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Escaping the Great Divergence

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1 What is Escaping poverty really about?

Peer Vries's new book, *Escaping Poverty. The origins of modern economic growth*, is not what its title might suggest, a frontal attack on the problem of wealth and poverty in world history. Indeed, there are already quite a number of books that already take on this grave problem. Peer's book does not directly address the origins of modern economic growth; rather, it examines a body of literature that has made a series of novel claims about those origins. His primary concern is the bundle of historical claims made in what is collectively known as the Great Divergence literature, written in large part by members of the California School.

This Great Divergence literature is not large. Even after more than fifteen years, it consists of 5 or 6 core book-length academic contributions. A sizeable penumbra of programmatic statements and responses to critics envelops this core, and all this is flanked by works by well-wishers and fellow travelers that address ancillary themes in what might be called a Great Divergence spirit. The key works are few in number and their distinctive historical claims are also few, but they are certainly dramatic. A great deal has already been written about them, not least by Peer Vries himself, so an initial question one should have about *Escaping Poverty* is just what this book proposes to add to earlier discussions.

The answer is, I believe, implicit in the generous attention the book devotes – the first substantive section of ten chapters – to a critical review of economists' current theoretical understanding of the causes of 'modern economic growth' (MEG), understood as sustained per capita economic growth on the basis of ongoing technological change and growing inputs of the factors of production. This MEG is held to have been initiated – to have been brought into a hitherto Malthusian World, or organic economy

– by the Industrial Revolution, beginning in Britain in the late eighteenth century, where after its achievements were imitated and adapted by other European countries and European settler societies. If economists have reached a consensus about 'what it takes' to enter the world of MEG, then the claims of the Great Divergence interpreters can both be better identified (how do they map against the economic 'consensus?') and evaluated.

If I have this right, then Vries regards the Great Divergence literature as a variant of the Industrial Revolution literature: Both are concerned with why Britain and Europe diverged economically from the rest of the world; both are concerned with determining why the Industrial Revolution began when and where it did. Here, I believe Vries is mistaken. Most participants in the Great Divergence literature take the Industrial Revolution, or a particular stylized and dated version of it, as a given. They acknowledge, indeed, insist on, its seminal importance, but their concern is not to better understand its 'inner workings. It is to establish claims about *before* and *after* the Industrial Revolution. By 'before', I mean the characterization of the economic performance and potential of pre-divergence societies across Eurasia. By 'after' I mean the reimagining of the future: what awaits us now that an aberrant historical interlude introduced by the Great Divergence/Industrial Revolution is fading into the distance?

Still, Vries's mistake is not a fatal one. He leads his readers through a leisurely, accessible review of economists' thinking about the relative importance to MEG of a long list of potential contributing factors. Are resource endowments a boon (staple theory) or a bane (Dutch disease)? Are high wages or low wages a spur to growth? Is specialization and foreign trade a path to development or a trap? Vries offers a well-informed review of these and many other debates from the perspective of a skeptical historian. He has neither drunk the economist's Kool-Aid, nor is he congenitally resistant to economic reasoning. At the end of this guided tour we are left with something less than a check-list of essential ingredients for economic success, but we do have a series of points of orientation that Vries then proceeds to compare with the arguments found in the Great Divergence literature. After a lengthy section identifying these arguments (Part II, Explaining the Great Divergence), he proceeds to assess them against the distilled wisdom of economics.

Is this overkill? After all, with one notable exception the characteristic Great Divergence contributions have been made by historians whose chief concern has been to make historiographical claims rather than contributions to economic theory. Those historiographical claims act as a filter of permissible economic causes of the Industrial Revolution, leaving a small

number of favored explanations. It would be a misunderstanding of the Great Divergence literature to suppose that it emerged out of a careful assessment of the many theoretical contributions found in the economic history literature.

2 Methodological moves

The overall design of *Escaping Poverty* pays the Great Divergence literature the compliment of taking the economics of its arguments more seriously than they deserve, or than many of its contributors probably intended. What this literature does seek to establish is that, whatever the specific causes, the Great Divergence has *shallow historical roots*, that, in the words of Peter Perdue, it was ‘a late, rapid, unexpected outcome of a fortuitous combination of circumstances in the late eighteenth century’.¹ It was a contingent event with far-reaching consequences.

