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Inhaltsverzeichnis

4	RUDY WEISSENBACHER Editorial
6	PEER VRIES The California School and beyond: how to study the Great Divergence?
50	VISHNU PADAYACHEE Capitalism of a special type? South African capitalism before and after 1994
78	RUDY WEISSENBACHER Keeping up appearances: uneven global development in a system of structural imbalances
122	Rezensionen
128	SchwerpunktredakteurInnen und AutorInnen
131	Impressum

PEER VRIES

The California School and beyond: how to study the Great Divergence?

1. Introduction

The biggest challenge for global economic historians is to explain the huge differences in wealth between rich and poor countries that began to emerge with the industrialisation of parts of the Western world. If they want to explain what, since the appearance of Pomeranz's book, is known as 'the Great Divergence', they would actually have to tackle four questions (Pomeranz 2000). The first one concerns the introduction of steam-power in production and transport; this, during the Industrial Revolution in Britain and parts of Western Europe enabled societies for the first time in world history to escape from Malthusian constraints. Then there is the question as to why the momentum of this first industrial revolution did not end in stagnation at a higher level but became a 'take-off' into *sustained* and *substantial* growth. Next there is the question of catching up: how did less developed countries catch up with or even overtake more advanced ones? Finally, there is the question as to why so many countries failed to do so, as a result of which the gap between such countries and developed countries was perpetuated and even widened.

In this article only the first question, dealing with the First Industrial Revolution, will be discussed. Why did the first escape from the Malthusian ceiling occur in Western countries, first and foremost Britain, and not in other parts of the world? That is one of the classic questions in economic history. With the emergence of the so-called California School of economic historians however, it has been posed in a new way by framing it explicitly in a context of global comparisons and connections, and it has received answers that are often quite different from the traditional ones. This text is

meant as an attempt to offer a critical, constructive evaluation that indicates what we can learn from the Californians, where they may be wrong, and what promising paths for future research they have neglected.

The reference to global comparisons and connections does not mean that I will discuss the entire globe. I will focus on only Britain and China. The decision to do so is, to some extent, pragmatic. It keeps the topic manageable. Yet there are also scholarly reasons. If we are to believe Jared Diamond, people of Eurasian origin, especially those living in Europe and Asia and those who migrated to America, had far better chances of becoming rich and dominant than those living someplace else, because different natural resources were available to the people of different continents (Diamond 1997). Although not everyone would so easily write off the Americas before Columbus, to me his main arguments sound plausible (Mann 2006). In Eurasia, at the eve of the Great Divergence, Britain and China are normally regarded as the most developed and richest countries. It is not by accident that in the work of the California School these two countries hold centre stage.

Comparing only two countries may look like evading the discussion on the origins of the great divide between rich and poor on a global scale. Personally I prefer to focus on countries as opposed to simply contrasting, as is often done, ‘the West’ with ‘the Rest’. Differences in the West and even more in ‘the rest’ of the world are too great to make that, *intellectually speaking*, a very promising endeavour. When it comes to comparative analysis, I prefer studies in which a couple of historical cases are analysed in depth and compared as a whole, over comparative studies in which large numbers of cases are compared variable by variable, looking for statistical correlations (for this distinction see Ragin 1987: chapter 3 and 4). One can and should in the end always add more cases.

My comparison will be synchronic and will focus on ‘the very long eighteenth century’, roughly the period from the 1680s to the 1850s, in which the great diverging of Britain and China actually took place. In the literature I review, this has become by far the most popular way to proceed. This does not mean that diachronic comparisons would not make sense. Personally, I’m increasingly inclined to think it would also be quite interesting and pertinent to analyse why Sung China (907–1276) did not ‘take off’ instead of focusing so exclusively on Qing China (1644–1912). Sung

China in many respects was more developed and more dynamic than Qing. Mark Elvin, in his pathbreaking *The pattern of the Chinese past*, contrasts the dynamism of China under the Sung and partly even under the Yuan (1278–1368) dynasties with what he regards as the beginning of technological stagnation and decreasing dynamism as early as in the fourteenth century (Elvin 1973: chapters 2 and 3, 1996: chapters 2 and 3). Eric Jones is even more explicit and claims that China came within a hair's breadth of industrialising in the fourteenth century (Jones 1988: 160). The question as to why there was no breakthrough under the Sung and why (probably) the Sung achievement was not even repeated, is still open and to my view simply neglected, also by the Californians (Jones 1988, 1990).

2. Eurocentric approaches

The debate on the causes of the Great Divergence is as old as the social sciences. Until quite recently, two or, if one regards dependency-theory and modern world-systems analysis as distinct approaches from 'ordinary' Marxist analysis, three 'schools' have dominated it. The most popular approach is still the one that builds on the legacy of Max Weber and his claim that the West underwent a uniquely intense process of rationalisation that resulted in the emergence of capitalist market economies, bureaucratic states and a disenchanted culture that was ideally suited to produce science, technology and a methodical way of living (Schluchter 1983, 1984, 1987, 1988, 1998). Although not many scholars would actually describe themselves as Weberians and hardly anyone would deny that in various respects Weber was simply wrong, he is still setting many research agendas. For example, David Landes' enormously successful bestseller on the wealth and poverty of nations has a strong Weberian flavour (Landes 1998). In this line of thinking the economic 'rise of the West' is almost identified with 'the rise of the market', a thesis that mainstream economist as well as increasingly popular 'institutionalist' colleagues, enthusiastically support (for institutionalist economics see Thomas/North 1973; North 1981, 1990, 2005). Weberians focus on developments in Europe. They regard its history as structurally and fundamentally different from that of the rest of the world. To them, the Great Divergence is the culmination of a long process, not something fairly

contingent that could have occurred anywhere. They regard what happens in 'the rest' as of no fundamental relevance to the main direction of modern Western history.

The second approach is the classical Marxist one that, notwithstanding its evident differences from the Weberian one, also shares a number of fundamental features with it. The differences are well-known. More interesting in the context of this article are the many similarities. Both approaches regard capitalism as the motor of modern economic development and as a Western invention. They both claim that its emergence in Europe explains the economic primacy of the West. They share the idea that Europe was different and more dynamic than the rest of the world over which it, not by accident, came to rule. From a Marxist perspective, though, the main challenge is not to analyse 'rationalisation' but to explain the transition from feudalism to capitalism (Holton 1985). Although that actually is hard to square with Marx's overall philosophy of history and so required ad-hoc constructs like the Asiatic mode of production and Oriental despotism, most classical Marxists came to the conclusion that the world outside the West lacked the internal dynamics to manage a transition to capitalism on its own (see Avineri 1969; Krader 1975).

Dependency theory and world systems-analysis are often regarded as neo-Marxist. They do indeed build on elements of classical Marxism, such as its focus on exploitation and 'unequal exchange' and on the history of capitalism as central to any understanding of the modern world. There clearly also exist major differences between these approaches and the classical interpretation of Marx's work. I will not deal with them here and will simply assume that the readers of this journal are familiar with the various theories about the "development of underdevelopment" that have been formulated by proponents of dependency theory and world-systems analysis. For adherents of these 'schools' too, capitalism functions as the lever of global development. What they have in mind is *not* a Smithian capitalism of fair and free competition but rather one of monopoly, collusion and even coercion, in which there is intense interaction between those who hold political power and the capitalists who are looking to find protection against the market. Braudel, whose influence on Wallerstein's historical analysis is not always sufficiently appreciated, even defined 'real' capitalism as an 'anti-market' (Braudel 1979, 1981, 1984). According to him, capitalism only

triumphs when it becomes identified with the state, or rather when it is the state (Braudel 1977: 64-65). Though not entirely enthusiastic about what the West did and does on the global stage, both ‘schools’ present capitalism as a dynamic force that *from its very beginning* was trans-national and, originating from the West, created the modern world-system by incorporating a fairly passive and non-developing ‘Rest’. In that sense they too are clearly Eurocentric.

Those following in the footsteps of Weber and Marx as a rule have not focused on demographical and geographical factors but reference to them has never been entirely absent from the debate. When it comes to demography, the Hajnal-thesis, referring to a Western European marriage pattern and its direct and indirect consequences for the economy, continues to be discussed (Engelen/Wolf 2005; de Moor/van Zanden 2005). There have always been scholars who attribute an important, ‘autonomous’ role in history to the natural environment, – think for example of Braudel and Jones, and recently Diamond. At the present moment in global history, the role of ecology is clearly quite prominent (Bentley 2005).

