

England as the source of the Great Divergence

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1 Historiography

When Oxford University Press published my *Growth Recurring: economic change in world history* in 1988, the *Journal of Economic History* did not review it.¹ I was told no suitable reviewer could be found. In those days there were scholars who deprecated work on such broad themes, saying they should be left aside until questions relating to conventional short period topics had been settled. The twenty-five years since 1988 have nevertheless produced a torrent of relevant publications and, ironically, some of the absolutist sceptics have become prominent in the global history movement. While remaining centred on the industrialisation of England – which cannot quite be wished away – the Great Divergence debate has sealed a shift of interest to contrasts between West and East. It has drawn into economic history, among others, people from mainstream economics and from general history; anti-capitalist and anti-Western ideologues in the social sciences; and specialist students of non-Western areas. Some of the work is derivative, which is perhaps understandable: scholars are attracted to subjects that others have pioneered and where there is active debate. This is especially so when they feel their techniques offer fresh perspectives or their favourite region has been slighted.

Writers about the majority of less-developed countries had some reason to feel excluded in that the focus was fast narrowing, if that is the word, to the relative experience of Qing China and the early modern West. It is commonly assumed that, however long the experiment might be let run, nowhere else, not even Song China or Tokugawa Japan, could have indus-

¹ E. L. Jones, *Growth Recurring: economic change in world history* (Oxford 1988). I am grateful for comments on the present article to John Anderson, Joseph Bryant and Charles Foster.

trialised independently. In his large and impressive book, Peer Vries presents reasons for selecting China among all non-Western societies to oppose to Europe, which in his case is proper because he is explicitly interpreting an existing literature.² It is hard to avoid asking, nevertheless, whether the history of China would have become quite so fashionable had its modern rise not been spectacular.

Members of the non-Western group have often entered the debate late and in consequence denounce opinions on the supposed merits of Western economies that are no longer held, if they ever were, by the mainstream. Such contributions are out of phase: Julian Simon explained why academics in countries poorer than the United States are disadvantaged, for example through not having access to all the most recent books and journals.³ Simon was of course writing before the Internet became ubiquitous, although despite that change of pace the gush of new printed work remains so fast that no-one, it seems, can quite keep up. Surveys of the history and historiography of (British or English) industrialisation continue to appear in print form, nowadays sometimes nested in studies of very different, so far mostly Asian, societies. The surveys can give the appearance of being up to date by adopting the slant of some recent dominant figure or contemporary school of thought. It is clearly difficult to incorporate a broad appraisal of a large literature with a sectional, even sectorial, approach that purports to explain the causes and geographical location of industrialisation. Two of the most recent analyses written by a single hand and reflecting a consistent vision – as opposed to textbooks that take much more as given – are those by Vries and the even newer article by John E. Wills, Jr.⁴ The latter author incorporates studies of upheavals well outside the ordinary purview of economic history and, enlightening as it is, will not be discussed in this consideration of the former's more focused contribution.

An exceptional scholarly range, not to mention intellectual confidence, is needed to appraise an impassioned literature that sometimes accuses anyone reporting internally-generated European growth of being triumphalist or worse. Vries makes a good job of handling a debate that he refers to as tiring (presumably meaning tiresome). By now there must be thousands of contributions and no-one could read them all. How to decide what to

2 Peer Vries, *Escaping Poverty: the origins of modern economic growth* (Vienna and Göttingen 2013).

3 Julian L. Simon, *The great breakthrough and its cause* (Ann Arbor 2000) 177-178.

4 John E. Wills Jr., 'What's New? Studies of Revolution and Divergence, 1770-1840', *Journal of World History* 25 (1) (2014) 127-186.

include? He emphasises work carried out by authors treating the topic directly, publishing in prominent places, and referring to one another. Despite the need to make innumerable distinctions, his evaluations are usually clear. His tone is temperate but he does not mince words when dismissing the more extreme writers, especially anyone who claims development alternated between Europe and Asia, an idea calculated to minimise achievement in the former. He dismisses writers of the 'similarity school' who accord no 'privilege' to anything Western and cope with the inconvenient fact of Western primacy by deeming it on the one hand to have appeared suddenly and late and on the other to have resulted from the chance acquisition of coal and colonies. The latter opinion seems impossible to eradicate from fashionable consciousness.

