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From Sociology and Economics to World History

Abstract: World History has only recently emerged as a distinctive and vibrant field of study. Its origins arose from a cross-fertilization of economics, history, and comparative sociology, and from particular conjunctions of people and places in the 1980s and 1990s. World historians now, thanks to unprecedented access to quantitative historical data and international networks of scholars, can develop increasingly precise, formal, and detailed accounts of changes and comparisons across historical periods. However, our goal is not to create new master narratives or theories that predict a grand historical trajectory for mankind; rather we seek to better understand the similarities and differences among societies, and the likely consequences of those similarities and differences.

Key Words: world history, intellectual history, rise of the west

New ideas in scholarship almost always involve some degree of chance. While 'normal' science proceeds by expanding our base of knowledge, and testing and refining the ideas at the leading edge of a field, paradigm shifts tend to arise at the intersections of fields where ideas from one discipline create new angles of vision or fresh insights into adjacent disciplines. The "California School" view of world history was born at just such an intersection, which arose in slightly out-of-the way places and partly by accident.

In the 1970s, when I began graduate study, world history did not exist as a field of study in doctoral programs. Indeed, it had rather a bad odor about it, being associated with sprawling and abstract schemes imposed on the history of varied nations and time-periods, as in the work of Arnold Toynbee and Oswald Spengler. There were a few daring historians who sought to identify longer-term historical trajectories – William McNeill's *The Rise of the West* being the outstanding example.[1] But even that book was considered something of a popular or teaching

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volume, and was firmly rooted in a "Western Civilization" perspective as the central story of history.

Historians of European countries often embarked on more general histories of "Europe", but this was mainly for teaching purposes, and even such histories rarely touched on adjacent Ottoman lands or Africa or Asia, except when they became European colonies. Only a few daring historians ventured into serious work on broader cross-cultural regions. Among the major regional historians who sought a more expansive view were Philip Curtin and Robert R. Palmer in regard to Atlantic history, Marshall Hodgson for the Islamic lands, and Joseph Fletcher for Central Asia, India, and China (the last of whom directly influenced me when I was a student).[2] However, their work remained highly controversial, even if widely admired.

One might have thought that the *Annales* school in France, building on the examples of

Fernand Braudel and Marc Bloch,[3] would have founded a school of regional and world history. Instead, their students went in the opposite direction, producing brilliant but more focused studies of long-term change in very specific regions of Europe – at the level of a province, a department, or even a village. Braudel's own sweeping later work on the history of capitalism was regarded as something of an eccentric masterwork, and had no emulators.[4]

The study of long-term historical change in global and comparative terms, at least from the mid-1960s through the 1980s, was occurring mainly outside of history departments. Its real home seemed to be in sociology, where the period from the 1960s to the 1980s marked what has become known as the "golden age" of comparative historical sociology, associated with such figures as Perry Anderson, Barrington Moore, Jr., Charles Tilly, Michael Mann, Theda Skocpol, Randall Collins, Reinhard Bendix, S.N. Eisenstadt, Andre Gunder Frank, and Immanuel Wallerstein.[5] These scholars were inspired by the classic studies of 'western' vs. 'non-western' social dynamics and economic development of Karl Marx and Max Weber, and took inspiration both from Marx's vision of the global expansion of capitalism and Weber's view of the rationalizing transformation of social life by law and bureaucracy. Both of these major social transformations were seen as beginning in Europe, transforming its historical development in distinctive ways, and then spreading throughout the world. The scholars building on Marx's and Weber's analyses undertook detailed studies of the similarities and differences in the trajectories of state-building, war, revolutions, state authority, and international trade across Europe *and* Asia.

While these scholars were able to draw upon the rich and rapidly expanding body of work by historians specializing in the study of various European and Asian societies, and on advances in theories of the state, the international economy, and organizations, they built their analysis on surprisingly slender quantitative founda-

tions. They relied on the traditional narrative of European history as progressing from the mainly agrarian Middle Ages to a Renaissance that gave birth to trans-oceanic trade and colonization, followed by the emergence of a modern world driven forward by the twin forces of reason and industrialization. By contrast, Asian societies were considered to be basically the same agrarian societies in 1800 that they had been in 1200. Their puzzle was thus to explain the sources and effects of European dynamism stretching across many centuries with the comparative stasis of non-European civilizations.

