

Useful Knowledge and Economic Change: What are We Explaining?

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Stating the Problems

Science and technology are basic to economic growth. Indeed, some would argue that they only true sources of economic growth. In contrast, all other sources of growth are finite, as classical economists recognized. Gains from becoming more efficient and expanding markets cannot continue indefinitely. But for their part, classical economists also did not assume that gains from science and technology could be infinite. Nor do all analysts today believe that science and technology will continue to yield new increases in productivity that will create ever more value. More specifically, some observers believe that we could experience resource bottlenecks and ecological problems that may severely qualify if not seriously undermine prospects for continued economic growth.

Uncertainties about the roles of science and technology in our present and future world inevitably influence the questions we bring to historical issues. Yet even those who counsel caution concerning predictions of future directions of scientific change and the impact of science and technology on economic possibilities must recognize that the application of new scientific and technical knowledge between the late eighteenth and late nineteenth centuries fundamentally transformed economic possibilities. For economic historians, science and technology is one clear area where economic practices in commercially sophisticated parts of Asia and Europe clearly differed. And while the important contrasts between China and Europe concerning the exploitation of frontiers that Kenneth Pomeranz has

demonstrated in several innovative ways must surely have contributed to the economic divergence of the late eighteenth and early nineteenth centuries, these differences were clearly not sufficient and perhaps not even necessary to motor the subsequent degree of economic divergence that took place. Europeans exploited new scientific discoveries and technical opportunities very effectively and people elsewhere, to be successful economically, had to gain access to these developments and take advantage of the new production possibilities they afforded.

There are at least four distinct questions and problems regarding science and technology that scholars interested in economic history have conflated in multiple ways. First, there is the question of how Europeans created the rapid advances in science and technology and have many of them apply to the economy quite swiftly. Secondly and separately from the origins issues there are thorny questions regarding where and how these technologies moved beyond their immediate environment of emergence and with what economic effects. While this issue is understood by at least some analysts to be distinct from the first question about “origins,” a third question is conventionally confused with the first, namely, how can sophisticated pre-industrial economies develop and apply useful knowledge—in particular, is the European sequence of changes the only set that was possible or plausible? We tend to assume or even claim that if we explain how Europeans developed their useful knowledge we have accounted more generally for how science and technology can come to play important economic roles. This is not in fact true unless we can argue persuasively that there was only one conceivable path toward useful knowledge that would lead to the kinds of changes that took place and that there aren’t alternative clusters of changes that could have had similar impacts. It is of course difficult to imagine alternatives and as long as we assume that

explaining the European case is equivalent to explaining how useful knowledge had to develop in general there is little intellectual incentive to consider the more complex and difficult problem. There is a fourth question that haunts China specialists and influences others as well. It is associated with one of the truly great minds to have devoted decades of study to Chinese history and culture, the late Joseph Needham. Put most simply the Needham question is “why, with its centuries of technological achievements didn’t China develop modern science?”

In this paper I begin with intrinsic problems with the formulation of the Needham question. I’ll suggest a general reformulation of what is to be explained with respect to China and then consider its implications for what we ask regarding European science and technology between the seventeenth and nineteenth centuries. Framing the issues of the relationship between science, technology and economic change as an empirical one for Europe I will then move on to consider how the issues of the relationship between science and technology to economic activities changes in the nineteenth century. If my discussion of these three questions (the first, second and fourth on my initial list) has any merit, I might be able to persuade you that we need to think more seriously about the third question—the generality of our understanding of the relationships between science and technology to the economy in China and Europe between the early sixteenth and late eighteenth centuries.

