well aware that complexity and variation made any such patterns far harder to trace than those based largely on European experiences alone. As a result, the old templates remain those to which some scholars turn, while others go about creating alternatives without clear patterns. This essay suggests components of a possible template to work on in order to direct at least some of our future efforts toward replacing the shop-worn templates with which we have worked in the past. It goes on to contrast the approach that it advocates with a recent example of the kind of scholarship that reproduces perennial non-falsifiable propositions in an extreme manner that manages to consider much new scholarship on Chinese economic and political history but only in a manner to show how the late imperial Chinese empire in general failed to become the modern British national state and in particular did not foster modern economic growth in the manner attributed by the author to the early modern British state.

It is hard to imagine any future template that is unrelated to the ones which we have been using. But rather than simply add new (and important, to be sure) concerns such as the Anthropocene as the site for pondering the interactions with and impacts on human beings with both our social and natural environments, global history also refines understanding of subjects already present in our studies and suggests the ways we should pursue them in our future research. This article offers an assessment of where historians are in understanding what Kenneth Pomeranz famously called the 'great divergence' in his 2000 book of the same name. It draws a sharp contrast between what can be considered useful and destructive approaches to issues of the origins of the modern world economy and modern economic growth.

We now study new subjects on a global scale. Some of these topics grow out of studies initially done of early modern Europe. Geoffrey Parker's *Global Crisis: War, Climate Change and Catastrophe*, for instance, is a prize-winning work that combines an assessment of a subject on which he is famous for in his earlier work, the military revolution



in Europe, with the issues of climate change, an issue of great interest in more recent years. The two are joined to reframe and infuse a subject initially conceived as an early modern European problem, the seventeenth-century crisis, seen by many European historians during the 1960s and 1970s as a political crisis but conceived initially by E. J. Hobsbawm in his 1954 *Past and Present* essay as an economic crisis of feudalism from which commercial capitalism would emerge. The last chapter of *Global Crisis* preceding the conclusion can be considered a restatement of a long-standing alternative to Hobsbawm's analysis, one that sees the sources of Europe's economic changes to lie in intellectual traits of European societies and their associated social and political contexts. This invoking of intellectual, social, and political features of early modern Europe are summoned to respond to Kenneth Pomeranz's explanation of the great divergence, a subject as the subtitle of Pomeranz's book claims, is about "the making of the modern world economy." The book is a more global view of what European historians of a Marxist persuasion viewed as the transition from feudalism to capitalism and to which their non-Marxist colleagues responded, much as Parker did to Pomeranz's work, by noting the historically distinctive character of European intellectual life, social organization and political institutions. The similarity of the responses to both Hobsbawm and Pomeranz does not mean of course that Pomeranz is a Marxist or channeling the spirit of Eric Hobsbawm in his scholarship. Pomeranz does in fact resonate in part with a different strain in Marxist historiography of the development of capitalism, one that stresses the relationships with other world regions forged by European powers and their elites; he highlights the significance of colonies and cotton, together with the location of coal, to explain the "great divergence". He arrives at his account for the origins of the modern world economy after rejecting numerous putative differences between the early modern Chinese and European economies, some of which had been raised in by previous scholarship, including my *China Transformed*.

Pomeranz highlights two aspects of British history, one about Britain itself and one linking Britain to another world region. The former is a feature of the natural environment and the latter is a product of British politics. Coal, needed for the new engines that would power the industrial revolution, was located in places from which it was easy move to the new factories. North American colonies were made up of both white settlers and in the south by African slaves who worked the land, and most importantly for the subsequent industrial revolution, planted cotton to be used by British factories to produce textiles that satisfied first domestic demand and then went on to be sold in Europe and in other world regions. The combination of the location of coal and formation of white settler economies and colonies depending on African slave labor were particular to Britain. In contrast, Parker in *Global Crisis* cites factors particular to Europe – the crisis of the universities, new kinds of learning, and responses to politically and religiously imposed limitations on intellectual enquiry and belief. What these two suggested explanations share is non-falsifiability. We have no way of proving how important any of these features were because they are particular to Britain and Western Europe respectively. Even when we trace plausible consequences from each that enabled industrial development we have

a hard time proving they were necessary in the sense of being the only conditions that could create an industrial revolution.

Precisely because other areas lacked these traits as well as many other traits found in Europe or in Britain specifically it is hard to know if they were necessary to create the industrial revolution. For instance, when my colleague and Marxist historian Robert Brenner famously argues that it is the absence of the agrarian social relations found in Britain that prevented the emergence of capitalism elsewhere, the importance of the system through which he sees the modern world economy emerging cannot be falsified because the enclosures and the social relations that attended them were particular to Britain.<sup>1</sup> Like Pomeranz's coal and colonies, Brenner cites traits that are British and not present elsewhere. To explain why Britain was the site of the industrial revolution which in turn was basic to the formation of the modern world economy may well invoke the traits each of these scholar highlights. Certainly they can be tied in closely with a narrative of other specific changes taking place in the English economy. What we cannot say from such explanations however is whether there could (or would) have been an industrial revolution elsewhere where such traits were lacking had Britain not first had its industrial revolution. Here, Parker's invoking of traits particular to Western Europe suggests the possibility that the industrial revolution would have been Western European even if not specifically British. Such a claim runs into the same problem of non-falsifiability but at least allows for a larger number of potential sites for the beginning of modern economic change because Parker is noting the absence of a scientific revolution and the immediate social and political contexts outside of Europe.

Historians make comparisons in which a certain case is typically taken as the standard with which others are compared. When differences among cases are very few, as is the case for the examples assembled by Jared Diamond and James Robinson in *Natural Experiments of History*, the significance of differences can be more directly and clearly posed. But Diamond and Robinson are very carefully curating their examples to fit strict criteria of comparability. Major questions posed by historians comparing economic and political experiences of many countries cannot possibly meet their standards. We are therefore left with an easy conflation of a particular country's experiences as the standard against which to measure others without knowing how the multiple causal mechanisms in the standard case are themselves entwined (beyond a certain kind of empirical description) and whether the particular causal mechanisms at work in the standard case are general or only an example of a more general causal relationship that can be satisfied in different ways. The outcome of these two conditions produces the problem of non-falsifiability built into comparisons of large-scale historical experiences – the problem has been present since Marx and Weber and limits our success to date in displacing their powerful and persuasive frames of reference even as generations of scholars have undermined many of their empirical claims.

1 T. H. Aston and C. H. E. Philpin (eds.), *The Brenner Debate: Agrarian Class Structure and Economic Development in Pre-industrial Europe*, Cambridge 1987.

The types of explanations for modern economic growth that are at the heart of the origins of the modern world economy offered by most all historians, including the famous and distinguished ones just cited above, are non-falsifiable, which is why they can be perennially promoted and equally frequently disputed. Note, any of the highlighted phenomena—coal and colonies, the scientific revolution, or English agrarian social relations—could in fact be factors helping to explain the industrial revolution. Pomeranz's factors have the virtue of being more concrete and specific than the others and we can therefore imagine counterfactuals in which either convenient coal or cotton producing (former) colonies are absent. Neither the scientific revolution nor agrarian social relations lend themselves very easily to counterfactual exercises because they are themselves so large and encompassing. They in fact can easily be slotted into evaluations of the onset of modern economic growth that do little more than observe every feature of British social and political life that was present when industrialization began and ascribe some unspecified importance to such straits. At the extreme we end up with a claim that the industrial revolution was British and the British made the industrial revolution. We are left unable to distinguish what we think were reasons for the great divergence that are distinct from the manner in which British practices led to an industrial revolution.

### **British Practices and Other Paths toward Modern Industrialization**

The distinction between a narrative highlighting some subset of British features versus an explanation that offers some account of factors that vary in ways that affect the likelihood of industrialization matters because without an explanation of the latter sort, we are left assuming that the only path to modern economic growth was the one taken by Britain. It can lead to assumptions about British practices as the only ones that could have created an industrial revolution without recognizing the possibility that other institutional formats might have served some of the purposes required for an industrial revolution. The literature on French financial institutions, for instance, shows sets of practices quite different from those in Britain, but certainly able to mobilize and deploy capital in the early modern era.<sup>2</sup> As we move forward to the nineteenth century we see the development of banking systems and financial markets that share features but also have distinct attributes. Late nineteenth-century British, French, and German banking systems for instance were quite distinct from each other. The relationship between firms and banks in Germany, for instance, was much closer and more direct than was the case in Britain; the French system continued to contain notaries as it developed banks; and the adoption of the corporation form also varied among common law and code-based legal systems.<sup>3</sup>

2 Ph. Hoffman, Gilles Postel-Vinay, and Jean-Laurent Rosenthal, *Priceless Markets: The Political Economy of Credit in Paris, 1660–1870*, Chicago 2000.

3 T. Guinnane, *Delegated Monitors, Large and Small: Germany's Banking System, 1800–1914*, in: *Journal of Economic Literature* 40 (2002) 1, pp. 73–124; id., R. Harris, N. Lamoreaux, and J.-L. Rosenthal, *Putting the Corporation in its Place*, in: NBER working paper 13109 (2007); Ph. Hoffman, G. Postel-Vinay, and J.-L. Rosenthal, *Entry, informa-*

What is desirable in terms of financial institutions varies according to contexts that include both earlier practices and the scales of capital required for industries, an amount that grows with the development of heavy industries in the second half of the nineteenth century. Much as variation is accepted in Europe, when scholars turn to comparisons of China with Europe, the Europe selected has been for earlier scholarship and remains for a few scholars today Britain and the differences between China and Britain are taken to be reasons for why China did not develop economically.

Pomeranz starts from an assessment of some similarities between Western Europe and China in order to establish a baseline from which significant differences might be identified; his “coal and colonies” highlights conditions in Britain very different from those in China’s most economically advanced areas. The much shorter enquiry into economic change in *China Transformed* followed the same method to highlight demographic similarities, the presence of well-developed long-distance commerce and rural industries in both China and Europe. The cluster of differences stressed in that work concerned European science and technologies, and in particular, following arguments of E. A. Wrigley, the importance of technologies to exploit coal as a source of energy to power new kinds of machines.<sup>4</sup> The explanations proffered by Pomeranz and myself had the virtues, I would argue, of having identified rather specific proximate causes for industrialization. We evaluated differences we thought not consequential for divergence as well as propose a variety of similarities not previously appreciated.<sup>5</sup> Such statements do not mean the different practices in China would or could have caused modern economic growth, only that they could have been combined with or changed into institutions that would support industrialization and modern economic growth without becoming like those in Great Britain – a phenomenon we already know to have been the case in continental Europe. The importance of the continental European work is that it was done on countries that by the end of the nineteenth century were clearly on paths of industrialization and modern economic growth. The challenge of much of the work to date on China is to suggest that such a possibility might also exist for some non-Western cases as well. One challenge to future work on China will be to test whether or not such causal linkages can be found.

### Formulating Comparisons with Falsifiable Propositions

For present purposes I move on to consider how falsifiable propositions can be formulated as the first step toward constructing social theories and historical explanations that

tion, and financial development: A century of competition between French banks and notaries, in: *Explorations in Economic History* 55 (2015), pp. 39–57.

4 E. A. Wrigley, *Continuity, Chance, and Change: The Character of the Industrial Revolution in England*.

5 K. Pomeranz, *The Great Divergence: China, Europe and the Making of the Modern World Economy*, Princeton 2000; J.-L. Rosenthal/R. Bin Wong, *Before and Beyond Divergence: The Politics of Economic Change in China and Europe*, Cambridge 2011; R. Bin Wong, *China Transformed: Historical Change and the Limits of European Experience*, Ithaca 1997.

more adequately assess the interplay of different causal chains in making modern states and economies. The shift in focus I want to stress begins by recognizing that Pomeranz's *The Great Divergence*, as well as my *China Transformed*, shared with other explanations mentioned above the basic problem of non-falsifiability. The great divergence debate continues the terms of engagement set by older approaches to historical change that have set out to explain unusual if not unique events – it is very difficult to explain their non-occurrence elsewhere because too many factors were present that cannot be found elsewhere and even if one's list is short, like Pomeranz's coal and colonies, other scholars argue these are not the right differences to highlight.

In subsequent work with Jean-Laurent Rosenthal in *Before and Beyond Divergence*, we shift the framing of the great divergence away from the English case specifically and consider instead the differences between China and Europe to ask whether or not amidst a large number of differences we can make sense of which ones might have mattered and which ones would not to explaining economic divergence. This method considers the possibility that institutions can vary and yet have similar abilities to foster economic production or exchange. If instead of simply cataloguing the many ways the Chinese economy and Western European economy differed, we consider how a given economic challenge, e.g., organizing long-distance trade, was addressed in both world regions we can gain a clearer sense of what differences were consequential for the great divergence. Rosenthal and I ended up focusing on an important political difference between China and Europe – political fragmentation in Europe and the presence of an unitary state for the Chinese empire. Our argument included two large and distinct claims: 1) Chinese political unity facilitated long-distance trade and the flourishing of commercial exchange more effectively than European fragmentation. 2) European fragmentation included a considerable amount of war making that gave craft producers an incentive to locate in cities where production costs were higher (high costs of food, higher rates of mortality), but the protection afforded against armies was worth the extra expense. From this second phenomenon we made the straightforward economics argument that the relative costs of capital and labor in cities meant that producers in cities were more likely to use capital than labor than those in the countryside. This contrast highlighted in the book made more likely the emergence of capital-intensive forms of production, which in turn means a greater likelihood of economically useful technologies being deployed. We turned the issue of divergence from a question of why was Britain the first industrializer to why was industrialization more likely to occur in Western Europe than in China. Our explanation for different likelihoods of industrialization was premised principally upon different demands for technological change in cities vs. countryside owing, *ceteris paribus*, to different relative costs of capital and labor.

At the heart of this explanation is a set of propositions regarding where capital-intensive technologies basic to industrialization will most likely occur. The propositions are falsifiable in the sense that evidence of capital-intensive technologies emerging in types of settings other than those we identify would minimally make our explanation less general and possibly untenable. Certain components of our explanation for the likelihood of

urban and rural locations for craft industries do not merely distinguish China and Europe but also account for changes within the histories of both world regions regarding urban and rural locations of craft industries – Chinese craft industries were in cities in the 10<sup>th</sup> and 11<sup>th</sup> centuries when warfare was more common and became increasingly rural in subsequent centuries, especially from the 15<sup>th</sup> century forward; while sixteenth and seventeenth-century European craft industries were more urban than rural when compared to Chinese counterparts, rural industries, often called proto-industries developed in some parts of eighteenth-century Europe, never becoming as widespread as they were in China which lacked the history of armies pitted against each other in the early modern era.

In *Before and Beyond Divergence* Jean-Laurent Rosenthal and I make the origins of modern economic growth the source of divergence rather than the making of the modern world economy which is Kenneth Pomeranz's focus. Our formulation of the problem entails a general shift to more capital-intensive forms of production enabled by new technologies that raise labor productivity. In the absence of an initial cotton textile revolution, Western Europe would have likely developed some other industrial activities, in particular those involving steam power which was the general purpose technology (GPT) of the industrial revolution that enabled a diverse variety of new forms of production. The explanation of divergence that Rosenthal and I offered thus addressed these more general possibilities for the start of modern economic growth and looked at traits particular to Europe rather than the relationships of Europeans to other world regions. For the development of steam power and its use for iron and steel production, in an imagined Europe without Britain there might well still have been the early nineteenth-century developments in Belgium and subsequent developments in Germany. Rosenthal and I argue that these kinds of nineteenth-century developments were more likely in Europe than in China because of the urban bias caused by earlier centuries of war making. Since our book appeared, the association between warfare and subsequent economic growth has been supported through the econometric research of Mark Dincecco and Massimiliano Gaetano Onorato showing clear connections between military conflict and the economic rise of cities in Europe through linking the geocode locations of urban centers in relation to sites of warfare.<sup>6</sup> This research helps confirm our hypothesis which concerns modern economic growth occurring in Europe rather than China. By making the question of the great divergence a more general one we recognize that even a profoundly transformative process like modern economic growth could take place in several ways and begin at several locations. The issues of what made Britain the center of the core of the European dominated world economy during the nineteenth century involves historical factors additional to those needed to explain divergence as the distinction between those areas experiencing widespread industrialization and those without modern economic growth. To explain the origins of the modern world economy and not just the

6 M. Dincecco/M. G. Onorato, "The Economic Legacy of Warfare: Evidence from Urban Europe" IMC LUCCA EIC Working Paper #6 Institute for Advanced Studies Lucca, 2006.

origins of modern economic growth and in particular to show how and why the British established a hegemonic position in the 19<sup>th</sup>-century economy leads us to consider their early modern era activities in other world regions, both in Asia and North America.

### **Early Modern British Commercial Capitalism and the Origins of the Great Divergence**

British political and economic relations with both Asia and North America matter to the origins of the modern world economy because they form the backdrop for English development of the cluster of advances in cotton textile technology that is the canonical focus of the industrial revolution. Early modern global trade that witnessed flows of Chinese tea, silks, and porcelain to Europe in part balanced by exchange for silver mined by the Spanish in the Americas also included the import of Indian cotton textiles by the English East India Company. English textile producers wished to compete more effectively with cheap Indian imports and the imposition of high import duties offered inadequate protection for domestic production to flourish. British labor was too expensive compared to Indian labor. In order for English cotton textile production to become competitive with Indian imports, new capital-intensive technologies were required that could allow labor productivity to rise so much beyond that of Indian textile workers that even with far higher wages, the textiles produced could be cheaper; differences in costs of capital and labor (factor prices) in England and India mattered.<sup>7</sup> British political efforts to restrict Indian cotton textile imports created tariff walls and policy aims more generally sought to foster British growth as Prasannan Parthasarathi has argued in *Why Europe Grew Rich and Asia Did Not*. But British policies could not be as pointed or as effective as subsequent policies enacted by later industrializers who could stimulate domestic substitutes for foreign technological processes and products made elsewhere. The productivity changes in British industrial output came about primarily due to the great differences in factor prices for capital and labor in Britain and India which made their cotton textile production processes so very different. For the British to outcompete Indian textile imports, they had to dramatically raise worker productivity to make their high wages no longer an impediment to domestic substitutes for imports.

The presence of high wages alone did not trigger the development of technological innovations in cotton textile production, or else we should expect to witness a similar development in the Netherlands, the other European site of high wages. As Jan Luiten van Zanden says, “the factors which hindered industrialization, such as high wages in the coastal provinces, were closely linked to the socio-economic structure which emerged in the seventeenth century. The slow industrialization of the nineteenth century is therefore ultimately linked to the difficult transition from the economic structure of the seven-

7 S. Broadberry/B. Gupta, Lancashire, India, Shifting Advantages in Cotton Textiles, 1750–1850: the neglected role of factor prices, in: *Economic History Review* 62 (2009) 2, pp. 279–305.



teenth-century commercial capitalism to industrial capitalism, a transition which was made more difficult by institutional rigidities on the labor market, the commodity market and in the apparatus of government.”<sup>8</sup> Instead, high wages were an impediment to the adoption of industrial technologies because countries with lower wages could adopt the same technologies more cheaply. The contrast between British and Dutch experiences highlights a basic difference between the conditions stimulating the development of new technologies in a high wage economy and their subsequent use in another high wage economy that was in competition for using the same technologies with places where lower wages prevail. High wages alone do not stimulate technological innovations to make such countries more productive. They are to the contrary an active impediment to technological diffusion when such vectors flow more easily to areas with lower wages for comparably skilled labor.

The particular arena of cotton textiles as the first site of industrialization owes its prominence to early modern global trade patterns. European states generally and the British state specifically clearly mattered to the particular way that the modern world economy emerged out of an early modern global economy powered by commercial capitalism. Maritime commerce was the sector of the economy producing fabulous profits for a small number of privileged merchants. It was their control over lucrative markets that gave them such prominent positions in economic life. It was the British state that controlled access to commercial capitalist participation in the East India Company because the Company was a state-chartered corporation, responsible for bringing Indian cotton textiles to Britain. Would have such imports been possible without a state-sponsored operation like the East India Company engaged in cotton textile imports? If cotton textile imports were organized in some other manner, including a more competitive market situation, the state would have had to play a larger direct role in creating the necessary institutional conditions for a market. One way or another, we have to give an account for why the British were off seeking goods in Asia in the first place and how then they could create the institutions needed to make trade possible. The reasons are as much political as they are economic. Political understandings of economic production and trade commonly called mercantilism were basic to the commercial capitalism at which the British and Dutch excelled.

Within Europe, mercantilism promoted economic competition between countries. Governments and economic elites believed that there was a finite amount of production that was possible given available technologies and institutions so that to become richer than their neighbors officials needed not only to encourage domestic production, but at least as importantly, they had to export their products and amass bullion in return as the indicator of wealth and hence both economic and political success in a competition

8 J. L. van Zanden, *Industrialization in The Netherlands*, in: M. Teich/R. Porter (eds.), *The Industrial Revolution in National Context: Europe and the USA*, Cambridge 1996, pp. 78-94; quote on p. 80.

9 But see new research on Swedish and English mercantilists overcoming such a zero-sum-game understanding of economy: P. R. Rössner (ed.), *Economic Reason of State*, 2016 (in particular contribution by Carl Wennerlind).



with other Europeans. Europeans' desired to search Asia for goods deemed desirable to Europeans and to capture control over European markets for Asian goods at the expense of their neighbors because this too would yield more wealth for their government and economy than their European competitors could achieve. The viability of European trade with Asia, in the seventeenth and eighteenth centuries, principally in Dutch and then British hands, depended in no small measure on silver mined in Spanish colonies in the Americas. In Asia silver was a principal good to exchange for Indian cotton textiles, as well as Chinese teas, silks, and porcelains. Without this silver extracted from American mines by European colonizers, it is not clear how Europeans would have been able to import as many Asian goods as they did.

Beyond Europe, mercantilist beliefs served the development of maritime commercial capitalism focused on bringing in Asian commodities paid for with American silver to be consumed by Europeans drawn to markets with new and wondrous goods. One historical irony of the European mercantilism through which British commercial capitalism was formulated was the subsequent development of industrial change in cotton textiles, a process that proceeded to undermine the very bases of the East India Company's commercial capitalist successes. The initial stages of industrialization undercut the commercial capitalism in cotton textiles that gave it birth. State roles in the economy would subsequently change in a world of industrial possibilities even as the importance of connecting wealth and power would be a lesson taught on a global scale by the end of the nineteenth century.

British political economy that shaped its relations to Asia and North America gives a set of historical reasons that the British industrial revolution began in textiles. But modern economic growth would likely have begun in Western Europe even if Britain did not have the particular relations it forged with other world regions because capital-intensive production utilizing new technologies was more likely to occur in Western Europe than in China. It is therefore important to distinguish the more general reasons for a great economic divergence at the same time as we pinpoint reasons for Britain beginning its industrial revolution in the manner familiar to history textbooks.

### **State Roles in Economic Change before and after Industrialization Began**

Europe's Asia trade stimulated various European efforts at import substitution, especially in silk and porcelains, as well as cotton textiles, which I have noted was a crucial stimulus for the cotton textile revolution setting Britain on a path toward its hegemonic position in the nineteenth-century global economy. The British state's pursuit of mercantilist competition with other European powers defined a number of its priorities which included prominently the expansion of fiscal and naval capacities in the eighteenth century. As Philip Hoffman has demonstrated, the European effort to expand their abilities to impose themselves militarily on peoples and places in other world regions had some

positive benefits for economic development.<sup>10</sup> But it is worth noting that neither the state policies of the British nor those of other European powers were taken with the explicit purpose of fostering modern economic growth. If they had positive impacts on economic growth, these were beneficial but unintended consequences.