The reason for the lack of any precise quantitative basis for exploring changes in Europe and Asia was fairly straightforward – the data did not yet exist. In the 1970s and 1980s, scholars had only begun to compile data on such basic facts as the size of cities, the growth of population, increases in output and capital stocks, and the level of prices and wages in Europe and Asia. The pioneering efforts of E. A. Wrigley and Roger Schofield and Charles Feinstein and Sidney Pollard for England; Jan de Vries for the Netherlands; Jacques Dupâquier and Fernand Braudel and Ernest Labrousse for France; Michel Cartier and G. William Skinner for China, and Ömer Barkan and Halil İnalcık for the Ottoman empire, to name just a few, helped make such data accessible to a wide range of scholars only in the 1980s and 1990s.[6] These data became the foundation for the new and exciting field of *cliometrics*, an emerging sub-discipline in economics that applied classic economic theory to quantitative historical data in order to test ideas about the causes and magnitudes of historical change.

Because of the availability of these new data, a wide variety of common-place ideas about economic history were turned upside down. Robert Fogel demonstrated that railroads had not been so productive, nor slavery so destructive, of economic gains in America as had been previously believed – work that earned him the Nobel Prize in economics.[7] Nicholas Crafts and Knick Harley showed that economic growth during the era of the Industrial Revolution in the late eighteenth century was much slower than had been previously believed; indeed almost as slow as in the entire sixteenth and seventeenth centuries.[8] İnalcık, Skinner, and others were also showing that there existed quite dynamic movements in regional growth, urbanization, prices and wages across the supposedly 'static' centuries of pre-modern Turkey and China.

I was fortunately, somewhat accidentally, swept up in these cross-currents early in my career. I obtained my PhD in sociology at Harvard in the late 1970s, where I studied with Theda Skocpol and S. N. Eisenstadt, and followed their work in their study of revolutions. As it happened, Harvard also had outstanding demographers, notably Nathan Keyfitz, who made me aware of the emerging work of Wrigley and Schofield on English population history. I arranged to visit Cambridge to become more familiar with the work of Wrigley and Schofield, and developed my disserta-

tion as an effort to understand how changes in England's population history had contributed to its Puritan Revolution in the 17th century.

In 1981, upon completing my Ph.D., I followed Skocpol to Chicago, where she took a position at the University of Chicago, and I took my first job across town at Northwestern University. I did not know it until I arrived, but Northwestern was becoming a hub of the new cliometrics, with Jonathan Hughes and Joel Mokyr on the economics faculty, and Avner Grief and John Nye as graduate students. At the University of Chicago, not only was Robert Fogel working in economics, but Halil İnalcık was teaching Ottoman social and economic history. Hughes and Mokyr invited me to join both the Northwestern Economics Department's cliometrics seminar and the Greater Chicago Economic History Group, and I began to extend my dissertation to a broad analysis of the role of population and price changes in revolutions and rebellions in England, France, the Ottoman Empire, China, and Japan, the work that became *Revolution and Rebellion in the Early Modern World*.[9] During these years, I also had the opportunity to visit the Demography program at the University of California-Berkeley, where I joined the Berkeley-Stanford Demography group led by G. William Skinner, and which often included Jan de Vries.