The reader will have noticed that up until now the word ‘China’ has not been mentioned. That is not unintentional. In Weberian and (neo) Marxist stories alike, Qing China received hardly any serious attention, in stark contrast to an industrialising Britain that figured as ‘the spearhead’ of the West. If it was mentioned at all, it was as the almost archetypical ‘non-developer’. It tended to be described as a rather immobile and closed economy; a specimen of oriental despotism with an Asiatic mode of production (see Du-Yul 1972). Until late into the twentieth century, with some rare exceptions, scholars agreed that Qing China was so backward and immobile that it did not even occur to them to seriously study why the first industrial revolution did not take place there (Blue 1999; Ho-fung Hung 2003).

3. The California School

Recently things have changed tremendously. Various scholars have almost completely re-written the economic history of China in the early modern era. They have found a willing audience. Their revisionism has had a major impact on debates on ‘the rise of the West’. It was often explicitly

meant to have that effect as it was part and parcel of an effort to combat Eurocentrism. The best-known amongst these scholars are Kenneth Pomeranz, Roy Bin Wong, Jack Goldstone, James Lee, Dennis Flynn and Arturo Giráldez, Robert Marks, John Hobson, Jack Goody, and of course the late Jim Blaut and Andre Gunder Frank. They are often called ‘the California School’ because many of them worked at universities in California. For the sake of convenience, I will also use this label, introduced by Jack Goldstone, in my text. Goldstone has just published a comprehensive account of the Californian interpretation of ‘the rise of the West’. To show the Californian ‘creed’, I can do no better than quote him: “Instead of seeing the rise of the West as a long process of gradual advances in Europe while the rest of the world stood still, they have turned this story around. They argue that societies in Asia and the Middle East were the world leaders in economics; in science and technology; and in shipping, trade and exploration until about AD 1500. At the time Europe emerged from the Middle Ages and entered its Renaissance, these scholars contend, Europe was far behind many of the advanced societies elsewhere in the world and did not catch up with and surpass the leading Asian societies until about AD 1800. The rise of the West was thus relatively recent and sudden and rested to a large degree on the achievements of other civilisations and not merely on what happened in Europe. Indeed some of these scholars suggest that the rise of the West may have been a relatively short and perhaps temporary phenomenon” (Goldstone 2008b: VIII). I will repeatedly refer to Goldstone’s book, as it is a fine summary of Californian thinking. In my critique of that thinking, I will use and often support the analyses of Joseph Bryant, who has given a much more critical summary (Bryant 2006, 2008).

The rise of the California School is part of a widespread dissatisfaction with Eurocentrism. Some members of the school can be quite extreme and more than anything else emphasise Europe’s backwardness. Until late in the early modern era, Europe was a backwater, so they claim. Its rise to primacy, moreover, was not only late and contingent but would have been unthinkable without Europe borrowing or outright stealing from other parts of the world, and without sheer luck. Until the Great Divergence of the late eighteenth century turned the tides, the really advanced societies of the world were to be found in Asia, first and foremost China.

An exponent of this extreme view is John Hobson, author of a book with the telling title *The Eastern origins of Western civilisation* (Hobson 2004). This book contains a systematic effort to ‘provincialise’ and ‘primitivise’ Europe. No opportunity is missed to point out that Europe was peripheral and marginal, a late-developer that actually profited from the advantages of its backwardness. Britain’s industrialisation is said to have Afro-Asian origins. A pivotal role in helping Europe to rise is accorded to China. If there is any original, home-made contribution of Europe to its own rise, it would be its expertise in using violence and manipulating the economy and, later on, its racist feelings of superiority.

Focusing on the economy, but definitely no less anti-Eurocentric, is Andre Gunder Frank. His *ReOrient. Global economy in the Asian age* i.e. the period 1400–1800, hammers home one clear message. Economic historians studying the early modern era must focus on the East, in particular on China, the world’s most developed economy. To focus on a ‘rising’ Europe for that period is a Eurocentric mistake: “[...] Europe remained a marginal player in the world economy until the second half of the eighteenth century with a perpetual deficit [i.e. in its trade with Asia, Peer Vries] despite its relatively easy and cheap access to American money, without which Europe would have been almost entirely excluded from any participation in the world economy” (Frank 1998a: 75). To make sure everyone gets the message, Frank even claims “[...] the Europeans did not do anything – let alone ‘modernize’ – by themselves” (Frank 1998a: 259). When the Europeans in the end rose, they did so by “climbing on Asian shoulders” with money they had somehow found, stolen, extorted or earned (Frank 1998a: 277). Although Frank still repeatedly and emphatically refers to Europe’s exploitation of the Americas, he fiercely rejects what he had still believed when he propagated dependency-theory: the idea that the Europeans by their activities there and in parts of Africa and Asia created a global economy of which they themselves were the centre.

Frank’s plea for all-out Re-Oriented has not fallen upon deaf ears, even though many of his claims are patent exaggerations. To claim that Europe’s role in the world economy was marginal, to give just one example, is absurd. Let me quote Victor Lieberman: “One may well ask how a region that conducted an intensive internal commerce and that in 1750 dominated the trade of West Africa, the entire New World, and much of maritime

Southeast Asia and coastal India could have been marginal to the world economy” (Lieberman 2003: 74). Robert Marks, however, as with many others, seems convinced, and writes in his popular textbook that Europe was “[...] a peripheral, marginal player trying desperately to gain access to the sources of wealth generated in the East” (Marks 2002: 43).

Less vociferous Californians confine themselves to emphasising that ‘the Rest’ was *not* backward, and ‘the West’ *not* that different. Again, some scholars take quite radical positions, developing a real allergy to *any* claim of difference (Goody 2004, 2006; Langlois 2008). That is not very convincing: no one living at the time would have mistaken London for Peking. Nevertheless, the idea that at least in many respects Europe and Asia were much less different than is claimed in traditional historiography (the so-called ‘Eurasian similarity-thesis’) has a wide appeal, also amongst people who are not Euro-bashers, like Jack Goldstone (Goldstone 2008b) and the even more nuanced John Darwin (Darwin 2007). I will discuss this thesis extensively later on in this article.

4. China as the centre of an early modern global economy?

In varying ways, all Californians ‘rehabilitate’ early modern China and thereby change the parameters of the debate on the Great Divergence. Some of them claim that China held a central position in what they regard as an already integrated global economy; others that its economy was one of the most highly developed, and definitely the biggest one at the time; some combine those claims. The study of China has now acquired a central place in the field of early modern economic history. Probably the best way to dissect their ideas and their relevance for the debate on the Great Divergence is to set them alongside the four concepts of centrality that Samuel Adshhead uses in discussing Gunder Frank’s position (Adshhead 2004: 16-17).

One reason to give pride of place to China might be the observation that it was the world’s biggest economy at the time. I don’t think anyone would want to argue with that. In the second half of the eighteenth century China alone was home to about one-third of the globe’s inhabitants. Suggesting that it would also have been the economy with the biggest GDP is then not exactly a wild hypothesis. Various authors would go further and claim

that it was also the country with the highest standard of living in the world. That too may be a good reason to focus on its economy. Pomeranz, whose ideas on this subject have had the widest resonance, is actually somewhat more cautious. The core of his ‘surprising resemblances-thesis’ consists of the claim that on the eve of the Great Divergence, differences in wealth between the most advanced parts of the globe, i.e. Western Europe, the Yangzi Delta, and parts of Japan and India, were minor, if not negligible. Some regions of Asia may well have been somewhat richer than Western Europe.

This allegation may sound quite sensational, but actually it is not that new or controversial. Even some well-known ‘Eurocentric’ historians think it might be correct. Paul Bairoch defended a comparable claim more than two decades ago (for a recent synthesis see Bairoch 1993: chapter 9). Fernand Braudel quotes him approvingly (Braudel 1984: 533-535). Eurocentric *par excellence* David Landes thinks that at the end of the eighteenth century the gap in real income between Western Europe and India and China was not bigger than 1.5 or 2 to 1 (Landes 1998: XX). Angus Maddison, to give one last example, is not so confident about the wealth and level of development of Qing China and not very positive about Pomeranz’s work, but still thinks that in 1820 Western Europe was ‘only’ about twice as wealthy as China (Maddison 2004, 2007).

