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Eric Jones
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The European Miracle

THE EUROPEAN MIRACLE

Environments, economies and geopolitics in the history of Europe and Asia

THIRD EDITION

ERIC JONES

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FOR JOHN HUGHES, REMEMBERING
OXFORD, PURDUE AND NORTHWESTERN

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Preface to the third edition

A retired farmer from just outside Oxford used patiently to instruct me in how to appraise cattle. I have long forgotten most of his teaching but I have never forgotten one of his anecdotes. An old Irish dealer said to him at Rugby market in 1904, 'Ah, my boy, if you spot a crowd coming down the road, go the other way and see if they've dropped anything.' It was in that spirit that I wrote *The European Miracle*.

The first plan of the book was set down in 1972. I have it still. It is not very different from the published text; very little long-term, comparative history was produced in the interim before I sat down to write in 1979. Earlier, when I was an undergraduate in the 1950s, one could just sense the fading of an empirical economic history written by people who sought the significance of the world of their own not-so-distant ancestors. 'People in whom tradition abides are less the product of their own time than of much longer earlier periods', as Don Haworth commented in his *Bright Morning*, of which the subtitle is *Images of a Lancashire Boyhood*. They understood the life of the past but longed to know what had made it tick. They approached the subject intrigued by the genesis of the factory system, awed by industrialization's feats of engineering, appalled by lives spent cramped in back-to-backs, and anxious to grasp how Britain's industrial supremacy seemed already to have slipped away. They wrote about what they found in old tracts, conned in the Blue Books, or traced on the ground. It was not all steam engines and cotton mills. They were more than train-spotters. They knew about economic institutions too but interest in these things faded. The last time I heard mention of the guilds was as an undergraduate, until I mentioned them myself in this book.

In the 1960s economic history fell increasingly under the spell of a technocratic cast of mind. It was in 1961 that I sat an arm's length away from Walt Rostow as he confidently introduced a normally sceptical Oxford audience to his concept of the 'take-off'. The concept took off all right, even if industrialization had in reality never grown so abruptly. Rostow was historically better informed than his (often political) adversaries have given him credit for, but his thesis was starkly mechanical. Lesser men turned it into a shibboleth. And then, quite quickly, even this brand of 'political economy' was drowned, at least in the United States, by a flood of cliometrics or new economic history drawing ever more exclusively on formal theory and published statistics.

Like Rostow, the pioneers of the new economic history were subtler in their approach than those who followed them. John Hughes, for example, to whom this book is dedicated, paused to write economic history through biography and even took a sabbatical to study the eighteenth-century English basis of American law. Others, however, rushed down a narrow chute into a world not so much informed by economic theory – in principle a good rather than a bad thing – as one wholly constructed on the assumptions of a small cadre of economists. A large share of economic experience was thrust outside the citadel. Paying any attention to it was dismissed as 'fishing expeditions'. Thus political governance, many of the institutions of the economy, every period before predigested statistics became available (say, before about 1700), and virtually all non-Western countries received little or no systematic treatment. This phase saw economic historians drive away their broader historical and lay audiences. Unsurprisingly, the subject had few friends when tertiary education was later assailed by financial cuts.

None of these phases, not even the earliest, paid much attention to parts of the world away from the major economies. The only non-Western exception was Meiji Japan, which was tacked on as an afterthought. During the 1960s the remainder of the world was left to development economics, a puzzlingly unhistorical genre, ashamed to admit the significance of the past for fear of upsetting the funding agencies. Economic history itself remained sternly nationalistic, as if the nation-state had been ordained to be the sole functioning economic unit. Moreover, the natural world did not get a look in; at least it received no coherent treatment. Even agricultural history, at which my own first researches were aimed, was completely unecological. The specialty was devoted to studies of landownership, with an occasional nod towards macro-economic implications. The environment was ignored.

It was against this background, and in the Irish drover's spirit, that I conceived *The European Miracle*. My method was to read the literature with perhaps five purposes in mind.

First, to distil generalizations about the experience of Europe as a whole. Even now, histories of the continent are seldom more than *seriatim* national histories. So-called European Studies typically remains fractured along those lines, as if the nation-state escaped being brought into being by historical processes and remains set apart, timeless and beyond analysis.

Secondly, to ask what part the natural environment may have played in European economic experience. My personal interest in natural history had a bearing on the choice of this line of investigation. Once again, what was involved was thinking about the continent as a whole, its similarities and differences. In retrospect this has involved dealing with factitious complaints about environmental determinism. But initially I was lucky. The environment, hitherto dismissed by economic historians as a pertinent category of knowledge, came into its own about the time *The European Miracle* was published, for reasons connected with the politics of the 1980s.

Thirdly, to investigate to what extent political arrangements in the form of nation-states, and more importantly functional relationships among those states, influenced Europe's economic growth. What difference did it make that no empire was ever successfully reconstituted in Europe after the fall of Rome?

Fourthly, to avoid prejudging the starting date and to be willing, even eager, to examine pre-industrial periods. I was not prepared to assume, a la Rostow, that economic life had jumped once and only once into modernity, with the industrial world owing nothing to the past.