At one point during this research, I attended a meeting of the British Economic History Association. Although I was studying countries in a period prior to industrialization, I nonetheless thought I should at least know what the latest scholarly view was on the origins of industrialization, so that I could anchor my work by knowing what followed. I thus asked half a dozen different scholars who were the leaders in this area what had caused the industrial revolution, only to be astonished that I received six very different answers. It was the advent of science, I was told by one; the revolution in agriculture that freed labor and boosted consumption, I was told by another. A third stressed the revolution in overseas trade and imperial conquest; a fourth cited as the key cause the changes in finance that arrived with the Bank of England and the national debt in the late seventeenth century. A fifth claimed that it was Britain's lead in literacy and mechanical craftsmanship; a sixth said there was no particular reason it happened in Britain at all, as it could easily have happened in France or Germany as well, anywhere that reason and learning were advancing as they had been in Europe since the 15th century.

None of these answers seemed to me wholly convincing, and none made any reference at all as to why science, or agricultural advance, or overseas trade, or craftsmanship or literacy could not have reached very similar levels in leading centers of civilization outside of Europe. So I made a mental note to myself that this problem seemed unsolved, and that I would someday return to it once I had completed my comparative work on revolutions.

That work, however, only deepened the puzzle, as the main finding of my research was that the great revolutions of the West and the great rebellions of the Middle East and China seemed rooted in very similar processes of population growth, urbanization, limits on agrarian output, shifts in prices and social mobility, and the responses of monarchic/bureaucratic administrations, that could be shown to resemble each other very closely across Europe and Asia. I thus returned to the broad comparative study of economic and political change in world history looking for clues as to why industrialization might have occurred only in Europe and not elsewhere.

In 1989, I moved from Northwestern University to the University of California at Davis.

I was again fortunate, in that the University of California also had a center of cliometrics, in the all-UC Economic History Group led by Alan Olmstead, Peter Lindert, and Gregory Clark. I was also fortunate that G. William Skinner, and his wife Susan Mann, the distinguished historian of gender and family relations in late Imperial China, moved to UC-Davis at the same time that I did. My move to California also brought me into contact with the outstanding social and economic historians of China working at UCLA (Philip Huang and Richard von Glahn) and at the University of California at Irvine (Kenneth Pomeranz and R. Bin Wong). I also began to receive invitations to conferences of historians interested in my approach to comparative history.

European scholars now were also taking an interest in building bridges between the new cliometrics and new global narratives of world history. Two groups, partly overlapping, were particularly interesting. One was the editors and contributors to the *Journal of the Economic and Social History of the Orient*, led by Harriet Zurndorfer, a very distinguished social historian of Imperial China at the University of Leiden. The second was the newly formed *Global Economic History Network*, formed by Patrick O'Brien of Oxford and the *London School of Economics*. O'Brien had been one of the leading historians of British Imperialism, and a pioneer in using cliometric techniques to question the value of the Empire for British economic growth. Both Zurndorfer and O'Brien brought together truly international groups of social and economic historians, drawing on colleagues in Turkey, China, India, and Japan as well as across Europe and the United States and Latin America.

Also in the late 1980s and the early 1990s, Jerry Bentley at the East-West Center in Hawaii was creating a new journal – the *Journal of World History* – and a new professional association: the *World History Association* (WHA). This was the spearhead for a larger movement among professors of non-European history and secondary school teachers to create a curriculum for high school and college teachers that would give a better balance to the study of western and non-western history. In an

increasingly globalized world, this was seen as vital to prepare students for careers that would likely extend across continents. Books and courses on world history began to shoulder aside the older "Western Civilization" story, which had emphasized European history from the Greeks to Western Imperialism, and which had dominated the study of long-term history in Europe and the New World.

Thus in the 1990s, we seemed to be in an entirely different world from the 1970s and 1980s. Serious studies of comparative patterns of world history, based on quantitative data as well as detailed social and political histories, was expanding in economic history programs and even returning to history programs. It must be said that the most prestigious, mainstream history departments in the United States and Europe – from Berkeley to Harvard, from Princeton to Michigan, were moving in a different direction in this period. They had moved away from the Marxist-inspired social history that had been dominant in the 1960s and 1970s, taking a 'cultural turn.' The leading figures of European history were focused on cultural history – the study of *mentalités*, political culture, education, sexuality, literary products, and family and village life. It was mainly in the less traditional research universities – places like the University of California campuses in Davis and Irvine, Ohio State University, the University of Hawaii, the University of the Pacific (in Stockton, California), and the University of British Columbia – that the sweeping views that Hodgson and McNeill had developed in the 1960s at the University of Chicago were being joined to a new generation of more precise and detailed social and economic history, in an effort to better pin down the precise trajectories and characteristics of long-term change.