The absence of deliberate intention matters because it contrasts with two kinds of late nineteenth-century state efforts regarding industrial economies that need to be separated from state efforts regarding economies before the industrial revolution. First, in late nineteenth-century economies that had both developed industrial sectors and were active in international trade, the late nineteenth-century depression presented government policy makers with hard monetary policy choices which affected international trade levels and could either stimulate or reduce production. Governments, both then and now, can achieve only two of the following three traits--free capital movements, a mechanism to manage exchange rates among currencies, and the monetary autonomy of sovereign country. Making choices among monetary policy options have been taken deliberately to influence domestic economic activity and levels of international trade. Quite separately, governments in countries that did not have well-developed industrial sectors could be attracted to taking actions intended to create the wealth and power that officials saw in Western countries. This too entailed conscious decisions by state policy makers to affect the economy.

As the leaders in industry and international trade were struggling to formulate policies, government officials in societies without much industry were thinking more about how government policies can foster industrial growth. Only after industrialization clearly begins could political leaders observe and desire to emulate those successes. Some states made deliberate efforts to industrialize. This happened in late nineteenth-century Germany fostering the formation of large business groups and in Russia with policies to encourage foreign capital and technologies to build factories in Moscow and St. Petersburg. Early twentieth-century Japanese governments encouraged the development of Japanese abilities to replace imports. Industrialization seeks to expand domestic production, to reduce reliance on imports and in some instances to create export competitiveness. Governments played increasingly large and conscious roles in setting policies designed to create wealth. In some ways, we can see in some world regions, especially East Asia, the pursuit of both wealth and power in the late nineteenth century that resembles early modern mercantilist states pursuit of wealth and power.<sup>11</sup> But there is a clear difference between economic possibilities before and after the economic divergence created by productivity differences in industrialized and non-industrialized societies. The ways in which state policies can take on modern economic growth as a goal could not be imagined before

10 Ph. Hoffman, *Why Did Europe Conquer the World?* Princeton 2015.

11 R. Bin Wong, Self-strengthening and other political responses to the expansion of European economic and political power, in: J. McNeill/K. Pomeranz (eds.), *The Cambridge World History Volume 7: Production, Destruction and Connection, 1750–Present, Part 1: Structures, Spaces, and Boundary Making*, Cambridge 2015, pp. 366–94.

the great divergence. Not surprisingly therefore state relationships to the economy before and after the onset of industrialization have varied across the globe.

The differences of state roles before and after divergence mean that explaining how modern economic growth occurs before and after initial cases of industrialization cannot be entirely the same. For the earlier era, we can explain the basic likelihood of economic divergence beginning in parts of Europe rather than parts of China due to the adoption of newly created technologies. Some of the technological innovation and development in the early industrial revolution has a clear demand side stimulus because the relative costs of capital and labor favor deploying more capital-intensive methods where capital is cheaper relative to labor. Where labor is cheap and capital dear, there is less likelihood of demand for technological innovations. Part of the difference in technologies was supply side determined – less a response to market incentives than an affirmation of social or cultural preferences for certain types of knowledge and solving certain kinds of problems. These preferences were not shared by all elites in Europe but those who did have such tastes could find countries where social networks and government policies were supportive. But state understanding of how policies could foster technological innovations became fundamentally different after the mid-nineteenth century when some science-based discoveries were directly exploited for their commercial value. The first industries to develop from such beginnings were the chemical industries which had expanded from textile dyes to fertilizers and explosives. German investments in science education fostered the success of German industries in these areas, manifesting a clear link between state social investment and economic development.

Returning to the era before the great divergence, I have already commented on the ways in which mercantilist state policies as well as the investments in military technology brought forth by the war-making dynamic to European state making had unintended positive impacts on the conditions that would subsequently prove the setting in which industrialization began. They were outcomes of the war-making dynamics which caused the move into cities, the unintended economic significance of which Rosenthal and I have stressed. The unintended economic consequences of early modern European political decisions contrast with the intended consequences of early modern Chinese economic policies which were, like European policies, not intended to create an economic divergence, but unlike European policies they were intended to expand output, address inequalities, and combat the negative consequences of seasonal and annual fluctuations in harvests.

The “great divergence” is both a specific though large and complex historical phenomenon and a more general and precise economic transformation. The literature to date does not distinguish adequately between the two. The former is a Britain-centered narrative and the latter is first a Western European-centered narrative with subsequent American and East Asian sites of divergence. We can develop falsifiable propositions to explain modern economic growth as a general process and we can even formulate propositions to explain why modern economic growth was more likely to begin in Europe than in China, China being one of several world regions where such comparisons could in principle

be made. But comparisons of this sort become wildly unwieldy if we turn to accounts of British emergence as the dominant world power in the nineteenth century. This rise was the joint product of state building transformations and economic developments which are historically connected but analytically separable. We know we can separate them analytically to understand each more fully because we conventionally compare British economic development with economic development in other countries and we separately consider British state building alongside other examples first of European state building and then state building in other world regions.

When the two processes are examined together with an intent to show how both state building and economic change in Britain “explain” its divergence from China we embark upon a very demanding set of contrasts which can be easily reduced to showing the many ways in which China was not and did not become Britain in either economic or political terms. This is however a very particular way to interpret the challenge of accounting for the great divergence and one that is antithetical to the proposals and distinctions this essay is putting forth as a way to distinguish among types of projects and analyses and to highlight in particular our need to formulate falsifiable propositions to be part of the bedrock of our social theories and historical explanations for the kinds of large-scale economic and political changes we see in the making of the modern world, mindful that many of these changes take place amidst the ruptures caused by the great divergence, whether the term covers of the making of the modern world economy or modern economic growth more generally. A recent publication by Peer Vries entitled *State, Economy, and the Great Divergence: Great Britain and China, 1680s–1850s* offers just such an “explanation” for the great divergence as the product of the state’s relations to the economy as part of a far larger set of summary contrasts between selected political and economic practices in the two countries. It deserves evaluation because it follows a method very different from that explained in this essay

### **Units of Analysis and Subjects Broadly and Loosely Connected to the Great Divergence**

Peer Vries is an historian who has participated in debates about the great divergence centered in workshops of the Global Economic History Network (GEHN) formed by Patrick O’Brien more than a decade ago. Unlike many of us brought into that project who had some formal training in economics, Peer Vries came to the subject of the great divergence having first published a historiographical work in 1995 contrasting social science and post-modern approaches to historical studies, *Verhaal en betoog. Geschied-beoefening tussen postmoderne vertelling en sociaal-wetenschappelijke analyse*, in which he analyzes the positions in the debate between postmodern approaches and social science approaches, and, as he explains, indicates his own position. It is thus a book of commentary on other people’s scholarship. His most recent book before *State, Economy and the Great Divergence* was *Escaping poverty: The origins of modern economic growth* (2013), part

two of which is entitled “Actual explanations of the Great Divergence” and is composed of twenty-nine sections, each ranging from three to eighteen pages in length. Each is a mini-essay on some broad topic related to the great divergence, including geography, institutions, factor endowments, human capital, consumption, inter-continental trade, ghost acreage. Much of this work is composed of summaries of other scholars’ work with the author’s criticisms and praise allocated according to a variety of criteria that range far and wide but end up with a statement in the “Concluding Comments”: “In my view Weber’s mega-cultural claim that the rationalization of economic life (read: ‘capitalism’), of public life (read: the legal-rational bureaucratic state) and of the mastery of nature and society (read: science and technology) had been pushed further in ‘the West’ than in ‘the Rest’ still is a very respectable claim that has not been refuted, can be turned into testable hypotheses and would deserve more systematic comparative empirical research.”<sup>12</sup> *State, Economy, and the Great Divergence* moves from a Weberian framing of modern economic growth to the many ways that state attributes and practices appear in Vries’s account to have fostered or contributed to the great divergence.

Vries asserts the appropriateness of the criteria creating successful early modern states in Europe for evaluating the Chinese state. Such an exercise makes sense when one’s goal is to offer a retrospective account of how early modern China did not become a strong nineteenth-century state in the manner of Britain or its continental competitors. Vries repeatedly shows how China was not a state conforming to European practices, which is hardly surprising since its agenda for rule, including both the challenges it faced and capacities in built were only partially similar to those of Britain and other European states. That is hardly news. The specific subject for much of the book, public finance, has no clear and direct significance for the origins of modern economic growth, however much the character of public finance tells us a lot about the contrasting character of the British and Chinese states. Vries wishes to argue that public finance comparisons are part of a larger story about British state making and as earlier generations of scholars have shown, the expansion of British revenues in the eighteenth century depended greatly on the country’s expanding trade. The causal arrows run from economic change enabling new forms of revenue collection so that economic change helps explain the possibilities for political change, rather than how state policies actually helped cause modern economic growth, even if Pomeranz’s “coal and colonies” does suggest a causal impact of policies on the economy which Vries does not find persuasive. Vries displaces the great divergence as an economic story to a review of state formation and transformation in early modern Britain and China.

An earlier generation of economic historians compared early modern Britain and China but such exercises have become quite rare, in part due to the stress on comparable spatial units of analysis necessary for making comparisons about economic change, a point raised in *China Transformed* and stressed both in Pomeranz’s *The Great Divergence* and in

12 P. Vries, *Escaping poverty: The origins of modern economic growth*, p. 436.

*Before and Beyond Divergence* by Rosenthal and Wong. For political issues the comparison of broadly similar spaces is less straightforward since one can claim a government has economic policies irrespective of its size. One can thus go ahead and compare these policies and their significance despite the different size of the polities. But an obvious question becomes what are the metrics according to which policies should be compared? Can the different challenges and capacities of large and small states existing in very different geo-political environments be usefully judged according to criteria relevant only to one of them? Vries has his metrics – judge the Chinese state according to how it differs from Britain’s early modern fiscal-naval state.

While Vries’s sort of comparison was essayed by earlier generations of scholars, the expansion of work in the global or world history field have made them a rare phenomenon. Most of us would take this as an indicator of improvement. Not so Peer Vries who produces passionate and opinionated prose in a register as akin to statements in the American presidential primaries as it is to the prose style of the scholarship on which he bases his understanding of Britain and on which he both builds his understanding of China and then attacks that same scholarship for what he deems a failure to understand the causes of the great divergence related to public finance. When this idiosyncratic exercise in comparison is carried out on the basis of a non-specialist’s reading of English-language scholarship and without access to either original sources or the major scholarship done on the field by Chinese and Japanese scholars, there is little reason to be surprised by the results the author reaches.<sup>13</sup>

### Chinese Public Finance (and the Great Divergence?)

The balance of this essay examines some of the style and method of Peer Vries in assessing early modern Chinese public finance, a subject that looms very large in *State, Economy and the Great Divergence*, the first two chapters of which are on fiscal systems in Britain and China. Given the scholarship available in English on the two countries’ fiscal systems it is not surprising that Vries can write more than twice as much about Britain as China. It is a bit jarring however for him to appear unaware of how much more limited his understanding of Chinese fiscal practices are because the scholarship available to him in English is so limited at the same time as he criticizes those of us who compare Chinese and European fiscal practices, saying “most of the Californians can compare Western economies with that of China without intensively studying them.” (p. 103) Unable to

13 The literature on early modern Chinese public finance in Chinese and Japanese is voluminous and detailed. Key works published by the mid-1990s are cited in *China Transformed*. Two major Japanese works published since then are Yamamoto Susumu, *Shindai zaiseishi kenkyū*. (A study of Qing dynasty fiscal history) Kyūko 2002 and Iwai Shigeki, *Chūgoku kinsei zaiseishi no kenkyū* (A Study of the Fiscal System in Late Imperial China) Kyoto 2004. Important Chinese works appearing in the past decade include Chen Feng, *Qingdai caizheng zhengce yu huobi zhengce yanjiu* (Studies of Qing fiscal policies and monetary policies), Wuhan 2008; He Ping, *Qingdai fushui zhengce yanjiu, 1644–1840 nian* (A study of Qing dynasty taxation policies, 1644–1840) Forbidden City Publishing House, 2012.

read original sources or the considerable scholarship in Chinese and Japanese on the early modern Chinese fiscal system, the author is in a poor position to evaluate how it worked or to contextualize the scholarship he draws upon to formulate his own assessment, let alone suggest he has done the intensive study necessary to make comparative judgments. Vries recognizes his linguistic limitations but suggests the information available to him is not a handicap but really indicative of a general problem with the system itself: "As far as I can judge on the basis of literature available in Western languages, information is scarcer and much harder to interpret than in the case of Britain. Which as such is already telling: Qing finances were not exactly transparent, in particular, for the Han Chinese" (p. 87-88). Vries, the linguistically limited foreign observer decides that what he can read in basically the English-language secondary literature actually indicates a problem plaguing the Chinese, including it appears both the historical agents and the scholarship that followed to analyze their activities.

Ignoring the limitations of his knowledge about China and applying metrics for judging state behavior according to British practices is a recurring problem in Vries's comparison of Britain and China. He knows far more about Britain than China and he adopts British practices as a norm he thinks a useful metric for judging the abilities of China. These are the limitations that scholars writing on Chinese history in Western languages have been able to mitigate but for those who remain comfortable creating standards this way, some direct evaluation of what is claimed needs to be undertaken. The limitations of Vries's approach to explaining state fiscal behavior and its possible relationships to the economy can be first demonstrated without even considering China.

### **State Revenues and Activities Compared (Where is the Great Divergence?)**

Vries's approval of the standard notion of the central government controlling a country's financial system and its revenues certainly makes sense for state makers in early modern Europe where countries were relatively small but what about when countries are far larger and lack similarly sized competitors around them? Certainly we know that in American economic history, much nineteenth-century government revenue before 1850 was mobilized at state and local levels; and these funds were put to economically productive uses, especially for creating canals. If we turn to the U.S. financial system more generally, the national level of government did not become very important until the late nineteenth century. Alexander Hamilton's efforts to establish a national banking system failed and banks were chartered at the state level and through these systems states raised monies for canal building and entrepreneurs were able to gain financing for their enterprises. How appropriate is it to judge the capacities of the American government and its structure by criteria used for a far smaller country like Great Britain? How much more relevant is the question when the country has a very large population as well as a territory roughly the size of Europe?

Let us consider briefly some of the problems attending Vries's understanding of the Chinese fiscal system which only considers certain parts of this fiscal system's operations.



Along the way, we should periodically remember that Vries does not bother to explain why the features of British fiscal practices that differ from China's contributed to the great divergence. Vries explains his empirical focus on the central government early on: "What I will try and compare here are taxes collected *for* and, directly or indirectly, spent *by* the central government. The reference to *central* government is quintessential. This text will not deal with revenue that is never put at the disposal for central government as it is collected *and* at the discretion of authorities other than those of the central state" (p. 70). Vries goes on to explain that European central governments did not control all money raised in their societies; much was done by lower levels of authority who, as Vries quoting Philip Hoffman tells the reader, "had their own ideas about how the money should be spent" (p. 70). Applying this criterion of central government control over revenue appropriate for distinguishing the European central state's expansion of power and authority in early modern times has been established by specialists studying early modern state formation, but why Vries thinks it prudent to use this standard for cases as different as early modern China or the early American republic he does not bother to explain. Rather, he asserts the generality of the European norm through his applying it to China and showing the many ways in which he has learned that the Chinese system did not meet British standards of central government control over revenues and that the Chinese government by his definition of state revenues taxed at a far lower per capita rate than the British did. His contrast would probably be true even if we more carefully make estimates of revenues than Vries is capable of providing based on his reading of English-language secondary sources. But such an exercise in revising his figures would be of little use unless we first sort through some institutional features of the early modern Chinese fiscal system that Vries does not discuss.

First, the revenues the eighteenth-century central government stored in provincial treasuries were in no simple sense controlled by the province. Instead, there was a provincial treasurer in each province who was an official in the Ministry of Revenue and it was officials in the capital and the province who together decided on how treasury reserves might be moved among provinces to meet needs deemed important by the central government. I have argued that the eighteenth-century Chinese state could use its resources more flexibly across space, which meant particular locales could raise less revenue per capita when they could anticipate an influx of resources when needed. Vries misrepresents this argument when he says it is untrue "for *all* the Western European countries for which I have found information" (p. 99). He apparently willfully ignores what the paragraph of which he quotes part of the topic sentence actually discusses which is about the Chinese mobilizing and disbursing revenues across great distances that span European countries. Looking at revenue mobilization and movement within European countries simply does not address the trait to which I referred. It is but one example of his thinking that comparing practices within a European country is commensurate with those across an empire with the territory of many European countries and the population of more than all of them put together. This is especially a problem when the point made is movement of resources across great distances as a substitute for raising more resources locally.



A second problem with the division between central government revenues and those of other lower-level actors in the Chinese case is that, unlike European scenarios in which it is reasonable to say the center and locale had different agendas for using the revenues, the same cannot be said for the Chinese, one of the subjects addressed in *China Transformed*. European rulers were anxious to amass more revenues to compete with each other and needed to build their military capacities in order to compete with each other. At the same time they had to extract their resources domestically in societies where well-established corporate orders of nobles, clergy and urban elites made their own claims on resources. In contrast, central and local officials in China shared a common agenda for expenditures in local areas to build social institutions designed to maintain social order and a healthy economy on which they believed social order was built. The differences from the European situation are two-fold. First, early modern China did not have a comparable set of corporate orders placing large claims on local resources distinct from those of the government. Second, provincial leadership with responsibilities over territories and populations comparable to European countries were not engaged in state-building and war-making competition. From central through provincial and down to local levels of government, officials shared a common agenda for rule that was furthermore shared with the elites of society. One well documented example comes from the formation of grain supply reserves in an empire-wide system of several thousand granaries funded by both officials and by elites. Sometimes surtaxes were levied on the land to initiate the building of new reserves; in the cases of some granaries, contributions from wealthy local people provided the infusion of resources needed. There is even some evidence that officials shouldered more of the responsibility in poorer counties than in wealthier ones more likely to have elites able and willing to provide for their communities.<sup>14</sup> One cannot say this system was funded solely by the government's routine revenues or even its special collections intended for particular projects. Nor can it be said that the granaries were not a major state undertaking. Looking simply at the amount of central government revenues allocated for building, expanding and rebuilding granary reserves would fail to address what the government was in fact doing.

A third problem emerges in Vries' assessment of 'welfare' policies. When looking at English poor relief, he appears at least indirectly to realize that state revenues can include funds that do not go to the central government since he claims Britain "had a *national* system of poor relief financed from taxation ever since their first Poor Law was enacted at the end of the sixteenth century" (p. 194). Vries neglects to mention that the funds are raised locally by substantial householders appointed by the justices of peace, themselves unpaid individuals, typically local gentlemen and merchants willing and able to take up responsibilities on behalf of the crown because such positions enhanced their influence

14 P.-E. Will/R. Bin Wong, *Nourish the People: The State Civilian Granary System in China, 1650–1850*, University of Michigan Center for Chinese Studies 1991; R. Bin Wong, 'Confucian Agendas for Material and Ideological Control in Modern China', in: Th. Hutters, R. Bin Wong, and Pauline Yu (eds.), *Culture and State in Chinese History: Conventions, Accommodations and Critiques*, Stanford 1997, pp. 303–25.

and power. The monies they collect and disburse for poor relief do not appear in the estimates of revenues collected by the central government. The people undertaking poor relief activities are not members of a bureaucracy or have formal government posts. The example of English poor relief does not fit the general distinction Vries wishes to draw between central and local power holders in European societies, but it can in fact fit his very careful “definition” of central revenue since the poor relief funds are used indirectly “for” the central government. This nicety however begs the larger issue of how to assess the roles of local notables and central government bureaucracies in levying and disbursing revenues. The distinction between the regional and local political agendas of aristocratic elites were often different than those of royal authorities, yet they could also agree in Britain and on the continent, with their kings and queens on some issues. English poor relief appears in some ways as less governmental than the Chinese granary system, since neither revenues raised by government officials or managed and disbursed by the government is entailed in the English case but is basic to the Chinese. We have some serious challenges to determine the most appropriate criteria to compare the two systems that Vries does not consider. The granary system and poor relief have different specific purposes, are organized with varying kinds of state involvement, and are implemented across territories extremely different in size and with populations of some 6.5 million and more than 270 million. The Chinese system may have indeed directly affected a smaller portion of the Chinese population than the English during the eighteenth century but is that really the only useful standard according to which two systems, one aiming to succor routinely the chronically poor and the other to ameliorate the widespread social impact of variable harvests and food supply availability should be evaluated? Could an eighteenth-century system covering a population some forty times larger than the English possibly be expected to perform the tasks intended in the English case? For Vries, the answer appears to be ‘yes.’

Vries goes yet further in his discussion of welfare issues with the purpose to contradict the observations that I and other China historians have made regarding the distinctiveness of the eighteenth-century granary system and its significance.<sup>15</sup> His critique depends on exaggerating the claims that I and others have made for the granary system’s uniqueness and sidestepping the points he cannot refute. *Nourish the People* put the Qing granary system into historical perspective and compared it with food storage systems mounted by other early modern states. No one has claimed that storing grain was uniquely Chinese, only that the spatial extent and degree of institutional integration from a central government ruling territories the scale of all of Europe’s many eighteenth-century governments was a distinctly Qing achievement. While Vries adds examples of scholarship on Dutch and Prussian practices to augment those I offered on Europe some 25 years ago (before much of the scholarship Vries relies on was even published, a fact that does not prevent him from chiding me for not knowing of research not yet published when *Nourish the*

15 P.-E. Will / R. Bin Wong, with J. Lee, and contributions by J. Oi and P. Perdue, *Nourish the People: The State Civilian Granary System in China, 1650–1850*, U. of Michigan Center for Chinese Studies 1991.

*People* appeared (p. 193-94)), the information does not change the basic contrast between the Qing system and the European cases that I did cite, with the exception of the Prussian evidence which concerns rural populations much more than urban ones. But how to think about the “state” in a German context is a bit challenging since unlike the British, French or Spanish cases where crowns distinct from aristocracies emerge, German-speaking areas were largely under the Holy Roman Empire and ruled by a variety of princes none of whom controlled the size of populations and economies of the British, French or Spanish rulers, not to mention some of them being smaller than Chinese provinces. These challenges of how to address comparisons between European cases and China when there is clear variation among European cases requires some effort to distinguish the contrasts between the world regions. For the Qing granary system this was reasonably straightforward for it was a single system covering the spatial equivalent of Europe and far more people. The authors of the granary volume Vries criticizes made no claims for these institutions fostering modern economic growth, nor that their impact was always large in a positive fashion; indeed we even suggested that these eighteenth century practices may have contributed the problems faced by the nineteenth-century state unable to keep up the granary system or manage subsistence problems in several parts of the empire. But this study and others I have undertaken do argue that the eighteenth-century Qing state did have an agenda for rule that highlighted the material welfare of its agrarian population in ways quite different from the priorities pursued by European rulers. Variations among European rulers regarding the manner and degree to which they cared about civilian food supplies is part of the contrast made in the book from 1991 that Vries so vigorously criticizes. However the specific issues of how to compare variations across Europe and China regarding food supply management should be addressed, the subject seems to have rather little to do with the putative subject of the book, the relationship between state, economy, and the great divergence in China and Great Britain.

Vries misunderstands or chooses to misrepresent other aspects of eighteenth-century Chinese public finance. For a fourth example of this inadequate understanding of early modern Chinese public finance, consider water control. The subject is important for at least two sets of reasons. First, water control projects were important activities making possible paddy rice agriculture and forging some key transportation links along rivers and canals, both of which were components of eighteenth-century China’s agrarian commercial economy. Second, Britain specifically, and with a few exceptions, continental European countries did not have many water control projects compared to China. Water control has long been noted as important in Chinese history and the history of other Asian countries. Interpretations have ranged from Karl Wittfogel’s model of Asian despotism founded on control of water control projects to those stressing the local nature of community-based water control projects assembled by several Japanese historians of late imperial China. Vries makes his assessment of Chinese water control unaware of the significance of the subject to the Chinese economy. Since he cannot summarize evidence and arguments made by Japanese or Chinese scholars who have done the bulk of research

or examine any documents himself, he has to rely for his material on what he can glean from scholarship published in Western languages.<sup>16</sup> Based on his inability to learn much about water control, he concludes “Central government in China had so little revenue as compared to governments in Western Europe and spent so much of it on the military that it simply cannot have spent a lot on water-management” (p. 191, fn 38). He does not however even draw upon the long available work on water control available in English and French by Pierre-Etienne Will and Peter C. Perdue. Pierre-Etienne Will identified a cycle of hydraulic interventions in Hubei marked by moments of intense official efforts to organize capital and labor to build or rebuild dikes and moments of private management and maintenance. Working a nearby area, Peter Perdue considered a shift from government acceptance of private land reclamation to settle peasants and expand production to official concern that land reclamation was having negative effects on ecological stability and the viability of water routes for transportation.<sup>17</sup> From work that includes both bottom up and top down perspectives we gain a picture of both official and private actors each having major roles in projects that affect and even create local communities at the same time as they can have clear consequences for the government, both on the revenue and expenditure sides of the ledger. We also gain an appreciation for the scale and complexity of water control as an exercise in resource management. Both Will and Perdue note the limited capacities of Chinese officials to manage water control problems. Perdue in particular frames the issues he sees in terms of the abilities of local wealthy people to pursue their private profits at the expense of public well-being and the state’s inability to stop them. But how do we decide what are reasonable expectations for Chinese state capacities to manage water control problems?