Pomeranz’s views on the wealth of the East have ignited a debate in which his ‘confidence’ has been qualified by authors who claim that at least in parts of North-western Europe real wages were higher than in even the richest parts of Asia. Yet in their estimates too, differences are not huge, especially not when compared to those that originated with industrialisation. Moreover, what, in particular for the Chinese side of the equation – where wage labour was almost non-existent – is actually needed, is information on *incomes* rather than *wages* (Allen/Bengtsson/Dribe 2005; Allen et al. 2005; Broadberry/Gupta 2005, 2006; for more detail on the China-Britain comparison see *Journal of Asian Studies* 61 (2) 2002 and 62 (1) 2003). In that respect the comparative analysis, by Bob Allen, of rural *incomes* in the Lower Yangtze Delta-region and Britain in the eighteenth century is interesting and promising (Allen 2004a). Anyhow, considering the Malthusian constraints that existed in all pre-industrial societies, differences in wealth simply *cannot* have been very big.

The question of course is what this might mean for the debate on the Great Divergence. It undoubtedly is interesting and important to try and assess the differing levels of wealth of the various countries that figure in that debate. For too long, apodictic claims have been made in that respect without a solid empirical basis. One, however, has to be careful not to assume too strong a relationship between wealth and (potential for) development. The chances of a country becoming the first industrial nation need not systematically increase with its wealth. The Dutch Republic, whose wealth in all probability was second to none until the 1820s, industrialised quite late. On the other hand, the costs of being the first industrialising nation *à la Britain* were quite low and could, in principle, have been covered by quite a few nations.

A third possible reason to focus on China and its economy might be that China actually dominated the global economy at the time. Personally I am anything but convinced that this was the case. Whatever the actual importance of early modern China in the economy of the world, it would in any case be far-fetched to claim it did actively and consciously try and set the pace of the global economy. The Chinese under Ming (1368–1644) and Qing rule did not make many lasting initiatives on a global scale. They all but stopped travelling to other continents, whether it was for trade or to invest. Nor did they settle there in substantial numbers. If so much silver ended up in China, this was not due to any effort by the Chinese. It was because others brought it. China's government was rather reticent when it came to contacts with non-Chinese, in particular when they came from non-tributary countries. All this makes early modern China a very unlikely candidate for being the active trade centre of the globe, notwithstanding the existence of many private initiatives and huge Chinese intra-Asiatic trade networks. The chances that China would in one way or another have passively dominated economic life on the globe by means of the sheer size of its economy also look very slim. Size and the characteristics of its intercontinental trade connections simply rule that out. What is clear is that even if China did 'dominate' the early modern global economy, that apparently did not create the right preconditions for an early take-off.

Finally, one may conceive of the centre of the global economy – at least in the early modern context – as the country that attracts the largest amount of bullion. It is this claim in particular – that in practice cannot easily be

distinguished from the form of centrality we have just discussed – that has become popular with regard to early modern China, with the not irrelevant restriction that one tends to exclusively focus on imports and exports of *silver*. For the sake of brevity, I will refer to this thesis as ‘the silver sink thesis’. It has two components. Firstly, the claim that from the end of the sixteenth century till the 1820s, China had a huge import surplus of silver. Secondly, the claim that this was a clear indication of the strength of its economy and of its involvement in global exchange. At the time bullion was widely considered as the measure of wealth *par excellence*: the country that accumulated most of it via its trade must therefore have been the wealthiest one with the most efficient economy. At least, that is, according to the defenders of this thesis.

With characteristic vigour, this thesis has also been propagated by Andre Gunder Frank (Frank 1998a: 75, 117, 127-128, 148, 175, 177-178, 185). For example, in a barrage of superlatives he writes: “China’s even greater [i.e. than India’s, Peer Vries], indeed the world economy’s greatest, productivity, competitiveness, and centrality were reflected in its most favorable balance of trade. That was based primarily on its world economic export leadership in silks and ceramics and its exports also of gold, copper-cash, and later of tea. These exports in turn made China the ‘ultimate sink’ of the world’s silver, which flowed there to balance China’s almost perpetual export surplus. Of course, China was only able to satisfy its insatiable ‘demand’ for silver because it had an inexhaustible supply of exports, which were in perpetual demand elsewhere in the world economy” (Frank 1998a: 127-128). Frank regards global trade as a game with winners and losers. He is convinced its winners lived in Asia, to be more precise in China (Frank 1998a: chapter 3).

The silver sink thesis was already cherished by Flynn and Giráldez before Frank promoted it. They think it is likely that two-thirds or maybe even approximately three quarters of all the silver produced in America between roughly 1492 and the 1820s, ultimately settled in China (Flynn/Giráldez 2004: 88, 1999: 23). For them too, this is an indication of China’s dominance and primacy (Flynn/Giráldez 2006: 240). It will not come as a surprise that Hobson and Marks support such claims. Hobson refers to “its perennial trade deficit with Asia” as “the clearest sign of Europe’s backwardness” (Hobson 2004: 171, 72, 77-78). Marks, who, as we have seen, regards

Europe at that time as being backward, claims that “[...] approximately three-quarters of the New World silver production over the three centuries from 1500 to 1800 eventually wound up in China” (Marks 2002: 80).

This view has found its way into textbooks. John and William McNeill, for example, consider the period from 1450 to 1800 as one in which a global trade system emerged and write that “[...] until about 1750, China remained at the centre of this system. [...] More than three-fourths of the silver [from America and Japan, Peer Vries] went to China or India” (McNeill/McNeill 2003: 201-202). David Christian voices a similar opinion: “That surpluses of silver gravitated toward Asia throughout this period also suggests the centrality of Asia in the emerging world system of trade.” To him Asia, and in Asia China, was the centre of the existing world system of exchange (Christian 2004: 385-386, 390, 404). Many more such quotes could be given. Even Pomeranz, who thinks the early modern global economy was polycentric, in a popular book written with Steven Topik explicitly points at the fact that: “New World gold and silver were shipped in huge quantities to Asia – perhaps fifty percent of these metals found their way to China alone” (Pomeranz/Topik 1999: 103).

Despite its popularity, this thesis is indefensible. An extensive rejection would require much more space than is available in the context of an article like this, so I can only refer to a forthcoming publication of mine, but it boils down to the following arguments (Vries forthcoming b). Firstly, close reading of the existing literature shows that the amount of silver that ended up in China was much smaller than proponents of the silver sink thesis claim; roughly one third of Latin American production in the eighteenth century in an extremely optimistic estimate. Silver remained very scarce in China as, for instance, shown in the fact that one could buy far more with it than in Western Europe. Moreover, much silver was brought to China because it yielded Westerners huge arbitrage profits when they exchanged it for gold. This reference to gold makes one wonder why one would have to focus so exclusively on silver. What about gold, copper or paper money? Of the globe’s gold production, the bulk went to Western Europe and stayed there. Britain for most of the eighteenth century was on a gold standard and it was a major copper exporter. The products that China exported in the second half of the eighteenth century when trade between China and Britain took off, either increasingly were not real manufactured

goods (e.g. gold and other metals, raw silk and drugs) or, in the case of tea (which by the end of that century had become by far the most important export commodity), a very peculiar kind of manufactured good that the British simply could not produce as it did not grow in their country, nor in any other country they had contacts with except China. This means that Sino-British exchanges do not provide relevant information on the relative productive efficiency of China's economy as compared with that of Britain. And then finally there is the fact that even if trade between China and Britain as such resulted in a drain of silver from Britain to China, the British earned a great deal of money by transporting and selling what they bought in China, far more than the Chinese did. Can one seriously expect them to have traded for decades with China without making any profit? These comments are not meant to re-install traditional Eurocentrism. In my view there definitely were important intercontinental trade flows, but not a global division of labour with a clear centre. *Intercontinental* exchange was simply too tiny for that: for a country like China it certainly was less than one percent of its GDP. The most important of those intercontinental trade flows and the one that grew fastest was that across the Atlantic that in the 1770 was about three times as big as that between Western Europe and the whole of Asia (see Vries 2003: 91-93; de Vries 2003: note 101).

Far less silver ended up in China than defenders of the silver sink thesis claim. The silver that did end up there often did so for reasons other than that of any kind of Chinese economic superiority. Nevertheless, China's silver imports were still huge. What exactly that means for the Great Divergence debate is unclear. While believing that its permanent silver influx did indeed, amongst other things, point at the strength of China's economy, Flynn and Giráldez think that the economy in the end would have been better off had the Chinese *not* exported so much of their products to acquire silver. Had they (re-)introduced a paper currency, all the inputs that were now used to acquire silver might have been used for producing something they could actually have consumed themselves (Flynn/Giráldez 2000).