To economists and economic historians, this immediately brings to mind the concept of path dependence, yet it is striking that this concept does not figure in any of the major Great Divergence books. The burden of path dependence is precisely to insist on the enduring influence, *under certain conditions*, of transient historical events. It was first applied to explaining enduring sub-optimality in technology standardization, but more recently, a series of influential papers have demonstrated the enduring influences of institutions and policies that themselves disappeared long ago. The Great Divergence interpretation of the Industrial Revolution could be seen as a highly aggregated, macro-level application of path divergence theory. Perhaps because Great Divergence interpreters emphasize contingency in order to argue that what was ‘accidentally’ set in motion – the economic rise of the West – can therefore easily be undone, the path dependence emphasis on ‘lock-in, or persistence in sub-optimality, appears to them to be unhelpful. Yet this is a point of convergence between the Great Divergence literature and current fashion in economics that might have been explored further. What is it; after all, that transforms a ‘mere event’ into a ‘fateful event’?²

If Vries’s critique arguably takes the economics of the Great Divergence

1 Peer Vries, *Escaping Poverty: The Origins of Economic Growth* (Vienna and Göttingen 2013) 438.

2 Recent work in economics is assessed in Nathan Nunn, ‘The Importance of History for Economic Development’, *Annual Review of Economics* 1 (2009) 65–92. For a historian’s discussion, see William Sewell, *Logics of History* (Chicago University of Chicago Press 2005).

literature too seriously, it tends to neglect the historical methodologies on which many of its arguments rest. In order to render plausible the contingent nature of the Industrial Revolution Great Divergent interpreters have developed two methodological reforms: reciprocal comparison, and institutional equivalence. The first, introduced by Wong and emphasized especially by Pomeranz, lays out ground rules for historical comparison that seek to negate the normative standing of the more advanced or better known of the entities being compared. Thus, China (or the Yangtze delta) should not be compared to Britain by identifying all the ways in which the former falls short of the standards of the latter, but by entering into a series of reciprocal explorations of equivalence, allowing the actual strengths and weaknesses of both comparators to be evaluated. Observed differences do not imply inferiority of one of the comparators *unless* they can be shown to have led directly to a distinctly inferior *outcome*. Short of this, one is left with a Scots' verdict of 'not proven'; one is left with equivalent ways of achieving the same outcome, which Pomeranz characterized as 'surprising resemblances'.

Institutional equivalence is my term for the comparative evaluations of Chinese and European institutions of all kinds (commercial, financial, fiscal, legal, organizational). Developed by Wong and Rosenthal, this is certainly the theoretically most ambitious contribution in the Great Divergence literature, and it, like reciprocal comparison, is focused on outcomes rather than inputs. They argue that the absence of Chinese institutions (inputs) to regulate markets, supply commercial courts, organize a public debt, provide mortgage instruments, and support coordinating functions and production facilities associated with urbanization are not necessarily signs of inferior economic performance. The outcomes, they argued, were broadly equivalent, for China supported large commodity markets that must have been supplied by long-distance domestic trade. Arguing from theory, Wong and Rosenthal reasoned that European and Chinese actors usually confronted a similar menu of institutional options but chose differently from this menu based on the economic logic of their situation. That is, people are assumed to be the same everywhere (culture is not decisive here), but they face certain objective facts (family structure, geography, political system) that are taken as exogenous. Rational, optimizing European actors would have chosen the same institutional menu options as the Chinese had they faced the same objective facts, and *vice versa*.

I have discussed these Great Divergence methodological moves in some detail, precisely because they are not discussed by Vries, but go far to explain how most of the factors contributing to the emergence of MEG

that he discusses could be rejected or ignored in the Great Divergence literature. The rules of comparative equivalence and the quasi-Coasian assumption that 'you always get the institutions you need' allow the Great Divergence interpreters to set aside as, at best, unhelpful much of the corpus of western social theory.

3 Escaping history?

Vries's critique finds major shortcomings in this literature's grasp of what is needed to 'escape poverty', but it is not much of an exaggeration to say that the Great Divergence interpreters were really attempting something else, to 'escape history': to approach the history of Asian societies without the heavy burden of political failure, economic backwardness and cultural inferiority hovering over every research question. Indeed, from the perspective of the discipline of history there is much to admire in the Great Divergence literature. The call for a 'reformed' comparative history has serious problems, as just noted, but it is a refreshing change from the tendency of recent cohorts of historians to dismiss large-scale and comparative history altogether, preferring to concentrate on 'microhistories of difference, diversity, locality, biography (that offer) a loving grasp of detail in search of diversity of human life worlds'.³ This literature has enlarged the field of vision of many historians and expanded historical discourse to include, once again, the social sciences.