The Needham Question Once Again

After doing research in biochemistry for nearly twenty years, Joseph Needham developed a fascination about Chinese science, devoting most of his time and energy after 1937 until his death in 1995 on leading the most

monumental research effort on a Chinese subject undertaken during the twentieth century. Needham's presentation of the many discoveries and inventions made in China before 1500 coupled with the evident material wealth and sophistication of Chinese culture that put parts of the empire well beyond Europe's most economically developed areas encouraged scholars to wonder why China could achieve produce these successes and then fall behind Europe at a later date. The contrast is often captured by pointing out how European used the Chinese inventions of gunpowder, paper and printing, and the magnetic compass in ways undreamed of by the Chinese themselves. As a well-educated Englishman of the early twentieth century Needham recognized that a serious answer to these large questions lay outside his domains of professional expertise. At the same time his wide reading allowed him to articulate sweeping views about politics, economy and society that were shared by many intellectuals concerning major differences between China and Europe. These views with their recent intellectual roots in Marx and Weber shared a common conviction in "progress" as a forceful dynamic potentially present everywhere but in practice blocked or disrupted by obstacles posed by politics and culture. Needham suggested that China's bureaucratic system of scholar officials stifled merchant initiative, while European feudalism made possible the emergence of a commercial class under competing European states, the combination of which encouraged both scientific and economic developments. Outside specialists on the history of science, Needham is probably at least as well known for these broad stroke claims comparing Chinese and European political and economic situations as he is for the detailed reconstruction and analysis of science and technology that he and his collaborators achieved. Yet his general views on Chinese politics and society were not the product of major research efforts as his views on

science and technology were.¹ Needham posed a sweeping question or puzzle that is a short hand for some major contrasts between Chinese and European histories. The first half of the puzzle about why China was ahead is answered by Needham more in terms of "what" they did with respect to specific areas of science and technology and "how" they made their advances. This translation of a "why" question into questions about "what" and "how" create problems of history that scholars can address. The second "why" question is far more problematic—"why didn't China have a scientific revolution?" This is a problematic question because it asks why something did not happen. It is both too easy and extremely difficult to explain a non-event. If you assume the event requires certain preconditions or causes, then you can easily eliminate the possibility of the event by showing the absence of any one of its asserted preconditions so the task of explanation is basically trivial; if however you consider that you are looking for "causes" for a non-event as opposed to the absence of causes for an event, the task becomes very complex since just defining a "non-event" like "no Scientific Revolution" is in fact very difficult. The problem may have become even more difficult with recent approaches to the "Scientific Revolution" in European Studies—as Steven Shapin introduced his 1996 book *The Scientific Revolution*, "There was no such thing as the Scientific Revolution, and this is a book about it." (Shapin 1996) Are China specialists then to seek an explanation for the non-presence of a non-existing phenomenon? Questions built upon the non-presence of a non-existing phenomenon do not seem likely to encourage the formulation of research hypotheses.

¹ A more recent example of people citing the authority of an accomplished scholar for ideas that are in fact common and problematic, not to mention quite distinct from the nature of the scholar's actual achievements is Jared Diamond and his *Guns, Germs and Steel*, the last chapter of which deals with intra-Eurasian comparisons and invokes arguments and evidence very different from those used for the book's main themes.

Fortunately, we may not have to follow this line of reasoning to develop our research.

The Relevance of Recent Work in Economic History

Many European economic historians gave up their “revolution” many years ago. Some observed the absence of persuasive quantitative indicators of major changes in a short period of time that would merit the term “Industrial Revolution.” Others argued that the concept of “revolution” suggested much too great a rupture in economic practices and an unwillingness to recognize the many small and gradual changes taking place over a far longer period of time that prepared parts of Europe, England in particular, to experience the cluster of change, many of which crystallized in activities of particular industries. One positive result of this kind of work has been to increase our understanding of many particular changes that contributed directly and indirectly to the much larger set of transformations previously known as the Industrial Revolution.² These benefits do come with intellectual costs, externalities that need highlighting. First, we often assume that phenomena found in Europe that we do not know of elsewhere are unique to Europe and thus play their unique roles in making for the pattern of change we observe. As social scientists, we then make a second assumption that the European pattern is the universal pattern of change so that if there is to be change of the sort we are studying, it has to happen as it did in Europe and thus if we don’t find this or that phenomenon elsewhere, the universal pattern is absent.