The annual meetings of the World History Association and the Social Science History Association, and then the new European Social Science History Conference, became forums where economic historians, historical demographers, comparative-historical sociologists, anthropologists and social historians – from all countries and specializing in the study of all societies around the world – came together to exchange ideas. World history, by the late 1990s, was becoming not only a field of scholarship, but something like a social movement in which new adherents were actively joining and recruiting and promoting a distinctive approach in study and teaching. It was at such meetings that I came to better the know the work of scholars such as Timothy Brook, Dennis Flynn, Arturo Giráldez, Peer Vries, Floris Cohen, and many others.

One particular meeting, organized by Harriet Zurndorfer at the University of Leiden in 1996, was particularly influential for me. This small conference focused on commerce in Asia, and included experts on medieval Java, ancient Mesopotamia, and Roman Egypt, as well as the more recent and (to me) familiar areas of sixteenth and seventeenth century India and China. As I carefully read the papers

for this conference, I was stunned to realize that there was virtually no difference in the practices being described in all these eras and places. The specific products might be different, but merchants were concerned about how much they paid for materials and for labor, what were the costs of transportation, and how much profit they could retain. The documents showed the same concern for distant markets, prices, and profits whether five or ten centuries before Christ or twelve or eighteen centuries later. The documents were also similar to those of 16th century London merchants and fifteenth century Genoese investors.

It thus became clear to me that it was a fundamental error to characterize world history as divided into a 'modern' era of capitalism, and a 'pre-modern' era of predominantly agrarian production and local trade. The patterns of capitalism seemed to go back as far as civilization, if one looked in the right places. Of course, I was well aware of Max Weber's distinction between 'primitive capitalism', based on short-term pursuit of trading profits, and 'modern capitalism', in which an ethic of rationally-organized enterprise and accumulation as a virtue guided behavior. Yet there seemed to me nothing primitive about the way in which Roman landlords in Egypt used early season flood data to vary their plantings and plan their sale dates to reap the maximum profits, or the way that Chinese merchants instructed their sons to carefully accumulate their gains to improve their family's position and not to squander them.

I thus reviewed the papers in this conference in an article that appeared in 1998, *The Problem of the 'Early Modern' World*, in which I argued for a new conception of world history. I argued that whether one looked at the activities of the Atlantic merchants of the seventeenth and eighteenth centuries, the Chinese merchants of Ming China, or the Muslim merchants of southeast Asia and the Indian Ocean in the thirteenth and fourteenth centuries, they had much more in common with each other, and with the merchants of Roman and even Assyrian times, than they did with the industrial producers of the late 18th centuries and later. All engaged in long-distance trade, rationally analyzed their markets, sought to obtain their products at the best price while guarding their quality, and aimed to maximize and accumulate profits. They even varied their designs and materials to meet demands in specific markets. Yet all were living in a completely different world than the manufacturer of the late eighteenth and nineteenth century, who relied on mechanized production in factories using steam power, constant innovation in production processes and materials, and making profits by producing goods more cheaply, rather than trading them more wisely, than competitors.

I therefore argued that it was misguided to divide world history into 'classical' and 'medieval' periods on the one hand, and 'Renaissance', 'early modern' and 'modern' periods on the other, in which the latter three periods, and especially the last

two, were seen as kindred or natural phases of commercial and industrial expansion, in contrast to the agrarian sameness of the classical and medieval times. This seemed fundamentally wrong, given the evidence I had seen. Rather, it seemed to me that all periods prior to the 'modern' (that is prior to 1750 in Britain and the Netherlands and perhaps 1800 to 1850 everywhere else in the world), were fundamentally similar in that, while quite rational and accumulative, they relied entirely on human labor, natural products, and clever trading to supply society with goods, while after 1750 there was a sudden shift to a world in which applying effort and insight to improving the productivity of manufacturing, and the use of machines and engines, became central to economic growth.