Vries lacks adequate historiographical context to appreciate the importance of water control issues to the statecraft tradition of policy making and implementation which both identified challenges and made efforts to meet them, though he could have demonstrated some understanding available by grappling with the texts excerpted and translated ably by Helen Dunstan in *Conflicting Counsels to Confuse the Age*, a book included in Vries’s

16 The major scholar of this subject is Japanese historian Morita Akira whose major works include *Shindai suirishi kenkyū* (A study of Qing dynasty water control history) Aki Shobō, 1974; *Shindai suiri shakaishi no kenkyū* (Studies of the social history of Qing dynasty water control) Kokusho Kankōkai, 1990; *Shindai no suiri to chiiki shakai* (Qing water control and local society), Chūgoku shoten, 2002. A crucial contrast of water control under Jurchen and Mongol rule (12<sup>th</sup> and 13<sup>th</sup> centuries) with the 16<sup>th</sup> through 19<sup>th</sup> centuries that resembles more state interventions of the 1950s and 1960s is *Bunsui to shihai: Jin Monggoru jidai Kahoku no suiri to nōgyō* (water allocation and governance: water conservancy and agriculture in Northern China under Jurchen and Mongol rule) Waseda U. Press, 2013. A recent study of the use of government funds for water control by Liu Wenyuan makes clear how ill-founded Vries’s assumption is that the government could not have financed water control: *Qingdai shuili jixiang yanjiu* (A study of borrowing government funds for Qing dynasty water control) Xiamen U. Press, 2011.

17 Un cycle hydraulique en Chine: la province du Hubei du XVI<sup>e</sup> siècle au XIX<sup>e</sup> siècle, in: *Bulletin de l’Ecole française d’Extrême Orient*, 68 (1980), pp. 261-87; P. C. Perdue, Official Goals and Local Interests: Water Control in the Dongting Lake Region during the Ming and Qing periods, in: *Journal of Asian Studies* 41 (1982) 4, pp. 247-65; Pierre-Etienne Will, State Intervention in the Administration of a Hydraulic Infrastructure: The Example of Hubei in Late Imperial Times, in: Stuart Schram (ed.), *The Scope of State Power in China* Chinese University Press, 1985, pp. 295-347.

voluminous bibliography. Vries notes a high-ranking official complaining about the lack of resources to maintain the Yellow River Conservancy without contextualizing the comment – the comment is by an official noted for his water control projects, some of which sparked debate and opposition, suggesting both the effort and expertise the official Jin Fu himself possessed and the importance of the Yellow River Conservancy as a government bureaucracy for managing the challenges of China’s ‘sorrow,’ so labeled because of the high silt content of the river (p. 99). The state also maintained a Grand Canal administration to dredge and maintain the north-south system of canals bringing rice from fertile lands in the southern half of the empire to the capital to feed some of the state’s military and other government officials. Instead, Vries states that the Chinese state was underfunded because it was undertaxed (p. 191 fn 38).

But he could have been a bit more cautious about throwing around some estimates of tax collections and guesstimates of national income and using those as indicators of the failure of the Qing state to have a public finance system that provided services and goods. Since he is well aware that China had large amounts of paddy agriculture and riverine and canal transportation and there is work in English as well on land reclamation both for the eighteenth century and earlier periods as well, a reader who knows some Chinese history might find it odd that Vries believes the state’s role in water control management could not have been consequential. Much funding for water control, just as for granaries, is not captured in the kinds of figures Vries relies on. Once again, it must be stressed to say that the Chinese spent more on water control and on food supply management than Vries recognizes does not mean that the issue in fact is much related to the great divergence.

Let me mention a fifth kind of problem with assessing British (and other European) cases of public finance with eighteenth-century Chinese public finance. In counting state revenue Vries chooses to leave out Chinese customary fees in principle because they do not reach the central government (and as he erroneously states “any part of the government at all” [p. 96]) and in practice because he cannot find useful data on the actual amounts. Since these collections of fees were not in statutorily defined amounts there is no easy paper trail for specialists to track; we are unlikely ever to have much, if any, systematic data on customary fees. To understand the existence of customary fees for the Chinese bureaucracy, which as Vries notes in chapter 4 of his work was an oft-discussed problem among Chinese officials, we need to think about how states paid for local government. The British were a bit extreme – they simply did not pay for local government. The local notables who executed the poor law were not members of a British bureaucracy charged with this as an official responsibility. They fulfilled a duty akin to what we call today an “unfunded mandate” but without even being government officials. By definition, the British could not have a problem levying and using customary fees because the central government did not pay for local government. The Chinese response to the problem of funding local government was quite different. The central government realized that local government certainly cost money but did not want to have to manage those costs as part of its budget and thus it set guidelines for fees collections but left it to local officials to

manage their own costs, including paying their own staffs of clerks and secretaries. This especially makes some sense when the center is governing an empire roughly the size of Europe and with a far larger population.

Vries summarizes some of the Western scholarship recounting the problems the Chinese had managing the levels and uses to which customary fees were put. The lack of formal accounts made it possible for county magistrates or members of their staffs to abuse their positions for private gain. The considerable discussion among Chinese officials signals the importance of the subject to them. Tracking the incidence of the problem over time and space is however difficult to measure. Simply noting the numbers of discussion of the problem may not be more than a very rough indicator of changing significance; one might even think that more discussion meant more effort to address the issue. Even if we successfully sorted through these problems the question remains how do we compare resources raised to pay for local government? That is the public finance question at stake with customary fees.

Regarding the public finance issue of customary fees, the British case is not very helpful because their local government costs are not paid; they are absorbed by local notables. To invoke Vries's criterion, were customary fees collected "for" the central government in the sense they were spent "directly or indirectly" on behalf of the central government? Perhaps a judgment call – certainly the central government depended on its local officials running effective administrations so funds collected for such purposes, even if spent indirectly, as customary fees were, might count by Vries's criterion. But if we exclude customary fees, what allows us to include monies collected to pay for English poor relief? Did the English central government depend on its local notables more than the Qing state depended on its local officials? Were poor relief funds raised and disbursed less indirectly than customary fees? The Elizabethan poor laws set rates statutorily, so perhaps we can include funds raised for poor relief but not Chinese customary fees because of a difference in the legal status of the funds. Yet the less legal status of customary fees might be weighed against the fact that it is government officials in local offices who are using the fees in China – individuals who in the eighteenth century were largely individuals chosen for their posts based on passing several exams and who owe their political positions and social status to a centrally organized system. They were members of a government bureaucracy and thus the state in a manner quite unlike justices of the peace and other local notables who acted as a non-official local government. The choices we need to make to apply Vries's definition of state revenues specifically and the state more generally point out the challenges of coming up with appropriate metrics for judging public finance, both revenues and expenditures. These are all issues of understanding eighteenth-century states, their capacities and agendas for rule. They are problems we face in comparing states. Beyond these issues regarding states, we have the mystery of discerning of how any of these features of public finance matter to the emergence of modern economic growth or to the origins of the modern world economy.

Vries has other observations on differences in British and Chinese public finance such as the development and expansion of public debt in eighteenth-century Britain and its ab-

sence in China. China did have lower per capita levels of taxation and no public debt, basic contrasts the literatures in English on eighteenth-century Britain and China noted by other scholars in the past, including those he criticizes most vigorously. The implication is that the Chinese state therefore did less than the British state did to promote modern economic growth. But the implication is left unexplored empirically. Instead, we hear from Vries what we have already learned through scholarship over the past thirty or more years regarding Britain building a fiscal-military state – a prime example of European state building but not one that the literature suggests created either modern economic growth or the great divergence specifically. What role differences in early modern British and Chinese public finance played in creating modern economic growth or the more specific issue at the center of Pomeranz's *The Great Divergence*, the origins of the modern world economy is left for the reader to imagine.

### **Not an Explanation of the Great Divergence but a Celebration of its Political Impact**

By stated intent, the book by Peer Vries is about state relationships to the economy and how these help us understand the economic divergence attending the onset of industrialization. Yet much of the book covers territory familiar to early modern European historians regarding the importance of mercantilism and the military to the formation of European national states. Vries summarizes a lot of literature on both, especially work on European fiscal-military states noting the entwined processes of expanding finances and growing militaries. The nineteenth-century British state is a major beneficiary of these developments and enters its relationship with China as a superior naval power. As Vries says when marking the outcome of the First Opium War between Britain and China states, “that hugely populous and, according to the California School, highly developed country proved to be no match for this tiny army and signed a humiliating peace treaty after what can hardly be called more than some skirmishes. China could have been whipped off the map in the second half of the nineteenth century” (p. 311). Leave aside the dig at scholarship that has suggested economic similarities as well as differences between early modern Chinese and European practices, the startling claim that nineteenth-century China could have militarily beaten to the point of swift extinction is one made about a situation well after the great divergence has clearly begun and really not related to explaining state-economy relations. It does more than offer a most extreme evaluation of the disparities of power between the two regimes; it avoids imagining what such a defeat, even if such had in fact possible, would have meant in terms of political consequences. Disregarding the plausibility of his assessment of what nineteenth-century Britain could have done to China militarily and politically had it wished, Vries's logic reminds an early twenty-first century reader of what the U.S. did in fact pursue and achieve in Iraq, creating the conditions for a failed state. Disturbing though these images be, the relevant question is what do they have to do with explaining the significance of state-economy



relations to the great divergence? Simply put, Britain's nineteenth-century naval power does not help explain the great divergence and however exaggerated Vries's assessment of that power's political significance might be, it contributes nothing to an account of the great divergence or explaining modern economic growth.

It is puzzling to consider that a book putatively about state relations to the economy as a factor in causing the great divergence can really be intended to address quite a different, if still familiar subject like the British-Chinese relations under the rise of Britain to its hegemonic position in nineteenth-century international relations and trade: "My book was primarily meant to determine and show *what* the differences were between China and Great Britain in the early modern era with respect to the importance, role, and function of their state apparatus and policies. As said before, in many respects the economic advantages and disadvantages of the way the two polities were set up and functioned in terms of its efficient infrastructural power and dynamism, including of course its military strength that enabled Great Britain to tell China what to do instead of the other way around, and its monetary, financial and institutional sophistication are fairly obvious." (434) The second sentence is not easy to read through, but its intent is quite clear – to explain why Britain pushed China around in the nineteenth century. Yes, the book is about "state apparatus and policies" but not because these help explain economic divergence, a subject the author never undertakes and in fact relieves himself from even considering near the beginning of the book. "I will here explicitly focus on the role the state may have played in the emergence of such growth in Great Britain and in its non-emergence in China during the very long eighteenth century. This is the first study in which these two polities are systematically compared from this angle. Therefore the bulk of my research had to be devoted to charting similarities and differences, which left me less room for the question of what all these similarities and differences might imply. Many of their effects are quite obvious. In some respects, however, determining their exact effects would require a separate new and extensive analysis – here I can only suggest the main questions it should deal with" (pp. 3-4). Vries trusts the reader to see what is obvious to infer from some of the similarities and differences among the Chinese and English states at the same time as he acknowledged that some would require some analysis. In the end Peer Vries brings us some very old news about the early modern sources of British hegemony in the nineteenth century. Along the way he tries to discredit some of the scholarship that has addressed the making of the modern world economy as an economic problem and other related efforts to displace the great divergence theme with a more general one of explaining in onset of industrialization and modern economic growth.

I have suggested in this essay that we have reached a point in the Great Divergence debate to seek falsifiable propositions for specific components of what we believe to be the origins of modern economic growth. Peer Vries, for his part, has displaced the issues regarding economic divergence in a different way that allows him to discuss British state making. In a moment of candor Vries reveals why he spends so much time recounting the early modern building of Britain's state because it tells us why Britain was so powerful in the 19<sup>th</sup> century. Consider once again his carefully worded concluding assessment of



the state and its relationship to the great divergence: “Mercantilism and the fiscal-military or rather fiscal-naval state, in my view, did not cause modern economic growth in Great Britain in the sense that they would be a sufficient condition for it, but considering the specific conjuncture in which Great Britain took off, I would certainly consider them a necessary condition for the emergence of the first modern industrializing economy” (p. 436). This statement on its own allows for some intriguing possibilities. First, the conclusion makes very clear the possibilities for other countries in other historical conjunctures to be pursuing other political practices. Second, should the author or some reader protest that this book to the contrary shows all the ways the British state was superior to the Chinese state in terms of public finances and military forces so there is no other specific conjuncture worth considering, we can still wonder how much some of the subjects the book raises really mattered to economic divergence since Vries chose not to demonstrate the relationships but simply divide them implicitly into the categories of obvious and too complex to evaluate without extensive analysis.

Vries’s work does little to advance discussions of the great divergence, either in terms of method, evidence, or analysis. Moreover, he does very little to explain how and why the Chinese state changed in the ways that it did in the nineteenth and twentieth centuries because he invokes a simple and incomplete standard of comparing China with Great Britain. Not only does he not show how differences between the two eighteenth-century states help create the great divergence, his assessment of the Chinese state ill prepares us for considering how the Chinese state has changed over time, how it remains very different from the British state even today as its role in the economy has in fact helped close the earlier economic divergence. This would seem a more important subject to explore than providing yet another review of the scholarship on the British fiscal-naval state coupled to a insufficiently informed assessment of scholarship on early modern Chinese public finance that does not in fact seek to explain the great divergence. Finally, Vries’s work is destructive because it aims to undermine unfairly scholarship that reconstructs and explains the nature of China’s early modern economy and political economy, as if all such efforts are attempts to undermine the reasons for the great divergence. While his hostile polemic and repeated mis-representation of work he does not like and his inclusion of commentaries on far more topics than are directly germane to his subject are both unusual, his approach to comparison of China and Europe exemplifies an approach that was once very common and may, alas, retain appeal for some European specialists.

### **Displacing Divergence**

Vries’s approach relies on describing political, economic, and often social changes in Great Britain as all elements of the unique compound creating a modern industrial economy and society and suggesting all examples of differences from any of Britain’s practices are contributing reasons to the great divergence. The only future for such scholarship is to repeat past arguments and disputes. For the study of history more generally it means the

shift to global history reduces debate among European historians regarding the causes of European changes and advances a critique of non-European parts of the world according to global history discoveries of the differences with Europe and disputing both claims of some similarities as well as many differences with Europe not being clearly consequential for explaining the great divergence. Peer Vries offers a relentless attack on the great divergence by mis-specifying what the basic topic is, namely origins of the modern world economy for Pomeranz and some others and the origins of modern economic growth for yet others. Along the way he mis-represents the intentions and findings of scholarship on China that he does not like for fear they diminish the superiority of Europeans and the British in particular. His main subject area, public finance has not figured prominently in the great divergence debate, and even if we accept all the limitations of fact and interpretation Vries suffers for his asymmetric treatment of British and Chinese public finance, the author still fails to provide a demonstration of how and why particular differences in public finance each mattered to the great divergence

Fortunately, we have far more scholarship pointing us toward a better understanding of the roles of war making, international trade, and fiscal state formation that themselves created the conditions in which the drive to create new technologies was motored beginning in the early modern era and gathering steam (literally and figuratively) in the modern era. At the same time, scholars have begun to distinguish more frequently the two distinct topics of the origins of the modern world economy and the causal mechanisms behind modern economic growth.

This shift of the focus of the great divergence from the particular experiences of Britain to conditions more commonly shared with some other parts of Europe is a more promising displacing of Kenneth Pomeranz's initial subject as an object of research than the displaced focus of Peer Vries's recent book which affirms the well-known importance of both economic and political changes to the making of nineteenth-century British global hegemony without explaining why an account of some of its salient features associated with public finance advances our understanding of the great divergence. The displacing of the origins of the modern world economy with the origins of modern economic growth that this essay advocates as the key issue in the great divergence will hopefully make it more likely that we can offer explanations of economic change in global history in terms of a common framework that offers explanations for what makes industrialization and modern economic growth more likely and what makes them harder to achieve. Such efforts can help us not only understand the past but perhaps also discover the relevance of some of those past practices to shaping our future choices and efforts.

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# **Making Sense of the Great Divergence. The Limits and Challenges of World History**

**Eric Vanhaute**

## **ABSTRACT**

Dieser Beitrag diskutiert den Einfluss der Great Divergence-Debatte auf die Forschungen zur Weltgeschichte, insbesondere bezüglich der Potentiale und Grenzen für eine Schärfung der globalen Perspektive in der Sozialforschung. Dabei stehen vier Argumente im Vordergrund. Als gegenwärtig bedeutsamste Debatte in der Welt- und Globalgeschichte hat die Diskussion um die Great Divergence die schon lange existierenden Dispute um Konvergenz/Divergenz neu ausgerichtet und klar erweitert, indem sie neue empirische Forschungsfelder erschlossen, neuen Ansätzen Raum verschafft und neue Daten und Wissensbestände verfügbar gemacht hat. Die Dynamiken der Debatte treiben sie über ihre eigenen Begriffe und Fragestellungen hinaus, indem allgemeinere Interpretationen des modernen Kapitalismus überprüft werden. Globale Forschung zu Prozessen der Integration und Hierarchiebildung im globalen Kapitalismus machen eine systemische Mehrebenenanalyse notwendig, die Vergleiche und Verflechtungsanalysen, strukturalistische top-down- und akteurszentrierte bottom-up Ansätze (inklusive der Untersuchung von Frontiers) einschließt. Dies führt zu epistemologischen Reflexionen über die Great Divergence-Debatte in ihrem aktuellen Zustand und über die Grenzen bzw. Herausforderungen einer weltgeschichtlichen Perspektive.

## **1. Explaining the Great Divergence: From the West to the East and Back**

World history took a different course after 1750. Great Britain and other industrializing nations made the successful transition from an organic to a mineral-based, fossil-fuel

economy, releasing the Prometheus of technology-based and capital-intensive growth.<sup>1</sup> This pushed their productive and military strength to unprecedented heights, resulting in an unparalleled, worldwide economic and geopolitical dominance around 1900. This process has been coined in different iconic terms, including the ‘Rise of The West’, the ‘European Miracle’, and the ‘Great Divergence’.<sup>2</sup> Soon after 1900, Max Weber wondered “to what combination of circumstances the fact should be attributed that in Western civilization, and in Western civilization only, cultural phenomena have appeared which (as we like to think) lie in a line of development having universal significance and value.”<sup>3</sup> For a long time, the Weberian research program was framed within Eurocentric paradigms. Whether one researched the origins of the industrial take-off primarily in Western European societies, like Max Weber or Karl Marx, or found it in the imperial space that Great Britain commanded, like Eric Williams, almost all research started from and circled back to Europe. The problem with his approach was that it left many hypotheses regarding the technological, institutional, social, political or geographical conditions within Great Britain, Europe or the West unchecked. It lacked a genuine comparative and systemic framework that helps identify which conditions were, in retrospect, necessary or sufficient to set Europe on its perceived industrial *Sonderweg*. Recently, new tendencies in Global and World History have fundamentally altered the contours of and the dynamics within this vibrant research field. In this context, a lot of scholars have re-oriented themselves, to use the expression of the late Andre Gunder Frank. They started looking across the Eurasian landmass in order to compare the European experience with that of China, East Asia or Southern Asia. The whipping debate about the remarkable rise of global inequalities in the last few centuries was, to a large extent, instigated by publications from the so-called ‘California school’. The authors included Andre Gunder Frank, Jack Goldstone, James Lee, Kenneth Pomeranz, Roy Bin Wong, Robert Marks, and others. Although their views often conflict with and contradict each other, they generally agree on a rough comparability in economic performance between China and Europe (or between the Yangzi Delta, its most developed region, and Britain and Holland) until sometime in the 1700s. Some of these scholars have also argued that Western Europe’s subsequent leadership owed much to its relations with areas outside Europe, which provided far greater relief from the ecological pressures created by early modern growth than East Asian cores could gain from their peripheries.<sup>4</sup> This intellectual return to the East is primarily motivated by the observation that the scientific and economic

1 D. Landes, *The Unbound Prometheus: Technological Change and Industrial Development in Western Europe from 1750 to the Present*. Cambridge 1969; E. . Wrigley, *Continuity, Chance and Change: The Character of the Industrial Revolution in England*. New York 1988.

2 W. H. McNeill, *The Rise of the West: A History of the Human Community*. Chicago 1992 [1963]; E. L. Jones, *The European Miracle: Environments, Economies, and Geopolitics in the History of Europe and Asia*. Cambridge 1981; K. Pomeranz, *The Great Divergence: Europe, China and the Making of the Modern World Economy*. Princeton 2000.

3 M. Weber, *Protestant Ethic and the Spirit of Capitalism*. New York 2003 [1930]. p. 13.

4 D. Little, *Eurasian Historical Comparisons. Conceptual Issues in Comparative Historical Inquiry*, in: *Social Science History*, 2 (2008), pp. 235-261

development of China in the centuries prior to the divergence makes it all the more puzzling as to why industrialization and the subsequent rise to global power took place in the West. The second trigger has been that since the late twentieth century, the economic and geopolitical dominance of Europe or the 'West' seems much less self-evident. The subsequent economic growth-spurts of Japan, the Asian tigers and China, combined with the latter's growing geopolitical importance, begs the question of whether we are witnessing 'The Rise of East Asia' and to what extent this rise also implies the 'Descent of the West'. Perhaps it points to a 'Great Convergence', a catch-up process in economic and political development between the two sides of the Eurasian landmass, or between The West and The Rest?

This general research interest has mostly been framed in economic terms: What are the causes of the wealth and poverty of nations? What induced the emergence of a new kind of sustained and substantial accumulation of wealth and growth? Why did this create new and unprecedented regional inequalities? In a recent overview of the debate, Peer Vries examined a wide array of explanations proposed by economic growth theorists and economic and global historians alike: natural resources, geography, labor, consumption, capital accumulation, trade, conquest, institutions, legislation, culture and religion, state actions, science and technology.<sup>5</sup> He stresses that none of the factors he studied can act as the one and only cause of the Great Divergence. There are just too many different factors acting in conjunction in different ways over time: "The Industrial Revolution and modern economic growth were neither foreseen, nor predicted or planned. It would be a major error to look at pre-Great Divergence history as a race between countries, which one would industrialize first."<sup>6</sup> Still, this begs the question: What is the historical story behind this remarkable global transformation? Was the great transformation mainly an internal European process with roots in its own history? Should the causes be sought in global shifts? Did coincidence play a major role? Moving beyond the discussions about the one and only 'prime mover', there is a growing opinion that the rise of the West was a 'contingent' (conditional, not required) process, a process that was not inevitable and could possibly not have happened. On the other hand, this change in the course of world history was not just random, it could not have occurred just anywhere. It was the result of a unique cumulative process, with roots inside and outside Europe.

Within a wide array of literature, three models of explanation can be discerned. The first, and clearly the most longstanding tradition, has a distinctly Eurocentric character. It chiefly evaluates the rise of Europe as a largely autonomous process, a result of internal changes. Since the 1990s a new school points to Asia's age-old predominance and recognizes many similarities between Western and Eastern societies until the nineteenth century. This model seeks an explanation for the divergence in a non-predestined and even accidental concurrence of circumstances. A third tradition distances itself both

5 P. Vries, *Escaping Poverty. The Origins of Modern Economic Growth*, Vienna 2013; see also J. Daly, *Historians Debate the Rise of The West*. London / New York 2015.