² We could put the phrase in quotes “the transformations previously known as the Industrial Revolution” but that would sound a bit too much like “the artist formerly known as Prince.”

Asian economic historians have addressed these negative intellectual externalities. To varying degrees I understand most members of our GEHN network to agree with some if not all the observations about patterns of commercial expansion in Asia and Europe that call into question the uniqueness of many features of early modern European economic history. I won't rehearse those arguments and evidence, parts of which one can find in "California School" writings. I will, however, highlight a subject that I've raised in earlier GEHN meeting papers that should be relevant to our concerns at this meeting.

The new institutional economics has had the salutary effect of encouraging us to evaluate the effectiveness of institutions for fostering economic growth. Douglass North and many influenced by him have argued for the importance of particular institutional clusters for reducing transaction costs and thus promoting economic expansion; key for North are contracts and courts. Other European economic historians have questioned the historical accuracy of North's characterization of the use of contracts and courts and this matters much more generally for it reminds us that there can be alternative institutional mechanisms for achieving similar economic results. Regarding transaction costs specifically, the expansion of long-distance trade in China appears to have depended on European-like contracts and courts very little if at all; instead, trust between buyer and seller was achieved through kinship and native place networks. Note that these mechanisms can be at work in very different political settings—in Europe we think of native place networks mattering for groups operating across fragmented political spaces whereas in China they worked both within a vast empire as well as across regional spaces linking the south and south-eastern maritime portions of the empire to Southeast Asia. My point here is two-fold: first, the frequency of specific mechanisms to reduce

transaction costs can vary both within Europe and between Europe and China (and other parts of Asia); second, these mechanisms work in distinct contexts which enhance or limit their effectiveness. For China, the spurts of commercial growth between 1400 and 1800 took place amidst domestic stability and security which the Chinese government was able to supply through strategies and policies quite different (and more involved) than those any European state could conceptualize let alone implement in this period. It thus seems plausible that Chinese government contributed to the conditions for commercial expansion in important ways even if these were not the same as those identified by North and others as important in European cases.³

The example of different institutional repertoires for reducing transaction costs suggests a parallel possibility in the study of science and technology, namely the existence of different institutional frameworks to encourage the production and/or dissemination of useful knowledge. I'll return to this subject in a later part of this paper. First however I want to remind us that the strategy I think much recent economic history encourages us to follow, namely of disaggregating complex historical processes of change and seeking to identify a range of causal linkages that are similar and different in various parts of the world, continues to meet vigorous and vocal opposition. Recent critiques of Ken Pomeranz's *Great Divergence* by Philip Huang and the team of Robert Brenner and Christopher Isett affirm the persistence of a faith in sweeping contrasts of China and Europe that do not engage new empirical results so much as they label them irrelevant to earlier theories of historical change, in their case those associated with

³ In a book project that Jean-Laurent Rosenthal and I believe we will finish "soon," this and related subjects are addressed in greater detail.

Marx's focus on the social relations of production.⁴ Analytically similar strategies could be mounted from a Weberian perspective as well. What is problematic about the Huang and Brenner-Isett critiques is not that they have a Marxist base or that they identify differences between Jiangnan and England. The real problems are their unwillingness to show how their particular differences concerning agrarian social relations and commerce they highlight affect other features of economic change; they abandon causal explanation quite swiftly in their assembling of multiple differences between Jiangnan and England that leads simultaneously to a level of abstraction that seems like “theory” and a level of historical particularity that in the end simply tells us that England and Jiangnan are two different places. Their labours can serve as a caution to us as we look more deeply into issues of science and technology—we will want, I suggest, to avoid conflating narratives of change in one place with general theory, theory which in turn explains differences in other places without engaging empirically much of what is in fact happening elsewhere. We want to guard against studies of European science and technology leading to assumptions previously quite common among economic historians that if they identified European dynamics of commercial growth they were locating general principles against a backdrop of assumed stagnation elsewhere. The realization that at least some features of European commercial growth were shared by other societies has narrowed the search for significant differences to explain the ‘great divergence.’ Science and technology are great candidates because we know how important they were in the Industrial Revolution and subsequent changes in production.