Frank too, thinks those silver imports in the end had negative effects for China. They fuelled economic dynamism and economic growth. That led to population growth, which in turn brought about what Elvin calls a "high-level equilibrium trap", a situation where labour is cheap and resources expensive and where opportunities to profitably invest capital in

labour-saving technology are lacking (Elvin 1973, 1996). Whatever the value of Elvin's explanation of China's predicament – on which more will be said later – to connect it to silver imports the way Frank does, is to seriously over-estimate the impact of such relatively tiny intercontinental trade on China's huge economy.

5. A world of surprising resemblances

Radical Californians may have a tendency to exaggerate the level of China's development and in particular its role in the economy of the early modern world. Their work, however, has forced even their fiercest opponents to admit that China's economy was much healthier and its global importance much bigger than traditional Eurocentric stories suggest. To find out *how* healthy and *how* important, a detailed analysis is needed of China's domestic conditions and dynamics. In that respect Pomeranz's work, which as we will see is definitely not blind to *global* political economy, is quite helpful, in particular his effort to describe the economies of Western Europe and China in terms of "a world of surprising resemblances" (Pomeranz 2000: Part I).

Although that phrase is frequently quoted, it is not always interpreted in the same way. In Pomeranz's book it often refers to the supposed absence of big differences in levels of *wealth* between various advanced early modern economies. Looking at real wages and income, as indicated, this claim is probably somewhat overstated but not terribly controversial. Including other indicators, like life expectancy, does not really change the picture, although here too, Californians tend to be somewhat overly positive about China.

It can also refer to a claim that there were no big differences in the level of *development* between various economies in Eurasia, which are supposed to have found similarly effective solutions to *their* problems. Pomeranz himself refers to "a series of balanced comparisons (that) show several surprising similarities in agricultural, commercial and proto-industrial development [...] amongst various parts of Eurasia as late as 1750" (Pomeranz 2000: 8). Here too the revisionists have an undeniable point. Too many claims about European 'exceptionalism' were nothing but a measure of the existing igno-

rance with regard to the history of the rest of the world. Early modern China had very sophisticated systems of production and trade. It was home to many innovations. It did know private property and property rights and had well-functioning commodity markets that came closer to Adam Smith's ideal than most of their counterparts in Western Europe. It was a highly developed commercial society. Its government clearly was not constantly interfering and thwarting development. It had a huge foreign trade. It went through a kind of consumer revolution and had an equivalent of what in European economic history is called 'the rise of domestic industry'. In agriculture, land management, the efficient use of fuel, and the production of textiles and ceramics, it was in many respects more, rather than less, advanced than Britain.

Fundamental in this respect is the fact that before the first industrial revolution, *all* economies, even the most advanced ones, were Malthusian, i.e. dependent for their wealth on the quantity and quality of their land. They all faced the same constraints. They did not massively use fossil fuels and what use they made of them was for heating, not as a power source. According to Wrigley (1988), the Industrial Revolution as it occurred in Britain was a process that ended this direct and full dependency on the land. Californian critics endorse his characterisation of the first industrial revolution as the emergence of a mineral-based energy economy (Goldstone 2002). Without it, *both* Britain and China, according to Pomeranz, would sooner or later have reached the Malthusian ceiling.

6. Problems with the surprising resemblances-thesis

We now have a much more positive view of China's economy on the eve of Western industrialisation. This is not 'unproblematic'. The more 'Eurasian' resemblances and equivalents are brought into prominence, the more miraculous if not downright inexplicable becomes the enormous gap that emerged during the nineteenth century between Britain and China. How can situations that are surprisingly similar produce such huge differences? If resemblances really were so striking, why did not an eastern society, e.g. China, 'take off', or at least find it easy to catch up when others did? The rise of the West does not exactly become less 'miraculous' either, if one,

like radical Californian critics do, constantly reiterates that it was poor and underdeveloped and could only become richer and more developed – and in the end industrialise – by stealing from the Americas and borrowing, copying and free-riding in Asia. If that were true, why did the ‘East’ lose its advantage and why did not the ‘West’ end up being like that more advanced ‘East’?

Joseph Bryant rightly feels uncomfortable with this urge to remove “all potentially invidious distinctions” between West and East (Bryant 2006: 417). For an author like Jack Goody, the distinct qualitative difference between East and West came only with industrialisation (Goody 2004: 60). In Bryant’s words such a “[...] world flattened of determinant social differences makes the local emergence of any historical novelty structurally inexplicable and restricts explanatory options to conjunctures aleatory or incidental” (Bryant 2006: 418).

One indeed finds many references to ‘luck’, of all sorts and varieties, in the works of the Californian critics. John Hobson believes that explaining the rise of the West requires reference to no less than five cases of Western luck (Hobson 2004: 313-316). Rosaire Langlois claims that “Europeans weren’t just lucky; they were lucky many times over” (Langlois 2008: 141). When Frank points at the windfall the West had when it acquired the silver and gold of Latin America or at ‘the decline of the East’, from which it profited, he also is basically referring to luck. These references to luck are usually accompanied by taunts at Eurocentric scholarship for interpreting the rise of the West in terms of ‘inevitability’ (Hobson 2004: *passim*, e.g. 3, 10, 11, 15, 18, 19; Marks 2002: 10-15; Darwin 2007: X, 50, 57). This is setting up straw men. I have never come across any serious historian who does so. Moreover, is everything that is not ‘inevitable’ thereby ‘luck’? The quite depressing implications for the poor of the world of the ‘fact’ that the rich would have become rich by sheer luck, apparently escapes these Californians.

Pomeranz is too serious a scholar to be as apodictic and polemical as some of his colleagues, but in his work references to ‘fortune’, ‘luck’, ‘accidents’, windfalls’ and the like also abound (Pomeranz 2000: the flap text and 12, 16, 62, 68, 241). The luck he has in mind is of a specific kind, namely the ‘fortuitous’ availability for Britain of extra resources in the form of coal and colonies. Explanations that refer to resources and the environment have become quite popular. They are also quite problematic. By their very

nature they are incomplete as the actual importance of resources depends on whether they are recognized as such; whether they are actually used, and on how they are used and to what purpose. Resources as such do not do anything. If only for that reason, there is no clear correlation between having ample resources and being rich. In the early modern era the Dutch Republic and Japan, to give just two examples, were not blessed by nature. They nevertheless were rich. The wealth of regions often was, and is, inversely related to their natural wealth (Reinert 2007: chapter 3). Spain and Portugal amassed enormous amounts of bullion and land over centuries. They continued to be poor and underdeveloped, which suggests that easy money can even be bad for an economy (Landes 1998: 171-173). It is not some abstract, overall category called 'resources' that in the longer run decides a country's wealth, but its productivity. Britain was indeed in a position to import a great deal of cotton from the periphery, but that would not have been much of 'a wind-fall' if it had not been able to efficiently turn that cotton into cheap textiles (Goldstone 2008b: 67-70).

Frank came up with an explanation, before Pomeranz did, in which resources play a fundamental role, so I will first comment on his ideas. For him, Europe's luck in laying its hands on American resources is subordinated to what he calls 'a global economic-demographic explanation'. He claims that China, because of its overall wealth, became densely populated and ended up in a high-level equilibrium trap. I do not exclude the existence of such a 'trap' in China. Yet more is involved in it than just a dense, growing population and relative factor costs, as Elvin, who introduced the concept, admits (Elvin 1996: 64-100). Nevertheless, even if China's economy, for whatever reason, indeed got stuck, how can this, or for that matter India's troubles, which were also quite real, in any meaningful sense of the word have *caused* Britain's industrialisation, the decisive element in the great diverging? How can one, with Frank, claim that 'the decline of the East' somehow explains the 'rise of the West'? (Frank 1998a: chapter 5)

I will not discuss Franks 'explanation' of the decline of the East, which refers to the fact that the region had entered a contractive B-phase of a long Kondratieff cycle. I simply fail to see what that in practice can mean and how it can explain the very different crises and their timing in, for instance, India, the Ottoman Empire or China and why in a supposedly globally-

integrated economy Europe would *not* be hit by that contraction. For more extensive comments I refer to my review (Vries 1998a, 1998b).

Frank's comments on the effect of global competition for European and Asian economies look more enlightening in that respect. The British had good reasons to try and do something about the fact that Asians were such highly efficient producers of goods that they, the British, wanted to consume themselves or sell to others. One such product would be Chinese porcelain. That challenge, in principle, had already been successfully dealt with by the 1750s, when the British produced and began exporting porcelain themselves. Silk textiles from China were less of a challenge; the bulk of British imports consisted of raw silks and those imports, moreover, tended to decline after the 1750s. By far the most important import from China at the end of the eighteenth century was tea. That simply could not be produced in Britain. Here import substitution could only work if the British found someplace in their empire where it could be grown. In the end, from the 1840s onwards, India would become that place.