I soon realized that this insight recapitulated the argument that E.A. Wrigley had made ten years earlier, in *Continuity, Chance, and Change*.[10] In that book Wrigley argued that England had been the leader in moving from an 'organic' society based on human and animal labor and agricultural products to an 'inorganic' society based on energy from mineral fuels (mainly coal) and materials and products made from minerals (mainly iron). When I had first read Wrigley's book, I thought he had just found a clever way to restate the old idea of the industrial revolution as being all about iron and coal. But I now realized he had identified something far more profound. Still, it was not merely the presence or use of coal and iron that enabled Britain to become an 'inorganic' economy. Many other societies had the same materials – even China, which had an early iron boom in the 12th century and continued to use coal for heating up through the 19th century (China remaining the world's largest coal producer to this day). What made Britain unique, and enabled it to increase its use of coal and iron many times over in the late eighteenth and nineteenth centuries, was its development of steam engines that converted the energy from coal into useful motion (including the motions of pumps to clear mines and giant bellows and hammers to forge and shape iron). It was thus the machines, and not merely the materials, that created the dawning of a new 'inorganic' age.

Excited by these ideas, I organized a conference at the University of California Davis in 1999 that brought together both traditional historians and economists and sociologists and historians interested in world history. We there debated (sometimes angrily) over how much similarity there was between pre-19th century Europe and pre-19th century Asian societies, and what changes, and when, constituted a distinctive shift toward the 'modern.' It was striking to see how deeply attached many historians were to the idea that European history had been dynamic, capitalist, and thus distinctive from at least the early Renaissance, if not the high Middle Ages, and who insisted that Ottoman and Chinese political and economic development had been far more static.

By contrast, the new generation of Chinese and Ottoman historians insisted on great advances in agriculture and trade in those regions from the fourteenth to the seventeenth centuries, as well as sophisticated large-scale commercial operations, elites open to heterodoxy and change, and living standards and government social programs on par with those of Europe.

The gathering was so contentious that no group volume could be assembled, but individual scholars were strongly motivated to produce work to defend their views. I also became more deeply aware of how close my views were to those of R. Bin Wong and Ken Pomeranz. Wong's book *China Transformed* was one of the first serious comparative treatments of Europe and China to note the relative sophistication of Imperial China's state organization and economy compared to that of seventeenth and eighteenth century Europe, while Pomeranz's book *The Great Divergence* made the most powerful argument yet that the relative wealth of 19th and 20th century Europe compared to Asia (the great divergence of the title) was a very late development, emerging from late eighteenth century conditions in which the levels of material life and economic activity in the most advanced regions of Europe and China were essentially the same.[11]

These key years, from 1997 to 2000, bracketed by the volumes by Wong and Pomeranz, saw an explosion of work from our group of connected world history scholars, which all made a similar claim: that Europe had not been economically or technically nor bureaucratically more advanced than China as late as the mid-eighteenth century. This work included my articles on *The Problem of the Early Modern World* and *The Rise of the West – or Not?*; Andre Gunder Frank's *Reorient*, which argued that China's being overtaken by the West economically was just a late and temporary event; the volume edited by Dennis Flynn and Arturo Giraldez (of the University of the Pacific in Stockton, California) on *Metals and Monies in an Emerging Global Economy*, which argued that far from Europe's exploitation of American silver being the driving force in the 16th century world economy, American production was just part of a global monetary regime driven by China's demand for silver, drawing on silver production in Japan as well as Latin America; and Robert Marks' (from Whittier College in California) book *Tigers, Silk, and Silt*, a brilliant study of Imperial China's expansion into Guangdong showing its commercial integration, exploitation of new materials, and agricultural innovation, which produced new wealth and paralleled European colonial expansion overseas.[12]

Because so many of these scholars (indeed all except for Frank) were based at California institutions, I labeled this group the "California School", and identified all of us as espousing a view that Europe's surpassing of other regions in material wealth was *not* a long-term process rooted in the general superiority or greater dynamism of European culture, organization, resources, politics, agricultural pro-

ductivity, financial acumen, or capacity for overseas trade compared to that in leading areas of Asia. Rather, we argued, Europe's leap forward was relatively late, came from similar conditions as prevailed elsewhere, and was more a matter of contingency and even luck than any systemic advantages over other major civilizations in Asia.