⁴ Huang 2002; Brenner and Isett 2002; Pomeranz 2002; Wong 2003.

European Science and Technology: What We Know and What We Believe

Economic historians generally agree that science and technology have been a crucial source of economic growth. This is most obviously true in the past century and a half. But the general proposition that technological changes made possible increased economic growth and social change occurs more generally. For instance, in a widely admired book *Revolution in Time*, David Landes makes an argument for the special importance of clock and watch making directly and indirectly for technological and economic change. He appeals to the arguments made about clock making in his *The Wealth and Poverty of Nations*, the most well-known work in economic history broadly conceived of the past two decades at least. Landes tell us that clock and watch making were important because they allowed Europeans to conceive time in a new manner that facilitated new kinds of economic practices. These activities further demonstrated and developed the fine motor skills and precision instrument making that Europeans put to great effect in a broader array of technical tasks. The inability of others to develop clock and watch making skills was symptomatic of their limited abilities to undertake technological changes needed for economic development. The argument appeals to common sense and Landes writes with energy and engagement. But is he right? Let us leave aside the problems of pinning down more precisely what Europeans, when and where began to use clock measurement of time to conceive time for economic purposes in new ways and the demonstration of how the skills used in watch and clock making actually transferred to other crafts. Consider simply that we know, thanks to Catherine Pagani's work on clocks in China that these European inventions were not only at the Ming court by the late sixteenth century but that by the eighteenth century Chinese clock makers and watch

makers in Suzhou and Guangzhou produced and repaired time pieces for a domestic market. Chinese were by no means unable to learn the skills needed to make clocks well in advance of the dramatic economic changes of the Industrial Revolution.

Alan Macfarlane and the late Gerry Martin more recently have made an argument for the importance of glass. Their glass story has some similarities to Landes' story about clocks. They show a sequence of changes after the thirteenth century in which glass is given star billing. There is little question that glass was important in Europe but how do we explain the failure of glass to take on a similar importance in Asia or its economic significance? Chinese had access to glass via the Silk Road, though not to the kinds of glass that Macfarlane and Martin spend most of their time recounting. Having access to glass, as peoples in Asia did, provided no guarantee that they would develop its uses. The glass story for Macfarlane and Martin then is really a story indicating a much larger set of European features. How do we decide how many of these features were distinctive and then what economic differences they made? We need both more empirical research on other places and more specific causal explanations for how glass of certain type mattered, in particular how to put particular causal mechanisms involving glass into larger contexts. Without such efforts we will find it difficult to assess how glass or Landes' clock making mattered to economic change more generally. Stories of European science and technology can end up much like stories of European commercialization—they end up describing what happened in Europe rather than explaining what is general about these changes.

Some scholars who have examined Chinese science and technology directly appear to have assumed that the explanations of European cases are general because they expect that if they can find something “missing” in

the Chinese case they can explain the absence of the changes we observe in Europe. Two kinds of economics arguments have been used in this manner. Mark Elvin has suggested that China's "high-level equilibrium trap" was due to a lack of demand for technological change because of abundant labour supplies and because Chinese became less curious about the natural world, turning introspective after the fourteenth century. Justin Yifu Lin suggests that labour-saving technological change could still make economic sense even in an environment of cheap labour and therefore stresses only the supply side of technological change; he states that China lacked the institutions such as universities to produce the supply of technological change that existed in Europe. Both scholars move beyond economics into a more broadly social and cultural arena. Elvin suggests that Chinese intellectually and politically turned inward in the fourteenth century, while Lin is contrasting the very different ways in which scholarship and learning more generally were organized in China and Europe.