The product at the heart of Britain's industrialisation was cotton. The main competitor in this case was India. Mechanisation of British cotton production, which, important as it may have been is not identical to '*the British industrial revolution*', definitely was (amongst other things) a response to that challenge. But that, of course, only provides a very partial 'explanation'. History is replete with efforts at import substitution that did not work. How could Britain respond so successfully that cotton textiles became its major export product? Here Frank claims that Britain, because of its relative under-population would tend to choose labour-extensive options in which machines would replace people. China, to refer to the other country central to my analysis, in contrast would, because of its 'relative' overpopulation, tend to select labour-intensive options, going down an 'involuntary' road.

This can only mean that Britain, which, according to Frank, was poor and backward and constantly sent huge amounts of silver to China, must have had high real wages and a sufficient amount of, actually quite cheap capital: All this notwithstanding its very heavy taxation and its enormous national debt. In China, that according to Frank was the most efficient economy of the world and its silver sink, real wages then apparently must have been so low and capital so expensive that it was not profitable to invest

in labour saving. Apparently, these contradictions do not bother Frank. It should, because it actually destroys his main thesis about China's centrality (Frank 1998a: chapter 6).

High wages in Britain indeed have provided an incentive to cut labour costs but the high level of these wages can not be explained by simple reference to demography. From 1750 onwards population increased sharply and labour still continued to be expensive. British wages were high because of high productivity in tradables and services. On top of that there was a long tradition of experimenting with mechanical solutions to problems in production, and coal was already widely used. Low interest rates facilitated borrowing for those who wanted to invest in machinery. Import substitution on this scale required a very complex and efficient system of manipulating supply and demand. In this case that meant keeping Indian textiles out, importing cheap cotton, increasingly from America, supporting exports of domestically produced cotton textiles, and so on and so forth. Such policies are only feasible in a well-organised mercantilist state (Allen 2004b). The weakening of India to which Frank refers, did indeed provide Britain with cheaper cotton imports and with a wider protected market; however, that weakening was caused, to a large extent, by British interference and could only be exploited with the support of Britain's rulers, which again shows that one can not simply ignore the role of politics in global economic history. For various other products and various other countries, including China, one could make similar comments. Being challenged and having high labour costs is only part of the story. A global economic-demographic explanation does not suffice: one must also take on board human agency, i.e. culture, institutions and politics.

My comments on Pomeranz's resource-based explanation have already been published. I will not extensively repeat them here (Vries 2001a). Pomeranz does not endorse Frank's idea of a "fall of Asia" that would have been "ecologically played-out" versus a Europe that still had "plenty of room left to grow" (Pomeranz 2000: 12). He denies that China at the end of the eighteenth century was already 'trapped' or in a worse predicament than Britain. My reading tends to claim that China at the time was closer to its Malthusian ceiling than Britain. I am ambivalent, though, about what that means: the closer one is to the Malthusian ceiling, the stronger the pressure

becomes to do something about it. The further one is from that ceiling, the more room one has to manoeuvre.

Whether Britain actually had more 'slack', i.e. unused resources or not, it definitely was much more actively engaged in using and finding resources, nearby in its Gaelic periphery, further away from home in Central and Eastern Europe, in the Americas and, in the end, in all corners of the world. Western overseas imperialism as compared to Chinese overland imperialism was much more focused on exploring, controlling and *utilising* the regions it incorporated, and it incorporated more of them (Abernethy 2000). China did hardly anything with the new territories it acquired in the eighteenth century. Its rulers even left their homeland, Manchuria, almost completely unexploited. Again proof, if proof is needed, that resources are not something one simply 'has' or 'does not have'. One cannot escape from studying society if one wants to know their impact. I will revert to this topic later on in my general critique of the way Californians approach the past.

7. A world of surprising resemblances? A closer look

Looked at more closely and shifting from a static to a dynamic analysis, the 'surprising resemblances' thesis loses much of its lustre. It turns out to deal rather loosely with time and place. With regard to place, I pointed at ambiguities in my review of Pomeranz's book (Vries 2001a). It is not always clear whether he is talking about Europe, Western Europe or Britain. At the other side of the equation, one finds references to Asia, parts of Asia, China and the Lower Yangzi region. What is clear is that he often switches. In the work of Wong, where the European state is compared to that of China, whereas there are enormous differences between state-formation in various regions of Europe, and in that of Hobson, where the East, meaning the Ottoman Empire, The Safavid Empire, India, China, Japan plus even parts of North Africa, is simply presented as one region to be contrasted to the West, one is struck by the disturbing vagueness when it comes to the regions with which one is actually dealing (Wong 1997; Hobson 2004). There is no such thing here as a perfect choice, but one should not switch as suits the argument. Differences between various parts of the 'East' as well as the 'West' were often enormous, so one has to be specific. When it

comes to chronology, Californians can be rather sloppy. The early modern era – with which most of their publications deal – was not an era of steady progress. Even in ‘rising’ Western Europe, improvement was neither continuous nor general (Goldstone 2008b: 79-83). Californians are easily seduced into selecting examples from different places and times to then put them all in one bag called ‘early modern Western Europe’ or ‘early modern China’. That can be quite deceptive and forms one of the main reasons I plead for comparing *specific countries* in precisely *demarkated periods* of time, in my case China and Britain in the ‘very long eighteenth century’.

Let us focus on chronology. In ‘Californian’ publications one constantly comes across references to the huge amounts of porcelain the Chinese exported to the West, mostly with the comment that Westerners did not even know how to produce it. That is a correct observation, but was no longer the case after 1750. Westerners by then had managed to produce porcelain and substitutes themselves, and imports from China would soon plummet. When it comes to silk textiles, China’s advantage had also disappeared by the 1750s. That is before one can find any serious sign of industrialisation anywhere in Europe. What continued to be exported, and increasingly so, was *raw* silk. Chinese cotton exports rose temporarily in the eighteenth century but could not stand up to British competition any longer, even before the end of the century. The importance of coal and iron for industrialisation can hardly be overestimated. It is often pointed out that Sung China had known an impressive coal and iron production (Hartwell 1967). In the period we discuss here, production of both, however, was at a *lower* level and showed no increase. In Britain the production of both coal and iron constantly increased.

Californians are rightly fond of calling attention to China’s high level of technology and its many inventions and innovations. Yet they tend to ignore that in this respect too, dynamism clearly abated. During the Qing dynasty, the number of discoveries in science and technology decreased very substantially, as Joseph Needham acknowledged and tried to explain (Cohen 1994: chapter 6.5). Goldstone is well aware of this: he actually mentions the fact in his book, but he does not really address its implications for his optimistic thesis about China (Goldstone 2008b: 122, table 7.1). Before 1800, as a rule, technological, ‘scientific’ and organisational knowledge did not accumulate. Change tended to be scattered and isolated. For China, that by

and large continued to be the case. Goldstone shows that Britain became an exception to this rule after 1688 (Goldstone 2008b: 26-29, chapter 8). I would go further and claim that Western Europe *as a whole* from at least the Renaissance onwards in this respect was on a different trajectory from China. Knowledge accumulated and progress became normal. Actually, the contrast was even bigger. Not only was dynamism slacking in Mid-Qing China; there are various examples of technologies and knowledge that disappeared. In the production of silk and cotton, with the passing of time, fewer machines were used and they tended to become simpler (Elvin 1973, 1996; Chao/Chao 1977; Li 1981).

One can find examples of changes over time and, more importantly, of relative or even absolute regress in other sectors of Chinese society too. All Californian critics refer to a huge increase of population under Qing rule as a sign that China's economy was doing very well. This is somewhat rash. There is much debate on the exact development of China's population at that time and population growth need not mean economic growth in terms of increasing wealth. It *can* also simply mean more people or even more poor people. Still however, no one can deny that China managed to decently feed an enormous population of over 300 million people around 1800. It did so, however, with an increasingly *rural* population. Goldstone claims that, for the early modern era, the level of urbanisation is an excellent indicator of the strength of an economy. He then mentions that China had a couple of huge towns and concludes that it must have had a vibrant economy (Goldstone 2008b: 83-84). That is somewhat rash too. Urbanisation in China in 1800 was substantially lower than in Western Europe. Only three per cent of its population lived in cities of over 10,000 people. In Western Europe this was over ten per cent (de Vries 1984: 349). The contrast with Britain is striking: in 1750 more than sixteen percent of its total population lived in towns of over 10,000 inhabitants. In 1800 that was over twenty percent. China's level of urbanisation between the Sung era and the beginning of the nineteenth century actually *decreased*. Population increases during that period were fully absorbed by the countryside (Chao 1986: 60-63). Things were very different in Britain.