Of course, such a radical new view promotes more controversy than agreement, and that has been the case. The responses ranged from highly critical to skeptical.[13] Yet more and more scholars have added important works built on serious consideration of the California school viewpoint,[14] or found that they had already come to believe it.[15] At the very least, an interesting dynamic has emerged, with an increasingly established field of world history, now including two major journals (the *Journal of World History* and *Journal of Global History*), doctoral programs in major universities, and a rapidly expanding body of research aiming to more thoroughly document, measure, and compare the precise nature of the changes in agriculture, economic growth, investment, living standards, political organization, military capacity, technology, and other factors across regions.

Important scholarly books addressing world history by a number of distinguished scholars have enjoyed great commercial success, such as those of David Landes of Harvard, Jared Diamond of the University of California in Los Angles (UCLA), and Gregory Clark of the University of California at Davis.[16] At the same time, intellectual networks and collaborative projects, such as the Global Price and Income History Group (headquartered at UC Davis), have grown worldwide. World history is now a world project, with crucial contributions coming from scholars from Tokyo and Taiwan, Buenos Aires and Bergen, Cambridge and Konstanz, Davis and New Delhi, Istanbul and Vienna.

For myself, the last eight years have left me struggling with a further problem, for I was not at all satisfied with the California School's explanations for the 'great divergence.' I accepted that that divergence was late, and not in any way predetermined. Yet I still felt a need to find a better explanation of how it arose. I have been focusing on the question of why Britain – and at first only Britain – become the leading center of machine invention and use. Was this the result of broad European trends that just emerged first in Britain, or were there specifically British antecedent conditions that produced this outcome? Why did other major societies – if they had the mechanical skills, the capital to invest, the long-distance trade and transport needs – not also develop in this way?

The notion that only Britain had sufficient access to coal, or to colonies, seems to me mistaken; the differences are minor, and the fact of access does not explain the creation of unique adaptations to make use of the resource. Britain had no cotton, yet created a textile revolution based on mechanization of cotton production; so

why should the presence or absence of coal be determinative of a coal-using power revolution? If China had developed coal-based industrialization and mechanization of cotton first, it would have been all too easy to explain that outcome based on its easy access to surface pit coal throughout China and its long history and plentiful crops of cotton. Explaining why Britain had a coal and cotton-based industrial revolution is much harder when most of its coal was in deep shafts that could not be mined without mechanical pumping and ventilation and hauling to the surface, and its cotton all had to be imported from distant lands.

So I have sought to understand the processes that were used to exploit those materials. Why did Britain first develop and apply steam engines to production? Where did steam engines come from, how did they develop, and how were they adapted to production? Why did people even believe that the first steam engines – clumsy, inefficient, irregular in motion – could ever be used to produce the smooth, regular, rotary motion necessary to turn machines for spinning fine fibers into quality yarn? Or why would they persist for the entire century that it took from the time the first steam engines were deployed in mines until they were later adapted to powering spinning machinery?