There are two large difficulties with these cultural and institutional arguments. First, it isn't clear how useful a general characterization of a Chinese turn away from study of the natural world really is since there continued to be a variety of efforts to study subjects such as plants, leading to major agricultural compendia after 1400. Second, it isn't clear that the Chinese didn't have institutions that could have encouraged scientific and technological knowledge. Certainly the civil service exams at times did include questions on quite technical subjects such as water control and Chinese officials were expected to have some competence in technical matters—more in fact than their European counterparts. It is easy to infer from the role of universities and their absence in China that these were crucial institutions not only empirically but theoretically as well—without universities one cannot expect the promotion of scientific knowledge: yet this

proposition as a theoretical argument suggests an inability to imagine alternatives and this seems imprudent since we could at least imagine a greater emphasis on technical subjects on the civil service exams stimulating a quest for such knowledge the equal of what universities did in Europe. The point of this and other counterfactual exercises is not to argue specifically that China could have had a “scientific revolution” just like that occurring in Europe as to make clear more generally that the institutional practices deemed important in the European case need not be necessary in more general terms for there to have been an increase in scientific knowledge.

The difficulties with distinguishing between a general explanation of technological change and its economic impacts from one that explains what happened in Europe is present in the very best work on technological change done by economic historians. Take as an exemplar the work of Joel Mokyr. *The Lever of Riches* gives a wide variety of stories about technological change and contrasts European stories with those gleaned from the far weaker English-language literatures on other parts of the world, including China. In his more recent *The Gifts of Athena* Mokyr develops a more explicit explanation of scientific and technological change and relates them more closely to economic growth and change. This is the best work linking science and technology to economic history that we have. Asking questions of this work can help us test the limits of what we know and can know and thus when our knowledge crosses over into the realm of belief.

First we can ask questions about the empirical contrasts Mokyr draws between China and Europe. China is an example for Mokyr of a more general class of centralized states. In *The Lever of Riches* Mokyr speaks of the obstructive nature of the despotic state, attributing far more real capacity to the late imperial state than it in fact warrants. In *The Gifts of Athena* he

gives a general statement of the argument: “Not all free-enterprise economies are necessarily technologically creative, and not all command economies are technologically stagnant. All the same, technological progress has a better chance in the long run in free, self-organizing market societies than in command economies. China’s technological superiority fizzled out in the centuries of the European Renaissance, and the much-feared Soviet technological advantage of the post-Sputnik years has melted away like the core in the Chernobyl reactor.” (223) Putting together in the same sentence sixteenth-century China with the Soviet Union in the 1960s as examples of command economies generalizes a bit too swiftly about similarities between the Chinese and Russian cases as well as their respective differences from market economies. Much of sixteenth-century China had a market economy; certainly there was as much of a market economy in sixteenth-century China as there was in Renaissance Europe. The general proposition about command vs. market economies, which might make some sense in comparing twentieth-century cases, seems ill conceived to characterize earlier situations.

Here is another version of the same argument later in the book “Both powerful and weak rulers can be intolerant and reactionary, but stronger rulers have more power to inflict stagnation on their economies under the guise of law and order. Could there not be a symmetric argument that the more powerful a ruler, the more technological progress he or she can bring about because by overruling the demands of special interest lobbies? There are such cases on record, but decentralized systems have tended on the whole to be more efficient than centralized ones in engendering technological progress because they did not depend on the personal judgment and survival of single-minded and strong-willed individuals. The ability to pick technological winners is never concentrated in the mind of a

single individual and is uncorrelated with political talent.” (239) Again, the depiction of centralized systems is a bit extreme, to the point of caricature about the “mind of a single individual.” It raises a second difficulty of thinking through what to expect in “decentralized” systems for we can easily imagine entrenched interest groups in decentralized systems effectively blocking the application of technological progress and even the production of such progress—indeed the Luddites remind us how long the former has existed in Western settings and the controversy over stem-cell research in the U.S. alerts us to the latter problem.