Comparing the Qing state with Britain, we again see different routes. Californians like Perdue and Goldstone describe Qing China as a part of a state-system having to compete with surrounding states and suggest that the

supposedly unique European state-system was not unique at all (Goldstone 2008b: 100-101; Perdue 2005: chapter 15). Again, one should be careful not to overlook differences over time and, more importantly here, in orders of magnitude. Qing China's 'competitors' until the beginning of the nineteenth century were not serious threats. China's biggest military effort in the eighteenth century consisted of the campaigns against the Zunghar Mongols, who in the end were crushed. At the time of final confrontation under the Qianlong emperor (1736-1795), there were some 600,000 Zunghars against over 250 million 'Chinese'. The arch-enemy of Britain during the eighteenth century was France, whose population over the century increased from over twenty million to about thirty million, confronting some five, to, at the end of the century – excluding Ireland – ten million Britons. These are entirely different kinds of competition with entirely differing effects. Britain developed an ever-stronger fiscal-military state, engaged in conflicts all over the globe. China had trouble defeating even quite small opponents. The Qianlong emperor, for example, was not successful in his campaigns against Vietnam, a country with only a couple of million inhabitants.

In the 1300s, China's government could indeed raise a fleet of 400,000 soldiers and sailors, as Goldstone points out. That of course is impressive. As is the fleet of Zheng He, which consisted of enormous ships manned by thousands of sailors and travelled half the globe in the beginning of the fifteenth century (Goldstone 2008b: 101, 27-28). At the beginning of the nineteenth century, however, there no longer existed a Chinese Navy to speak of. In 1809-1810, central government had to ask the English and the Portuguese for help in combating pirates (Antony 2006). The contrast with Britain and its Royal Navy couldn't be bigger. Between the beginning of Qing rule and the first half of the nineteenth century, China's army tended to become smaller rather than bigger, in any case in comparison to total population. In comparison to what was the norm in European states, where the size of armies had increased enormously and permanent change in organisation and armaments had become the norm, it had become small and inefficient. When it comes to expansion one sees a similar trend: China did expand over time, but as compared to Britain, its expansion was small.

In revisionist literature it has become common usage to refer to China's efficient bureaucratic rule. Over time, however, this bureaucracy, relatively speaking, became much smaller. From the beginning of Qing rule onwards,

it never counted more than a mere 20,000 to 30,000 officials for China as a whole, whereas total population increased sharply. Considering the growing complaints about corruption towards the end of the eighteenth century, one may also query its supposed efficiency. Here too the direction of developments in Britain was different. The Chinese state did *not* become stronger over the seventeenth and eighteenth centuries, in terms of 'hard power' at least. Important European states did.

Let me just give one final example of differing trends. Paper money was invented in China under the Sung rule. Under the Qing dynasty, the state no longer issued it. Neither did it coin any silver or gold currency. In the eighteenth century the importance of copper as currency increased. Again, developments in Britain went in an opposite direction. In the eighteenth century the country was on a gold standard and had a central bank. Overall, Qing China seems to have undergone far fewer institutional changes than Britain, which had its financial and military revolutions.

A fundamental problem with the 'striking resemblances' thesis is that, in trying to establish the relative efficiency of various economies, it tends to concentrate on providing a static cross-section at a specific moment in time. Various indicators used to show 'surprising resemblances' or even Chinese 'advantages', however, do not fare well over time in Qing China in absolute or in relative terms, i.e. as compared to Britain. That means they cannot provide satisfactory answers to questions one has to address when trying to solve the riddle of the Great Divergence. Questions like: on what trajectory is an economy? What options are open to it and how easy or complicated is it to choose them? How much 'potential' does it have? A static analysis is not capable of providing answers to such questions, particularly not when that analysis tends to be rather 'loose' with regard to time and place, as Californian analyses often are. On top of that, there are differences that are not mentioned (enough) but do deserve close attention.

8. Neglected differences and differing trajectories: modes of production

Resources and trade, I would say, get more than their fair share of attention from Californians. The importance of trade tends to be greatly

over-rated when it comes to the amounts of traded commodities, but more analysis of trading as a generator of income would be welcomed; that is, of trading as providing a service for which one gets paid. In early modern mercantile capitalism – as I would claim in *all* capitalism – big profits are made not in producing something but in buying and selling it and in financing. If one depicts the British as mere middlemen or transporters and the Chinese as actual producers and if one then suggests the Chinese would somehow profit more and have a stronger economic position, one misinterprets the logic of mercantile capitalism. Giving an extended analysis would of course lead us too far afield. So let me just give one example, the tea trade. The Chinese produced tea whereas the British ‘only’ transported it. But in the process, in the end, the British traders (and the British state), earned much more than their Chinese counterparts, let alone the Chinese producers. For the British economy income from services was enormously important: even in the nineteenth century, the first industrial nation normally had a deficit in its balance of trade that was more than compensated for by its income from services and by income from foreign assets. Even an industrialising Britain was about as much a service economy as it was a commodity producing economy, earning a great deal of money as carrier, insurer, financier and investor (Cain/Hopkins 2001). Comparative studies would be most welcome here.

The phenomena normally associated with the first industrial revolution, however, primarily concern changes in the mode of production. Whatever may have been their effects, differences in this respect between pre-industrial China and Britain were enormous. They ought to be a central topic in the debates. In the past they often were analyzed, mainly but not exclusively by Marxists when they discussed China’s ‘household mode of production’ or ‘involution’. They still are. However, modes of production no longer seem to really be ‘en vogue’ (Gates 1996; Brenner/Isett 2002; Huang 2002; Isett 2007). This is a very complex and wide-ranging topic that can only be treated very cursorily here. For further information I have to refer the reader to the literature.

For the sake of brevity and argument, one might construct the following, extremely simplified and stylised dichotomies in a comparison of the way in which production was organized in China and Britain. China’s agriculture by and large was much more land- and labour intensive than that of Britain.

Whereas in Britain most agricultural production took place on large farms, in China farms almost without exception were very small (see e.g. Huang 2002; Brenner/Isett 2002). Let me just give one example: in around 1800, an average farm in Southern Britain was about 150 acres; in the North that was about 100 acres. In rice-growing regions in China it would be roughly some 5 acres. In 1750, the amount of agricultural land in Britain per agriculturist was about forty-five times as big as in China's Lower Yangzi region. In China's energy system, the relative importance of human labour was much bigger than it was in Western Europe, and that of fuels much smaller. In absolute terms the importance of animals for the economy of Western Europe, and in particular Britain, was striking and much bigger than it was in China (Malanima 2006; Wrigley 1988: chapter 2). Production in China, in particular in agriculture, looks less capital-intensive in terms of implements and animals than in Britain, in particular in rice-growing regions where, in Francesca Bray's terms, it was very 'skill-oriented' (Bray 1986).

In China the household continued to be by far the most important unit of production. In Britain waged labour became increasingly important. Whereas the percentage of proletarians in China's total labour force was negligible and certainly amounted to no more than five percent, waged labour in Britain was becoming the rule rather than the exception. In the countryside it was already more than fifty percent at the end of the seventeenth century and about three-quarters around 1850. Landlords in China were not managers of large farms but 'tenurial landlords', i.e. landowners who rented out their land in small parcels to peasants (Chao 1986: chapters 7, 8). One finds this overwhelming predominance of small peasant cultivators in China as compared to Britain, not only in rice-growing regions, where according to Bray it would be logical, but also in places where other grains and, even more surprisingly, products like tea, sugar, tobacco, cotton or silk were grown. For all these crops, one never finds any reference to plantation-like cultivation, either inside or outside China Proper (Gardella 1994; Mazumdar 1998; Chao/Chao 1977; Li 1981; Xu Dixin/Wu Chengming 2000: chapter 10). In the regions from which the British imported these goods, inside as well as outside their empire, large-scale, centrally-coordinated growing and processing on 'plantations' was the rule.

Those Chinese families of small peasants, especially the women, spent substantial amounts of their time producing goods, especially textiles, for

the market. This domestic industry as a rule was organised as a '*Kaufsystem*', with each stage of production and distribution being dealt with by an autonomous 'entity' that bought its raw materials and sold its products. These petty-commodity producers fell back on large numbers of middlemen and brokers. In comparison to the situation in Britain, putting out production was exceptional (Eastman 1988: chapters 6-8; Li 1981; Rowe 1998; Zelin 1997). There are examples of a putting-out system in China in silk textiles production and of big manufactories in silk textiles as well as porcelain production, but that is quite exceptional. In Britain co-ordination via management and concentration of capital were on the increase. In China, co-ordination via the market and the substitution of commerce for management continued to be the rule (Elvin 1996: 20-63). Its commercial organization was very sophisticated and shaped the patterns of commodity production as it was buyer-driven and extremely flexible but based on small producers (Hamilton/Chang Wei-an 2003).