Finally, and contrary to the nationalism or isolationism then prevalent in the economic history profession, one aim was to seek some standard of comparison against which to assess what had happened in Europe. My interest in ecological history and the history of science undoubtedly prompted this approach. Scientists had long since learned not to conduct experiments without 'controls' against which the results may be judged. This procedure led quickly into comparative history, with the other large societies, continents or civilizations falling into place as the obvious comparisons or contrasts. Of course there are conceptual problems related to assuming, as it were, that there were fruitful and less fruitful trajectories that societies might follow. But many of the objections are red herrings. Sometimes they deny any possibility of comparison, apparently out of resentment at what is called 'privileging' Europe's efforts. They thus start by prejudging whether any European circumstance or innovation possessed merit. They end by defending autocracy and poverty which were far more unyielding than any in Europe's past.

All five approaches could be found somewhere in the literature of the 1960s and 1970s if one looked hard enough, but synthesizing them was rather unusual and clearly struck a chord. Few of the lines of criticism have seriously undermined the edifice of *The European Miracle*, as I hope the Afterword in this edition makes clear. The 'edifice' perhaps differs from many works on 'big picture' issues in two ways. One is that no attempt is made to flesh out a full narrative but rather to distil the essence, trying to add touches of historical plausibility by a selection of allusions. The other is that no single explanatory key is paraded. I found none in my researches and do not believe that such a complicated set of processes can be accounted for so neatly. Nor have the natural scientists who turn to this type of study persuaded me that the benefits of painting at the species or global level (I am far from denying the excitement of making the attempt) are worth the loss of detail or abolition of the fortuitousness in the historical record. The text that follows, then, is a dense one. The achievement of the first continent to attain sustained economic growth can realistically be portrayed in no other way.

Preface and acknowledgements

Oscar Wilde expected to be met at the Pearly Gates by St Peter bearing an armful of sumptuously bound volumes and declaring, 'Mr Wilde, these are your unwritten works.' I have often felt that *The European Miracle* would turn out to be among St Peter's armful for me. As a narrative task the subject calls for unbounded reading; as an analytical challenge it inspires awe. Yet as a research topic there is a compensation that has been useful to me on my travels, that some pertinent material may be found anywhere, even in the mobile vans of rural library services. I am indeed indebted to assistants in many sorts of library in three continents, and especially, since all the material is never in any one place, to those who engaged in slow-motion wrestling with inter-library loan schemes for me.

Beyond portability, it seems to me important for the health of economic history that more of its practitioners should try to build houses with the bewildering variety of bricks baked in our individual researches, at the risk of dropping a few bricks on specialist toes. Recent works by a number of authors have indicated some renewed interest in universal and *very* long-term history (the term was first used by Hartwell (1969)). Not many of these writers have been professional economic historians, and I think that we too should try to reach the wider audience, if we believe that taken all together our work has something to say. Here is my own interpretation of *very* long-term development in Europe, paying special attention to the period from about A.D. 1400 to 1800 and to comparisons with Asia. The system which appeared then in Europe is obviously important for the history of that continent and as the progenitor of the United States, Canada, Australia, New Zealand, Latin America and South Africa as well as of much that has happened in the rest of the world.

I have not looked for a neat determinist or historicist model. Criticisms of models of those kinds seem too strong (e.g. Bauer 1971). Neither have I necessarily pinned economic outcomes to pure choices of economic behaviour. The key influences in bringing the European economic system into being seem to have been political decisions made within a favourable natural environment, this environment imparting a direction but not precise marching orders. The choice of economic actions has to be examined within that evolving frame, at least for the periods under discussion. In brief, Europe appears to have had environmental advantages and while these did not guarantee particular responses, or any response, their absence in Asia may have made development harder there. I do not however favour the view that it would have been out of the question for parts of Asia to have industrialised spontaneously, even before Europe. It was just more difficult and a case may be made that it was becoming increasingly difficult in mainland Asia even in precolonial times. Asians lived under much worse and economically distorting oppressions. Europe alone managed the politically remarkable feat of curtailing arbitrary power, thus reducing risk and uncertainty, encouraging more productive investment, and promoting growth.

Were I to seek a general theory of history in the face of the arguments that this is the *ignis fatuus* of scholarship, I should stand further back and survey the world in the *very, very* long term, since the Palaeolithic. The prospect of an economic history of man as a successful species, articulated around the big population cycles identified by McEvedy and Jones (1978), the big overlapping migrations, and the fundamental advances in agriculture, is an exciting one. At that level there would be a class of phenomena from which generalisations might be drawn and against which the experience of independently creative cultures might be tested. But with Europe's achievement of sustained economic growth leading to the first industrialisation – by definition a unique case – there is no such class of phenomena and therefore no appropriate overarching theory. This necessitates a pragmatic retreat from the exposed outworks of theory pushed forward into history by Hicks (1969) and North and Thomas (1973; cf. Jones 1974b). For all the conceptual difficulties, anyone who has written on a topic like the present one is bound to hanker for someone who will take his work as *reculer pour mieux sauter*. For the present, in the absence of general theory, I have found the comparative method offers a measure of control over conjecture, and some hope of seeing the wood as well as the trees. Accordingly I have emphasised comparisons and contrasts between the experiences of Europe and Asia.