For all the admiration I feel about Vries's handling of the intricacies of scholarship, cautionary words on method are in order. They relate to much of the literature and not solely to his approach, but because he is the author in view this is a suitable place to raise the issues. First, then, despite his scepticism about much that economists have written, he has been markedly influenced by their reductionist manner. This involves the *seriatim* treatment of explanatory factors in an attempt to drill down to a final cause that would explain the rise of the West in terms of some single variable. Authors who have not themselves done detailed research into the complexities of the past tend to argue in this way. Unfashionably 'historical' though the approach has become, there are still authors who do present sequential models working from early and general features towards later and more specific ones, and who are perhaps worth more attention.

My second impression is that the chronology of the strictures is not always as precise as it should be. Mixing chronologies – the 'vintage problem' – is all too frequent in the literature, and here again Vries must be our starting point. A habit exists of running together work of different vintages, accompanied by ignoring publications by individual scholars beyond one piece singled out for criticism. This holds authors to account for opinions they may since have amended, as though no-one is permitted more than his or her first throw of the dice. On occasion Vries treats authors who are merely reporting elements in the literature as if they are asserting the point themselves. He is therefore not wholly immune from commonplace failings in the Great Divergence debate.

Thirdly, it seems a pity that he limits himself to contrasts between Western Europe and Qing China. Two-way comparisons may prove inconclusive since each is liable to reflect back only the other. The danger is of

treating their differences as if they were of universal rather than special significance. This is a variant of the small- n problem. Lieberman argues that exercises like Weber's attempt to account for the divergent histories of only two regions, Europe and China, are foredoomed unless extraordinarily strict conditions are met – that with only one experiment and just two actors we can never be certain what caused the divergence.⁵ It is a contentious matter but the current prominence in the literature of a Europe-China framework draws Vries in the same binary direction. He urges that societies in the Islamic world and elsewhere were non-starters. He notes that the California School, with which he is not enamoured, even ignores Japan and speculates that this is because the 'Californians' see it had no coal or colonies and therefore cannot be assimilated to their ruling notion that only these can have yielded early growth. Yet Japan was a late nineteenth-century industrial success.

Vries does describe explanations that rely on single factors but for his own part rejects the genre. He exposes the 'shockingly inconclusive' dogmatism of economists – how 'every position imaginable' is taken and history is envisaged as fate, with no room for agency. Much economics offers only inducement models which tacitly assume that, given the right set of prices, the response of rational, maximising individuals must follow. This evades the deeper sources of varying responses. I am struck myself by the propensity of economists, whose remit is the state of the market, to search instead for the sources of growth in the state of the art. Hence the physical means of production – cotton machinery, steam and coal – are where the spring is so often made to bubble up. Fortunately technological determinism of this type does not appeal to him. Admittedly economists are nowadays flirting with far broader topics, such as geography, culture and above all institutions. None of these has however added much by way of certainty and it is possible, for instance, to dismiss neo-institutionalists as serving up 'merely more of the same Samuelsonian weak tea', as Deirdre McCloskey puts it.⁶ Vries, at any rate, while not totally dismissing the influence of these protean factors, finds them under-specified.

5 Stanley Lieberman, 'Small n 's and big conclusions. An examination of the reasoning in comparative studies based on a small number of cases', *Social Forces* 70 (2) (1991) 307-320; and Lieberman, 'More on the uneasy case for using Mill-type methods in small- n comparative studies', *Social Forces* 72 (4) (1994) 1225-1237.

6 Deirdre N. McCloskey, 'Austrians, Anti-Samuelson, and the Rhetoric of Quantification: A Comment on Daniel Klein's *Knowledge and coordination*', *Studies in Emergent Order* 7 (2014) 19.

2 Revaluing Europe and China

Vries does not accept the downplaying of early modern European advance based on the preconception that this was a laggard region able to forge ahead only about 1800. He admits (and in influential quarters nowadays it will be seen as an admission) to being Eurocentric. Why not – wasn't it Joel Mokyr who said that the opposite of Eurocentric is eccentric? The other side of the Great Divergence debate has involved revaluing the economic performance of Qing China (and occasionally other Asian societies, though in far lesser degree). One benefit has been to spur further research into Chinese economic history. As a result depictions of China as successfully authoritarian and economically stagnant over long periods are no longer acceptable. But for all the activity now revealed, Vries does not see China under the Qing as promisingly inventive. If one wishes to find a Chinese dynasty when growth and (up to a point) industrialisation were live possibilities, the place to look is the Song. He believes progress during the Song tailed off quite soon but notes that the period is neglected. It is odd that the California School makes so very little of the example, which surely demonstrates Asian creativity, not to mention primacy. It is odder still that the case is almost completely ignored in economics, given that the subject's concepts are paraded as timeless verities; economists should not take fright at early episodes.