The market clearly was very important in Qing China's economy. The total amount of goods traded was, of course, enormous, as Californians love to point out. Yet, as a percentage of total production I think it must have been less than in Britain. Producing for subsistence was more common in China as there were more peasants who shied away from producing only for a market. Cash crops of course were grown, but their importance continued to be relatively small (Xu Dixin/Wu Chengming 2000: chapter 6). The amount of fertilizer that entered interregional markets was much smaller than Californians claim (Yong Xue 2007). The decreasing level of urbanization must have had its effects on the sale of agricultural products. When it comes to factor markets, China's labour market was definitely less developed and less important than that of Britain as a much smaller number of people were working outside their home and outside any family setting. Considering its much higher interest rates, its capital market does not look very efficient. There was a lively market for land.

It may very well be that 'the Chinese mode of production' yielded about as much per capita as that of Britain over the eighteenth century and even later. But did it have the same potential for further growth? Are there not inherent limits to increasing productivity in a system based on a household-mode of production? There is less 'free labour' that can be hired and 'fired' as one pleases. Does that not set a certain limit to efficiency? People

focusing on subsistence in all probability will buy less in markets. Does that not restrict specialisation? Will not households tend to avoid investing in labour-saving implements, in particular when they are big and expensive?

These are hard questions to answer. What is clear, however, is that in China's economy dynamics were at work that differed from those at work in Britain's economy, as already demonstrated in previous comments with regard to changes that occurred between the Sung and the Mid Qing eras. The Industrial Revolution in Britain has traditionally been associated with breakthroughs in the use of energy and in technology, and with 'the rise of the factory', which counts as a symbol for concentrated production and the use of wage labour. Compared to China, Britain was already on a much more energy-intensive route *before* industrialisation. It made far more use of animals than China did and was already the biggest coal user in the world in 1700, in all probability burning five times as much of it as the entire rest of the world. There was an inclination to look for mechanical solutions and utilise implements in production, even before industrialization is supposed to have started. In manufacturing, large-scale, centrally co-ordinated production too was on the increase, whether it was in the form of putting-out or manufactories. It seems that even before the eighteenth century, China had chosen a different path, which of course created certain path-dependencies and lock-ins. We see no increase in the use of coal and iron, no improvement in the quality of iron utensils and implements (Xu Dixin/Wu Chengming 2000: chapter 16), a decreasing importance of sophisticated machinery and a continuation of decentralised modes of production, with peasant households continuing to be by far the dominant productive entity.

This means that the developments we traditionally associate with industrialisation were *less improbable* as a continuation of ongoing developments in Britain than they would have been in China. This, I want to emphasise, does not imply that industrialisation as it occurred was a necessary or even a logical outcome of preceding developments in Britain or would have been impossible in China. Britain simply had already been experimenting for quite some time with a type of solution that might more easily lead to an industrial revolution.

Things of course look different in case the traditional image of Britain's industrialisation has been revised, or the way in which Britain industrialised has turned out to be merely one of various possible ways, not the only and

necessary one. Actually, both of these developments happened. The image of the first industrial revolution in Britain has been revised substantially over the last decades. It apparently was less revolutionary than previously thought, both in its pace of change and rates of growth. The role of steam and steam engines was less prominent than traditional stories suggested, as was the importance of big factories and other big units of production (Floud/Johnson 2004). Moreover, it is no longer widely held that there is 'a model' for nineteenth-century industrialisation, let alone for industrialisation in general (Cameron 1985; O'Brien 1986; Verley 1997). A growing awareness has emerged of the importance of flexible and dispersed production in industrial societies (Sabel/Zeitlin 2002; Hamilton/Chang Wei-an 2003). The concept 'industrious revolution', originally meant to be quite distinct from that of 'industrial revolution', is quickly gaining popularity with various scholars now trying to blur, or in any case tone down, the distinction between the two (De Vries 2008). Much thought is given to the idea that there might be a labour-intensive form of industrialisation (see Sugihara 2002). The phenomenon is not unknown in Europe, but is especially suited for Asian conditions of high population, low wages and small entities of production. It is even supposed to have stood at the beginning of a specific East Asian path of economic development (Pomeranz 2001; Sugihara 2003).

Nevertheless, even if we take on board new perspectives on how countries might industrialise, as we *should*, and which opens many venues for interesting comparative research, one cannot deny that technology, increasingly science-based, as well as steam-power and factories, did play a substantial role in Britain's industrialisation and in the end in all nineteenth-century instances of industrialisation. Coal, steam and factories did make a fundamental difference for Britain, as Californian critics explicitly underline. The steam engine, however, was not an 'accidental' solution to a 'Malthusian' problem. Britain had a tradition of trying to harness energy in production. One must, moreover, not lose sight of the fact that in Britain: "Innovation was a broad process, pervasively embedded in many industries, even those that were essentially matters of hand technology (and) [...] present across virtually all activities that comprised the British economy at that time" (Kristine Bruland in: Floud/Johnson 2004: 186). This broad process of innovation that had already started decades before actual industrialisation

contributed substantially to total economic growth in Britain. Up until the second half of the nineteenth century fifty percent of all growth in productivity came from non-mechanised sectors of the economy. It was a precondition for its industrialisation and had *no* parallel in China. Neither do we see the kind of interaction between scholars, engineers, tinkerers, artisans and entrepreneurs and the ‘Baconian’ efforts to try and apply science in China (Cohen forthcoming; Goldstone 2008b). Anti-Eurocentric historians eager to point at similarities in science and technology between Britain or rather Western Europe on the one hand and Eastern societies, especially China, on the other hand, easily tend to exaggerate this point. Frank’s claim that there was no such thing as a Scientific Revolution, that it definitely was not European and that in any case it did not matter for industrialisation, is one of the many examples in his work of revisionism being pushed too far (Frank 1998a: 186-205).

In this context more attention might be given to what one may anachronistically call ‘social science’, or rather ‘social engineering’, i.e. all those ways in which one can try and organise things and people more efficiently. It would be interesting and relevant to know more about the macro-economic effect of institutions and institutional innovations in the early modern era. Here too one sees the application of knowledge, the effects of which are still underestimated as scholars studying industrialisation tend to focus on ‘hard science’ and ‘hard technology’.

9. Neglected differences and differing trajectories: culture, institutions and politics

In all these respects, Britain had developed a culture of innovation. Apart from Goldstone, there are not many Californians who discuss this. On the whole, they hardly discuss culture at all. Some don’t because it simply is not their field; the majority because they do not like cultural explanations, in particular when they imply there is something ‘special’ to Westerners. Frank, as usual, is quite extreme: “A derivative observation is that Europe did not pull itself up by its own economic bootstraps, and certainly not thanks to any kind of European ‘exceptionalism’ of rationality, institu-

tions, entrepreneurship, technology geniality, in a word – of race.” (Frank 1998a: 4; for a similar line of reasoning see Blaut 1993, 2000).

Why pointing at some European ‘exceptionality’ would have to imply racist thinking, fully escapes me. While in many branches of history ‘culture’ has become the all-encompassing key concept, and it has become impossible *not* to talk about it, many Californians and global historians in general tend to shy away from it. To claim with Landes “[...] that culture makes all the difference” is an obvious case of exaggerating (Landes 1998: 516). There clearly are good reasons to be careful with assuming the existence of fundamental, structural and long-lasting differences between cultures. Cultures change, as do the perceptions of their impact as can be observed in the fascinating career of the concept ‘Confucianism’, regarded by some at some moment in time as the main hindrance to China’s development, by others at other moments in time as its main support, and by yet others as simply irrelevant. (Pye 2000; Zurndorfer 2004). Yet to neglect it as many Californian critics – again not all – do, is a big mistake (Vries 2001b). It eliminates all ‘agency’ from history. As Adshead correctly points out: in Frank’s Sino-centric *ReOrient* there actually is no attention whatsoever paid to Chinese history or Chinese geography: China is nothing but a place. What the Chinese actually do and think plays no role whatsoever (Adshead 2004: 17-18). Apparently the old Marxist habit of denying any real autonomy to the ideological superstructure lingers on: “[...] technological progress [...] even more than institutional forms, is a function of world economic ‘development’ much more than it is of regional, national, local, let alone cultural specificities” (Frank 1998a: 186). One can only be surprised that so active an activist as Frank ends his career as a global historian by almost entirely ignoring agency.