The current exercise seals my transformation from Hedgehog into Fox. The rites of that passage have embroiled my family and friends. None of them is to be held to account for the abandonment of scholarly caution that synthesising on this scale has required. I feel a special debt for their encouragement, nevertheless, to my former colleagues in the Department of Economics at Northwestern University, especially John Hughes and Joel Mokyr. Because I have since been working in Australia I am more than ordinarily grateful to those friends in Britain and the United States who have continued to write me letters, suggesting and even sending books, photocopies and clippings to help keep me in touch. Coming and going I have also been dependent on their hospitality, as on that of my parents. Thus my thanks go to Bob Dodgshon, Patrick Dillon, Malcolm Falkus, Max Hartwell, Michael Havinden, Geoffrey Hawthorn, Clifford Henty, Clifford Irish, Bill Kennedy, Noel King, Peter Large, Lew Lewis, Bob Machin, Derick Mirfin, John Naylor, Bill Russell, Colin Tubbs, Nick White, and Stuart Woolf in Britain; to Lou Cain, Stan Engerman, Matt Enos, and Bill Parker in the United States; as well as to Betty Vinaver and the late Eugène Vinaver in France. I have had memorable conversations with Betsy Hoffman at Northwestern, John Gould and Gary Hawke at Wellington, N.Z., Sir Frederick Russell, formerly Director of the Plymouth Marine Biological Research Station, James Lewis at the University of Bath, and.Barry Turner at Exeter University. I have been glad, too, of the similar interests of John Anderson who has taught a course with me on economic change in the *very* long term.

Sabbatical leave entitlement under the terms of my Australian appointment, the first non-teaching leave I have had, enabled me to tackle this task. For my leave I was a Visiting Professor at the University of Exeter and I am grateful to Professor W. E. Minchinton and the members of the Department of Economic History for their courtesy. What Arnold Toynbee called the 'dumb show' of historical monuments, made real, in Somerset where I took a cottage, many of the events and processes discussed here. Physicists say that their best work is done in bed, bath, or 'bus, and while I might normally be tempted to retort with the Australian expression, 'half yer luck!'; this year I was able to order my own thoughts while walking the Somerset orchards. The literate quality of English life proved stimulating, though in connection with literacy I must apologise for using the author–date system of referencing. It is ugly and interrupts the eye and reduces precision and flexibility in attributions, but does help to keep down publishing costs.

Another means of keeping down costs has been to have my wife and my children, Deborah and Christopher, fetch and carry library books and compile the bibliography. The works cited in the bibliography printed here are almost entirely restricted to those referred to in the text. My wife also went out to work to help pay for a visit we made to former colleagues in the United States during my leave. These are the least of the many debts she and the children are owed.

E. L. J.

Haselbury Plucknett, Somerset
January 1980

Introduction to the second edition

The subject of this book is a big one: Why did economic growth and development begin in Europe? They had after all been more likely to emerge in other parts of the world. The book, then, is concerned with how technical change, structural change, and income growth all got started, that is to say with a complex of issues at the heart of economic history. It is also concerned with historical geography insofar as place (in the sense of natural environment and the areal differentiation of political society) affected the shape of economic change. Comparisons with areas outside Europe are therefore made in an attempt to see what was special about the European case.

With a canvas as broad as this, touching on the experience of three-quarters of the world's population over several centuries and obliging us to rely on secondary sources, we should be prepared to consider all sorts of explanations. There is no completely satisfactory, uncontestable theory on this scale. The vehemence with which the exponents of one scheme or another assert that theirs is the answer shows that social scientific history remains immature, and that some of its practitioners are not yet mature enough to live with the uncertainty associated with hypotheses that are hard to bring to a conclusive test.

In the circumstances comparative history seems a less predetermined way than grand theory for weeding out explanations that may have local explanatory power but lack any general application. If we confine ourselves to studying the history of a single economy we may easily mistake its peculiarities for universal rules. The depth of research that the historian can attain by focusing on a single nation actually heightens this danger. Admittedly, as one reviewer of *The European Miracle* complained, a comparative approach does not guarantee that we will be able to tell exactly 'which pellet brought down the bird'. The only way to do that is by embracing a theory that has decided in advance which is the key variable. What comparison should do is eliminate a large part of the pattern of the shot (to adopt the same metaphor). There is, in any case, not the least reason to suppose that any single factor or relationship had an all-powerful effect on economic development. We will hang loose, as they say. This book is an exploration, not a gospel, and starts from the assumption that any broad historical evolution has a configuration of causes. In this Introduction I shall however mention a few minor changes of emphasis from the original picture I drew of that configuration.

WHAT, WHERE AND WHEN

Historical studies of growth typically seek to identify how a rise in average real income per head began – that is, the causes of an upturn in the trend of gross national product divided by population. This approach tends to home directly on presumed changes in income and cuts away the context of prior and facilitating change. In reality no sustained rise in per capita real income is likely to have begun in a stagnant and unaltered economy. It is fruitless to concentrate on growth shorn of its matrix of developmental and structural transformation, especially as there are virtually no figures on early movements of national income.

One of the reasons why there are few if any data aggregated at a level which modern economists take for granted is that the nation-state itself did not exist throughout most of history. Instead, it was a European political invention of the very period in which we are interested. Earlier economic changes must be referred to different and varying geographical units. Historical statistics are by no means lacking, especially those on taxes and prices, but we lack them at whole-economy levels. Few societies took even a population census before the present century. This means that miscellaneous, indirect indicators of economic change have to be discovered and compounded together.