6 P. Vries, *Escaping Poverty*, p. 55.

from the classic Eurocentric and the (sometimes referred to as) Asia-centric explanations. It departs from an increased interaction between the West and the East, from which European countries were able to gain the most benefits after 1500. Thanks to several comparative advantages, this increased interconnection enabled them to strengthen their position in the areas of trade, knowledge and state power.

The founding literature about 'The Miracle of The West' sketches the rise of Europe essentially as an internal process. As a consequence of key differences in social and cultural life, Europe was able to break away from other regions in the world. Europe's position in the global system changed dramatically between the fifteenth and the nineteenth centuries, when it became the absolute dominant power in the new global system. This important change is the result of a new internal dynamism within the European world, contrary to an assumed stagnant Eastern society. This explanation model relies on Max Weber's research program, which asserts that the West distinguishes itself via a steady and systematic rationalization of thoughts, actions and institutions. The differences between Europe and the non-West grew increasingly larger. Industrialization seems to flow automatically out of this Western dynamism. This vision is shared by disciples of Max Weber (rational state), Adam Smith (market economy) and Karl Marx (capitalist production relations) alike. For many who adhere to the Weber premise, culture makes the difference: the development of new, Western cultural patterns related to labor, discipline, freedom, knowledge, etc. The West was the first area to develop modern, rational institutions: a modern state-system, a modern bureaucracy, an efficient military apparatus while also promoting individual property rights, and a more or less efficient and 'free' market economy. Within this framework, strong arguments have been made for a range of prime movers, such as Europe's extraordinary drive for invention and innovation and openness to borrowing ideas from others (David Landes, Carlo Cipolla); the fundamental shift in European values, such as the rise of individualism (Allen MacFarlane) and the rise of bourgeois values (Deirdre McCloskey); the unique set of institutions and property rules (Douglas North); and an unprecedented marriage of science and technology (Joel Mokyr).

Within the last two decades, new and comparative datasets undermined the image of Europe's gradual lead in the centuries before 1800. According to these comparisons, the Asian continent created at least 60 percent of the world's wealth in the eighteenth century while containing 66 percent of the world population. According to some estimations, per capita income in East Asia (without Japan) was comparable with that of Western Europe around 1700. So the gigantic reversal of fortunes mainly occurred after 1800. Studies that distance themselves from a Eurocentric approach perceive the world until the eighteenth century as a place of major similarities. Due to China's dominant position in the early-modern world economy, comparisons usually concentrate on Europe versus China. Just like Western European countries, China developed productive arable farming and intensive industrial and commercial systems. The organization of property rights and markets was not inferior to Europe, nor was the political organization less developed. Like other commercial societies in those days, growth was limited by the boundaries of



organic agriculture systems. Assuming what they call a reciprocal comparative perspective, these authors made the compelling claim that it can no longer be taken for granted that centuries before the Industrial Revolution, European states experienced exceptional economic, legal, institutional and political frameworks, allowing for the formation, integration and operation of more efficient markets. Some authors have further minimized Europe's rise as a short-term interlude within the long-term dominance of Asian civilizations, due to a combination of sheer luck and downright violence (Andre Gunder Frank, John M. Hobson). Kenneth Pomeranz made the most compelling case not to consider the European path as a 'normal' outcome of history.<sup>7</sup> Commercial capitalism and the Industrial Revolution did not arise as the result of a long, progressive process; they arose from necessity. Contrary to China, which could profit from its large, united empire, the European continent gradually stalled in an ecological bottleneck: scarcity of energy and scarcity of raw materials. The responses to this bottleneck (coal and industrial technology; colonization) gave Europe a considerable advantage afterwards: more efficient technical knowledge and a network of colonies (an Atlantic trade system). Until the nineteenth century, models of social and economic development in the main centers of development around the world remained based on agrarian, organic-energy economies and they did not create huge regional inequalities. Why one eventually triumphs over the other is not the result of providence; it is a concurrence of circumstances in which coincidence plays a major role.<sup>8</sup>

Recent publications have labeled the revisionist image of the world before 1800 – a world of striking similarities – as too one-sided or even wrong.<sup>9</sup> They do not advocate a return to former Eurocentrism, but argue that major imbalances in economic and political power were not coincidental; they sprang from a different social organization in the West and East. In his recent book, Vries diverges both from neo-classical growth theories and the revisionist writings of 'the Californians': "Whatever the outcome of that debate, it simply is a myth that the economic history of early modern Europe would be the history of the rise of a Smithian market (...) Actually it goes for all major countries that ever took off." He adds: "The coming of modern economic growth was not a natural continuation of previous economic history, be it on a different scale: it was quite unnatural. It was not something that was bound to occur if only certain blockades would disappear." The revisionism of the Californian School is, in turn, "very salutary, but I think there are very good reasons to claim that revisionism went too far." Instead of a world of remarkable similarities, Vries sees "a world of striking differences".<sup>10</sup> The question remains, to what extent Europe's changing global position can be explained from an internal dynamism. Since the Late Middle Ages, contacts with the outside world changed Europe's position on diverse levels. First, its own capitalistic trade system gradually incorporated

7 K. Pomeranz, *The Great Divergence*.

8 R. B. Marks, *The Origins of the Modern World. A Global and Ecological Narrative from the Fifteenth to the Twenty-first Century*. Lanham 2006.

9 P. Vries, *Escaping Poverty*.

10 *Ibid.*, 47, 401, 433.

other parts of the world in such a way that the fruits of this system chiefly served the core. Second, Europe created a unique knowledge system via accumulation, imports and adaptation. This knowledge system was the breeding ground of industrial acceleration in the nineteenth century. Third, Europe applied this knowledge and technology to the development of strong state systems and of unseen military strength. The result was near absolute political and military dominance in the nineteenth century. A wide set of explanatory stories have aimed to understand Europe's changing role within a global perspective and have stressed different external key factors: geography and climate, making Eurasia, and Europe in particular, the most favorably endowed regions in the world (Jared Diamond, Eric L. Jones); interactions among societies in Eurasia, instigating Europe's recovery since the Late Middle Ages (William McNeill, Janet L. Abu-Lughod); European imperialism enabling its states to dominate peoples and resources beyond their shores (e.g. the use of African labor: Joseph E. Inikori; Eurasian invasion of flora and fauna: Alfred Crosby; a globalizing Europe-centered division of labor: Immanuel Wallerstein). The 'Rise of the West' completely upset relations on a world scale. The convergence of internal societal transformations and external expansion beyond its old borders propelled Europe from the periphery to the center of global events. In the remainder of this essay, I will argue that the Great Divergence debate both enlarged and redirected the long-standing convergence/divergence dispute in social sciences (part 2) and that its dynamics push it to go beyond its own terms and to transcend its own limits by rethinking the history of global capitalism (part 3). I will conclude with two more general sets of epistemological reflections, one on the Great Divergence debate as it stands now, the other on the challenges of today's World and Global History (part 4).

## 2. Understanding Convergence and Divergence

Researching the Great Divergence has triggered a wide array of research, including different sets of data, different research strategies, different scopes, scales and units of analysis. The central question is whether these units – regions, states or the world economy – permit meaningful comparisons and to what extent the units of comparison are connected within broader webs or systems of interaction. Using multiple spatial frameworks has tended towards more narrative approaches, and trans-regional comparisons have retained spaces of varying sizes and definitions alongside nations and global systems as units of analysis. Regardless of how the Great Divergence debate fares in future research, it has influenced and stimulated work on various other areas and periods. This impact is clear in the way it avoids the sharp categorical distinctions central to other approaches within modernization and globalization studies. It does not a priori deduce a place's prospects from its location within global networks, it suggests the possibility of multiple paths of development, it stresses several continuous, rather than dichotomous, variables, and it

makes global ties influential, but not decisive by themselves. It stresses that regional units of various kinds and sizes remain important to the story of global economic history.<sup>11</sup> This tension between diverging scales of analysis, between comparison and connection, prompts one of most fundamental debates within the field of World and Global History. How can we understand processes of regional convergence/integration versus divergence/hierarchy in the 'modern world' within a global framework? How do we relate tensions of divergence within a context of increased connections? This debate goes to the core of social sciences. Over the past two centuries, social sciences developed a dominant view that the modern world shows a pattern of more or less linear development in which all positive trends over time converge into a more homogenized world.<sup>12</sup> By and large, left and right shared the same belief in the inevitability of progress and the linear upward pattern of social processes. This ideology of ultimate, positive convergence of all states and peoples reached an apotheosis in the three decades after the Second World War. At the same time, a number of analysts began to contest this linear model, arguing that the modern world was also one of heterogenization and polarization.<sup>13</sup> When analyzing the social world, the linear versus polarizing models of historical development became a debate about whether the various zones or countries would converge to an approximately equal standard of economic, political and cultural structures. A global perspective shows that, despite the many ways in which there has been convergence, there has been simultaneous and strong polarization. Much of this can only be observed if different scales of analysis are interconnected, if regions are not analyzed as self-contained units, and if the global is not seen as an undifferentiated macro process. The need for a global and historical perspective instigated three, interrelated research strategies facilitating multilayered and multifocal frames of analysis. The first compares individual cases in 'a two way mirror', equating both sides of the comparison (reciprocal comparative analysis). The second strategy analyzes the interactions and interconnections between societies or systems, and how those patterns of contact shift (network analysis, translocal/transnational analysis). The third takes human systems in which various societies and their mutual contacts are given shape as the central unit of analysis. Examples include economic systems (the current world-system), migration systems, ecological systems (climate, disease), and cultural systems. Human societies are always linked together by several of these systems and act in reaction to these systems (systems analysis).

The debate about the Great Divergence has yielded large-scale comparative studies on differences in geography, ecology, population, resources, wages, institutions, state building, and so on. Key issues in comparative history are the questions: What is compara-

11 K. Pomeranz, *Writing about Divergences in Global History. Some Implications for Scale, Methods, Aims, and Categories*, in: M. Berg (ed.), *Writing the History of the Global. Challenges for the 21<sup>st</sup> Century*, Oxford 2013, pp. 117-128.

12 I. Wallerstein (ed.), *The World Is Out of Joint. World-Historical Interpretations of Continuing Polarizations*. Boulder/London 2014.

13 R. Palat, *Dependency Theory and World-Systems Analysis*, in: P. Duara, V. Murthy and A. Sartori (eds.), *A Companion to Global Historical Thought*, Wiley Blackwell, 2014, pp. 369-383.

tively being measured and how? How does one avoid explanatory reductionism, methodological nationalism and analytical synchronism? Scholars of the 'Californian school' have made a strong case for the method of reciprocal comparisons, precisely to avoid approaching non-Western histories from the stylized facts of European history and to turn away from pre-determined world views.<sup>14</sup> The method of reciprocal comparison can give historical research more analytical rigor, by forcing researchers to formulate problems, ask questions, look for answers and develop explanations in a more structured and systematic way. The questions about methodology and sources remain intensively debated. Which units are fit for comparison and why? Which assumptions and models underlie any comparison with a global ambition?<sup>15</sup> Moreover, historians making comparisons often face the challenges of a lack of data and scholarly work to create comparable accounts from widely differing sources, compiled under very different assumptions and purposes.<sup>16</sup> Some collaborative networks responded to this challenge by compiling large-scale sets of quantitative-economic data over time and space, such as prices, wages, and estimates of GDP (The Global Price and Income History Project; The Madison Project). However, GDP estimates going beyond the nineteenth century are tentative at best, useless at worst.<sup>17</sup> Wage-based proxy for living standards remain perilous, since until deep in the twentieth century outside Western Europe wage labor was not representative of productive relations and took different positions in different societies. Still, if carefully contextualized in regional stories, these data can serve in reciprocal comparative analyses. For example, recent historical research on Asia has produced some partial and regionally-specific evidence to suggest that standards of living in Western Europe and maritime provinces of China and South India may not have differed perceptibly before the late eighteenth century.<sup>18</sup> Comparative research explicitly raises the question of spatial dimensions. By definition, world-historical research challenges conventional chronological and geographical frames. It stresses both areal integration and differentiation.<sup>19</sup> Much historical work continues to be done at a local, regional or national level in order to achieve control over information and sources. This tension can regenerate national

14 R. Bin Wong, *China Transformed. Historical Change and the Limits of European Experience*, Ithaca, NY, 1997; K. Pomeranz, *The Great Divergence*; G. Austin, *Reciprocal Comparison and African History: Tackling Conceptual Eurocentrism in the Study of Africa's Economic Past*, in: *African Studies Review*, 50 (2007), pp. 1-28; P. Parthasarathi, *Comparison in Global History*, in: M. Berg (ed.), *Writing the History of the Global. Challenges for the 21<sup>st</sup> Century*, Oxford 2013, pp. 69-82.

15 J. De Vries, *The Great Divergence after Ten Years. Justly Celebrated yet Hard to Believe*, in: *Historically Speaking*, 12 (2011) 4, pp. 10-25; id., *Reflections on doing global history*, in: M. Berg (ed.), *Writing the History of the Global. Challenges for the 21<sup>st</sup> Century*, Oxford 2013, pp. 32-47.

16 M. Berg, *Global History. Approaches and New Directions*, in: M. Berg (ed.), *Writing the History of the Global. Challenges for the 21<sup>st</sup> Century*, Oxford 2013, pp. 1-18.

17 P. O'Brien and K. Deng, *Can the Debate on the Great Divergence be Located Within the Kuznetsian Paradigm for an Empirical Form of Global Economic History?*, in: *Tijdschrift voor Sociale en Economische Geschiedenis*, 12 (2015) 2 [= E. Vanhaute (ed.), *Escaping the Great Divergence? A Discussion about and in Response to Peer Vries's Escaping Poverty. The Origins of Modern Economic Growth*], pp. 63-78.

18 B. Li and J. L. Van Zanden, *Before the Great Divergence? Comparing the Yangzi Delta and the Netherlands at the Beginning of the Nineteenth Century*, in: *The Journal of Economic History*, 72 (2012) 4, pp. 956-989.

19 W. L. Lewis, "Geographies", in: J. H. Bentley (ed.) *The Oxford Handbook of World History*, Oxford 2011, pp. 36-53.

frameworks and essentialize features of a nation's history. This is clear in some efforts to resurge institutions as main drivers of unequal development. For Acemoglu and his associates, economic performance is largely explained by a country's institutions, and in many cases these stem from early colonial choices. While settler colonies, for example, usually created a liberal property rights regime that promoted growth, in other colonies Europeans reinforced or introduced coercive institutions. This 'reversal of fortunes' argument posits a single critical intervention and one dichotomous variable (good or bad property rights), ignoring any effects of subsequent global connections.<sup>20</sup>

A wide range of recent World History studies has favored a network perspective. Moving away from comparative histories brings up a whole new set of questions and subjects about connectedness, entanglement, reciprocity and circulation. New metaphors, such as flows, networks, webs, chains and new epithets such as trans, inter, cum and meta aim to translate the experience of border-crossing interconnections. This includes topics like human and labor migration, chains and networks of commodities, and long-distance trade, including methods of navigation, finance, tariffs and price movements, and price convergence. This angle explicitly questions spatial frameworks, creates decentering narratives, and gives agency to the parties involved. It can also favor horizontal stories of entanglement, which risk leveling out history.<sup>21</sup> Connections of whatever kind are created and redefined in a world that is not flat. Stratification and inequality define the direction and the impact of networks. Societal relations configure the world on different levels or scales. In order to understand how they influence each other, a global framework has to integrate connections and networks within (overlapping) scales and (overarching) systems. Over time, these societal systems have grown from small to large, from mini-systems such as chiefdoms, meso-systems such as civilizations, to the world-system of today. They have gotten larger, more complex, more hierarchical and more intertwined, reconfiguring connections and networks time and again.

Over the last two decades, cross-regional comparative and interconnective research has gained a wealth of new knowledge about the 'birth of the modern world'. In order to understand why processes resembled or differed, why interactions went one way and not the other, one needs to understand the systemic logics that combine those patterns. A systems perspective does not narrow the lens to the 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'. A 'world' is not a constant; it is bound by nested human activity. It refers to social change that can only be understood in specific contexts of space and time. For that reason, no single delineation can be absolute. On the contrary, choosing a space and time perspective (where? when?) is linked to an intrinsic substantive choice (which social change?). Consequently, a global or world perspective cannot apply exclusive frameworks of space and time and cannot draw fixed boundaries. Neither do these worlds consist of fixed scales; they overlap from small to large. Interactions be-

20 D. Acemoglu and J. Robinson, *Why Nations Fail. The Origins of Power, Prosperity, and Poverty*, New York 2012.

21 P. Vries, *Escaping Poverty*.

tween external boundaries or internal scales create zones of contact and interaction that we call frontiers. This is where different scales and social systems come together. Scales and contact zones or frontiers are central concepts of analysis in contemporary world history and global studies.<sup>22</sup> Rather than reducing an entity to the properties of its parts, a systems perspective focuses on the arrangement of and relations between the parts that connect them into a whole, creating a 'world'. Systems have a strong internal cohesion but are also open to, and interact with, their external environments, resulting in continual evolution. World-systems are open systems with operational closure, reproducing the very elements of which they are composed.<sup>23</sup> From the moment these patterns of reproduction have become irreversible (and the factors that can prevent its deployment have become too weak or are no longer present), a system is functioning and has replaced former systems. Systemic interactions between communities and societies are two-way, necessary, structured, regularized and reproductive.<sup>24</sup> 'Worlds' refer to these nested interaction networks, whether these are spatially small or large. Until recently, world-systems did not cover the entire surface of the planet. Only capitalism could transform itself from 'being a world' to 'the historical system of the world'. A comparative world-systems perspective is a strategy for explaining social change that focuses on whole, inter-polity systems rather than single societal units. The bulk of world-systems analysis has been engaged with the so-called modern world-system, historical capitalism.<sup>25</sup> Historical capitalism combines a globalizing economic unity (based on extensive trade and exchange relations and a hierarchical division of labor) with a multitude of political entities (states, bound together in an inter-state system) and a multitude of cultures (civilization traditions as world religions and state-bound, group-bound, class-bound and gender-bound identities, tied together by a universalistic geo-culture). Research into systemic processes of convergence and divergence should be based on three basic and interrelated questions. First: What constitutes the system? What are the factors of internal coherence and integration? Second: How does the system reproduce internal hierarchies and stratifications? Lastly: Where are the boundaries of the system? What constitutes its frontiers? A research strategy of incorporating comparisons turns away from the search for invariant hypotheses based on more or less uniform cases. Its goal is to give substance to historical processes through comparisons of its parts, conceptualizing variations across time and space.<sup>26</sup>

22 E. Vanhaute, *World History. An Introduction*. London/New York 2013; H. Cottyn, *A World-Systems Frontier Perspective to Land. Unravelling the Uneven Trajectory of Land Rights*, in: *Journal of World-Systems Research*, 2017 (forthcoming)..

23 A. De Wachter and P. Saey, *Trajectories of Regions and Spatial Integration in the Worldsystem*, in: *Tijdschrift Voor Economische En Sociale Geografie*, 96 (2005) 2, pp.153-167, here 165-166.

24 Ch. Chase-Dunn, H. Inoue, T. Neal, and E. Heimlich, *Global History and World-Systems*, Paper Institute for Research on World-Systems, University of California, Riverside, 2014 (<http://irows.ucr.edu/papers/irows86/irows86.htm>).

25 I. Wallerstein, *World-Systems Analysis. An Introduction*, Durham 2004.

26 P. D. McMichael, *Incorporating Comparison within a World-Historical Perspective: An Alternative Comparative*

### 3. The Geometry and the Frontiers of Historical Capitalism

The Great Divergence is part and parcel of the chronology and geometry of historical capitalism. Capitalism, as a social system, developed as a complex of stratified time, stratified space and stratified social power relations. There is a persistent perception, in both scholarly communities and popular opinion, that the recent rise to power of an array of non-traditionally powerful countries is inverting an age-old trend of global divergence. This rhetoric of globalization and global convergence by and large obscures long-term global stratification, the reproduction of hierarchies in global power relations, together with the emergence of new inequalities.<sup>27</sup> A structural-historical view contends that the processes associated with globalization tend to reproduce stratification and hierarchy in the capitalist system and that 'globalization' as a concept mainly serves to legitimize neoliberal 'modernization'.<sup>28</sup> A global and historical systems-analysis reveals the insistent multi-dimensional nature of global capitalism. Cycles of global expansion contributed to the political upward mobility of a limited number of non-core countries, while states in the core remained politically and economically dominant.<sup>29</sup> A considerable body of academic research confirms that the stratified structure of the world-system has remained remarkably stable over time, despite (varying levels of) upward and downward mobility.<sup>30</sup> The processes associated with global growth do not benefit all countries equally. They contribute to the reproduction of hierarchy and stratification in the system. In order to untie global processes of divergence and convergence, we need to map and understand the interaction between short-term fluctuations and long-term change in global capitalism. A dominant focus on 'massive and large-scale change' in the short-term still leads to a large body of scholarly research that disregards long-term continuity and stratification in the global system of power relations. Structural stratification remains

Method, in: *American Sociological Review*, 55 (1990), pp. 385-397; Ch. Chase-Dunn and Th. D. Hall, *Rise and Demise: Comparing World-Systems*. Boulder 1997.

- 27 M. Cox, *Power Shifts, Economic Change and the Decline of the West?*, in: *International Relations*, 26 (2012) 4, pp. 369-388; D. Flemes, *Network Powers: Strategies of Change in the Multipolar System*, in: *Third World Quarterly*, 34 (2013) 6, pp. 1016-1036; L. M. Jacobs, *Shadows of the Future. A Network Analysis of the Structural Evolution of the Global Power System between 1965 and 2005*. Unpublished PhD Thesis, Ghent University, 2014.
- 28 G. Arrighi, B. J. Silver, and B. D. Brewer, *Industrial Convergence, Globalization, and the Persistence of the North-South Divide*, in: *Studies in Comparative International Development (SCID)*, 38 (2003) 1, pp. 3-31; S. D. Sharma, 'The Many Faces of Today's Globalization: A Survey of Recent Literature', in: *New Global Studies*, 2 (2008) 2, pp. 1-27; R. P. Korzeniewicz and Albrecht, *Thinking Globally About Inequality and Stratification: Wages across the world, 1982-2009*, in: *International Journal of Comparative Sociology*, 53 (2012) 5-6, pp. 419-443; E. Vanhaute, *Historicizing Global Studies. About Old and New Frontiers of World-Making*, in: *Potentials and Challenges of Global Studies for the 21<sup>st</sup> Century*, Global Europe. Basel Papers on Europe in a Global Perspective, No. 105, Institute for European Global Studies, Universität Basel, 2014, pp. 50-59.
- 29 R. Clark, *World-Systems Mobility and Economic Growth*, in: *Social Forces*, 88 (2010) 3, pp. 1123-1152; E. L. Kick and B. L. Davis, *World-System Structure and Change*, in: *American Behavioral Scientist*, 44 (2001) 10, pp. 1561-1578.
- 30 J. Kentor, *Capital and Coercion: The Economic and Military Processes that have shaped the World Economy, 1800-1990*. New York 2000; S. J. Babones, *The Country-Level Income Structure of the World-Economy*, in: *Journal of World- Systems Research*, 11 (2005) 1, pp. 29-55; M. C. Mahutga, *The Persistence of Structural Inequality? A Network Analysis of International Trade, 1965-2000*, in: *Social Forces*, 84 (2006) 4, pp. 1863-1889.



one of the – if not *the* – most defining features of the global system of power relations today.<sup>31</sup> The work of the Italian-American political economist and sociologist Giovanni Arrighi is a challenging attempt to reconcile the political economy of capitalism with the call of global history to understand convergence and divergence, integration and hierarchy beyond established core-periphery relations.<sup>32</sup> His work shows in a comparative, incorporated and historical way how modes of production, circulation, consumption, and distribution are organized, and how they created and transformed modes of reproduction. Since this perspective has no meaning outside the system-bound world-historical coordinates, it rejects both abstract localism and abstract globalism.<sup>33</sup> Internal logics and transformations are formative to the system as a whole: “The globalization of historical capitalism must instead be represented as involving fundamental structural transformations of the spatial networks in which the system of accumulation has been embedded.”<sup>34</sup>

Since historical capitalism goes through cyclical phases of expansion and contraction, it continuously creates and recreates zones of contact or frontier zones. It is frontier-making through the recurrent waves of geographical expansion and socio-ecological incorporation of nature, land and labor. This coercion to put human and extra-human natures into the service of capital accumulation has gradually extended the zones of appropriation. These zones produce ‘cheap natures’ in the form of labor, food, energy, and raw materials in order to encounter capital’s rising costs of production.<sup>35</sup> Capitalist incorporation and expansion had been fuelled by the opening of the ‘Great Frontier’, a metaphor for an interconnected set of shifting frontiers. Frontier expansion provided an astounding wealth of nature that reduced production costs and increased profitability for centuries to come. For example, each successive food regime “has particular conditions for cheap food, and each relatively stable set of relationships are expressed in a world price governing production, circulation and consumption of food (...). The food regime is premised on forms of enclosure across time and space. This dimension is critical because enclosure alters ecological relations: substituting world-extractive for local-extractive processes, thereby foreclosing local futures for a capitalist future driven by variable and unstable

31 G. Arrighi and J. Drangel, *The Stratification of the World-economy: An Exploration of the Semiperipheral Zone*, in: *Review* (Fernand Braudel Center), 10 (1986) 1, pp. 9-74; Ch. Chase-Dunn and Bruce Lerro, *Social Change: Globalization from the Stone Age to the Present*. London / New York 2014.