To answer these questions, I have had to spend a good portion of the last eight years studying the history of science and technology, a rather different direction than I had expected to go when I started my studies. The beginnings of my answers are presented in a paper titled *Efflorescences in World History* and two books – *Why Europe?* (2008) and *The Rise of the West and the Origins of Modern Economic Growth*.[17] These have forced me to reverse my ground, at least partially. While I still believe that there were no major material, political, or economic differences or advantages in Europe before 1750, there *were* major differences in intellectual history and scientific insights that go back to 1600. These put Europe first, and then Britain, on a very different and distinctive intellectual path, such that by 1750 the world of science in Europe had been completely transformed, while nothing comparable occurred in any of the other major civilizations of Asia. Indeed, Asian societies appear to have narrowed their vision and reinforced their traditional notions of natural philosophy and progress during precisely the same period, from 1650 to 1750, that Europe's ancient traditions in natural philosophy were overturned and transformed.

Here again I have been fortunate. My former colleague at Northwestern, Joel Mokyr, had also come to focus on intellectual trends during the European enlightenment as a prime mover of European Industrialization (Mokyr 2004, 2009).[18] He introduced me to the work of the brilliant historian of science Margaret Jacob at UCLA. I also benefited from discussions (thanks again to the Global Economic History Network) with Nathan Sivin, the leading historian of Chinese science today, the

remarkable Dutch historian of technology Floris Cohen, and other distinguished historians of science: John Henry from the University of Edinburgh, Robert Westman of UC San Diego and Steven Shapin (then of UC San Diego, now Harvard).

While these scholars certainly disagreed among themselves, and indeed showed me how divided the field of science studies had become, I began to perceive a pattern in the events that led up to the discovery of atmospheric pressure and the atmospheric engine – the critical earliest form of the steam engine. First, it became clear that while all the world's major civilizations had made great and similar advances in mechanics, logic, mathematics, astronomy, chemistry, botany, and agriculture, *only* Western Europe had discovered the phenomenon of atmospheric pressure.

All societies of course were familiar with winds pressure and had used it for centuries for sail power and windmills, and all societies were familiar with the power of expanding steam. But no societies could use either wind or steam expansion to exploit coal or wood resources to create motive force through machines. Windmills of course had driven machinery, including pumps, sawmills, grinding mills, and diverse other operations. Yet such machinery remained dependent on the fickle winds or on human or animal power, and could in no way tap the vast resources of wood or coal for energy. The latter energy sources were crucial for providing heat: whether to cook food, warm a residence, or power industrial processes such as smelting, glass-making, firing ceramics, or brewing. But the great leap of being able to convert heat energy to motive power for machinery remained beyond any society's grasp.

The expansion of steam against atmospheric pressure was capable of small tricks, such as spinning a mounted globe or opening a heavy door. But heating steam to a temperature and pressure where it could do repetitive work against atmospheric pressure was impossible; given that atmospheric pressure is roughly 15 pounds per square inch, doing work that would accelerate objects against that force would require steam pressures of several times atmospheric force, a pressure that pre-industrial metallurgy could not contain without bursting. Indeed, even in the late 18th century in Britain, James Watt – the inventor of the modern steam engine – refused to build engines that would work at any greater pressure than just above that of the atmosphere, for fear of fatal and machine-wrecking explosions.

The key to building practical steam engines was realizing that either the pressure of the atmosphere itself, or of heated steam, could do useful work when *pressing against a vacuum*, as in an evacuated cylinder. The problem with the science of most civilizations, however, was that the presence of a real vacuum in nature was either unknown or argued to be impossible. Even in Western Europe, the dominant school of natural philosophy, based on Aristotle, denied that a vacuum could be created and sustained on earth.

How then was it possible for steam engine technology to develop? That is too long a story to relate here, but the main parts were: (1) a repudiation of Aristotle's physics in favor of Baconian empiricism and Cartesian mechanics, which occurred all across Europe in the course of the sixteenth to eighteenth centuries; (2) an embrace of empirical experimental programs as a superior pathway to nature knowledge, with precedence over pure logic, as championed by Robert Boyle and Isaac Newton in England; (3) the publicizing of Boyle's experiments with vacuums, Newton's optic and mechanics, and the work of Denis Papin with steam, all of them associated with the Royal Society of Britain, to a broad stratum of mechanics, instrument makers, businessmen, and professionals as part of the spread of a scientific culture in eighteenth century Britain; and (4) the willingness of British entrepreneurs, inspired by Bacon's visionary writings, to seek the aid of scientists and instrument-makers in applying the latest scientific concepts and instruments to the improvement of machines and production processes. I argue that it is this marriage of engineering and entrepreneurship that created the process of modern economic growth, replacing the older search for greater efficiency in using known processes and gains from trade with the search for new materials and processes to create new products and new markets.[19]