If the Great Divergence debate has on the one hand inspired favourable evaluations of Chinese economic history, on the other it has meant waving away independent achievements in Britain and North-Western Europe. These need to be rescued from assertions that they came late in time and depended on coal, colonies and disagreeable forms of capitalism. Micro-history paints instead a picture of Europe in ferment. The continent can certainly be shown to have adopted Asian notions like algebra and 'Islamic' crops, though the extent of the agricultural borrowing is now under challenge.⁷ In any case diffusionism cuts both ways. Casually citing the emperor Qianlong as telling Lord Macartney that China needed no crude European wares is not evidence that the spread of Eurasian technology was a one-way street. And while ideas did travel across Eurasia and around Eur-

7 Michael Dekker, 'Plants and Progress: Rethinking the Islamic Agricultural Revolution', *Journal of World History* 20 (2) (2009) 187-206

ope in the distant past, independent discoveries are too often scouted. England's supposed borrowings are a case in point: they are exaggerated relative to home-grown developments.⁸ In the seventeenth century Celia Fiennes noted the case for 'curing the evil itch of over-valuing fforeign parts.' Much of the international and even more the inter-continental diffusion of ideas lay in the background of industrialisation proper. Its value is in showing how inventive and innovative preindustrial European societies were.

3 Coal and colonies

To understand the Great Divergence what is needed is to show that in late pre-industrial centuries there was development conducive to actual growth in the West, notably England. On the reckoning of the California School, England was treading water, reaching ecological limits, running up against resource constraints. In the eyes of some like-minded scholars it was by 1800 close to sinking, to be floated only by the discovery of coal and the full agricultural adoption of clover.⁹ On this reading, the European economy was drowning but surfaced overnight (historically speaking) into a regime of puddled iron, machine-spun cotton, large factories, steam engines and coal mining. All this, purportedly, happened only about 1800. In reality it grew out of prolonged experimentation. This took place in certain English regions much more than in others, a fact that offers under-remarked clues to the growth process. Because he is following the 'core' literature, Vries does not probe far into regional processes, though – well-read scholar that he is – he is aware of them.

The argument here is that understanding growth and industrialisation will require more sensitive regional investigation than it has received. It is one direction in which his survey might, indeed should, be extended. The counterpoint would be to show that Qing China or some substantial part of it had reached a level of productivity equivalent to eighteenth-century Europe, was technologically promising, but was blocked only by a lack of cheap coal coupled with a self-denying ordinance over colonisation. Re-

8 Eric L. Jones, *Locating the Industrial Revolution: inducement and response* (Singapore 2010) 31-32.

9 This thesis is generalised in T. Kjaergaard, 'Denmark's Ecological Crisis in the Eighteenth-century', *Economia e Energia Secc. XIII-XVIII* (Florence 2003) 905-915, and disputed in Eric L. Jones, *Revealed biodiversity: an economic history of the human impact* (Singapore 2014) 4-8.

search has muddied the waters by elevating Qing Chinese achievements and income levels but has not seriously suggested there was a close approach to industrialisation. The intellectual evolution of Qing China did not favour solving technological problems. Karel Davids has recently argued that different religious contexts separated Europe and China after 1500: knowledge circulated less freely in the latter, which had no patent system, no prizes for inventors and no strong intellectual interest in technology.¹⁰ Elsewhere the issue has been side-stepped by suggesting that not merely China but East Asia as a whole adopted desirably labour intensive rather than capital intensive productive systems.

The disputants continue to box, even shadow-box, with well-worn categories of information. When we come to the developmental heart of Europe, England, they mostly treat it as a unity. They cannot of course sustain this position, so they dwell on London and the North, accounting for the industrialisation of the latter because it was where the coal was. Repetition makes this sound acceptable to modern ears: what I tell you three times is true. This plays into the hands of the California School because it places industrialisation late in time, with coal-generated steam forcing open the bottleneck of limited energy inherent in an organic economy.