Income growth for whole societies (or their 'average' members) may have begun quite early. Some major authorities claim that average incomes were rising in Europe as long ago as A.D. 1000, although the rise must have been glacially slow at first. Attaining economic growth is the means of solving the dire human problems caused by want, and it does matter desperately. 'A failure to maintain economic growth means continued poverty, disease, squalor, degradation and slavery to soul-destroying toil for countless millions of the world's population' (Beckerman 1974:3). If we deal with this subject, as we do here, via a discussion of shadowy, long-run, prestatistical, aggregate movements, we seem to be writing history far removed from the experience of individual people: dehumanized history, someone has called it. But consider, if we go to the other extreme and write biographies we can hope to deal with only a handful of people, and worse, we have no way of telling whether the chosen subjects were representative. Human strivings were no less agonizing because there were a lot of them. A statistician who was charged that his discipline was 'soulless' retorted that social statistics are frozen tears. Broad-gauge history makes good sense and is not as inhumane as it may look at first glance.

A concern with economic growth does not necessarily mean an indifference to questions of the distribution of income. (It may be fairer to see it as a matter of putting first things first.) One of the points stressed in the text is indeed that supplying more and better public goods had become almost a defining characteristic of European governments by the eighteenth century. The most significant were the actions classed here as disaster management. These included in particular the imposition of quarantines to halt the spread of epidemic diseases among human beings, of *cordons sanitaires* to shut out movements of infected cattle, the payment of compensation for the slaughter of infected herds owned by farmers, and the emergence of measures to redirect cereal surpluses to districts where high prices threatened to produce famine. In poor and vulnerable societies, the gains from administrative measures like these were large. The payment of compensation for beasts slaughtered because they had come into contact with sick animals suggests an altogether different picture of eighteenth-century administration and peasant life than is usually painted.

Europe came to outclass Asia and the rest of the world in its range of policies to avert or cope with disasters. Assertions to the contrary (Wong and Perdue 1983) do not stand up because they rely on the evidence of the remarkable famine precautions discovered for early Manchu China by Will (1980) while ignoring the context: famine was only one disaster; China was not Asia; and even the Chinese counter-famine measures shrivelled away just when Europe's competence became so apparent (Post 1977).

Public goods are defined as those from the use of which no one can be excluded. As a result of this nonexclusion principle, the unprecedented range and scale of provision by European governments improved welfare for the population at large. The implications of this for the well-being of Europe's poor are almost wilfully neglected in the historical literature. The significance should be assessed against the proper standards, which are those of most of Europe in the Middle Ages and those of most of the rest of the world until very recent times. It might be objected that the poor were taxed disproportionately to pay the costs of counter-disaster measures, but some public goods are so vital for life and health that even 'forced saving' to acquire them via taxation would have been a positive step.

Economic development may be viewed as involving change before gain. Development implies changes in the economic structure because of reduced employment in agriculture. This happened at first with the emergence of part-time manufacturing on farms and in farm cottages, which was a hidden form of structural change. The 'proto-industrial' sector created goods for sale, and reciprocally, among farmers specializing in growing food for the cottage workers, created the very market to absorb the goods.

Modern studies do show that development and growth run together, in the sense that structural change is positively associated with income growth. The association is not however very close, and there is more to development than just structural change. Other early aspects included investing in building roads, bridges and harbours and in river and canal navigation, to permit the wide distribution of bulk loads throughout Europe. Previously, bulk trade on any scale had tended to be confined to the Mediterranean basin, some of the Chinese waterways, the Sea of Japan, and (more in prospect than in reality) the 'Indonesian Mediterranean'. In all this we must see that Europeans were neither the only nor remotely the first active traders. Differences in trading activity around the world were in degree, not in kind, and many features of European development were those of a rather late-settled area catching up with the leading regions. What happened to distinguish Europe was the swollen emergence of bulk trade over quite long distances, multilaterally, in everyday commodities, and not simply in the luxuries that had always dominated long-distance trade.

We are not able easily to detect turning points in the early history of trade, for lack of statistical evidence, and as a result our explanations of the history of economic change are unlikely to be clear-cut. Economies are intricate things – there is now a literature which admits that their nature is better conveyed by biological rather than by the usual mechanical metaphors. Economies are related to the other features of social life in various and inconstant ways. The challenge is not that of fixing the focus on economic growth, which only becomes rapid or measurable in recent periods, but of judging which preceding developments were connected with the eventual rise of average income.

One phenomenon that was closely involved was the integration of markets, including the eventual merging of local, price-segregated markets. Commodity markets in Europe were integrated quite early, but while this was necessary it was clearly not sufficient for very significant growth to result. China long had integrated markets without any sustained rise in per capita incomes. The Islamic world would accept a single coinage. The banking system of Mughal India was capable of honouring drafts with which the very Marathas who were in revolt against the Mughal empire could for a time be bought off, like (to put it in European terms) a Dane-geld. By themselves, however, these sophisticated practices were not enough to bring about much growth.

Nevertheless the most fundamental change may still have lain in the emergence of markets, but not in commodity markets so much as in markets for land and labour. What Europe achieved in addition to bulk commodity markets was the formation of quite efficient factor markets able to transact in land and labour. This required a more profound dissolution of cultural and political rigidities, and thus deeper and more dangerous shifts in society, than the mere acceptance of extensive trading in goods. Non-market exchanges of labour typically persist later than those of goods. It has been suggested that this is because the costs are higher as a result of difficulties in measuring a tenant's or worker's marginal product or in monitoring his effort (Posner 1981:181n.12). However, it would seem to have as much to do with considerations of appropriating the product – that is, with power relationships.