Once the evolution of the steam engine was underway, it gave first Britain, then Europe a great advantage in the deployment of energy for processes ranging from basic production to warfare. Exploiting not only steam power, but the process of rapid scientific discovery through empirical research programs into organic chemistry, electricity and radio and semiconductors, the West gained advantages in production of fertilizers, power machinery, transportation, communications, and computing that powered its 19th and 20th century advantages over other world regions.

However, this was clearly *not* a result of any long-term superiority or accumulated advantages over other civilizations. The West's classical (e.g. Greek) science and Christian religion had argued as strongly against the presence of vacuums, universal gravity, and evolution as had the traditions of any other great civilization. What empowered the West was that it was the first major civilization to set aside its classical and religious inheritance, at least regarding natural science, in favor of knowledge gained from disciplined empirical research programs using scientific instruments. As we have seen, any other civilizations that adopt such programs, and couple them to engineering efforts and pursuit of modern economic growth, can narrow the West's material advantages, as Japan, then Korea and Taiwan and China and India are now doing with vigor.

When studying world history, I have come to respect the highly distinctive approach of many different scholars, and tried to learn from all of them. Some

are specialists in particular products (textiles, engines, crops), other are specialists in particular processes (state formation, overseas trade, finance, changes in wages and prices, population, the history of philosophy or science), while still others are specialists in connecting changes in a particular region or time-frame. The result is that world history has a tendency to throw up dozens or even hundreds of puzzle-pieces – bits of data, regional narratives, biographies, sequences of events and relationships – that must somehow be fitted together in ways that make sense. Far from being a set of established national narratives, which can at best be compared like mounted lab specimens, world history consists of so many interlocking actions and events that they are like a prism or a kaleidoscope, presenting shifting views and images depending on the angle from which one examines them.

Given my training as a comparative-historical sociologist, my approach is to try to fit these pieces together in ways that respond to specific questions: why did certain changes or departures happen at a particular time, and a particular place, and not elsewhere? Because I have come to lean heavily on economic data, I also ask what evidence can be used to support certain answers and to falsify others. At the same time, because I have learned from historians that history is not merely a set of determinate processes that can be formally modeled, but includes contingent outcomes arising from the interaction of many individuals and groups that produces unintended and unforeseen consequences, one must be open to seeing a given outcome as possibly produced by many different causes or pathways. Ideas thus need to be tested by counterfactual analysis, as in my essay *Europe's Peculiar Path: Would the World be Modern if William III's Invasion of England in 1688 had Failed?*[20] We need to examine whether if one pathway for change was closed, which others remained open? Why where these pathways seemingly available and taken in some places, but not in others?

To summarize, my approach to world history is not to seek a master process or overarching narrative that subsumes all national histories into a set of specific patterns. Rather, my goal in world history is to try to identify why different societies, at different times, resembled each other in some ways but differed greatly in others, and to identify the most likely consequences of those similarities and differences. My view is perhaps closest to that of evolutionary biologists, who do not believe evolution has a necessary end, only that it has understandable mechanisms, which can best be discovered by close examination of the characteristics of species and their development and interactions with each other and their environment over time. There is in fact now a branch of world history analysis that explicitly approaches world history through the use of formal physical and evolutionary models.[21]

We are fortunate that globalization has not merely impacted production and trade, but has also benefited scholarship. Today's scholars are able to utilize bod-

ies of data, and organizations and forums and international networks of scholars engaged in generating historical analysis, that did not exist just a few decades ago. I would say that in the 1990s, a new golden age in the study of world history had just begun, and is still gathering momentum.

Notes

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