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

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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.

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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.[1] 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) has 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.[2] 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.[3]

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.[4] 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.[5] 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 10th and 11th centuries when warfare was more common and became increasingly rural in subsequent centuries, especially from the 15th 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 Massimiliamo 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.[6] 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 19th-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.[7] 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."[8] 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 th[9]e 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.[10] 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.[11] 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. Geschiedbeoefening 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."[12] *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.[13]

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 kenkyű. (A study of Qing dynasty fiscal history) Kyűko 2002 and Iwai Shigeki, Chűgoku kinsei zaiseishi no kenkyű (A Study of the Fiscal System in Late Imperial China) Kyoto 2004. Important Chinese works appearing in the past decade include Chen Feng, Qingdai caizehng 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.[14] 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. Huters, 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.[15] 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.[16] 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.[17] 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 Shobô, 1974; Shindai suiri shakaishi no kenkyű (Studies of the social history of Qing dynasty water control) Kokusho Kankôkai, 1990; Shindai no suiri to chiiki shakai (Qing water control and local society), Chűgoku shoten, 2002. A crucial contrast of water control under Jurchen and Mongol rule (12th and 13th centuries) with the 16th through 19th centuries that resembles more state interventions of the 1950s and 1960s is Bunsui to shihai: Jin Monggoru jidai Kahoku no suiri to nôgyo (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 jiexiang 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 XVIe siècle au XIXe 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 absence 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 19th 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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