The history of economic performance is indissolubly bound up with the choices of geographical arena and precise period to be studied. The triad of questions, what, where, and when? is usually answered – the Gordian Knot is cut – by the almost unthinking assumption that what really mattered in economic history was the 'industrial revolution' originating in and diffusing outwards from late eighteenth-century Britain, together with the spread of imperialism, considered to have been solely a Western phenomenon and strictly negative to boot. 'In the beginning there was England. And contentment vanished from the world' (Berliner 1966:159). As a result of this orientation we have a truncated view of the stream of change, and the evolution of the pre-industrial and precolonial worlds has become hard to recognize and assess dispassionately.

The geographical issue splits in two. First there is the question of the proper scale of the units of study. Second, once that has been decided, there is the question of selecting the appropriate historical examples to be studied. The main contending units are regions, nation-states, and the empires which were sometimes virtually coextensive with culture areas or civilizations. Nation-states are the most popular but least suitable of these categories. They are artefacts of European historical process, hammered together from job lots of feudal fragments. They had not come fully into being at periods when it is reasonable to seek the early experience of development. In short, they are anachronisms. They are also anatopisms. Having largely been forced on the rest of the world since 1945, they do not capture or contain the whole relevant historical experience outside Europe.

Economic activity is more plausibly regional, and it is interesting to note the rediscovery of the region by economic historians during the last few years, trumpeted as if an original finding. Yet regions present almost insuperable problems of definition (someone once dismissed them as metaphysical units used by geographers). They need to be redefined continually according to the fortunes of the economic activities they contain and for which they are often rather clumsy proxies. This makes them uncertain vessels in which to pour history, which is necessarily about change through periods over which the regional assemblages may not be stable. A further difficulty is that regions are not independent of larger or overlapping polities. Although farming, in particular, is quite usefully discussed in terms of ecological regions, it should not be forgotten that these remain subject to political influences such as taxation by incongruent units.

The conclusion is likely to be that we need multivariate geographical units, which are hard to identify or to find specific evidence about in practice. This somewhat tangled problem strikes me as comparable to the colligation problem – the problem of when to start – in historical analysis. No doubt the commonsense solution in both cases is to choose the date and unit most convenient to the analysis one has in mind, though since the chronological and geographical setting may set arbitrary bounds to the answers one can expect, it is important not to allow oneself to become a prisoner of the initial choice. Historical studies often do suffer in this respect because historians define themselves as students of periods and places rather than of problems. They rationalize this, understandably enough, in terms of the load of specific facts to be learned, but it too often constricts their thought about issues.

For our purposes the third major category mentioned, the empire, is the most suitable unit. We do have to remain aware of the possibility of contradictory economic fluctuations in the various subunits, and also add a consideration of the substitute for empire which emerged in Europe's case. The substitute was the states-system, whose common processes affected its constituent nation-states and regions alike.

The choice of unit for study in the history of growth has conventionally been the nation-state, and the first part of the world chosen has typically been Britain. Although lately there has been a breakaway which has carried with it a change in the unit of central focus, the largest body of thought does remain what I call the 'Little Englander' school. This school implies that England, or Britain, was or became so unlike everywhere else that she, and she alone, could spawn industrial revolution. This insularity has, or should have, suffered a hard knock from recent research on France, which turns out to have matched many of Britain's eighteenth-century achievements. Envisaging British economic history in a European context, instead of insisting that Popery and wooden shoes both began at Calais, almost seems a result of British scholars coming to terms with the Common Market. If so, there is still a surprisingly determined nationalistic opposition. The Little

Englanders prejudge the issue of the timing of economic change by flatly assuming that all relevant change took the form of a late and decisive industrialisation. The misfortune of this point of view is that if Britain truly were unique, her history could shed no light on the experience of other countries, even those near neighbours that industrialised so suspiciously quickly afterwards. It is much more instructive to look on Britain as affected by forces that were acting on the whole European continent, out of which she narrowly emerged as the first industrial power on the basis of technical change in the manufacture of cotton and iron.

At the opposite extreme are the 'One-Worlders.' They seem to be an extension, almost a reductio ad absurdum, of the world-system school of Immanuel Wallerstein and his followers. The original world-system view was Eurocentric. The system was envisaged as driven by an exploiting economic core of countries in northwestern Europe. This giant economic geography represented a liberating shift from internalist British or European work, although the central hypothesis of exploitative relations between the core, semi-periphery and periphery has not stood up to critical tests by independent scholars (e.g., O'Brien 1982). The train of thought is quite run off the rails by One-Worlders, who argue that all late premodern economies were permeated by the ripple effect of trade with Europe, and that this, rather than internal considerations, determined that the fate of the non-European world would seem to be dismal.

Mao Tse-tung pointed out that heat may be applied to a stone or an egg, but the fact that a chicken comes from one and nothing from the other is actually due to their internal structures. So it was with the non-European economies when European trade and violence touched them. They responded according to their own organization and circumstances, not merely according to the fierceness with which Europe laid hands on them.

The period before 1800, with which this book deals, was in any case free of massive European influences on most of the Middle East and Asia, especially China. An historian of Indonesia has spoken of the eighteenth century as Asia's own century (Van Leur 1955:271). A Turkish historian has observed that the Ottoman empire was an autonomous structure affected by international military rivalries rather than the plaything of commerce and relationships to the market (Sunar 1980:574–75n.2). The common assumption that the fate of Third World economies was always made more dismal by Western imperialists is in any case unwarranted. Lloyd Reynolds (1983; 1985) has amassed considerable evidence that many of these economies were already growing in the imperialist world of the late nineteenth century.