Mokyr has a more general argument about conditions affecting technological change that seems to me to be both important and mis-specified. He states that “the development of useful knowledge as a source of economic dynamics is influenced by political economy far more than is often realized. Consequently, economic development and performance were often held back by political processes that arrested the growth of useful knowledge.” (231-32) Yes, political economy matters but not only in the ways Mokyr suggests. Governments don’t simply interfere with the development of useful knowledge. Chinese officials, for example, took as part of their vocation as political leaders the assembling and distributing of agricultural knowledge knowing that promoting such knowledge would have some positive social value. They expected those people who would benefit from particular kinds of knowledge to utilize the information made available to them. They also realized that a considerable amount of agricultural knowledge is necessarily local knowledge. Peasants can only use so much outside knowledge and its utility often depends on local adaptation. Some knowledge was useful for extending arable to hill land areas while other information applied to more intensive use of land and labour resources in lowland cores. There was therefore no large-scale systematic vision of what

were the logically correct approaches for utilizing agricultural knowledge. Instead, useful knowledge came in smaller modular units that could be combined in different ways.

A second key feature of the state's role in communicating useful knowledge was the absence of a market nexus for transmitting knowledge production. Neither officials nor elites expected to make money directly from disseminating potentially useful information. Does this mean they had fewer incentives to assemble and disseminate information than if there had been a more market-oriented system? Not necessarily. A market-driven set of incentives presumes that people creating useful knowledge can control its distribution and make those who use their ideas pay for such an opportunity. The fewer the people likely to use the technology and the larger the potential gains from adoption, the more likely such a system will be plausibly effective. In scenarios where the potential users are many and the likely gains more modest per user, the costs of assigning and enforcing property rights might not necessarily be balanced by an increased amount of technological change.⁵ When the adaptations are variable and local, as they are for much of agricultural change, assigning value to what is transmitted becomes even more difficult. Without government officials and elites assembling and disseminating agricultural information as a political and social service, such knowledge would likely have travelled more slowly and covered less ground.

When it comes to specifying what “decentralization” means and in particular to identify the range of optimal levels of “decentralization” it turns out that it is difficult to pinpoint what is desired. After admitting that it is difficult to know what degree of “decentralization” is desirable, Mokyr tells us

⁵ This is difficult to predict easily since more persuasive arguments would depend upon creating production functions for technological change; while we imagine we can understand the demand side, it is very hard to specify the supply side factors.

that “All the same, some measure of decentralization is probably desirable.” (282) To reach this conclusion Mokyr actually recognizes how difficult it is to associate political decentralization or fragmentation with technological change. He recalls for instance the first half of Needham’s discoveries, namely that technological advances in China surpassed those in Europe through the twelfth or thirteenth century, from which he concludes that “The first point to be made is that although there is a correlation between political pluralism and technological creativity, it is quite clear that pluralism is neither a sufficient nor a necessary condition for technological creativity.” (279) He goes on to say, “Second, political fragmentation is no guarantee that technological creativity will persist.” (280) And he reaches a conclusion he considers problematic for others but seems to me would apply to him as well: “Third, and most serious, both Jones and North fail to fully acknowledge the enormous costs and hazards of political fragmentation.” (280) Given these three observations it is difficult for me at least to know how much faith to put into his conclusion that “some measure of decentralization is probably desirable.” This will more likely depend on specifying further the contexts within which this political feature operates. And it will also depend on making more specific what “decentralization” actually means in operational terms. If and when we do so, we are brought once again to specifying the political and social contexts within which our propositions about science and technology are claimed to hold. This gives us little necessary analytical purchase on other political and social contexts unless we can persuasively make a move from context-specific explanations to more general ones.