32 G. Arrighi, *The Long Twentieth Century: Money, Power, and the Origins of Our Times*. New York 1994; id., *Adam Smith in Beijing. Lineages of the Twenty-First Century*. London 2007; J. F. Abbeloos and E. Vanhaute, *Cutting the Gordian Knot of World History: Giovanni Arrighi’s Model of the Great Divergence and Convergence*, in: *Journal of World-Systems Research*, 17 (2011) 1, pp. 89-106.

33 P. D. McMichael, *Food Regimes and Agrarian Questions*. Halifax / Winnipeg 2013, p. 12.

34 G. Arrighi, *Spatial and Other “Fixes” of Historical Capitalism*, in: *Journal of World-Systems Research*, 10 (2004) 2, pp. 527-539, here 538.

35 J. W. Moore, *Transcending the Metabolic Rift: A Theory of Crises in the Capitalist World Ecology*, in: *The Journal of Peasant Studies*, 38 (2011) 1, pp. 1-46; Id., “The Capitalocene, Part I: On the Nature and Origins of Our Ecological Crisis. Part II: Abstract Social Nature and the Limits to Capital” (2014), in: <http://www.jasonwmoore.com/Essays.html>; Th. D. Hall, *Incorporation into and merger of World-systems*, in: S. J. Babones and Ch. Chase-Dunn (eds.), *Routledge Handbook of World-Systems Analysis*, London / New York 2012, pp. 37-55.



market, rather than socio-ecological needs”.<sup>36</sup> Frontiers generate shifting sets of ‘localized’ activities to secure access to labor and land for ‘globalized’ commodity production (primarily agricultural, forest and mining goods). The sites where this happens become frontier zones. Frontiers connect the expansion of global commodity chains with the creation of unequal geographical and social spaces. As Beckert states in his fascinating story about global cotton: “The geographical rearrangement of economic relations is not just a noteworthy element of capitalism or an interesting aspect of its history; rather the shifting recombination of various systems of labor, and various compositions of capital and politics is the very essence of capitalism. (...) These frontiers of capitalism are often to be found in the world’s countryside, and the journey through the empire of cotton reveals that the global countryside should be at the center of our thinking about the origins of the modern world”.<sup>37</sup> Frontier expansion has often been associated with problems of social, economic and ecological sustainability. This results in the apparent need for these frontiers to be continually shifting towards new areas. Frontiers embody historical processes of both incorporation and differentiation that create and reorganize spatial settings. Frontier zones do not vanish after incorporation; they are permanently replicated by converging and dialectical processes of homogenization (the reduction of frontiers) and heterogenization (the creation of new frontiers).<sup>38</sup> Analytically, a frontier perspective can grasp the imbalances of incorporation processes, emphasizing the role of the margins and friction zones. Due to the incomplete nature of incorporation, frontier zones are the prime locus of negotiation processes about socio-economic commodification and socio-cultural assimilation. This frontier-focus requires research into similarities and differences, into connections and systemic changes. Frontiers determine exclusion and inclusion; they enforce new rules while giving space for resistance. Frontier zones have been the locus of both confrontation (war, resistance, lawsuits, intolerance, plunder, extraction, sabotage, ecological degradation, segregation) and cooperation (biological symbiosis, marriage, economic partnership, political bonds and treaties, celebration, conversion, gifts). Constant renegotiation forms a fundamental process in the shaping of ongoing, accelerating, retreating or stagnant incorporation processes. ‘Peripheral’ agents, such as peasant and indigenous movements, act within these ‘fault lines’. Frontier processes create concrete spatial settings, structured by asymmetrical power relations.<sup>39</sup> It is not the finiteness of frontier processes, the prevailing idea of a homogenizing world (convergence), but their permanence, the constant reproduction of instances of heterogenization (divergence) that must be questioned in world history.

36 P. D. McMichael, *Food Regimes*, 9.

37 S. Beckert, *Empire of Cotton*. A Global History. New York 2014, pp. 440-441.

38 E. Vanhaute, *World History*; Hanne Cottyn, *A World-Systems Frontier Perspective to Land*.

39 S. Sassen, *When the Center No Longer Holds: Cities as Frontier Zones*, in: *Cities*, 34 (2013), pp. 67-70.

#### 4. Limits and Challenges: Can World History survive Success?

Up to now this paper has addressed three major arguments. First: the Great Divergence, the single most important debate in recent World and Global History, both enlarged and redirected the long standing convergence/divergence dispute in social sciences. It unlocked new fields of research, introduced new approaches and created new data and knowledge. Second: the dynamics within the Great Divergence debate push it to go beyond its own terms and to transcend its own limits. Perspectives and methods tested within the Great Divergence debate challenge more general interpretations of the history of global capitalism. Third: global research into the processes of integration and hierarchy of global capitalism need to adopt a multilayered systems-perspective. Systems-analysis incorporates comparisons and connections in an integrated, hierarchical frame, and it allows for a combined, structural, top-down (geometry) and agency-driven, bottom-up (frontier processes) approach. I will conclude with two more general sets of epistemological reflections, one on the Great Divergence debate as it stands now, the other on applying a global focus in social research.

The Great Divergence debate has sharpened the discussions on the potentials and limits of a global or world-historical perspective. It has opened up fixed narratives that universalize particular, space-time bound experiences. On the other hand, it risks recreating new, fixed histories embedded in regional specificities. The only way to avoid new, fixed master-narratives or re-emerging essentialist regional/national stories is to continuously query new knowledge with comparative, interconnected and systemic research. In this perspective, the Great Divergence debate has compelled us to rethink some fundamentals of historical research. It shows how a change of perspective can change the whole story.

1. World historians are forced to invent and reinvent geographical schemes, to question the limitations of regional frames, and to debate how to connect and integrate the various spatial scales. Regions in a world-historical perspective are not a given; that is why they lack a spatial precision as countries. They are also multidimensional and overlapping, from the big Afro-Eurasian ecumene, maritime regions, border areas and rim zones, to small-scale social agro-systems. Within a given region, people share clusters of traits or connections that are different from those that they have with people beyond that region. Interacting regional histories make the world economy; a developing world economy also re-makes regions.
2. Capitalism is not an invention of eighteenth century England; it has its origins on a world scale from its start in the long sixteenth century (recalling Marx's famous quote that "world market and world trade date from the sixteenth century and from then on the modern history of capital starts to unfold").<sup>40</sup> This change in the time/space perspective makes it clear that historical capitalism is something completely different

40 R. Palat, *Dependency Theory*.

from the expansion of a free, Smithean market economy. It developed, using Braudel's phrase, as an anti-market where exceptional profits are reaped and monopolies are safeguarded; it makes use of the relentless competition between states. Still, the image of 'striking similarities' in the Great Divergence debate departs from the (mostly intrinsic) idea that agrarian market economies all over the world have the intrinsic potential to develop into capitalist growth centers. Capitalism develops where new, transnational commercial-financial elites ally themselves with assertive, mercantilist states. The commercial-agrarian empires in the eastern part of the Eurasian continent were not built on such alliances between capitalist and political elites. The new world-system, dominated by a European center, disturbed the existing balances of power at the expense of former regional empires. As Ravi Palat stated, "Economic agencies in Europe operated on an ever increasing scale in contrast to those in societies based on wet-rice cultivation where the size of economic agencies tended to become smaller and more specialized over time. This was the crucial difference between interstate systems in Europe and Asia: the former was predicated on capital accumulation, the latter was not. Moreover, the expansion of trade networks generated by the intensification of rice cultivation and the spread of craft production also led to dense networks of trade. The very density of trade networks meant that no single person or agency could monopolize lucrative lines for any substantial length of time".<sup>41</sup>

3. Most participants in the Great Divergence debate probably agree that its roots need to be explored in all their complexity, in order to cover the enormous range of transformations and innovations that arose with the emergence of modern economic growth. Despite the call for more holistic methods of analysis, interpretation schemes in the Great Divergence debate tend to remain monocausal; they still focus on the 'why not' question.<sup>42</sup> For example, China's 'failure' to precipitate the world's first scientific or industrial revolution has been explained in a variety of ways: political centralization, the stifling cultural hegemony of the elites, and the lack of independent institutions (David Landes); technological stagnation from the fourteenth century (Joseph Needham, Joel Mokyr); the success and efficiency of the commercial-agricultural system dominating state policies (Kent Deng), causing a 'high equilibrium trap' (Mark Elvin) and keeping wages low (and thus preventing the search to labor-saving inventions) (Gunder Frank, Bob Allen). In addition, the growth limits of a world-empire, in contrast to a world-economy (Immanuel Wallerstein), and the lack of a colonial empire (Kenneth Pomeranz). Back in 2007, Arrighi argued that we need "a more comprehensive model", since "the really interesting question is [...] how and why China has managed to regain so much ground, so quickly after more than a century of political-economic eclipse. Either way, a model of the Great Divergence must tell

41 R. Palat, *Convergence Before Divergence? Eurocentrism and Alternate Patterns of Historical Change*, in: Summerhill: Indian Institute of Advanced Study Review, 16 (2010) 1, pp. 42-58; id., *Power Pursuits: Interstate Systems in Asia*, in: *Asian Review of World Histories*, 1 (2013) 2, pp. 227-263.

42 J. Daly, *Historians debate the Rise of The West*. London / New York 2015.

us something, not just about its origins, but also about its development over time, its limits, and its prospects.”<sup>43</sup>

4. The Great Divergence debate both enlarged and redirected the long-standing convergence/divergence dispute in social sciences. It unlocked new fields of research, introduced new approaches and created new data and knowledge. It has sharpened the discussion on the potentials and limits of a ‘global’ or ‘world’ perspective. It has opened up fixed narratives that universalize particular, space-time bound experiences. These dynamics within the Great Divergence debate push it to go beyond its own terms and to transcend its own limits. Perspectives and methods tested within the Great Divergence debate challenge more general interpretations of the history of global capitalism. In addition, it urges historians to contextualize, rethink and sometimes reject concepts forged within Western social sciences. This is illustrated by the unceasing debates about the nature of (capitalist) economy, nation-states and states, formal and informal institutions, useful knowledge, and so on. While some authors stress the need for a more genuine supra-regional perspective, superseding the disjuncture between European and non-European knowledge, others conclude that this urges us to retreat into more particularistic frameworks: “Two-way comparisons may prove inconclusive since each is liable to reflect back only the other. The danger is of treating their differences as if they were of universal rather than special significance. (...) and not concern ourselves so much with the Great Divergence between Europe and the very different circumstances of China”.<sup>44</sup>

The proliferation of Global and World History in research and education over the last two decades has been impressive. This generated a swelling stream of publications on a wide variety of themes; some of them became bestsellers. World History has got out of the catacombs of Clio’s realm, to become ‘a house with many mansions’ that will stand for a long time to come.<sup>45</sup> The global building has become a landmark; it arouses admiration and envy. It also creates confusion since its size and composition is constantly changing. How does its global design relate to the many parts of the building? When and why does the house accept new occupants? Who designs the new mansions? Does rapid growth affect the outline and stability of the building? Is it still clear what belongs under the roof of Global and World History? Nevertheless, having become a strong brand, Global History has made an impressive march through the institutions, creating associations, networks, journals, series, periodic conferences, educational programs, and professorships. This has generated ongoing debates about content, methodology, data

43 G. Arrighi, Adam Smith in Beijing, p. 32.

44 E. L. Jones, Peer Vries on the History of Economic Growth, in: *Tijdschrift voor Sociale en Economische Geschiedenis*, 12 (2015) 2 [= E. Vanhaute (ed.), *Escaping the Great Divergence? A Discussion about and in Response to Peer Vries’s Escaping Poverty. The Origins of Modern Economic Growth*], pp. 79-92.

45 P. Vries, *Escaping Poverty*.

and sources, scales and units of analysis.<sup>46</sup> I conclude with what I see as five central ambitions in current world-historical research.

1. A world history perspective deconstructs both theories with universal aspirations derived from the historical experiences of the peoples of Northwestern Europe and North America, and the assumption of the state as a basic, self-enclosed and self-evident unit of analysis. By doing so, world historians have opened new windows on the global past and constructed visions related to this past from twenty-first rather than nineteenth-century perspectives.<sup>47</sup> The past shows itself to us as a complex of stratified time, stratified space and stratified social power relations. It calls for a holistic systems perspective; it aims at creating new meta-narratives. Specialization is an inevitable part of the production of new knowledge, but since history emphasizes contextual understanding, new knowledge is of very limited significance without on-going attempts at integration and synthesis. Global thinking does not decentralize or resurrect new dichotomies ('clash of civilizations'); it links and combines; it questions existing hierarchies (time, space, social) without flattening out history.
2. A world history perspective questions self-evident causalities and stories of path-dependency. Patterns observed in a global frame are often as much the outcome of geographical and historical contingencies as they are of historical necessity. Much of our social theory is prone to teleology, seeking the roots of an inevitable present rather than exploring contingency of past experiences.<sup>48</sup> World History does not reconstruct a singular march of humanity toward modernity; it portrays messy worlds and a multitude of historical experiences. It constructs visions of that past that are capable of accounting for both fragmentation and integration on multiple levels (local, regional, national, continental, and global). It builds frameworks that permit historians to move beyond the issues that have been dominating social sciences since the nineteenth century: cultural distinctions, exclusive identities, local knowledge and the experiences of individual societies and states. It facilitates the study of large-scale, border-crossing comparisons, processes and systems.<sup>49</sup>
3. A world-historical perspective adopts multiple spatial scales; it does not erase regional frames, it reinvents them. Interacting regional histories make up the world economy; a developing world economy re-makes regions. We need more bottom-up, regionally-focused research, especially on today's 'global South'. The research must have global structures and dynamics as its objective. New research perspectives like reciprocal comparisons (regions as subunits), integrating comparisons (cycles as subunits) and

46 P. Manning, *Navigating World History. Historians Create a Global Past*. Basingstoke 2003; P. Stearns, *World History. The Basics*. London / New York 2011; A. Komlosy, *Globalgeschichte. Methoden und Theorien*. Stuttgart 2011; M. Berg, *Global History. Approaches and New Directions*.

47 J. H. Bentley, *The Task of World History*, in: J. H. Bentley (ed.) *The Oxford Handbook of World History*, Oxford 2011, pp. 1-16; R. Palat, *Dependency Theory*.

48 K. Pomeranz, *Scale, Scope, and Scholarship*.

49 J. H. Bentley (ed.) *The Oxford Handbook of World History*, Oxford 2011.

frontiers (processes of integration / resistance) allow for a more bottom-up oriented focus within global research.

4. A world-historical perspective encourages more inter-disciplinary and trans-disciplinary approaches and alternative academic models based on teamwork, networks, collaboration and joint projects across the globe. In order to move world history to a new stage, it is very important that we remove the mental and material barriers that prevent the development of this kind of integrated research programs.
5. Last but not least, a world-historical perspective pushes for more cosmopolitan thinking; it questions old and new processes of integration, differentiation, adaptation and resistance. It creates emancipating stories; stories that connect human actions within a broader human-made world. It allows peoples to re-imagine their future. This is not a plea for legitimizing stories, but for a morally charged program. World History does not trade a national perspective for other exclusive frameworks, either global or sub-national. It does not essentialize new concepts like the non-West, the Global South or the subaltern. It tells us about the complexity of both the past and present worlds. It makes moral claims about the way in which the world functions today and how it could function tomorrow. Since differences and diversity are basic components of the human story, the global perspective shows that understanding and handling differences is an important moral skill. Claims, interpretations and evaluations cannot be made solely within the framework of our own known world; they must reflect the complexity of human history. With the global perspective, history strikes back. It integrates time and place, deals with interactions and the hierarchy of scales in the human world. This makes it a barrier against the threat of an undifferentiated multitude of new stories, and it advances the levels of ambition, time, place and themes, of questions and answers. Historicizing does not create a new, totalizing master-narrative, only a lack of historical knowledge can do that. A global perspective is, by definition, highly ambitious; it interrogates processes of 'world-making', of social change, in a broad time-space context. It compares, it connects, it incorporates, it systemizes. Global and World History deconstruct world-making processes and construct new world-making narratives. That is why the global perspective is inclusive. It includes outer worlds and outer times in our world; it includes 'us' in our narrative.<sup>50</sup>

50 This text is completed in Fall 2015.

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# LITERATURBERICHT

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## **Between World and Nation: The long 19<sup>th</sup> Century in a Global Perspective\***

**Zinaida Chekantseva**

The present book is an integral part of the new major academic six-volume edition of World History, conducted by the Institute of World History of the Russian Academy of Sciences upon the initiative of academician Alexander Chubarian – the editor-in-chief of this book series.<sup>1</sup> Stressing the aim and scope of this edition, Lorina Repina, corresponding member of the Russian Academy of Sciences, emphasized some crucial aspects of this project: “This series must have definitely scholarly nature and give an up-to-date insight into world history. It must be world history with no Eurocentrism, revealing both historical and cultural specifics of each region, and centuries-old expansion, extension, and intertwining of ties among nations and cultures, aiming to combine the global and civilization approaches with the anthropologic one in order to contribute a ‘human dimension’ to history. Its structure is predicated upon a coherent combination of chronological and topical principles, including sections, which propose a comparative perspective. Each separate volume will take some chronological periods as a basis of its structure, which allows for an overview of the history of various nations in their interaction.”<sup>2</sup>

The project required a long lead time as well as the creative effort of many Russian scholars of various generations and epistemological orientations. Authors of this volume represent not only the Institute of World History but also other Russian academic institutions and universities. In a way, one may consider this publication as a collective portrait of the contemporary professional community in the field of history in Russia.

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\* Review of: V. S. Mirzekhanov et al. (ed.s): *The World in the 19<sup>th</sup> century: On the Way to the Industrial Civilization* (= *World History*, vol. 5, edited-in chief by A. O. Chubarian), Moscow: Nauka, 2014, 940 p. (russian).

1 This work was financially supported by grant № 15-18-00135 of the Russian Science Foundation.

2 L. P. Repina, *Tsivilizatsionnaya paradigma v staryih i novyih modelyah mirovoy istorii. Istoriya i teoriya tsivilizatsiy: v poiskah metodologicheskikh perspektiv*, Vladivostok 2015, p. 14.

The narration of world history in the nineteenth century consists of three unequal parts, beginning with a detailed introduction that describes the general concept of the volume, its structure, and the main epistemological approaches (written by V. S. Mirzekhanov). The first part “The Development of Industrial Society: Globalizing Trends” consists of eight chapters, which depicts key processes of the industrializing society: “Industrial Revolution” (A. V. Revyakin, V. S. Mirzekhanov), “Economic Growth, Demographic Transition, and Mass Migrations” (V. A. Melyantsev, V. S. Mirzekhaov, S. B. Volfson), “Social Processes” (A. A. Isserov), “Languages of Culture of the 19<sup>th</sup> Century” (I. V. Kondakov, V. S. Parsamov), “Education and Science” (A. N. Dmitriev, N. V. Rostislavl-eva, M. V. Loskutova), “Medicine in the 19<sup>th</sup> Century” (A. M. Stochik, S. N. Zatravkin), “Religion and Church” (S. G. Antonenko), and “Politics and Society” (N. P. Tanshina, M. P. Aizenshtat).

The second part “World-system of the 19<sup>th</sup> Century” consists of a theoretical introduction, “Empire and Nation in ‘the long 19<sup>th</sup> century’” (A. I. Miller), and four sections. The first is about “Europe and the World: A Treacherous Path to the Global Political System,” which is divided into ten chapters; the narrative is structured along country-specific and regional topics: “Pax Britannica: Great Britain” (M. P. Aizenshtat), “Pax Britannica: Dominions” (A. A. Isserov, A. N. Uchaev), “Pax Britannica: India” (L. B. Alayev), “France: From Napoleonic Despotism to the Parliamentary Democracy” (A. V. Revyakin), “The Sunset of Spanish Empire” (I. Yu. Mednikov), “Portugal: the Decline of Great Empire” (A. P. Chernykh), “Netherlands: Minor European Country – Major Colonial Power” (G. A. Shatokhina-Mordvintseva), “Belgium: Kingdom and Empire” (A. S. Namazova), “South-East Asia” (V. A. Tyurin), and “Sub-Saharan Africa” (A. S. Balezin). The second section, “The Transition Century of Western and Eastern Monarchies: From Ancien Regime to the Modern Age,” presents the following chapters: “Russian 19<sup>th</sup> Century” (V. S. Parsamov), “Habsburg Monarchy in the 19<sup>th</sup> Century” (E. V. Kotova), “Ottoman Empire in the 19<sup>th</sup> Century” (S. F. Oreshkova, M. S. Meyer), “The Arab World: the Long Search for Renewal” (B. V. Dolgov, E. A. Prusskaya), “Iran under Qajar Dynasty” (A. I. Polischuk), and “China and the World: Contradictory Modernization Processes” (O. E. Nepomnin). There are five chapters in the third section dealing with the overall theme of the “National Idea, Emergence and Development of Nation-States”: “Japan on the Way to the Great Powers” (S. B. Markarianz, E. V. Molodiakova), “Germany: Implementation of a National Dream” (A. G. Matveeva), “Italy in the 19<sup>th</sup> Century: the Risorgimento” (Z. P. Yakhimovich, A. A. Mitrofanov), “North Europe: on the Way to Welfare” (V. V. Roginsky), and “The Formation of Nation States in South-Eastern Europe” (O. E. Petrunina). The fourth section contains three chapters: “Western Hemisphere: Continuity and Changes” (A. A. Isserov), “The USA: on the Way to Might” (B. M. Shpotov), and “Latin America: Age of Independence” (M. S. Alperovich).

The third part of the volume on “Inter-State and International Relations in the 19<sup>th</sup>–early 20<sup>th</sup> Centuries” includes three chapters: “Napoleonic Wars and the Vienna System of International Relations” (V. V. Roginsky), “World Order, Wars and Foreign Relations in the mid-19<sup>th</sup> Century” (V. V. Roginsky, V. N. Vinogradov), and “World Politics of the



Last Third of 19<sup>th</sup> Century—early 20<sup>th</sup> Century” (A. V. Revyakin). The volume ends with a conclusion, “The World in the 19<sup>th</sup> Century: the Historical Outcome and Orientation to the Future,” by V. S. Mirzekhanov, the editor-in-chief of the entire volume.