As mentioned above, Vries does not espouse simple technological solutions, observing that coal did not launch the Industrial Revolution, at least not in cotton manufacturing. He does favour the idea of a compressed period of industrialisation, but not one that rose abruptly, *ex nihilo*, amidst a sea of backwardness. Moreover, he notes that the emphasis on Britain's luck in sitting on coal seams is a little beside the point. It is chronologically lacking, offering no explanation of why things happened when they did. Coal mining had to be developed by entrepreneurship and hard work and in any case its development was not at first aimed at generating power for industry. Rather, coal was supplied, once transport permitted, to meet the demand for domestic heating: the bells were rung 'for rejoicing at ye coals coming to Oxford' on the arrival of the first shipment there in 1790.¹¹ Oxford was not, nor (with all deference to Lord Nuffield) was it destined to become, a major manufacturing centre. What it was, was cold and damp.

A better way of looking at regional processes than attributing every-

10 Karel Davids, *Religion, technology, and the great and little divergences: China and Europe compared, c. 700-1800* (Leiden 2013).

11 Mary Jessup, *A History of Oxfordshire* (Chichester 1975) 95.

thing to northern coal is to recognise that political and other changes, above all those affecting both the overland and coastal transport sectors, were fostering market expansion well before any conceivable 'take-off'. An increase in agricultural output was involved in this, with a concomitant shipping of produce. Wrigley has noted that agricultural historians have concentrated on food production to the exclusion of agriculture's supply of industrial raw materials and energy for use outside the farm sector.¹² He calculates that the land's supply of products other than food rose six-fold between 1300 and 1800, mostly after 1600, which was between two and three times greater than the growth of population. There could be no more eloquent testimony to a late pre-industrial vigour that intensified market exchange and owed nothing to coal or colonies.

4 English regional diversification

As for manufactures, during Tudor and Stuart times most people lived in small communities where goods were made for local consumption. Towns were too small for much specialisation to arise when the radius of their market area was seldom as much as fifteen miles. But market expansion meant competition with nearby settlements; trends over time are consistent with competition slowly eliminating weaker businesses in many small towns. Larger settlements won out, or grew bigger through slow-acting advantages which are hard to trace when business records are so scanty.

The vast bulk of manufacturing firms were tiny family businesses where acquiescence on the part of members could usually be assumed and decisions were rarely written down. The records of disputes over wills and similar cases that came to court may be the most likely sources but seem not to have been examined on a sufficient scale. Nevertheless we find products increasingly associated with individual settlements, so that place-names become brand names in indirect but plausible testimony to growing specialisation. No-one who reads, say, Defoe's classic *Tour* can doubt that the English economy was regionally specialised; his book was written very early in the eighteenth century, with much development of transport and communications still to come.

Assembling information about this process is painfully slow, requiring detailed knowledge of English geography and delving into the welter of

12 E. A. Wrigley, 'The transition to an advanced organic economy: half a millennium of English agriculture', *Economic History Review* LIX (3) (2006) 435-480.

fugitive sources yet coming up with generalisations. It is not surprising that attempts at this have been few or that Vries found little on the topic integrated into national economic history. Local histories themselves cite multitudes of causes which often seem to be little more than correlates of growth. Advantages of resource endowment are put forward without due consideration of whether their use was instead induced by the expansion of the trades for which they were inputs. Misleadingly enough, location and resources are more conspicuous than commercial competition among sets of tiny businesses.

There is space here to consider only one industry. Let us take leather, the tanning and use of which were immensely significant in a horse-drawn economy. This is a neglected industry despite having been the fourth largest by value as late as 1851, ranking only after cotton, wool and iron, and employing 350,000 hands.¹³ It has been suggested that footwear production concentrated in Northampton because this had the advantage over other towns a similar distance from London through being situated near major droving routes.¹⁴ Welsh drovers brought cattle – ‘leather on the hoof’ – to be fed in the East Midlands during spring and early summer, before being driven on to Smithfield market in London. Consider alternatively the rise of tanning in Wantage, Berkshire, which possessed one of the largest, if not the largest, tannery businesses in the kingdom in 1800.¹⁵ That location has been attributed to the tan bark available from oak trees in the adjacent Vale of White Horse. Yet Wantage was adjacent to the traffic in cattle driven along the Berkshire Ridgeway by Welsh drovers and is even closer to London than is Northampton. And there were other towns that shared the locational advantages of both Northampton and Wantage, meaning that these features cannot fully account for the rise of their leather trades. The Wantage tanneries collapsed through bankruptcies very early in the nineteenth century and were not re-established; they could not compete with expanding enterprises in larger places.