This lack of attention to agency also shows in a certain reticence to talk about the importance of institutions, whereas amongst economists such discussion has become quite fashionable. Frank thinks that their importance is over-estimated. A major thesis of his book is “[...] that institutions are not so much determinant of, as they are derivative from, the economic process and its exigencies, which are only institutionally instrumentalized rather than determined” (Frank 1998a: 206). For him the dynamics of human history are driven by fundamental economic forces to which institutions respond. One wonders what “the economic process” and “its exigencies” can

refer to if not to institutions and agents. Others simply do not pay much attention to them as they happen to be interested in other things or because they think that in the field of institutions too resemblances are surprising. Roy Bin Wong does point at substantial institutional differences between Western Europe and China but thinks that macro-economically those differences did not make a real difference before industrialisation and were not “designed to promote industrialization” (Wong 1997: 151). Yet, even so, they may still have had positive effects for economic growth in general and played a part in the coming about of industrialisation (van Zanden 2008) Is it really probable that institutions like Britain’s national bank, its funded public debt, its chartered companies, its Parliament – all with no equivalent whatsoever in China – made no difference to the economy? What about its systems of law and taxation, its monetary and financial systems?

Referring to these institutions means referring to the state. When it comes to their political organisation, differences between Britain and China could hardly have been bigger, no matter whether one looks at its structure, policies or trajectory (Vries 2002; forthcoming a). Till at least the 1820s, Britain’s state was first and foremost a fiscal-military state. Taxes were much higher than in China and increased continually. Its tax system was completely different. It had a huge national debt, something unknown in China. Relatively speaking, its army and especially navy were much bigger. The navy was far bigger even in absolute terms. Those parts of government that dealt with finance and the military were much better developed. To any impartial observer it was clear that in the case of a clash, the British state would defeat the Chinese state (Arrighi 2007).

Even before industrialisation, Britain’s state had acquired much more ‘infrastructural’ power than the state in China (see for that expression Mann 1993). Central government played a large role in the economy and actively supported certain developments (Daunton 1995; Ron Harris in: Floud/Johnson 2004 chapter 8). It was fiercely mercantilist, focusing on creating a strong state and a strong economy which in contemporary thinking implied having an empire. Britain clearly did not have its colonies by accident, let alone they were ‘a windfall’. Pomeranz emphasizes their importance: “[...] the fruits of overseas exploitation were probably roughly as important to at least Britain’s economic transformation as its epochal turn to fossil fuels” (Pomeranz 2000: 23). He, however, never analyses how Britain got its

overseas possessions and, more importantly, what it took to exploit them. Frank ignores the role of the state completely (Frank 1998a). Hobson does provide an excellent analysis of the workings of Britain's fiscal-military state in his co-production with Linda Weiss (Weiss/Hobson 1995), an analysis he synthesises in his book of 2004, without however indicating what that means for his overall view on Eastern and Western civilizations.

The costs in people and resources of Empire for Britain were enormous, so enormous that various scholars claim they, at least in direct monetary terms, surpassed the benefits (O'Brien/de la Escosura/Engerman in: O'Brien/de la Escosura 1998). The direct and indirect benefits, however, were also significant. In that respect, one clearly finds new and more 'positive' interpretations of British mercantilism that, ever since attacks on it by Adam Smith, has had a bad press amongst mainstream economists and economic historians considering it as inefficient and as an obstacle to development. The line of reasoning that was already prominent in the work of Braudel and Wallerstein, namely to see it as a strategy to successfully strengthen the economy of Britain as a state and country, is now continued in publications that present much more detailed analyses (Ashworth 2003; O'Brien 1998; Ormrod 2003; Reinert 2007; Winch/O'Brien 2002). Perspectives have changed so much that in some of these publications mercantilism is almost presented as a predecessor to the policies applied in so-called 'developmental states' (Amsden 2001; Ha-Joon Chang 2002, 2003; Johnson 1995; Lindert 2004; Reinert 2007; Schwartz 2000; Wade 2004; Weiss/Hobson 1995; Woo-Cumings 1999).

The policy of China's government can best be described as 'agrarian paternalist'. Rulers wanted to govern lightly, focusing on providing security and wealth for their people. They only interfered when they thought that security and wealth, and the existing social order were endangered, for example by miners who were regarded as very unruly people or by foreign traders who might have a bad influence on their subjects. They were in no way dependent on merchants for their income. They could rely on their land taxes for income. The typical Western alliance between power and profit was absent (Antony/Leonard 2001; Dunstan 1996, 2006; Leonard/Watt 1992; Rowe 2001; Vries 2002, forthcoming a; Wong 1997, 1999). The same goes for the ongoing and fierce interstate competition that was the

motor behind economic development and imperialism in the West (Greenfeld 2001; Arrighi 2007).

The approach of the Californian critics is innovative, but as with all innovators they tend to neglect what they probably regard as old-fashioned. Classical topics like the study of modes of production and especially of culture, institutions and politics get quite short shrift in their work, which is a pity. If culture and institutions and the state indeed matters so little, one wonders why in all countries where industrialisation was on the agenda, one sees these fierce debates between ‘modernisers’ and ‘conservatives’ about cultural and institutional change. In this respect I can only fully endorse the following observation by Elvin: “Most Chinese thinkers of the key transitional decades, roughly 1890–1920, saw the West as qualitatively and challengingly different, no matter whether they were conservatives or radicals [...] it is hard to see how one could argue that, in general, they were mistaken in their virtually unanimous basic evaluation of the old social and ideological patterns as being in some regards incompatible with modernization” (Elvin 2008: 181).

10. Concluding remarks

The California School has changed the way we look at the economic history of the world, especially the pre-industrial world of Eurasia. It has rightly pointed at the enormous importance of Asia in the economy of the early modern world and at its very high level of development. It has done so in a couple of years. It is no longer possible to write a book on the rise of the West like the one David Landes wrote only ten years ago, with immense success. That alone is a major feat. One should not, however, thereby be tempted to confront it uncritically. The biggest compliment one can make colleagues in scholarship is to seriously engage with them.

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Abstracts

In this article the author presents a description, analysis and evaluation of the fundamentally new interpretation of the economic history of the early modern world that is defended by authors who have collectively become known as the California School, the most important among them being Kenneth Pomeranz, Roy Bin Wong, Andre Gunder Frank and Jack Goldstone. The author in particular analyses their claim that in the period from roughly 1400 to 1800 the most advanced economies of Eurasia formed a world of ‘surprising resemblances’ and that the Great Divergence between ‘the West’ and ‘the Rest’ only originated with industrialisation and must be interpreted as a fairly contingent and recent phenomenon, basically due to differences in the availability of resources. The author claims that ‘the Californians’ have a tendency to exaggerate the resemblances between Western Europe and East Asia and should be more specific when it comes to time,

place and the differing historical trajectories of various regions. Finally, he claims they should pay far more attention to political and military developments and to the role of culture and institutions.

In diesem Artikel präsentiert der Autor eine Beschreibung, Analyse und Bewertung der grundlegend neuen Interpretation der Wirtschaftsgeschichte der frühmodernen Welt, wie sie von Autoren wie Kenneth Pomeranz, Roy Bin Wong, Andre Gunder Frank und Jack Goldstone vertreten wird, die gemeinsam als die *California School* bekannt geworden sind. Er analysiert insbesondere ihre Behauptung, dass in der Periode von ungefähr 1400 bis 1800 die höchst entwickelten Ökonomien Eurasiens eine „Welt von erstaunlichen Ähnlichkeiten“ gebildet hätten und dass die *Great Divergence* zwischen dem „Westen“ und „der restlichen Welt“ erst mit der Industrialisierung entstand und daher als ein ziemlich zufälliges, rezentes Phänomen gesehen werden soll, das letzten Endes mit der unterschiedlichen Verfügbarkeit von Ressourcen erklärt werden kann. Der Autor argumentiert, dass die Kalifornier stark dazu neigen, die Ähnlichkeiten zwischen Westeuropa und Ostasien zu übertreiben, und dass sie hinsichtlich Zeit, Ort und Entwicklungspfaden von verschiedenen Regionen präziser sein sollten. Schließlich fordert er mehr Aufmerksamkeit für politische und militärische Entwicklungen sowie institutionelle und kulturelle Faktoren.

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