There seems little reason to suppose that geographically far-flung, but usually tenuous, trade contracts running back ultimately to Europe were what made the whole premodern world revolve. Certainly some of the distant repercussions have long been known, for instance the chain reaction of disturbances set up far to the west by fur traders on the east coast of North America, but an attenuated contact is not the same as a world market. One-Worlders tend to avoid the effort at quantification that might decide the point. Their argument in itself is not new. The geopolitician Halford Mackinder was accused of ignoring the experience of the 'people without history' who occupied the lands (almost) discovered by Columbus. Yet like Mackinder we do not need to study the entire world to grasp the beginnings of European development or even to provide adequate contrasts with European experience. As Mackinder's biographer savagely but tellingly retorted in his defence, insisting that people did dwell and make history outside the Eurasian land mass is like saying that a cupboard is not empty because it is full of air (Parker 1982:234–5). Such considerations led me to set Europe's economic emergence against a backcloth of the other major economies of the (European) early modern or late pre-industrial period, defined as very large populations organized in single polities. Mackinder was dealing with the distributions of power, not culture; and we are dealing with major economic changes, not marginal cultures or obscure channels of trade. Small societies are anthropologically interesting and provide special opportunities to investigate economic behaviour under virtually controlled conditions, but they are by definition not the heavy-weights of world history. It was the Europeans who rose to power and it was they, above all, who traded in bulk – mostly, it must be added, with one another.

The proper units for our purposes are therefore the European states-system and the great contemporary empires. The states-system was a set of interacting parts. The structure of individual nation-states, not to mention regions, of course influenced economic performance, but this was secondary to the systemwide influences of a common civilization, reactive politics, and supranational markets. Europe was bonded economically as much as politically. A better understanding may be gained of the total economy of the states-system by comparing and contrasting it with the other large economies of the time, organised as they were in political empires, situated in Asia or at any rate in Asia and the Middle East. Their different, and in Europe's early modern period ultimately downward, trajectories are not explained primarily by Western imperialism. A different imperialism is to be indicted. This was the command economics imposed by dynasties from the steppes of central Asia: Ottoman, Mughal and Manchu, all of them latter-day models of the Mongol onslaught. It was this, not the hydraulic agriculture of a timeless Asian Mode of Production, nor Western trade or conquest, that determined the fate of the East. Steppe imperialism was what made the difference, by clamping in its selfish grasp the customary agricultures and nascent trade sectors of the 'early modern' Islamic Middle East, India and China.

Why should we investigate these matters during Europe's 'long early modern period' from about 1400 to 1800? The deeper springs of Europe's growth certainly arose earlier, and earlier periods also need a glance if we are to notice the potential that had already been revealed by Eastern societies. The book does mention these former times, but concentrates on the period when Europe's acceleration became patent and overtook all others. That period permits a close look at the divergence.

Debate about the divergence is a silent, unexpressed, incoherent struggle. Various authorities make stern or impassioned comments about the timing of Europe's advance, with or without contrasting it to Asian experience, but they mostly ignore one another. They debate by default.

Transcurrent points, the dates when Europe forged ahead of China in the realms of science, were long ago calculated by Needham (1967). In more directly economic spheres, views about the passing points range over many centuries according to the authority. Part of the discrepancy stems from the choice of 'controls' (i.e., the precise other areas with which Europe is compared). The general issue is seldom addressed head on, but Issawi (1980) has devoted an article to 'what points in time and in what fields the West overtook and surpassed ... the Middle East.' His answer is that in most respects, except the military, Europe was ahead by the fifteenth century at the latest.

Four leading authorities – Cipolla, Kuznets, Landes and Maddison – have separately stated the opinion or actually estimated that in strict growth terms Europe was edging up as far back as A.D. 1000 (collected in Maddison 1982:255n.3). This does not necessarily imply any particular trend elsewhere, but it does have implications very different from the Little Englander view of a 'great discontinuity' at the industrial revolution. One widely used industrial revolution textbook even claims that until the middle of the eighteenth century the British economy was 'relatively stagnant' (Deane 1979:18). Bairoch and his collaborators also indicate that the income gap opened only after 1750 (Bairoch and Levy-Leboyer 1981).

There are thus early and late schools of thought, geographical inconsistencies, and differences over the proper focus of economic enquiry. Syllabuses tend to be dominated by the 'late' school. Two otherwise different but widely held interpretations converge on a late discontinuity. The former is the Little Englander view, which sees Britain industrializing unaided in an income-stagnant world – in a phrase, 'taking off'. The latter is the imperialism view, which sees British or European industrialization achieved at the expense of non-Western societies, blighting growth prospects there, especially in India. The history of native Asian imperialisms is neglected in favour of implying that without Western imperialism the East might have achieved industrialization by itself.