Of course, the Great Divergence literature rejects important parts of the (Eurocentric) intellectual legacy of those disciplines. It also – certainly in the work of Marks, Pomeranz, and Parthasarathi – tries to shift its economic arguments away from markets and commercial life and toward ecological and environmental forces. They prefer to discuss quantities of output rather than prices, and sustainability rather than demand. The economic game is one played by humankind directly across the table from Mother Nature, and all Eurasian societies are assumed to be playing this game with essentially the same deck of cards. It is from this premise, an incorrect one in my view, that they can claim that the core regions of eighteenth-century Eurasia all faced a common ecological crisis and that none of them – least of all Western Europe – was equipped to overcome this crisis. All the players would have succumbed, but one got lucky. The Industrial Revolution allowed those who mastered it to cheat Mother Nature, at least for a

3 Dipesh Chakrabarty, *Provincializing Europe* (Princeton 2000) 18.

while. This is a claim that suits modern sensibilities and anxieties, but it is a claim that certainly deserves critical scrutiny.

Vries does not address the Great Divergence literature's strategic invocations of environmental history as proof of surprising resemblances, but he does tackle its assertion of another surprising resemblance: that Qing China and eighteenth century South Asia were essentially capitalist. Vries correctly notes that this claim of surprising resemblance rests on an incomplete definition of capitalism. The assertion of equivalence is based on the undisputed widespread existence of commercial life. But, he notes correctly that every serious definition of capitalism goes beyond this to require factor markets (capital and labor), specific organizations of production, and institutions for investment and accumulation. Is he being reasonable in demanding evidence for these things? The Great Divergence interpreters may well claim foul, asking that we should take their word for it that the absence of specific institutions does not imply an absence of functional equivalence via some other, unspecified, channels. But most readers would, I expect, agree with him that something important is missing.

4 Predicting the Great Divergence

Vries deploys an interesting methodological move of his own. The emphasis placed by the Great Divergence interpreters on contingent events invites consideration of a probabilistic approach to the occurrence of a British (rather than a Yangtze delta-focused) Industrial Revolution. Nick Crafts did just this a generation ago in comparing Britain and France.⁴ In an intellectual atmosphere still influenced by the New Economic History's fascination with counterfactuals, Crafts took very seriously the proposition that economic development should be regarded as a stochastic process, and that our *ex post* knowledge that the decisive innovations were, in fact, English should not color our *ex ante* assessment of their probability. He concluded that 'France has unfairly and prematurely been written off as inferior to the English.'

The spirit of this approach (deeply uncongenial to most historians) is to consider the *ex ante* probability of particular outcome in two settings (Britain-France; Britain-Yangtze delta). When one finds surprising resemblances – both economies are organic, both have widespread commodity

4 N.F.R. Crafts, 'Why Britain was First?', *Economic History Review* 30 (1977) 429-441.

markets – one might assign even odds (perhaps equally low!) for their being first. But Vries, after noting these similarities, goes on to find that every other category of comparison he considered reveals not surprising resemblances but ‘striking differences’. In agriculture, energy supply, domestic industry, urbanization, labor markets, foreign trade, law, and commercial institutions he assesses much lower odds to China than to Britain. These factors may not fully explain Britain’s industrial revolution (there may still be much room left for contingency, or luck), but, in his words, ‘the chance that Qing China might have become the world’s first industrial nation... was about nil’.⁵ Stronger still, he regards the claim of surprising similarities as, ‘to put it bluntly, quite weird’.⁶

Vries’s confident dismissal of the claims to equivalence made by the Great Divergence interpreters is based on his review of a long list of what used to be called ‘preconditions’ to take off. Many readers will agree with his assessment, of course, but not everyone. Neither he nor, for that matter, any Great Divergence interpreters pause to consider the claims of Malthusian fundamentalists. Gregory Clark represents this position.⁷ He claims that ‘England in 1800 was not much richer than in most of its history since 1200’, that it was trapped in a Malthusian vise in which fluctuations in real income depended wholly on population change rather than on technological advance. The breakout from this regime, in his telling, owes little to earlier technological developments, capital accumulations, or institutional achievements. This was a world in which no good deed went unpunished. In this sense, the similarities between Britain and China that Vries dismisses as too general and abstract – they were both commercial, organic economies – may be all that is really relevant in assessing the odds for ‘escaping poverty’. Those odds were low all-round; the Industrial Revolution was simply not predictable.