The contemporary world, which lives “in the twilight zone between the passing of the national era, and the emergence of the cosmopolitan era” (Ulrich Beck), desperately needs to reinvent an appropriate language and conceptual framework. At this level, the humanities and the social sciences reconsider the perennial epistemological questions: What is the ideal status of sociohistorical knowledge in society? What is the specific nature of historical knowledge and its approaches? What language is indispensable to represent human-scale historical reality? How to overcome an inevitable ideological and sociocultural dependence of a cognizer? And so on. By allowing the vision to be decentred, the global approach invites a historian to study geographic areas and its population thoroughly while promising advanced opportunities to study events and processes at various scales. However, as reflected by discussions in scholarly literature, those opportunities are to be gained if and only if scholars make maximum use of the most recent studies.

While reading the fifth volume of *World History*, I tried to estimate in general where the novelty of this volume lies. How, in what way, and how far did the authors of this massive volume (939 pages) succeed in changing our interest in world history in the nineteenth century?

History of the nineteenth century in a global perspective is unachievable without a reflection on the relativity of timelines. This is due not only to a multiplicity of calendar systems in the world but also to the fundamentally heterogeneous historical time frame. In the book under review, the concept of *the long 19<sup>th</sup> century* (1789–1914) is the temporal framework in which searching for coordinates in a multilayered historical matter is undertaken. The conventionality of the chronological framework chosen for the study is evident. For instance, Jürgen Osterhammel wrote that to close the nineteenth century “with a sudden fall of the curtain in August 1914” is not completely valid because “only when the war was over did humankind realize that it was no longer living in the nineteenth century.” To open a long nineteenth century with the French Revolution is also conventional as its impact on contemporaries in their present world took place only in the West.<sup>3</sup> The constructivist nature of the concept of a long nineteenth century is balanced out in the book with the presence of many more nuanced systems of periodization within the boundaries of states, regions, and continents, included in a description of historical events and in country-specific, regional, and topic essays.

Substantial features of the nineteenth century contrast with others in multivalent ambiguousness. This period is largely comprehended as a transitional one, one which prepared modernity. However, the simplicity of the notion modernity is misleading. Much depends on the research approach chosen by the scholar. If we understand modernity

3 J. Osterhammel, *Transformation of the World: a Global History of the Nineteenth Century*, Princeton 2014.

as a project of modernization (successful/ unsuccessful, finished/ delayed), like the book offers, then usual teleological thinking, inherent in the idea of evolution, inevitably creates images of a development from simple to complex. Despite paying attention to gaps, declines, and retreats, these images are still not less convincing. Although F. Braudel wrote that history moves as a Spanish procession, where each step forward is followed by two steps backward, other approaches are also available. For example, M. Foucault suggested perceiving modernity not as a chronological period with certain features but as an attitude (epistemological and ethical at once) of a researcher who studies the epoch. This perception assumes thoughtful use of a notion – representation, that is to say studying the ways of thinking/ sensing/ acting indigenous to specific persons – rather than some abstract historical process. Moreover, the theory of “multiple modernities” that is under discussion for the last 15–20 years still remains a challenge when using it with historical materials. One thing is clear; the inclusion of the “man-sized reality” is impossible without broad anthropological understanding of culture and reflexive positioning of a scholar. Alongside the idea of duration, the idea of transformations’ asynchrony in various parts of the world is of great concern to the concept of the volume. Furthermore, it is convincingly demonstrated that the choice of modernization tools depended on a ratio of economic orders as well as on a situation of state institutions and social elites in various countries.

There is no East-West division of the world in the book. Such notions and the dichotomy itself are problematized in the introduction, and their relativity is determined by the volume’s structure and the narrative organization in the synthesis chapters. In terms of space, the long nineteenth century is presented in the light of a world-system, that is to say a centre and a periphery, where colonial and continental empires played a significant role along with nation-states.

The theoretical introduction, gravid with ideas and generalizing characteristics, promises an emphasis on dynamism and divergence of the development processes, as well as a global view on occurrences, processes, events, and historical figures with due consideration of recent approaches to study world history as a history of civilizations, history of transfers, transnational history, connected history, entangled history (p. 14). The structure of this volume is organized in accordance with global and local perspectives. Its first part provides a general description of the most important phenomena of the emerging industrial world: industrial revolution, economic growth, demographic transition, mass migrations, social processes, languages of high culture, education, science, medicine, religion, church institutions, and politics. The conjunction between local and global is illustrated in the introduction and conclusion as well as in a substantial analytical chapter, “Empires and Nations in the Long 19<sup>th</sup> Century,” which introduces the second, the largest part of the volume.

National narratives described the nineteenth century as the age of nations and nationalism and imperial projects of that period as unrelated to nation-building. This book convincingly shows a strong necessity to intensely rethink the interrelations between national and imperial. Nation-building was woven into imperial practices both in the

metropole and on the periphery. This approach opens up new opportunities for understanding the nature of nationalism, integrational imperial projects at the end of the century, and the originality of the first modernity in general. Imperial projects outside of Europe and national transformations in Europe are presented side by side in the volume. It is interesting but not yet sufficient to show how they were interwoven and mutually remodeling each other. Additional perspectives and research approaches of recent studies in new global history,<sup>4</sup> not included in specific sections of the volume, are crucial.

The third part examines interstate and international relations in the nineteenth and early twentieth centuries. This part of the volume remains Eurocentric. International history is described in a linear fashion and within the disciplinary framework of international relations, with its typical key topics: diplomacy, wars, and geopolitics. As in many traditional studies of international relations, the world appears as the scene of international discord, conflicts, compromises, and alliances among national actors. Meanwhile, intellectual transformations of the field in recent decades – for example, interdisciplinarity due to the cultural and spatial turns, and also a renewal of social history related to them – has brought a fresh perspective. Historians have started to search for combined methods of micro- and macroanalysis on global and local levels. International relations are no longer considered as only the results of governmental actions in connected and entangled history but also as important factors in the development of nations, tightly bound with transfers of ideas, concepts, technologies, materials, civilization values, etc. Additionally, colonial aspects of international relations are naturally intertwined with European history. Everything that is written in the introduction on these problems presents a new perspective for interpretation: “formation of empires led to strengthening of national at the expense of colonial and to their active convergence”; “overlapping of universalism and nationalism, of national states and empires”; “imperial idea and colonial culture became a part of the mass culture of metropolitan countries” (pp. 16–18). However, there is not much on the mass culture of the metropolitan countries in the book. It raises, however, the question how it was perceived in colonies both on the elite and mass levels? What can postcolonial studies tell on the matter?

Many of these inferences are still open and lively debated in world historical literature.<sup>5</sup> It is a problem whether marking the *nineteenth* century as a separate period is possible at all because there is a high risk of falling into the trap of Eurocentrism, and, as this volume demonstrates, to abandon such an orientation is no easy task. It denies the fundamental role of this age in the transformation of the whole world: many factors and processes, described in the book, primarily affected Europe. For instance, an essay in the volume on culture examines only European high culture and states that it influenced the whole world (p. 19) despite that modern humanities research, including new global history,

4 See, for instance, P. Boucheron / N. Delalande, *Pour une histoire-monde*, Paris, 2013.

5 See, Peter N. Sterns: Rethinking the Long 19<sup>th</sup> Century in World History. Assessments and Alternatives, in: *World History Connected* 9 (2012) 3, URL [http://worldhistoryconnected.press.illinois.edu/9.3/forum\\_stearns.html](http://worldhistoryconnected.press.illinois.edu/9.3/forum_stearns.html) (accessed: 18 May 2015).

has gathered plenty of evidence for a mutuality of cultural influences. There are studies demonstrating that the influence of peoples outside Europe on the industrial revolution's processes, as well as on other regions and continents, was significant. Much of a stable representation of Eurocentric foreign affairs in the nineteenth century is due to the lack of attention to the global transformations in the history of international relations until recently.<sup>6</sup>

This volume gathered and mastered a wealth of empirical knowledge. The majority of essays are well written. It is a good, highly interesting read. However, many relatively new topics and subjects of world history are not fairly represented. Studies of memory; images; discourses; discourse structures; myths, which shaped historical memory; historical representations of the past epoch; and value conflicts in societies are not examined in the volume. The emphasis, rather, is on political, economic, and, to a lesser degree, social history. The latter only received scant attention because the sociological turn, reflected in the topic (see "Social Processes") and country-specific essays, did not inform specialists of the history of specific European and non-European countries. Social changes are comprehended in long-condemned terms and notions, and social processes are habitually only linked to economics and politics. The cultural, and therefore the anthropological, component of these processes is not represented.

Some chapters lack modern historiographic matters. For example, colonization, as the main embodiment of communicative practices of European and non-European peoples, is understood in line with traditional colonial history although the introduction argues persuasively for the need to appeal to the postcolonial historiographic tradition of the last 50 years (p. 18–19). Moreover, cultural practices, including communicative ones, could be found in traditional and renewed history of transfers in the research field of ideas and concepts, scientific discoveries and technologies, information, managerial and educational skills, etc. The volume falls short of such perspectives – as well as entangled and connected ones – as they could not be presented because of the apprehension about these approaches in the Russian historiographical tradition has only just begun.

The majority of country-specific and regional essays strictly follow the informative and impersonal requirements of conservative educational and encyclopaedic thinking. Perhaps, it is a conscious choice of the editors, which allowed various methodological preferences of authors to be combined. However, such a choice has some disadvantages, which appear noticeably in the imbalance between the ratio of the new approaches declared in the introduction and some survey essays, and the content of a considerable part of the volume.

But besides all criticism, this book is a fresh and original contribution to the debate about world history. An experienced reader can learn a lot about the history of various nations of our planet while contemplating the state of Russian modern history studies. A less informed reader, interested in the historical process, will read many pages of this

6 B. Buzan / G. Lawson, *The Global Transformation. The Nineteenth Century and the Making of Modern International Relations*, in: *International Studies Quarterly* 57 (2013) 3, pp. 620–634.

volume, including those written in accordance with the long-established rules of a historical profession and its practice of explaining “as it was in actual fact,” with pleasure. It is possible that this volume will inspire some critics to ask “simple” questions. What is capitalism? What is the difference between petite and big bourgeoisie? Why is there so little about material culture? Should we oppose culture and technologies? How has the understanding of a state and a society changed? Why is there nothing on mentality, imagination, and so on in the first part of the volume? Beyond doubt, this questioning will stimulate the next “rewriting” of history that actually allows the historical knowledge to maintain its social value as an open and truly non-complete human experience.

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## BUCHBESPRECHUNGEN

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**Emmanuel Akyeampong et al.:**  
**Africa's development in historical**  
**perspective. Cambridge: Cambridge**  
**University Press, 2015, 539 S.**

Reviewed by  
Felicitas Becker, Cambridge

There has been a dramatic revival of interest in African economic history in recent years. A handful of economists have started using African history as a quarry for their arguments; most famously, Daron Acemoglu, Simon Johnson and James Robinson have used African data to argue for the decisive role of institutions in creating or obstructing economic development. At the same time, the rise of comparative history has revived the question how Africa fits into such global-scale schemes of progress and regression. Against this background, Gareth Austin's long-standing research into the conditions of agricultural growth is finding a wider audience, and several of his students, including Leigh Gardner, Morten Jerven and Ewout Frankema, are using advances in data processing to re-examine long-known and locate new sources of numerical information, making African economic history more numerate than

seemed possible earlier, given the quality of the sources.

The edited volume under review here brings together economic historians such as Austin and Inikori with others not specifically known as economic historians, such as Christopher Ehret and Linda Heywood, a historical linguist and an urban cultural historian respectively. Their entry point into African economic history is less through re-examining quantitative evidence or Acemoglu-style regressions, than via the desire to see applied to Africa some of the theoretical developments in economic history that have so far focused on other areas. In particular, they engage with two of them. One is the deployment in the history of developing countries of what is known as 'new institutionalism': the insistence that it is above all else the nature of a society's institutions that determines its economic outcomes, rather than, say, natural resource endowments or commercial exchange. The other is the debate on the 'great divergence': in other words, on the reasons why industrialization happened in Europe and not in other places with a sophisticated pre-modern material culture and strong economy, such as China's Yangtze Delta.

The result is an absorbing, fascinating, and at times frustrating book. It takes an enjoyably wide perspective on economic

history, considering the economic role of cities (Heywood), changing patterns of trade (Akyeampong) and entrepreneurship (Olukoju), the role of malaria (Weil) and pre-colonial warfare (Reid), as well as that of mission culture (Nunn), the improvement of transportation in the colonial period (Chaves, Engerman and Robinson) and 'Imperial Peace' (Bates) among others. Some of the chapters present syntheses of long-pursued research; others very fresh material, and depending on their own specialization, every historian of Africa is bound to find plenty here that is of interest. Rather than summarizing every chapter, the rest of this review proceeds by picking up on some recurrent themes that, I think, help examine the promise and the problems of the editors' undertaking.

Given the enormity of the topic addressed, easy agreement either among the authors or among their readers was not to be expected. There is nevertheless one point which emerges from this collection fairly uncontroversially. This is that there was indeed a 'divergence' in the history of Africa, relative to other continents. Africa was not always the poorest place on earth. Christopher Ehret's discussion of archaeological and linguistic evidence shows that, taking a long-term view, parts at least of Africa did well in terms of urbanization, population density, technological innovation and market development until several centuries after the birth of Christ.

The measures that Ehret uses for Africa's prosperity in the distant past, though, carry a certain baggage with them, whose problems become more evident as the reader works her way through the chapters: they implicitly take for granted the association of economic development with

political centralization, with the role of states and the kind of political economy states tick over by. Given that a relative lack of states is a distinctive feature of African history, this reader wonders what this approach misses. Some authors here clearly take urbanization and political centralization as correlates, and both as preconditions for development, in a way reminiscent of long-standing ideas about the history of civilisations. But even Ehret, who is among these authors, points out that technologies, including iron-working, advanced also outside states in Africa. Inikori in particular points to the prosperity of some stateless societies in Africa. Given the salience of such societies in African history, the question of how to define and assess their economic status hovers in the background of this collection somewhat uneasily. We know very well that many pastoralists, for instance, passed as wealthy in their own and their neighbours' minds, nomadic lifestyle notwithstanding. But how should economic historians parse their wealth-in-cattle?

The focus on the sort of centralized social formations recognized as progressive and wealth-creating in normative histories of early economic development also carries problems in the context of the history of the slave trade. Some of the strongest states in early modern and pre-colonial Africa lived off enslavement and slave trading. Were such states developmental, then, or were they predatory? Or does the question have to be considered on a case-by-case basis? Inikori's chapter on the economic effects of Africa's participation in the Transatlantic trade is clear that the slave trade created 'deep economic inefficiencies'. But for the Americas, whose 'new institutionalist' his-

toriography the editors of this volume cite as an inspiration, there have also been influential claims that slavery created proto-modernisation, e.g. in the sugar industry. In Africa itself, the control over labour enabled by slavery arguably helped intensify agricultural production and supported export-trade participation. Thus Inikori's summary dismissal of slavery as economically disadvantageous sits awkwardly with the implicit assumption, in other parts of the book, that centralization of power (often for the purpose of controlling labour) is a proxy for development.

The implicit normative model at work here connects centralization to economic specialization, thus to greater material sophistication, accumulation of wealth and, ultimately, economic development. There are good reasons why variants of this model have persisted; it is eminently plausible. But in one way or another, they involve a factor which the editors to this volume are reluctant to engage with: the role of the environment, more specifically resource endowment, in economic development. In their introduction, the editors dismiss resource constraints as a cause of Africa's current poverty. They argue that resource constraints are permanent, but Africa's relative position among continents, development-wise, has changed – ergo resources cannot be the cause of Africa's position. The problem with this reasoning is that the meaning of the term 'development' has changed enormously over the longue durée the editors so rightly insist on examining. If Africa's resource endowment was adequate to keep it comfortably in the middle, or even front, of the field in 1500, that does not mean it was still adequate, let alone advantageous, in 1800 or 1900.

It seems regrettable to this reader that the editors' dismissal of resource endowment approaches also precludes them from taking seriously long-standing arguments about the role of people shortage and land abundance in African history. For example, in a chapter about the non-developmental role of certain cultural institutions, Olukoju discusses the conspicuous spending of major traders in nineteenth-century Yoruba towns as a form of unproductive, wasteful consumption. If he recognized the possibility that creating and maintaining followers was a serious political problem, he might instead look upon it as the pursuit of patron-client ties; thus as an element of a political economy that turned in part on the need to parlay wealth in things into wealth in people. He might still want to argue that this kind of political economy was not necessarily developmental. But it would prevent him from taking cultural values as an independent variable, and finding Yoruba culture flawed by its unaccountably flamboyant, wasteful values.

Similar points apply to Platteau's chapter on the (as he sees it) anti-developmental role of egalitarian values in many African societies. His argument is focused on a well-known anthropological claim: that witchcraft beliefs are really a way of making judgments about economic inequality and preventing or reducing economic stratification. Accumulators who might otherwise take the economy forward, so the reasoning goes, are reined in and held back by the threat of witchcraft accusations. The connection between witchcraft and egalitarian discourses in contemporary Africa is not in dispute. But there is reason to doubt whether it does much to hold back accumulators. Many societies



where moral mistrust of the rich flourishes have also seen successive generations of successful, at times quite ruthless accumulators. And witchcraft beliefs are nothing if not ambivalent: Martin Chanock, for instance, has shown that big men could also use witchcraft accusations to control followers, in particular, wives.

The kind of discussion of cultural features as obstacles to development exemplified in Platteau and Olukoju's chapters is noteworthy also for treating these features as independent variables: they ask what economic effects cultural practices have, but not why they exist or how they change. As mentioned above, there are ways of connecting cultural features to livelihoods; to the horizon of possibilities offered by environments and other structural factors. Granted, the attempt to do so needs to take care to avoid functionalist reduction. But it may not be much of an improvement to reduce obstacles to development to cultural features instead.

Evidently, then, this is a book worth arguing with and over. It is also worth noting that the refusal to accept environmental limitations as a crucial factor can be seen also as a way of living in hope. If factor endowments are to blame, there is no easy answer to the question of how these limitations can be surmounted. By contrast, institutional factors, whether political or cultural, are clearly very malleable. It is a different question, though, how open they are to the kind of directed change that would be required to implement recommendations derived from institutionalist analyses. And are environmental limitations really that inflexible? Austin's chapter on agriculture and Manning's on population suggest otherwise. This volume pro-

vides plenty of food for thought on these questions and many others, and it is to be hoped that it is not the last of its kind.

**Jan Hüsgen: Mission und Sklaverei. Die Herrnhuter Brüdergemeine und die Sklavenemanzipation in Britisch- und Dänisch-Westindien (= Missionsgeschichtliches Archiv, Bd. 25), Stuttgart: Franz Steiner Verlag, 2016, 238 S.**

Rezensiert von  
Adam Jones, Leipzig

Im Leserraum des Unitätsarchivs in Herrnhut hängt das Ölgemälde von Maria, einer ehemaligen Sklavin aus der Karibik, die nach ihrer Befreiung fünf Jahre in Deutschland lebte und dort 1749 starb. Für mich schien dieses Bild die humanitäre, universalistische Weltanschauung der damaligen Herrnhuter Brüdergemeine zu verkörpern, die viel früher als andere Protestanten mit der Missionierung entfernter Erdteile begannen und bei denen trotz großer sozialer Unterschiede jede Person in gewissem Sinne als „gleich vor Gott“ galt. Zwar wusste ich, dass die Herrnhuter von Anfang an „notgezwungen“ Sklaven auf ihren Plantagen in der Karibik und Suriname besaßen, aber in der Historiographie wurde der Plantagenkauf paradoxer Weise als indirekter Beitrag zur Sklavenbefreiung und zur „Neuordnung der gesellschaftlichen Verhältnisse“ dargestellt.

Das auf eine Dissertation zurückgehende Buch von Jan Hüsgen über zwei Karibikinseln – das britische St. Kitts und das dänische St. Croix – stellt dieses Bild des Verhältnisses der Herrnhuter zur Sklaverei im 18. und frühen 19. Jh. grundsätzlich in Frage. Es stützt sich auf eine beeindruckende Menge an Archivmaterial in Deutsch, Dänisch und Englisch, nicht zuletzt aus dem Unitätsarchiv. Es behandelt drei Phasen: die Sklaverei, die Abschaffung und die Zeit danach.

Das Kapitel über die Sklaverei befasst sich zum einen mit dem Diskurs der Brüdermission (Gleichsetzung von Sklaverei und Leibeigenschaft), zum anderen mit den ökonomischen Strategien in der Karibik; denn die Sklaven wurden in erster Linie – ob auf einer Zuckerplantage, im Haus oder in einer Schmiede – als „Teil des Missionsvermögens“ und „nicht als Mitglieder einer gemeinsamen Religionsgemeinschaft“ wahrgenommen. Hüsgen hat zahlreiche wirtschaftshistorische und demographische Daten zusammengebracht. Vorsichtig argumentiert er, dass die hohe Geburtenrate auf der Missionsplantage Bethel auf eine bessere (wenn auch ökonomisch begründete) Behandlung als auf den nicht-missionarischen Plantagen hindeutet. Besonderes Augenmerk widmet er dem Kauf und Verkauf von Sklaven und den verschiedenen denkbaren Motivationen dafür. Er zeigt, dass zumindest einige Sklaven Widerstand leisteten, und dass manche als Strafe verprügelt bzw. verkauft wurden.

Das folgende Kapitel beschreibt die langjährigen Konflikte zwischen der Brüdergemeine und der britischen Anti-Slavery Society in den 1820er bis 1840er Jahren. Auch nach der Abschaffung der Sklave-

rei in den britischen Kolonien im Jahre 1834 weigerten sich die Herrnhuter ein Jahrzehnt lang, ihre Sklaven auf St. Croix freizulassen. Die Hauptentscheidungen wurden nicht in der Karibik, sondern – wegen der hierarchischen Struktur der Brüderunität – in Herrnhut getroffen. Noch 1844 konnte ein schottischer Reise-schriftsteller polemisch fragen: „Wo in der Welt ... gäbe es außer in Herrnhut noch eine Kirche, [welche] die Sklaverei nicht verurteile?“

Dass viele Sklaven nach der sogenannten Emanzipation „lieber in ihrem unfreien Stande bleiben wollten“, lag, wie Hüsgen im 4. Kapitel zeigt, nicht daran, dass sie mit dem Verhältnis zur Mission zufrieden waren, sondern an der fehlenden Autonomie, die mit der Emanzipation einherging. Hier wäre ein Vergleich zur Kapkolonie (Südafrika) angebracht gewesen, wo die Herrnhuter sich ebenfalls mit dem Problem der Sklaverei auseinandersetzen mussten und der Sekretär der Brüdergemeine in England, Peter Latrobe, eine ähnliche Vermittlerrolle wie in der Karibik spielte. Wie in Südafrika stellten die langen Übergangszeiten (*apprenticeship system*) im Grunde nur eine Verlängerung der Sklaverei dar, und in beiden Fällen fehlte den emanzipierten Sklaven der erforderliche Grundbesitz, um sich als selbständige Bauern zu etablieren.

In diesem Kapitel erfahren wir viel über die damalige Brüdergemeine. Man verwendete dieselben Bibelpassagen, die in der Zeit der Sklaverei für eine Predigt gegen das Streben nach körperlicher Freiheit gedient hatten, jetzt dazu, einen „vernünftigen Gebrauch“ der Freiheit zu predigen; d.h. die ehemaligen Sklaven wurden damit ermahnt, weiterhin auf den Plantagen zu ar-

beiten. Einerseits betont Hüsgen die „Exklusivität und Hierarchie“ sowie die starke Sozialkontrolle der sich um die Missionare entwickelnde Gesellschaft, andererseits die Möglichkeit sozialer Mobilität für beide Geschlechter. 1856 kam es zur ersten Ordinierung eines schwarzen Pfarrers in der westindischen Brüdergemeine. Hüsgen analysiert die Rolle von Missionsschulen und die Debatten innerhalb der Brüdergemeine darüber, ob man für den Unterricht anstatt Mitglieder der englischen Brüdergemeine (die wegen ihrer Sympathie für die Sklavenemanzipation ungeeignet erschienen) schwarze Lehrer einstellen solle. Generell sind Hüsgens Urteile quellenkritisch und differenziert. Mir gefällt besonders die Art, wie er Zitate kommentiert und zwischen den Zeilen liest. Der Text wird durch fünf Diagramme und zehn Tabellen bereichert. Leider gibt es keine Karte, keinen Index und viele Schreibfehler. Trotzdem stellt dieses Buch einen Meilenstein in der Historiographie der Herrnhuter und der Karibik dar.