The 'early' school may treat Europe's rise as an unfolding of possibilities latent wholly and solely within Europe, or it may set it against a backcloth of other societies. At least these scholars do recognize the early stirrings in Europe. 'An understanding of the subsequent widening gap in relative economic performance must be sought in the period when the gap originated, which was prior to 1500 A.D.', declares one reviewer of *The European Miracle* (Crotty 1983:194). Others tend to narrow the critical period by depicting Europe as having been backward compared with T'ang or Sung China or the Abbasid caliphate of Baghdad, indeed from the Fall of Rome until the Middle Ages. They thus trace Europe's relative success to the High Middle Ages, long before there were dominant trading interactions with other cultures. In this vein North and Thomas (1973:157) proclaim that 'the industrial revolution was not the source of modern economic growth. It [i.e. the growth] was the outcome of raising the private rate of return on developing new techniques and applying them to the productive process'.

The most cogent summary by an economist may be that of Kuznets (1964:21). His opinion is that the European countries in their pre-industrial phase, which he defines as before the share of the work force in agriculture fell below 60 percent, enjoyed per capita incomes several times higher than most less-developed countries in the 1960s. They were more developed than most other parts of the pre-industrial world and had already undergone a long period of growth and expansion.

Kuznets's views on early growth, expressed over twenty years ago, might have been expected to have a big impact on research programmes. Yet although lip service is not infrequently paid to them, they inspired little research and even less teaching apart from the period beginning with the 'industrial revolution' of the eighteenth century. Recency is mistaken for relevance. But the game in town was an older one, and it was leapfrog. Europe had to catch up to earlier economic revolutions in China and leap over them; Indian science had to be absorbed and surpassed, and so did that of the brilliant Islamic Middle East. Kuznets himself did not investigate these other episodes of rise and decline in historical detail. He left the agenda open, and the present book considers it. What it looks at are the conditions conducting to the period of Europe's rise and the reasons specialist authors may have for describing Asia and the Middle East as 'frozen' or 'slumbering' during comparable periods.

CONTROLS

The obvious comparisons or contrasts with nascent Europe are thus to be found among the other large societies of the long early modern period, which were in Asia. For this purpose the term 'Asia' is the merest label of convenience, particularly as it is also used to include the Near and Middle East. Nothing is meant to be implied along the lines of a universal or immutable 'Asian' behaviour pattern. In the sense meant here, 'Asia' is merely a geographical expression for where the other vast organized populations of the world happened to be located (they still are) during the long early modern period of Europe's history.

Whether to tackle these other economies via a general model or one by one depends partly on taste and partly on the space available. In many ways it might be preferable to do both. A scheme is certainly required to organize the strings of special cases that constitute the histories of particular areas. On the other hand, both writer and reader need to keep a natural curiosity about possibly significant details that may have influenced economic performance. The three main Asian empires are tackled separately here, though the common theme of the effects of conquest empires on huge customary agricultures is emphasized. Grouping together the Islamic empires, Ottoman and Mughal, as one category would have made sense too, including also the smaller Safavid empire in Persia, but the Chinese empire usually needs its own coverage. In any case every individual empire lay across a different indigenous society. Each had its own ecological setting, though the theme of higher disaster risk in 'Asian' than European environments can be discerned.

Asian environments were generally far from biologically unproductive. Most of India, in particular, had abundant warmth and moisture for growing crops – if the monsoon came. When the rains did come, plant production outstripped anything in Europe's chilly fields. In the seasons when the monsoon failed there was tragedy. The difference lay not in average productivity, which was higher in south and southeast Asia than in Europe, but in the greater variance about the mean. Large shocks which destroyed capital works as well as numbers of people were commoner than in Europe, although given the nature of historical evidence this is hard to demonstrate statistically (Pryor 1985; Jones 1985). A greater frequency of disasters increased uncertainty as well as risk, and thus militated against long-term investment.

Political risk was even more acute. Economies are politically embedded, and this is decisive for the way they perform. The customary Asian political form, the empire, produced little incentive or an actual disincentive for important groups to engage in productive investment. The largest social group, the peasantry, had low incomes and little surplus. Peasants were without redress against arbitrary taxes, to the point that eating any small surplus that a harvest did provide made more sense to them than hazarding it in new ventures. Merchants were necessarily inferior to warriors in societies where status accrued to military conquerors. Some merchants did grow rich, but unless they themselves became landed officials (which was open to them only in the Chinese case) their wealth remained at risk of confiscation by public officials. Individual merchants might bribe their way to influence, but emperors never needed to rely on them as impecunious European kings did, and they did not gain influence as a class. They never succeeded in hollowing out the Asian empires into bourgeois states.

Land ownership in these empires was not usually hereditary. The *jagirdars* of Mughal India were moved around by the regime; the sons of Chinese scholar-gentry had to sit the classical examinations. They could form little permanent attachment to the land and lacked the incentive of European landowners to invest in the productive capacity of estates for the sake of their offspring. The procedure was to milk the peasantry, without crossing the line of bringing on peasant revolt (though they were not always good judges of that).

The empires did not become service states. Sultans and emperors amassed vast wealth but received incomes that were nevertheless small relative to the immensity of the territories and populations governed. Even with a will, which they did not have, they would have lacked the central government budgets needed for the economic development of their lands. Thus the customary economy of the agricultural sector, the limited market sector, and the command elements of these systems all lacked the makings of sustained advance, despite the burst of productivity that followed each initial establishment of peace and order by the conquerors.

These were vast but brittle systems. Faced with defeat, or an end to the military successes that had brought them into being, they turned inward. Mounting revenue needs tended to crowd out productive investment on the required scale. Tended is a key word. Particularist historians will want to object with counter-instances of the achievements of one empire or another. History is very full. Our aim is to identify the general tendencies, and these simply were not cumulative development and growth on the European plan.