5 Escaping poverty in 1600 rather than 1800: what difference does it make?

This is not Vries’s position. He holds that modern economic growth was the result of ‘a deep, slow evolution out of centuries of particular condi-

5 Peer Vries, *Escaping poverty. The origins of modern economic growth* (Vienna and Göttingen 2013) 401.

6 *Ibidem*, 402.

7 Gregory Clark, *Farewell to Alms* (Princeton 2008).

tions unique to early modern Europe'.⁸ But this wording suggests a search for origins in the sixteenth and seventeenth centuries (rather than, say, the early middle ages) and if there is one concession that Great Divergence interpreters have made to their critics in recent years it is that they may have dated the point of divergence too late. Wong and Rosenthal, for example, write of a process of divergence dating from the seventeenth century.⁹ This may appear to be nothing more than a strategic retreat designed to accommodate some inconvenient facts amassed over the past decade by what might be called the Quantitative Great Divergence interpreters. The original claims of Pomeranz, Wong, Parthasarathi, et al., asserted a divergence in real earnings beginning around 1800 and growing rapidly thereafter. Such claims naturally invite quantitative statement and testing, but the original literature did not pursue this.

Not surprisingly, economics historians devoted to quantitative measurement have hastened to exploit their comparative advantage and fill this void, and there is now a substantial and growing literature that documents wages and prices across Eurasia, establishes standards for cross-society real income measures, and thereby seeks to specify both the timing and the size of a Great Divergence.¹⁰ The patterns revealed by these recent studies remain scantily documented, but they all, in one way or another, reveal substantial and growing gaps between Europe and Asia, and more specifically, between Northwestern Europe and the rest of Eurasia (a 'Little Divergence') dating from no later than the first half of the seventeenth century.

Vries acknowledges this new literature, but relativizes its importance, perhaps even more than the Great Divergence interpreters themselves.

8 Vries, *Escaping poverty*, 438.

9 R. Bin Wong and Jean-Laurent Rosenthal, *Before and Beyond Divergence. The Politics of Economic Change in China and Europe* (Cambridge MA. 2011) 9, 209. Kenneth Pomeranz also raises this possibility in 'Ten Years After: Responses and Reconsiderations', *Historically Speaking* 12 (2011) 24.

10 Robert C. Allen, 'The Great Divergence in European Wages and Prices from the Middle Ages to the First World War', *Explorations in Economic History* 38 (2001) 411-447; Robert C. Allen, et al. (eds.), *Living Standards in the Past. New Perspectives on Well-Being in Asia and Europe* (Oxford 2005); Stephen Broadberry and B. Gupta, 'The Early Modern Great Divergence: wages, prices and economic development in Europe and Asia, 1500-1800' *Economic History Review* 59 (2006) 2-31; Stephen Broadberry, 'Accounting for the Great Divergence', *LSE working paper* 184 (2013); Li Bozhong and Jan Luiten van Zanden, 'Before the Great Divergence? Comparing the Yangzi Delta and the Netherlands at the Beginning of the Nineteenth Century' *Journal of Economic History* 72 (2012) 956-990; Jan Luiten van Zanden, et al. 'Wages, Prices and Living Standards in China, 1738-1925: in comparison with Europe, Japan, and India', *Economic History Review* (2011) 8-38.

Per capita incomes in Northwestern Europe may have been higher than in Asia from an early date, but, these differences did not 'point at fundamentally different economies *in their potential*'.¹¹ Yet, if there is a 'bottom line' to his extended critique, it is that divergence was the product of 'a deep, slow evolution out of certain centuries of particular conditions unique to early modern Europe' – an evolution in which human capital, innovation, competition, the early emergence of a middle-class market, and the European states system – none of them contingent factors – figured prominently.