13 J. Yeats, *The natural history of the raw materials of commerce* (London second edition 1871) 298.

14 P. R. Mounfield, ‘Moving the industrial flywheel: the origins of Northampton’s footwear manufacturing industry’, *Mounfield Publications website*, accessed 18 March 2014.

15 John Parrott, *The largest tannery in the kingdom: three centuries of tanning in Wantage* (East Challow 2009).



Illustration 1. Business premises founded in 1839 in Banbury, Oxfordshire. It symbolises the commercialising of English agriculture brought about by the improvement of communications. (photo by the author, 23rd August, 2014).

Local histories do not always see developments in transport and communications as the vector of competition among settlements; they do not probe into the consequences for town businesses, nor speculate whether the process was directional and where it might have been heading.¹⁶ Investigating the transport sector is not helped by the dominant narrative of turnpikes and canals, which relies on disparaging earlier improvements and concentrates on absolute gains without recognising the significance of proportionate gains in successive periods. God may or may not be on the side of the big battalions but economic historians are. Their assumption is commonly that pre-industrial change must have been self-limiting.

That the rate of growth in traditional sectors other than agriculture would have doubled per capita national income between 1780 and 1860 is dismissed as negligible compared with the massive growth of non-traditional sectors that made up the classic Industrial Revolution. I wish someone would double my income! No-one doubts the scale of industrial change or the novelty of its productive forms and it is obfuscating to say that granting preceding achievements their due is *ipso facto* an attempt to

¹⁶ But see John Chartres (ed.), *Agricultural marketing and trade 1500-1750: chapters from the Agrarian History of England and Wales IV* (Cambridge 1990).

downgrade the Industrial Revolution. What is at stake is whether preceding changes were leading nowhere, so that to all intents and purposes industrialisation did spring out of thin air. That impression is conveyed by many scholars, not just those who think it was a triumphal procession carried on the backs of colonials and coal miners.

Constructing an account of late pre-industrial change is not made easier by the limited size and scantily recorded nature of improvements in business organisation and management, something relegated to the margins in favour of manufacturing production, in fact to 'the rubbish heap of history'. Douglas Farnie wrote a defence of merchants, claiming they are 'portrayed as middlemen performing no useful function'.¹⁷ Farnie averred that 'business fulfilled its primary function not through the manufacture of products but through their sale', adding that 'manufacture was essentially a simple process (...) in contrast commerce remained the most difficult and demanding sector (...) and rarely benefited from technical innovation (...)'. These strong opinions are not really telling us that merchant endeavour in finding fresh overseas markets – although *sine qua non* – was what led to revolution in the cotton industry. They point nevertheless to the one-sidedness of viewing all important change as technological. Merchants were creating big businesses when manufacturing was still done by hand.

The aggregate effect and direction of change is masked by the dominant tale of growth on the northern coalfields. This view is not inconsistent with Vries's interpretation, for he is well aware of the economic energy not only in late pre-industrial England but in other parts of north-western Europe, notably the Netherlands. Happily there is no room in his account for a static Western economy on the brink of an ecological challenge from which only coal and colonies could deliver it.

Market growth based on the linking of small settlements by incrementally improving transport admittedly takes us only so far. It is not evidence that the powered industrial advances of classic industrialisation were guaranteed to emerge from it. Nevertheless this was a likely outcome given the accumulation of mechanical tinkering in England's workshops. The sheer density of competitive activity made breakthrough technologies a live possibility, though higher-level cogitation and experimentation were needed too. Economics has little useful to say about the relatively free religious and political culture of the areas where the eventual breakthroughs were to come. Nor is much comment made about the wide distribution of small,

¹⁷ Douglas A. Farnie and David J. Jeremy (eds.), *The fibre that changed the world: the cotton industry in international perspective, 1600-1990s* (Oxford 2004) 27-28.

investible capital sums among the inhabitants of such districts. The assertions of economists about the forces at work are contradictory, as Vries is at pains to show their views often are. He might as well have quoted the late T. S. Ashton, appropriately a Lancastrian, on economic theory – the fruit of that tree, Ashton said, is too often the apple of discord.