It may help to distinguish explicitly between two kinds of explanation. One closely resembles laboratory and experimental sciences, which make predictions about process and outcome based on particular factors within

specified initial conditions. The other is far closer to those offered in historical sciences like astronomy, evolutionary biology and geology. In these latter cases principles are developed that can be used to explain what happened. These principles are far weaker on prediction when the phenomena to be explained are like the panda's thumb or the Grand Canyon, unique events defined by the contexts within which more general principles act. I suggest that what Mokyr and others who have done the best work on the interface of economic history and technological change offer us are explanations of what has happened in Europe which give us at most limited purchase on explaining what happened elsewhere before the nineteenth-century world of scientific and technological transmission from the West to Africa and Asia. The issues of transmission, which generate the second of the four questions I identified at the beginning of this paper, deserve separate treatment, which I lack the time, space, and skill to embark upon here. Instead I turn to the problems and implications of distinguishing context-specific explanations from more universal ones.

Levels of Generality, Types of Explanation and the Need for Self-Congratulation

I suggest that the best research we have to date fails to make a clear case to explain the relationship between science and technology on the one hand and economic growth on the other as a general relationship. We generally recognize that the relationship evolves toward a tighter set of connections as we move into the twentieth century, especially the closing decades of that century. But at the same time many among us continue to search for and in some cases think we've found explanations of how science and technology relate to economic change more generally in earlier periods—that we can generalize from our knowledge about what happened

in European history to general propositions to explain what didn't happen in other histories, such as the Chinese case. From the analytical parallels to what we've learned about commercial expansions between the fourteenth and eighteenth centuries and from the difficulties that scholars have in specifying necessary and sufficient causal relationships between science-technology and the economy, I suggest we are better off assuming we have to date largely answered the first of the four questions I presented at the beginning of this paper, namely, "how did scientific and technological change affect European economic growth?" This is not the same as the third question of "how do scientific and technological change affect economic growth in general?"

I make this suggestion for both positive and critical reasons. The positive reason is that a decision to recognize our greater success at explaining the European case than the problem in general should encourage us to explore more closely the kinds of explanatory claims that are empirically and conceptually plausible for comparisons among Europe and other places such as China. The critical reason concerns the ways in which claims about European science and technology have been used to support the tone of cultural smugness and superiority that abound in a work like David Landes' *The Wealth and Poverty of Nations*. A fifth and final question we might ask is why does it matter if the way in which science and technology affected economic growth in Europe is the only way, and thus the universal or general way, in which such change could take place? How would it matter if there might have been other paths to a similar result? It seems to me that it matters for some because they want reassurance that they are, amidst multiple challenges in our contemporary world, still culturally superior. It matters more to conserve old beliefs than to create new knowledge.

Lest I be too easily mis-interpreted, I by no means am arguing that any and all work that identifies important differences between Europe and other parts of the world are acts of chauvinism—I have spent too much of my own research time engaging in comparisons to be party to such an enterprise. I am however arguing that we will make more progress (yes, I believe in intellectual progress if not all other forms) when we specify more closely the kinds of problems we are seeking to solve and recognize the level of generality that our explanations can support. For his part Joel Mokyr has quite a different review of culture, it seems to me, than David Landes has. For Mokyr “Culture can be a brake, but cars with their handbrakes on can move, if at a slower speed, and doing so for a prolonged time does wear the break down. Perhaps that is as much as we will ever be able to say about the deeper cultural roots of economic growth.” (251-52) Mokyr ends up being a universalist connected to the same sets of beliefs that animated Joseph Needham. Both scholars discern a general forward motion to historical change that carries us all forward, perhaps with lurches and not all at the same speed, but we are all sharing in a common journey. And yet, so much of what Needham and his collaborators have taught us about China and what Mokyr has synthesized from European experiences are what have been distinctive features of each. The Needham question will not be answered by looking at Athena. But perhaps that question was never a good one to ask in the first place, in which case we should look for what gifts we can find wherever they might lie.

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