This seems to me to be a rhetorical misstep, the sort of thing that can happen in an extended critique where an argument that seems useful for an immediate task stands in contradiction to the larger architecture of the author's analysis. If we set it aside, we might then ask – as I think Vries should have asked – what changes if the Great Divergence becomes a debate about a parting of the ways circa 1600? What is 200 years in the larger scheme of Eurasian history? I believe it makes a big difference to the nature of the claims the Great Divergence interpreters can make about the larger meaning of what everyone agrees on – that there really was a divergence. It is also a difference that would necessarily diminish the force of the arguments that Vries has arrayed against them.

First and foremost, an earlier dating to the divergence removes its close association with the Industrial Revolution. Explaining the Great Divergence becomes something essentially different than explaining the Industrial Revolution. Second, much of Europe joins with Asia in becoming part of the 'problem'. Perhaps the aversion to confronting culture as a variable in the story, so evident of the Great Divergence literature, could be overcome if the focus of attention were 1600 rather than 1800. Finally, the ghost of European imperial power would cast less of a pall over such a refocused debate, so that 'ghost acres' and silver hoards could not be rolled out as *deus ex machina*.

In short, the Great Divergence interpreters would be giving up a great deal. As I stated at the outset, their historical analyses have not been primarily about the Industrial Revolution (here, Vries may have been misled by their 'strategic' embrace of an old-fashioned vision of that event), but about before and after. They argue that *before* the Industrial Revolution, China (and other Asian societies) were normal rather than backward. Indeed, Peter Perdue's review of Pomeranz's 2000 book bore the title, 'Lucky England, Normal China'. England's good luck did not reflect nega-

11 Vries, *Escaping poverty*, 44 (emphasis added JdV).

tively on China. But an earlier divergence among still organic economies may be less amenable to radically contingent explanation. With respect to *after*, the work of the Great Divergence interpreters seeks to allow people today to re-imagine the future. The world, or at least Eurasia, was once 'flat' and is now becoming 'flat' once again. This is the normal state of affairs in global history. The nineteenth and twentieth centuries, with their stark hierarchies of economic and political power, were an aberration. It rested on no secure foundations: what had been gained because of a conjuncture of transient events around 1800, can just as easily, and just as quickly, be lost. A divergence beginning at least 400 years ago, and not so easily characterized as contingent, erodes considerably the comforting message (or discomfoting, depending on your perspective) of the Great Divergence literature.

It changes an interpretation of the present and the anticipated future, but it does not change its reality. That reality is of once poor societies now rapidly growing, but that growth is not occurring as part of some reversion to type; it is the product of massive, often painful cultural and institutional changes. As Vries put it with disarmingly simplicity: 'If culture were *not* important, then why did it take so much cultural change for countries to catch up?'¹²

Peer Vries has written a book that explores the origins of modern economic growth as interpreted by economists and that critiques recent accounts of the origins of the Great Divergence as seen from a global historical perspective. He can do both because he sees them linked together in concern for a common event: the Industrial Revolution. In this essay I have argued that he is mistaken in making this close association, and have explored some of the ways the Great Divergence interpreters might better be understood. This leaves largely unattended the many insights Vries offers his readers about the origins of modern economic growth, the subtitle of his book. This I must leave for other reviewers, but I can say in conclusion that Peer Vries brings to this task a brave, skeptical mind. He has, it seems, yet to encounter the scholar to whom he feels the need to defer without first offering a moment of vigorous intellectual resistance.

¹² *Ibidem*, 398.

About the author

Jan de Vries (PhD. Yale), an economic historian of early modern Europe, is the author of *The Industrious Revolution: Consumer Demand and the Household Economy, 1650 to the Present* (Cambridge University Press 2008) and, with Ad van der Woude, *The First Modern Economy. Success, Failure, and Perseverance of the Dutch Economy from 1500 to 1815* (Cambridge University Press 1997). He has addressed themes in global history and the history of globalization in a series of articles, including: 'The Limits of Globalization in the Early Modern World', *Economic History Review* 2010; 'The Great Divergence after Ten Years: Justly celebrated yet hard to believe', *Historically Speaking*, 2011; 'Reflections on Doing Global History', in: Maxine Berg, ed., *Writing the History of the Global* (Oxford Oxford University Press 2013); and 'Understanding Eurasian Trade in the Era of the Trading Companies', in: Maxine Berg, ed., *Goods from the East: Trading Asia, 1600-1800* (Palgrave Press, forthcoming). He is Chancellor's Professor of History and Economics, emeritus, at the University of California at Berkeley. See: <http://history.berkeley.edu/people/jan-de-vries>.
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