Economic history can, however, chart the sequences, one of the most influential being the emergence of a community of watchmakers in south Lancashire, small independent producers who solved a number of horological problems technically more challenging than the first tasks in making cotton-spinning machinery.¹⁸ They included men who were hired away by the cotton-spinners to work on their machinery. Nothing was inevitable about the creeping process of growth, nothing that absolutely ensured it would give rise to a cascade of new technologies. But its occurrence undermines those who wish to deny that anything was progressing in Europe and that European growth resulted from luck and greed. Such a version does not square with the slow welling of technological change within the religious and sociological culture of the small independent craftsmen of south Lancashire. That this county is coal-bearing was obviously relevant to development but in the larger scheme of things the presence of coal was not the salient consideration. If coal needed to be brought in, the extent of the shipping of other raw materials into and around eighteenth-century England shows that it could have been transported at acceptable prices.

An argument that a concentration of artisans was sufficient to account for industrial invention would fail to persuade, even when this took place on the coal fields. Deterministic solutions to the riddle are not worth seeking: so much else was in the air or rather in the regional culture. Co-operation as well as competition was involved, with ideas circulating among tight-knit communities. In the framework knitting part of Nottinghamshire specialised ancillary trades had developed by 1739 – frame-smiths, setters-up, sinker makers, stocking-needle makers, joiners and turners – and were staffed by as many hands as there were actual stockingers. Among this galaxy and those around other manufacturing plants there was enough low-grade experimentation to generate novel ways of handling both processes and products. The plethora of ingenious devices available in late pre-industrial England, and in Western Europe as a whole, shows how vital an economy this was.

Devising larger machines and hitching steam power to them was a

¹⁸ Charles F. Foster and Eric L. Jones, *The fabric of society and how it creates wealth: wealth distribution and wealth creation in Europe 1000-1800* (Northwich 2013).

crucial step, as (to insist) no-one denies. Yet our intense gaze on major technologies in key industries obscures the underlying ferment. In some directions this included failures. They were all part of the learning process. A great deal of small-scale and scantily recorded invention and innovation took place; it was not all Eureka moments. The tale of abortive efforts also helps to indicate how interconnected was early industrial experimentation, something demonstrated long since by the chapter on 'Early Spinning Machinery' in Wadsworth and Mann, *The Cotton Trade and Industrial Lancashire*.¹⁹ General purpose technologies emerged that spread rising productivity among unrelated firms and trades. A network that was almost ecological in its intricacy had emerged in Britain – the matrix of industrialisation without which, it is reasonable to believe, change would not have been so rapid nor spread so widely in modern and traditional sectors alike. A similar process occurred, albeit on a smaller scale, in several parts of north-western Europe, where concentrations of industry existed that were neither wholly isolated from one another nor from what was taking place in England. Had similar circumstances – or acceptable substitutes – not been present in Europe and the early United States in the late preindustrial period, industrialisation could not have spread so far or so fast.

Revisiting the detailed spread of industry across the Western world would be instructive; the fact that conditions were both similar and different offers a purchase on the problem. The history of all societies in the world is interesting and some are especially worthy of study in the hope of identifying how they got as far as they did and why they did not get further. But the incontestable fact is that Britain and North-Western Europe were the first to attain and sustain high rates of growth and industrialisation. Compared with unlocking the Western puzzle other topics, fascinating though they are in themselves, can be of only second-order importance. The Chinese achievement was impressive but altogether differently organised. As Winston Churchill said, 'however beautiful the strategy, you should occasionally look at the results.' In short, it may be best to tackle the Little Divergence and not concern ourselves so much with the Great Divergence between Europe and the very different circumstances of China. Vries has done a sterling job in analysing the Great Divergence literature but perhaps we should now move on.

19 A. P. Wadsworth and J. De Lacy Mann, *The cotton trade and industrial Lancashire* (New York 1968).

About the author

Eric L. Jones is Emeritus Professor, La Trobe University, and Professorial Fellow, Melbourne Business School, University of Melbourne. Well before the debate about divergence began, he wrote *The European Miracle: environments, economies and geopolitics in the history of Europe and Asia* (1981, third edition 2003), a work expressly stated as universalistic though often misrepresented. It was an early foray into large-scale comparative economic history, followed by *Growth Recurring: economic change in world history* (1988) and other books and essays. His recent work deals with pre-industrial development in England, as spurred by expanding markets and diverging regional change. The process is depicted as aleatory rather than determinist yet as cumulative in practice, with the odds almost inadvertently favoring eventual coal-based industrialization: see *Locating the Industrial Revolution: inducement and response* (2010), *The Fabric of Society* (2013) [with Charles Foster], and various articles.