**Michael Kemper / Artemy M. Kalinovsky (eds.): Reassessing Orientalism: Interlocking Orientalologies during the Cold War (= Routledge Studies in the History of Russia and Eastern Europe, vol. 23), London: Routledge, 2015, 236 S.**

Reviewed by  
Matthias Battis, St. Petersburg

The present volume continues what Michael Kemper & Stephan Conerman (eds.) have begun with *The Heritage of Soviet Oriental Studies* (2011), namely to “integrate the Russian and Soviet traditions of Oriental studies into current Western debates on the study of the Orient, and also of ‘Orientalism,’ where the Eastern European and Central Asian cases have so far hardly been visible” (p. 3). Both books argue that, on the whole, Edward Said’s thesis of Orientalism as “a Western style for domination, restructuring, and having authority over the Orient”<sup>1</sup> also applies to Soviet Oriental studies: “Our assumption is that Soviet studies on the Orient fit very well into this [Saidian, MB] definition[.]” Kemper and Conerman argued in 2011.<sup>2</sup> Likewise, in *Reassessing Orientalism* Artemy Kalinovsky and Kemper write that the connections between “power and knowledge, between research and policy, between a discursive ‘Orient’ and real administrative power exercised there, are all present in [...] the Soviet case” (p. 3). At the same time, both books do not merely apply Said’s thesis of Oriental studies as

a handmaid of imperialism to the Soviet case, but qualify it by highlighting the domestic specificities of Soviet Orientalology. *Reassessing Orientalism*, moreover, goes further than that by placing the development of the discipline “in a broader Cold War setting[,]” (ibid.) and by scrutinising the global connectedness of Oriental studies across the geopolitical and ideological divide.

Like its predecessor, the present volume argues that several factors need to be taken into account to better understand the nexus of Orientalist knowledge and power in the Soviet case: The huge political pressures under which Soviet Orientalists worked (this is especially apparent in Alfrid Bustanov’s chapter on Soviet Oriental studies in Kazakhstan); the active integration of “national cadres,” including scholars from a traditionally Muslim background, into the academic process (see Bakhtiyar Babajanov’s chapter on the careers of *madrasa* graduates-turned Soviet Orientalists); the fact that the discipline “became part of a broader dynamic of nation building within a Soviet framework” (p. 2). (I will elaborate on this later); last but not least, the distinct anti-colonialist and internationalist agenda of Soviet Oriental studies, which entailed an explicit critique of West-European Orientalist scholarship<sup>3</sup> (see both the introduction and Masha Kirasirova’s chapter on US and Soviet imaginaries of the modern Middle East).

However unlike its predecessor, which focussed on the domestic history of Soviet Oriental studies, *Reassessing Orientalism* aims to “transcend the framework of domestic dynamics and develop a focus on processes of interaction with other scholarly traditions” (p. 4). In particular, it com-

pares “the Soviet production of knowledge on the Orient with the ways Oriental studies developed in the US [...]” (p. 3.). For that reason, the book contains a chapter by Ruud Janssens on the history of Japanese studies in the United States, which “points to the necessity to nuance Said’s ideas about Orientalism” (p. 146). Moreover, the volume argues that Cold War rivalry led to the politicisation and institutional expansion of Oriental studies in both countries and, more importantly, that this scholarly and political rivalry led to direct and indirect interactions between scholarly communities in the USSR and US. Challenging “the widespread assumption that Soviet Oriental studies developed largely in isolation[,]” (p. 6) the authors place the discipline in an international and “interlocking” web, in which “different actors in scholarship and politics, on national and international levels, are so closely linked to each other that any movement of one of them will also affect the functioning of the others” (p. 4).

“Orientalology in both the USSR and the United States was as much about the ‘other superpower’ as it was about the ‘Orient,’” (p. 3) reads one of the volume’s main arguments. While it is a compelling sentence, it is arguably also a slight exaggeration. After all, the volume itself provides ample evidence of the importance of the *internal* rationale of Soviet Oriental studies: Babajanov’s excellent (if somewhat misleadingly titled)<sup>4</sup> chapters, for example, quotes a prominent Soviet Orientalist who described Islamic manuscripts and their study as “‘an important resource for the histories of the peoples of the USSR as well as an integral part of the ‘culture-building’ of the new nations” (p. 88).

Armina Omerika stresses that “[i]n both Bosnia and Serbia, Oriental studies were established primarily to serve the purposes of national historiographies and were thus closely linked to the national projects of the two sponsoring republics” (p. 154). Bustanov, too, argues that the various Orientalological disciplines in the Soviet Union “were deeply embedded in Soviet nationality policies” (p. 48).

Still, the volume’s objective of exploring Soviet Orientology in a global and Cold War context (and the stress on the importance of the ‘other superpower’) is entirely justified. The authors are right to maintain that “current research on ‘Orientalism’ has a tendency to focus on particular national schools, in isolation from each other [...]” (p. 4) – a sin that I am myself somewhat guilty of. Moreover, elsewhere in the book this global and comparative Cold War perspective is more present: Masha Kirasirova, for example, argues that the Soviet vision of the Middle East as “a site for internationalist solidarity” and the American view of it as “a vulnerable *terra incognita* [...] reflected both superpowers’ fears about the other superpower” (p. 36). Similarly, Kalinovsky’s topical and fascinating chapter on the study of the “Soviet East” in the US during the 1970s and 1980s demonstrates how anti-Soviet sentiment led to a politicised and flawed scholarly agenda that promoted engaging (Central Asian) Islam in a Cold War struggle against the Soviet Union. Kemper’s chapter on Soviet strategies at 1960 International Congress of Orientalists in Moscow tells the intriguing story of how Soviet Orientalists, in particular “the Tajik politician-com-scholar [Bobodzhon Gafurov]” (p. 200), managed to skilfully exploit the event by impress-

ing Western participants with high quality and rather traditional scholarship, while presenting participants from non-Western countries with more propagandistic and anti-colonial works.

*Reassessing Orientalism* provides fascinating insights into the practical “relationship between knowledge of the Orient and domestic and foreign politics” (p. 2f). But it also sharpens our theoretical understanding of (Soviet) Orientalism by adding a comparative Cold War-perspective to it, even if this perspective is not always entirely obvious to the reader. On the whole, the book makes a compelling argument for the need to take into account the global dynamics of the Cold War in order to understand the development of Soviet Oriental studies and, in particular, the paradox why a discipline that after 1917 was “officially called upon to transform from a tool of oppression into an instrument of liberation” (p. 3) turned into a more effective instrument of politics and state power than had been the case before the revolution.

#### Notes

- 1 Edward W. Said, *Orientalism*, London 2003, p. 3.
- 2 Michael Kemper/Stephan Conermann (eds.), *The Heritage of Soviet Oriental Studies*, Abingdon, Oxon/New York, NY 2011, p. xiii.
- 3 Vera Tolz, *Russia’s Own Orient: The Politics of Identity and Oriental Studies in the Late Imperial and Early Soviet Periods*, Oxford/New York 2011) pp. 85–111.
- 4 While the chapter speaks of “[m]adrassa graduates at the Soviet Institute of Oriental Studies”, implying a central metropolitan institution, it does in fact refer to the Institute of Oriental Studies of the Uzbekistan Academy of Sciences.

**Corinna R. Unger: Entwicklungspfade in Indien. Eine internationale Geschichte 1947–1980, Göttingen: Wallstein Verlag, 2015, 319 S.**

Rezensiert von  
Ursula Rao, Leipzig

Welche Impulse gehen von den großen Entwicklungsprojekten aus, die im unabhängigen Indien von internationalen Geldgebern angestoßen wurden? Welche Entwicklungswege haben sie durchlaufen und wie haben sie Indien geprägt? Mit diesen Fragen im Blick durchforstet die Autorin Corinna Unger Archive in Berlin, Bonn, Bremen, Chicago, College Park, Hohenheim, Koblenz, New Delhi, New Haven, New York, Rom, Sleepy Hollow, Tübingen und Washington und schreibt eine detaillierte Geschichte der Planungen, Erfahrungen und Reflektionen markanter Projekte der ländlichen und städtischen Entwicklung. Ihre Fallstudien sind das indische Genossenschaftswesen, Community Development Projekte, die „grüne Revolution“, sowie Stadtplanung in Chandigarh, Rourkela und Kalkutta.

Das Buch verfolgt zwei unterschiedliche Argumentationsstrategien. Teil I thematisiert die historische Abfolge verschiedener Projekte zur Förderung ländlicher Entwicklung und agrarischer Produktivität. Entwicklung erscheint hier als geschichteter Prozess, dessen Charakter sich durch die wiederholte Abfolge von Erfahrung, Kritik und Neukonzeption ausprägt. Teil II betrachtet Fragen industrieller und

städtebaulicher Entwicklung aus dem Blickwinkel deutscher und US-amerikanischer Investoren. Es zeigt sich, wie nationale und institutionelle Diskurse und Praktiken konkrete Projekte prägen und wie Projekterfahrung auf internationale Entwicklungsparadigmen zurückwirkt. In beiden Teilen wird zweierlei deutlich. Entwicklungsdynamik gestaltet sich im Prozess des historischen Lernens und zugleich in der akuten Aushandlung konkurrierender Interpretationen, Ideologien und Interessen. Entworfen als Beitrag zur Wissensgeschichte zeigt das Buch am konkreten Fall der indischen Nachkriegsentwicklung, wie Wissen sich im Transfer entwickelt, entfaltet und erneuert. Das Buch lebt von der „dichte“ Beschreibung konkreter Steuerungsvorgänge bekannter Projekte, deren Entwicklungshandeln selten in diesem Detail beschrieben wurde. Das Buch antwortet nicht auf die weitergehende Frage, ob aus diesen Fallbeispielen Schlüsse für aktuelle Debatten über Praktiken des Wissenstransfers im Entwicklungskontext gezogen werden können. Im Ansatz formuliert die Autorin Kritik an Theorien der neunziger Jahre von James Ferguson und James Scott. Sie argumentiert, dass die Bürokratisierung der indischen Stadtplanung durchaus nicht zur Entpolitisierung urbanen Geschehens geführt hat. Vielmehr zeigt ihre Diskussion von Planungsdynamiken, wie politisiert der Einsatz von Experten ist und wie deren Arbeit von Fragen nach Machterhalt, Einfluss und Ideologie durchsetzt ist. Neuere Ansätze, wie z. B. die Thesen zu „traveling Models“ (Richard Rottenburg), Entwicklungshandeln als global vernetztes Unternehmen (Thomas Bierschenk) oder Wissensproduktion als Grenzziehungs-

prozess (David Mosse) spielen keine Rolle. Im Vordergrund dieses Beitrags zur Globalgeschichte steht eine regionalwissenschaftliche Frage, nämlich „warum Indien [...] zwischen den späten 1940er und den frühen 1980er Jahren bestimmte Entwicklungspfade und die mit ihnen verbundenen Modelle privilegierte, und andere verwarf, einige ausprobiert und andere ignorierte“ (S. 11).

Teil I beginnt in der Kolonialzeit. Die Autoren weist nach, dass die Sorge um Ernährungssicherheit ein durchgängiges Motiv indischer Politik ist, auch bereits lange vor der Unabhängigkeit. Allerdings gab es immer wieder neue Antworten auf die Frage, wie die wachsende Bevölkerung ausreichend und gerecht mit Lebensmitteln versorgt werden könne. Die erste Phase der postkolonialen Agrarpolitik markierte die Neuauflage des von den Briten eingeführten Genossenschaftswesens. Die staatlich subventionierte Zusammenarbeit in Kooperativen bot sich dem blockfreien Land als idealen Kompromiss zwischen Förderung solidarischen Handelns und marktwirtschaftlicher Ausrichtung an. Da Kooperativen alleine nicht in der Lage waren, die substantiellen sozialen und wirtschaftlichen Probleme des Dorflebens zu lösen, verabschiedete die Nehru Regierung in den 1950er Jahren das Community Development Programm, das durch Bildungsangebote, Bodenentwicklung und moderne Anbaumethoden niedrigmoderne Entwicklung fördern sollte. Die Veränderungen waren schleppend und die Bilanz in den 1960er Jahren ernüchternd. Man diagnostizierte unzureichende agrarische Produktivität, das Überdauern eines rasanten Bildungsgefälles und massiver Ungleichheit zwischen Kasten und

Geschlechtern. In diesem Kontext trafen die bereits in den 1940er Jahren laut gewordenen Rufe nach einer Intensivierung der Landwirtschaft auf offene Ohren. Die von amerikanischen und internationalen Geldern geförderte „grüne Revolution“ ließ die Produktivität hochschnellen und beförderte – nicht zuletzt durch die hartnäckige Verhandlung Delhis – die Entstehung einer indischen Düngemittelindustrie. Der Modernisierungsschub brachte wirtschaftliche Erleichterung, förderte aber zugleich soziale Ungleichheit, Massenarbeitslosigkeit und führte die negativen Konsequenzen des schnellen Bevölkerungswachstums vor Augen. So blieben dann niedrigmoderne Modelle, die auf gemeinschaftliches Lernen und Bildung für die Ärmsten setzen, immer auch populär. Entwicklungspolitik zeigt sich als ein Mischgelage verschiedener sich überlappenden und in Indien heimisch gewordener Modernisierungsansätze.

Im zweiten Teil des Buches ändert sich die Narrative und es geht um die kritische Evaluierung verschiedener Versuche von Stadtentwicklung. Unger diskutiert die Naivität des städteplanerischen Optimismus, mit dem ausländische Planer eine moderne Hauptstadt (Chandigarh) und ein produktives Hüttenwerk (Rourkela) zu begründen versuchen. Die als Autostadt geplante monumentale Anlage von Chandigarh wurde von der lokalen Bevölkerung nicht als modern und demokratisch, sondern autoritär und menschenfeindlich wahrgenommen. Gleiche Fehler sollten in Rourkela vermieden werden, wo auf Druck der indischen Regierung angrenzend an das Hüttenwerk ausreichend Wohnraum geschaffen werden sollte. Niedrigbauweise führt zu einer zersiedelten Landschaft, die durch die lebens-



praktische Aneignung der Einwohner ein ländlich-agrarisches Flair erhielt. Das mit Verzögerung an Produktivität gewinnende Hüttenwerk schafft zugleich eine Insel der Modernität, deren Ausstrahlungskraft jedoch gering blieb. Stadt und Land standen weiter im krassen Gegensatz. Die daraus folgende wachsende Landflucht forderte Großstädte wie Kalkutta heraus. Nicht zuletzt die Sorge um eine kommunistische Revolution förderte das Engagement amerikanischer und internationaler Entwicklungsagenten. Unger untersucht den weltbankfinanzierten Umbau der Calcutta Metropolitan Development Authority, der dazu beitragen sollte, solide Verwaltungsstrukturen zur effektiven Planung und Durchführung von Infrastrukturmaßnahmen zu schaffen. Das Projekt führte die Weltbank auf neues Terrain. Es ging nicht wie zuvor um die Förderung eines klar umrissenen Projekts, sondern um die zeitbegrenzte Unterstützung staatlicher Institutionen bei der Erfüllung dauerhafter Entwicklungsaufgaben. Das Projekt wurde zum Lehrstück über neue Formen der Entwicklungspolitik und führte zugleich die Grenzen der Arbeit mit staatlichen Institutionen vor. Es folgten weitere ähnlich angelegte Projekte, zugleich entstand die Saat für einen Abkehr von Fokus auf den Staat und die wachsende Förderung von Zivilgesellschaft.

Ungers Buch hat es nicht ganz leicht. Weil sich die Autorin sehr gut bekannte und viel diskutierte Entwicklungsprojekte vornimmt, überrascht die große Narratives des Buches nicht. Zugleich ist das Buch aber auch gerade deshalb von hoher Relevanz. Denn es nimmt Themen auf, die bis heute die Gemüter erhitzen und nichts von ihrer Aktualität eingebüßt haben. So

führen Diskussionen um Gentechnologie immer wieder zurück zur kritischen Evaluierung der „grünen Revolution“. Das im wirtschaftsliberalen Indien exponentielle Städtewachstum beförderte infrastrukturelle Maßnahmen, die typische Themen der 70er und 80er Jahre spiegeln. Unger wagt keinen Ausblick und thematisierte nicht ausdrücklich die Brisanz ihrer Arbeit für das aktuelle Indien. Doch ihre detailreiche Arbeit spricht für sich. Die Autorin zeigt durch die genaue Analyse vielfältiger Quellen, dass Entwicklung ein mehrlagiger, komplexer und in Teilen unplanbarer Prozess ist. Sie weist am Detail nach, wie sich politisches und administratives Handeln verschränken, nationale und internationale Interessen überlappen und globale und lokale Wissensbestände amalgamieren. Sie zeigt vor allem ausführlich, wie genau sich dies im indischen Kontext vollzieht und schreibt so eine Geschichte jener Prozesse, die das moderne Indien fundamental geprägt haben.

Das Buch ist allen zu empfehlen, die sich mit Entwicklungspolitik und Entwicklungswirklichkeit beschäftigen. Es enthält Lehren, die weit über Indien hinaus reichen. Es ist ein Buch für Fachleute, das auch für Laien gut verständlich ist. Die materialreiche Schrift wird gleichermaßen Historiker, Soziologen, Ethnologen, Humangeographen und natürlich Indienwissenschaftler interessieren.



**Stuart Harris: China's Foreign Policy,  
Cambridge: Polity Press, 2014, 240 S.**

Rezensiert von  
Katja Levy, Berlin

Der Autor dieses Buches war unter anderem in den 1980er Jahren als Staatssekretär im Department for Foreign Affairs and Trade der australischen Regierung tätig und ist heute Emeritus an der Australian National University im Fachbereich Internationale Beziehungen mit einem Forschungsschwerpunkt auf Chinas internationale Beziehungen.

Beide Aspekte dieser beruflichen Karriere spiegeln sich in dem Buch wieder: ein umfassendes Detailwissen um historische und aktuelle Ereignisse, das vom jahrelangen Austausch mit hochrangigen außenpolitischen Akteuren profitiert hat, sowie deren sensible Interpretation und ein feinsinniges, diplomatisches Gespür für die vielfältigen Zusammenhänge von Ereignissen und ihre Auswirkung auf Chinas Außenpolitik.

„China's Foreign Policy“ ist vordergründig ein umfassender Band über die Außenpolitik der Volksrepublik China seit 1949 mit dem Schwerpunkt auf der gegenwärtigen Außenpolitik. Auf einer zweiten Ebene geht dieses Buch aber weit darüber hinaus, indem es versucht, bei allen Fragen explizit die Perspektive Chinas herauszuarbeiten. Diese Perspektive folgt konsequent dem Leitmotiv „Herausforderungen und Verletzlichkeiten“ („challenges and vulnerabilities“).

Dem eigentlichen Hauptteil des Buches vorangestellt sind ein Vorwort, einer Karte Chinas (auf der Taiwan als Provinz der Volksrepublik China aufgeführt ist), ein Abkürzungsverzeichnis sowie eine Chronologie wesentlicher historischer Ereignisse beginnend mit dem ersten Opiumkrieg 1839 und abgeschlossen mit einem Eintrag über die Ernennung Xi Jinpings zum Staatspräsidenten 2013. Im hinteren Teil des Bandes befindet sich ein kurzer Anmerkungsapparat, ein umfangreiches Literatur- sowie ein Stichwortverzeichnis. Jedes der acht Kapitel von „China's Foreign Policy“ ist durch ordnende Leitfragen vorstrukturiert. Da vielen Kapiteln zusammenfassende Schlussabsätze fehlen und das Buch weder über eine gedankenstrukturierende Einführung noch über ein Schlussfolgerungen ziehendes Fazit verfügt, eignet sich das Buch eher zur kompletten Lektüre als zum stellenweisen Lesen einzelner Passagen.

In Kapitel 1 geht es um den Einfluss historische Ereignisse, der bis in die Gegenwart auf die chinesische Außenpolitik einwirkt. Im anschließenden zweiten Kapitel beschreibt Harris, wer in China das Sagen in der Außenpolitik hat und zeigt dabei – nicht ganz neu – auf, dass sich auch in China inzwischen die Akteure in diesem Politikbereich diversifiziert haben und zahlreicher geworden sind. Kapitel 3 gibt einen allgemeinen Überblick über Chinas Außenpolitik und seine Rolle in der internationalen Politik, wobei der Autor auch hier viel Wert darauf legt, der chinesischen Perspektive genügend Raum zu lassen. In Kapitel 4 stehen die bereits erwähnten Verletzlichkeiten, wie sie die VR China empfindet, im Fokus, und zwar auf vier Ebenen – die Verletzlichkeit des

politischen Systems, Chinas historische Verletzlichkeit, die geographischen Verletzlichkeiten Chinas, insbesondere an seinen Land- und Seegrenzen, sowie die spezifische Verletzlichkeit, die die VR China nach Harris gegenüber der Dominanz der Vereinigten Staaten im internationalen System empfindet. In den anschließenden drei Kapiteln geht der Autor in seiner Analyse mehr ins Detail. Er nimmt sich Chinas Militär vor, schaut, welche Rolle es in der aktuellen Außenpolitik Chinas spielt, wie bedrohlich seine Modernisierung und Aufrüstung wirklich sind und wie auch in diesem Bereich die Verletzlichkeiten der aufsteigenden Großmacht zum Tragen kommen (Kapitel 5). Es folgen die Betrachtung der Wirtschaftspolitik und ihre Wirkung auf die Außenpolitik (Kapitel 6) und die Beschreibung der vielfältigen bilateralen und multilateralen Beziehungen der VR China zu ihren Nachbarn sowie ausgewählten weiteren Ländern und Regionen. Dass Europa in diesem Kapitel keine bedeutende Rolle zugeschrieben wird, lässt sich erstens mit der tatsächlich vergleichsweise geringeren Bedeutung Europas in der Gesamtschau auf die Weltpolitik erklären, und hängt zweitens auch mit der Perspektive des Autors von „down under“ zusammen. Erstaunlicher ist an dieser Stelle jedoch die komplette Ausblendung der sino-australischen Beziehungen sowie der Beziehungen Chinas zu den lateinamerikanischen und karibischen Staaten, die spätestens seit der letzten Jahrtausendwende für China deutlich an Bedeutung gewonnen haben. Auch der Nahe Osten wird mit keinem Wort erwähnt. Dass der ehemalige Außenpolitikpraktiker der australischen Regierung Australien als Teilgebiet weglässt, ist unverständlich, wird auch

nicht erklärt und ist schade, weil die Leser/innen hier besondere Einsichten erwarten dürften.

Das Buch endet schließlich nicht in einer erhellenden Quintessenz über die Verletzlichkeit der VR China in der internationalen Politik, sondern mit einem weiteren Kapitel, das – unter dem eher irreführenden Kapiteltitle „Foreign Policy in Transition“ – ähnlich wie die vorhergehenden anhand einer überbordenden Fülle von Daten, Fakten und Wissen, diesmal die Außenpolitik Chinas auf ihre innenpolitischen Einflüsse abklopft. So endet das Buch etwas abrupt und sperrig.