In addition the Islamic empires never fully solved the problems of succession to the throne. Europe's past is besmeared with wars of succession – Austrian, Polish, Spanish – but the struggles of the Orient were endemic and more consistently destructive of internal order. Attempted solutions, such as keeping the heir in a 'cage' until he was needed, virtually guaranteed inexperienced rule and poor economic management. Few men confined since boyhood in a harem, fawned on by eunuchs and concubines, were likely to acquire the expertise or self-discipline needed to preside over empires otherwise barren of checks on arbitrary power.

This view of the prospects of the major Asian economies attaches to the particular Ottoman, Mughal and Manchu dynasties. It is not Wittfogel's view, nor Marx's, nor Engels's, of an endless Asian Mode of Production. Asian economies were not locked forever in an environmentally determined posture where central control over irrigation rights put a permanent stop to progress. Asia *had* changed, long before Europe. Income growth may have become 'frozen' in these centuries, but the freeze has identifiable historical roots in particular invasions from the steppes. The economies then became command hierarchies imposed on customary agricultures. These weakened investment in human and physical capital, slowing and diverting for the duration of the empires much further growth of the market.

Investment and its political determinants may be challenged as the focus of study. The key to the empires may not after all have lain in frustrated investment. (Yet if the system is nothing, systematics is all. History otherwise becomes a chaos of anecdotes and special cases.) Perhaps after all the economic performance of empires centred on less tangible matters like religion, culture, ideas, or law. Marx, certainly, lamented that Asian history seemed to be nothing but the history of religions. He would have preferred to ground it on the materialism he believed to lie beneath (Worsley 1984:104–5, 169, 243–4). Heaven knows, if we may so put it, he deserves some sympathy. Religions are not immutable, and Asia's history from far back is one of recurrent missionizing by one faith after another. Ideas in general are malleable, at least over historical time. The puzzle is, how far do they adapt to the deeper circumstances of

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natural environment or political incentives? To what extent are they formative? Asian religions were in any case only superficially incompatible with economic growth, and if the Christianity of Europe seems all too comfortable with it we can only remark that Christianity had been around for a long time without producing growth. With regard to religious, cultural, ideational, and legal influences, the issue is what lags, what rachet effects, do they insert in material change? The answer, no doubt, lies somewhere between simple materialism and the undiluted history of ideas. The material ground is however more certain, and in an introductory essay the stress may justifiably be placed on the politics and natural environment that influenced it.

EUROPEAN EMPHASES

The plan of the European section moves from prehistory or *very* long-term environmental matters by overlapping stages through the history of technical change, the stimulus of the Discoveries, and the formation of the market, to the nature, origin and implications of the European states and states-system. What is this, one reviewer of the first edition demanded, but the invisible hand of Adam Smith with a helping hand from the state? The interpretation may indeed be distilled into an argument for the combined and synergetic effect of environment, market and state.

First, the environment: This subdivides into features of site, location, and the disaster profile. Among site characteristics, the resource endowment is not very helpful in explaining change. Resources are a function of the available technology and have no economic meaning until a technology has been invented to employ them. The North American Indians knew about oil but had no conception or means of using it as petroleum. In Europe's case the most relevant aspect of the resource endowment was probably the way it was dispersed across a geologically and climatically varied continent, since this provided an inducement to trade.

Another site feature was the discontinuous distribution of good land for growing grain. Europe possessed a number of so-called core areas of high arable productivity, each the home of a denser and richer population than the area around it. One reason why Europe remained politically decentralised may have been that the larger core areas were much of a muchness, and the occupants of any one of them found it hard to dominate the others.

Site characteristics also include climate. A number of reviewers read an emphasis on climatic change into the first edition. This surprises me, because it is not there. I agree with my colleague John Anderson, who has shown that incremental changes in long-run climatic averages were offset by economic adjustments, and that some of the supposedly depressing climatic effects were more likely the results of other events, such as the fall in population after the Black Death (Anderson 1981). The closest he or I come to attributing major economic consequences to shifts in weather (not climatic) variables or to any other changeable element of the physical or biological environment is in discussing disaster shocks (Anderson and Jones 1988). The central point about these is that they strike hard and fast, and although costs can be spread by insurance devices (themselves not a free good), disasters are an environmental phenomenon that cannot be wholly avoided.

The locational advantages seem to have been twofold. Europe, at any rate western Europe, was a long journey from the central Asian epicentres of the invasions which from time to time captured other parts of the Eurasian periphery, notably India and China. Distance was some protection, as was a forested landscape unsuited to cavalry warfare. On the other hand, once adequate sailing ships were built, the western European seaboard was found to lie conveniently opposite some of the richest seas and most exploitable and least defended lands in the world. The mention of shipping technology, nevertheless, introduces the wild card of the environmental pack. By itself geography explains nothing. Historical events, including specific technological innovations, are needed to put site or location to given uses. Yet geography may not be altogether dismissed. The layout of the world does affect the relative costs of economic activity under any one technology. Europe overall was not as disfavoured with respect to trade, political variety and capital accumulation as its rather chilly northern location and short growing season might suggest. Indeed, from the standpoint of disease a northerly location may have been a positive asset.

The cultural environment of Europe was unique in one deepseated sense. In a classic paper, Hajnal (1965) has demonstrated that marriage participation rates were lower, and the female