Wie in dieser Kurzzusammenfassung angedeutet, gibt sich der Autor recht wenig Mühe, die Leser/innen durch aufwendige Leserführung auf bestimmte Erkenntnisse zu stoßen. Vielmehr muss die Leser/in, wie ein „Trüffelschwein“ zwischen den unendlich vielen Fakten und Informationen die „Trouvaillen“ des Buches selbst für sich entdecken. Mit solchen glücklichen Fundstücken meine ich die wertvollen Einsichten, die der Autor bezüglich der chinesischen Sicht der Dinge aufzeigt und unter dem Oberbegriff „vulnerabilities“ zusammenfasst. Sie machen den bedeutenden Mehrwert dieses Buches aus: Hier versucht ein in praktischer Politik erfahrener und gleichzeitig in der Wissenschaft bewandelter Autor sachlich die chinesische Perspektive auf die großen und mittelgroßen Fragen der Weltpolitik aufzubrechen, und zwar ohne jede Anbiederung an das chinesische Regime. Er schlägt sich somit nicht auf die Seite der „Chinaversteher“, denen leicht der Vorwurf anhängt, jede Handlung dieses Staates verteidigen zu wollen, sondern er erklärt schlicht, wie die Welt aus der Perspektive Chinas – geprägt

durch seine Geschichte, die gegenwärtige weltpolitische Gemengelage und seine eigenen geopolitischen Gegebenheiten – aussieht. Außenpolitik sollte eigentlich grundsätzlich auf einem solchen sachlichen, aber empathischen Verstehen basieren. Nur so lassen sich die Situation und das Verhalten der anderen internationalen Akteure einschätzen und ihr künftiges Handeln prognostizieren.

Man kann sagen, dass „China's Foreign Policy“ – abgesehen von den genannten regionalen weißen Flecken – das gesamte Grundwissen über die Außenpolitik Chinas enthält. Der Autor hat Fakten, Daten und wichtige Zusammenhänge minutiös und in großer Vollständigkeit nicht nur zusammengetragen, sondern macht sich auch die Mühe, bestimmte Begriffe, die in anderen Überblickswerken über die chinesische Außenpolitik eher oberflächlich abgehandelt werden, einsichtig zu erläutern. Ausführlich erklärt Harris z.B. unscharfe Begriffe wie „Verantwortlichkeit in der internationalen Politik“ (S. 60 f.) oder „Strategische Partnerschaft“ (S. 52). Diese bravourös gemeisterte Mammutaufgabe bewahrt den Autor leider nicht vor einigen wenigen Missverständnissen. So hält er beispielsweise den chinesischsprachigen Fernsehsender „New Tang Dynasty“ für ein Instrument der chinesischen außenpolitischen „Soft Power“ (S. 72), obwohl es sich dabei um ein äußerst chinakritisches Medium der Falun Gong handelt.

Man kann dieses reichhaltige Buch auf mindestens zwei Arten lesen: Für die einen Leser/innen, die das Buch vielleicht mit weniger Vorwissen zur Hand nehmen, ist es eine sehr umfassende Einführung in die Außenpolitik der VR China. Für die anderen, die schon über eine gewisse

Vorbildung in diesem Bereich verfügen, ist dieses Buch eine Fundgrube von wertvollen Innenansichten darüber, was China in der Außenpolitik „wirklich denkt“. So beschreibt Harris z.B. die strikten chinesischen außenpolitischen Prinzipien der Souveränität und territorialen Integrität nicht, wie die meisten anderen Autoren, als originär chinesische Außenpolitikprinzipien, sondern sieht sie eher als von der chinesischen Führung übernommene Prinzipien des Westfälischen Friedens (S. 54). An anderer Stelle erklärt Harris, dass das Chinas Anti-Sezessionsgesetz von 2005, das ein militärisches Eingreifen bei einer eventuellen Lossagung Taiwans vom Festland ermöglicht, auch als Gegenstück zum US Taiwan Relations Act gelesen werden kann – als eine Regelung nämlich, die weniger ihre Wirksamkeit in der Implementierung entfaltet als vielmehr im alltäglichen diplomatischen Miteinander für allerlei politische Entscheidungen als Vorwand genutzt wird und somit der Erleichterung so mancher diplomatischer Verhandlung dient (S. 43).

Das Buch eignet sich somit für Studierende und Interessierte an der chinesischen Außenpolitik und ist insbesondere auch außenpolitischen Politikpraktiker/innen zu empfehlen. Letztere wünschten sich allerdings wahrscheinlich eher eine konzentriertere Form der besonderen Erkenntnisse dieses Werkes, ohne dass sie sich durch die gesamte Faktenlage arbeiten müssen. So wird dieses Buch wohl leider viele seine wichtigsten Adressat/innen in der praktischen Politik eher schwer erreichen.

**Paul Nolte (ed.): Die Vergnügungskultur der Großstadt. Orte – Inszenierungen – Netzwerke (1880–1930) (= Städteforschung A: Darstellungen, vol. 93), Köln: Böhlau 2016, 180 S.; Len Platt, Tobias Becker, David Linton (eds.): Popular Musical Theatre in London and Berlin 1890–1939, Cambridge: Cambridge University Press, 2014, 284 S.**

Reviewed by  
Antje Dietze, Leipzig

Modern forms of popular culture emerged at the intersection of accelerated urban development and increasing transnational interactions in the decades around 1900. The urban entertainment sector was a hotspot of globalisation, characterized by intense cross-border transfer and exchange, making cosmopolitan culture accessible for the majority of leisure-seeking urban dwellers. Only just recently however, has research in this field begun to catch up with developments and debates in transnational and global history, and vice versa.

These two edited volumes give an overview of current trends, new conceptual debates, as well as of remaining open questions in research on popular culture in the modern metropolis. The volumes stem from conferences organized by interconnected research projects at Freie Universität Berlin and Goldsmiths College, University of London. These projects dealt with urban entertainments from a transnational perspective, spanning from the late 19th cen-

tury to the 1930s. [1] The volumes bring together established scholars as well as younger researchers from the fields of history, theatre studies, musicology, literature and cultural studies.

The volume entitled “Die Vergnügungskultur der Großstadt” (amusement culture in the metropolis), edited by historian Paul Nolte, traces different forms of urban popular culture in one city. Without having explicitly named the city of Berlin in the title, most of the contributions centre around the German capital. The chapters analyse operettas, musical comedies, community celebrations and festive culture, circus, dance, visual culture, theatre, opera and concerts, cultural offerings in taverns, ballrooms and summer gardens, as well as workers’ culture. The notion of “amusement culture” is used here to avoid social limitations (it includes both lower and middle class as well as elite culture) and also extends beyond purely commercial entertainments (also comprising of publicly sponsored and non-profit ventures), which ultimately enables the volume to reveal the diversity of urban popular culture and the different social and spatial entanglements within which these forms were embedded.

The volume “Popular Musical Theatre in London and Berlin”, edited by a team of German and British researchers including Len Platt, Tobias Becker and David Linton, has a slightly different focus. The chapters closely analyse related forms of musical entertainment – especially operetta, musical comedy, revue, popular music, song and dance – in London and Berlin. Attention is paid however to contextualize these cities and their entertainment zones (West End and Friedrichstraße) within

larger transnational transfer and networks of exchange which extended to Paris, Vienna and New York.

The volume takes popular musical theatre as an example to study historical transformations of cultural cross-border entanglements. It is organized in three sections: first “mechanics of transfer and translation” in the wider context of Anglo-German relations at a time when these were characterized by intense connections, competition, but also open hostility. The second section addresses “Atlantic traffic”, most notably the growing cultural influence of the US. A third section analyses representations of “others” on stage, which brings questions of nationality, gender and race to the forefront. The strong focus, consistent composition and many references between the chapters result in a useful overview of the development of musical theatre. It encompasses the emergence of new, highly cosmopolitan genres, the collapse of cultural connections during the First World War, and the further transformations of transnational exchanges and perceptions up to the 1930s.

Together, these books represent important trends in studying and conceptualizing the cultural sector in the metropolis at the turn of the 20th century. First of all, both volumes reflect upon the position of culture and entertainment in the wider history of urbanisation, globalisation and modernisation. In the introduction to their volume, Platt, Becker and Linton underline the strong interconnections of popular culture and modernity. Theatres and entertainment venues were prominent sites of the modern urban experience. Musical theatre itself was going through a radical modernizing process in the period in ques-

tion. It also staged, reflected and promoted modern and cosmopolitan life styles and attitudes, often in dream worlds that stood in stark contrast to daily life.

In a similar vein, Nolte in his introduction makes the most welcome plea to move culture and entertainment from the margins more to the centre of both urban history and the study of modern societies at large. He frames the cultural sector as a constitutive element of the modern metropolis rather than a supplement or decoration. His argument however falls slightly short of its declared ambition when he subsequently defines the amusement culture as a sphere where modernity is “doubled”, reflected, and aestheticized. It is certainly relevant to further explore urban popular culture’s function to experiment with, express and process the complexities of modern life. Then again, the notion of re-enacted modernity cannot fully grasp the productive force and structuring effects which urban popular culture had within rapidly and radically transforming societies. It was a new cultural field, a growing public sphere and a booming economic sector, with lasting consequences for sociability, knowledge formation, identity building, consumerism, city development and much more.

Many of the contributions give insights into these effects, and more particularly into the interactions between different social spheres of production. Kaspar Maase’s chapter (in Nolte), for instance, discusses knowledge circulation in urban visual culture. It emphasizes pedagogical concerns regarding the urban youth’s exposure to supposedly indecent or harmful knowledge through cinema, the popular press, dime novels, postcards and shop window

displays. These debates reflected the flip-side of modern urban culture: it made unregulated and undisciplined knowledge accessible and was therefore perceived as a serious threat to established morals and authority. Beyond Maase's contribution, other chapters move in a similar vein while addressing changing gender relations and the emergence of new subjectivities like the "modern girl" (Frey; Bailey; Gardner in Platt et al.); or people of colour and concepts of race both on and off stage (Linton and Platt; Lewerenz, *ibid.*).

The two volumes at hand form part of a trend to focus more explicitly on the cultural transfers, cross-border entanglements and globalisation processes which thoroughly shaped the field of modern urban entertainments. The cosmopolitan character of new cultural genres in the modern city has always been evident and was the subject of controversial debates by contemporaries. This aspect has not necessarily been reflected in existing research approaches which to the most part retained a focus on single cities or on national frameworks.

In contrast, the volumes at hand include comparative approaches, transfer analyses and studies of networks and circulations. Kerstin Lange and Tobias Becker, who each contributed to both volumes, are among the authors who draw upon the concept of cultural transfer. They focus on actors, networks and mechanisms of cultural exchange in the fields of dance (tango as a transatlantic import to Europe) and musical theatre (interactions of British musical comedy and continental operetta). Special emphasis lies on processes of appropriation, but likewise also on processes of rejection which were heavily influenced

by travelling artists, dance teachers and translators. These transfer agents developed various strategies to manage cultural differences. They turned cities into transfer hubs, adapted cultural genres and practices, but also started critical debates about the effects of growing transnational entanglements and cultural "invasions".

With case studies like these, the two books outline the very complex web of cultural interconnections which Berlin and London were involved in. They also illustrate that focusing on these cities alone is not enough to explain the mechanisms of cultural exchanges between them. These metropolises were nodes in a wider city network, with their relevance and functions, as well as the extent and direction of cultural interactions, changing over time.

The volume on popular musical theatre succeeds particularly well in mapping out larger European, imperial, and transatlantic entanglements. Its authors outline how genre formation, audience preferences and production structures evolved – starting with Paris and Vienna as pioneers and long dominating centres of musical comedy and operetta production. Later on, London and Berlin emerged as transfer hubs with their own local brands of popular musical theatre that subsequently became exported and adapted elsewhere. Meanwhile, transfer flows from Europe to the US slowly reversed, with American revues, ragtime and jazz crossing the Atlantic and adding new themes and forms to the evolving cosmopolitan culture.

Token together, the volumes point to a lasting challenge for research on popular culture in the modern metropolis. As the extent and variety of spatial connections and networks has been further revealed,

the question is how to conceptualize them. One proposal, by theatre historian Peter W. Marx (in Nolte), uses the concept of circulation to rethink interactions between different spaces and social spheres, thus bringing together the two fundamental questions of the function of urban culture in modernisation, and the sector's transnational (or trans-urban) connections. He takes the example of actress Jenny Groß to analyse the entanglements of theatre and the fashion industry against the background of a rapidly evolving consumer culture. Metropolitan culture and lifestyles, he argues, were not place-bound, but a sphere of circulation of goods, imaginations and emotions that extended beyond the city space – to other metropolises, but also to suburban and rural areas. Taking this transgressive character of urban culture seriously thus calls for a de-centring of the metropolis in research on urban culture and entertainments.

Other contributions highlight the fundamental tension between cultural differences on the one hand, and growing transnational convergence on the other. They all show that while the metropolises were going through similar processes of modernisation, and were further connected by intensifying exchanges, this did not necessarily lead to homogenisation. The authors outline the complex relationship between local idiosyncrasies, national or racial differences and stereotypes and a converging cosmopolitan style of musical theatre in all these cities.

They also show how these cultural boundaries and imaginations were constantly transcended and mixed up. After the First World War for example, Vienna style operettas were mass-produced in Berlin and

exported to Britain disguised as “Austrian” or “continental”. Musical comedies and operettas were being marketed by local or national branding, while the transnational biographies and careers of many of the composers, writers, translators and stage directors made any definition of the cultural “origin” of the product almost impossible (see chapters by Becker and Platt). Demarcating and explaining cultural differences and exchanges becomes difficult under such circumstances.

Between place of production, exchange routes, transformations in transfer, local reception and different cultural imaginations and identity constructions, the contributions reveal a complexity of various aspects of space and spatial interactions in modern urban culture. They open up a promising route to further unravel these entanglements in a more systematic manner within subsequent research.

#### Notes

- 1 DFG project “Metropole und Vergnügungskultur. Berlin im transnationalen Vergleich, 1880–1930”, Freie Universität Berlin; conference: “Metropolenkultur im Transfer. Orte – Inszenierungen – Netzwerke. 1880–1930”, in cooperation with Institut für vergleichende Stadtgeschichte an der Universität Münster (2010). DFG and AHRC project “West End and Friedrichstraße: Popular Musical Theatre in London and Berlin, 1890–1939”; Freie Universität Berlin, Goldsmiths, University of London; conference “West End and Friedrichstraße. Cross-Cultural Exchange in Popular Musical Theatre, 1890–1939”, Freie Universität Berlin (2012).



**Hans Jürgen Puhle: Protest, Parteien, Interventionsstaat. Organisierte Politik und Demokratieprobleme im Wandel, Göttingen: Vandenhoeck & Ruprecht, 2015, 380 S.**

Rezensiert von  
Thomas Plötze, Leipzig

Sein aktuellstes Buch sei „ein gewisses Wagnis“, so Puhle in seiner Einleitung des Buches (S. 13). Es sei vor allem ein Wagnis, weil es ein Sammelband mit insgesamt 15 Aufsätzen des Autors aus 50 Jahren wissenschaftlicher Tätigkeit ist und die Auswahl der Beiträge anhand spezifischer nachvollziehbarer Überlegungen zu treffen gewesen sei. Eins kann man vorwegnehmen: Dieses Wagnis ist gelungen. Die Entwicklung der Argumente in den jeweiligen Artikeln und zwischen ihnen ist für den Leser klar nachzuvollziehen. Darüber hinaus geben die Artikel einen guten Einblick in das vielfältige Oeuvre des Historikers und Politikwissenschaftlers.

Inhaltlich zusammenfassen lassen sich die Beiträge auf die Frage, wie sich moderne Staaten (Massendemokratie, Marktwirtschaft und Wohlfahrtsstaat) historisch herausgebildet haben und welche unterschiedlichen, aber auch zum Teil überlappenden Entwicklungsmuster dafür verantwortlich gewesen sind. Puhle nähert sich dieser übergeordneten Frage in einer historisch vergleichenden Perspektive, die sowohl Länder Europas als auch Nord- und Südamerikas berücksichtigt. Denn, so schließt Puhle am Ende seiner einleitenden

Reflektion, es sollten „alle systematischen Sozialwissenschaftler eigentlich (und am besten immer) auch Historiker sein“ (S. 13). Seine historischen Vergleiche haben für Puhle einen konkreten Startpunkt im 19. Jh., welches die Zäsur und den Übergang zur modernen „Staatswerdung“ initiiert. Eine zweite Zäsur verortet Puhle an der Schwelle zum 21. Jh. mit der Krise des keynesianistischen Interventionsstaates (1970er Jahre) und den zunehmenden „transnationalen und globalen Vernetzungen und Entgrenzungen“ (S. 7). Zwischen diesen zwei Zäsuren möchte Puhle seine 15 Aufsätze eingeordnet sehen, die einer „„altmodischen“, aber immer wieder spannenden und aufregenden Weberianischen ‚Konstellationsanalyse‘“ folgen (S. 12). Mit deren Hilfe werden die für Puhle entscheidenden Prozesse der Demokratisierung, Bürokratisierung und Industrialisierung für die moderne Staatswerdung entlang der 15 Aufsätze verdeutlicht.

In den ersten beiden Artikeln (Artikel eins: „Parlament, Parteien ...“ und Artikel zwei: „Transformationen des deutschen Konservatismus ...“) zeigt Puhle historisch, dass Demokratisierung durch politische Akteure errungen oder erkämpft wurde, wobei dies nicht ein linearer Prozess sein muss, sondern durch Gegenkräfte – in seinem Beispiel des deutschen Kaiserreiches war es ein konservativ-agrarisches Bündnis – zu einer „plebiszitären Diktatur“ (S. 49) mutieren oder verkommen kann (für eine vergleichbare Argumentation siehe hierzu den Artikel 14 über die Bauernbewegungen und deren politischen Einfluss auf Transitionen in der außereuropäischen Welt). Dass daran der Konservatismus, als politische Bewegung und Programm um 1900, einen erheblichen Anteil an dem



eingeschränkten Prozess der Demokratisierung hatte, steht für Puhle außer Frage. Er zeigt weiter, dass dieser nach 1945 in einen Reformkonservatismus mündete, somit an der Demokratisierung Westdeutschlands erheblich beteiligt war, letztlich aber um 1980 „starb, weil er keinen Gegner mehr hatte“. Deswegen sei er zu einem „kollektiven Lebensgefühl“ (S. 63) Deutschlands geworden. Demokratisierung ist somit ein Prozess, der zwischen Akteuren, wie zum Beispiel Parteien (Artikel 3: „Was kommt nach den ‚catch-all-parties‘“) und Interessenverbände (Artikel fünf: „Interessenverbände im Wandel“), mit bestimmten Mitteln (Artikel vier: „Populismus: Form oder Inhalt?“) erkämpft wird. Gleichzeitig vermittelt Puhle mit den drei darauf folgenden Artikeln dem Leser, dass Demokratisierungsprozesse in unterschiedlichen Linien und entlang unterschiedlicher Entwicklungsmuster verlaufen können. Dabei fokussiert er vor allen auf den Vergleich der Demokratisierungsprozesse in Europa und Amerika (Artikel sechs: „Demokratisierungsprobleme in Europa und Amerika“) und auf Transitionsprozesse und Konsolidierungen der Demokratien in Südeuropa (Artikel acht: „Demokratisierung, Europäisierung, Modernisierung“). Analytisch auf den von Huntington eingebrachten Demokratisierungswellen aufbauend, argumentiert Puhle, dass Demokratisierung und deren Konsolidierung auf einer Funktionalität von sieben Teilregimen beruht (u. a. wirtschaftliche Stabilität, Parteiensystem, Interessenvermittlung, Eliten, usw.). Die Probleme der demokratischen Konsolidierung werden dann in einem siebten Artikel („Embedded Democracy“ und „Defekte Demokratien“) mit dem vom Puhle mit entwickelten Konzept der ‚de-

fekten Demokratie‘ analytisch gefasst. Wenn man so will, stellt dieser Artikel und das Konzept die analytische Ausformulierung der im ersten Artikel aufgeworfenen These der Fragilität und Nicht-Linearität von Demokratisierungsprozessen dar.

Auch wenn der Frage der Demokratisierung als wichtiger Prozess der modernen Staatswerdung ein wesentlicher Teil der wissenschaftlichen Arbeit Puhles darstellt, sieht er auch durch seine historisch-vergleichende Perspektive, dass Demokratisierung ‚meist‘ am erfolgreichsten mit dem Prozess der Industrialisierung (S. 154) vollzogen wurde. An diesem Punkt ist er an einer zentralen Frage der Transitionsforschung angelangt: eine erfolgreiche Demokratisierung ohne Industrialisierung ist nur schwer möglich. Allerdings lässt Puhle zunächst offen, ob Industrialisierung vor oder nach der Demokratisierung oder gleichzeitig mit ihr voranzutreiben sei. Dieser Frage begegnet Puhle zunächst historisch vergleichend in Europa (Artikel neun: „Vom Wohlfahrtsausschuss zum Wohlfahrtsstaat“), indem er die Entwicklungsmuster Großbritanniens, Frankreichs, Deutschlands sowie Spaniens (Artikel zehn: „Probleme der spanischen Modernisierung im 19. und 20. Jahrhundert“) gegenüberstellt. Trotz unterschiedlicher Entwicklungsmuster zwischen diesen Ländern sieht Puhle gewisse Parallelen hinsichtlich des Industrialisierungsprozesses. Für ihn wird deutlich, dass Industrialisierung einen Verwaltungsapparat (Bürokratie) bedingt, der die Steuerung und Lösung von Problemen übernimmt. Bürokratisierung kommt, ganz im Sinne der Weberianischen Perspektive, somit eine zentrale Funktion zu, da Puhle zufolge der Verwaltungsapparat die Industrialisierung initiiert oder begleitet, die sozialen Folgen

(durch einen Wohlfahrtsstaat) abfedert und gleichzeitig durch Demokratisierung die politische Teilhabe der Bevölkerung gewährleistet. Somit ist auch klar, dass Demokratisierung und Industrialisierung nur als gleichzeitige und gleichgewichtige (doppelte) Transformation gelingen kann (das sogenannte „Reißverschlussverfahren“) (Artikel elf: „Nationalismus und Demokratie in Europa“, S. 282).

Diese Erkenntnisse überträgt Puhle schließlich auf die Staaten Nord- und Südamerikas und stellt, zumindest für Südamerika, unvollständige Industrialisierung und Staatsbildung (damit einhergehend unvollständige Bürokratisierung und Demokratisierung) fest (Artikel zwölf: „Das atlantische Syndrom“ sowie Artikel 13: „Unabhängigkeit, Staatenbildung“).

Ist somit das gewisse „Wagnis“ des Sammelbandes vollends geglückt und kann eine Leseempfehlung ausgesprochen werden? Ja, doch nicht ohne Einschränkung: Die 15 Aufsätze plus Einleitung sind empirisch und theoretisch außerordentlich gut nachvollziehbar. Zur Verdeutlichung seiner Argumente und Thesen in Anbetracht der heutigen sozialen und politischen Krisentendenzen hätte sich der Rezensent allerdings ein abschließendes Kapitel gewünscht, in dem die in der Einleitung aufgeworfenen Zäsuren mit den ausgewählten

Artikeln reflektiert und womöglich neue Fragen oder gar weitere Thesen im Lichte derzeitiger Probleme aufgeworfen werden. Erleben wir beispielsweise eine Veränderung des „Industrialisierungsprozesses“ in Europa mit einer Erosion der sogenannten „freien und sozialen Marktwirtschaft“ durch Tendenzen einer gewissen „Re-Feudalisierung“ ökonomischer und sozialer Verhältnisse, die auch Auswirkungen auf den Prozess der Demokratisierung in den einzelnen Ländern Europas und der Europäischen Union insgesamt haben? Wenn also, wie Puhle suggeriert, Demokratisierung, Bürokratisierung und Industrialisierung nur in ihrer Dynamik verständlich werden, sind sie dann nicht auch in ihrer Richtung, auch in den sogenannten „Industrieländern“, umkehrbar und werden somit gefährdet?

So wünschenswert diese inhaltliche Klammer in Form eines abschließenden Kapitels wäre, so zeigt dieses Buch doch die notwendige Brücke zwischen einer breiten historischen Perspektive und einer prägnanten sozialwissenschaftlichen Analyse. Das Buch ist ein bemerkenswerter Einstieg für diejenigen Leser/innen, die sich bisher noch nicht mit Puhles Werk auseinandergesetzt haben, denn es gibt einen hervorragenden Überblick über dessen analytische Perspektive und reichhaltiges Wirken.

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