Why the Scientific Revolution Did Not Take Place in China —or Didn't It?

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This essay makes the case for two conclusions. First, why the scientific revolution did not take place in China is not a question that historical research can answer. It is becomes a useful question primarily when one locates the fallacies that lead people to ask it. Second, a scientific revolution, by the criteria that historians of science use, did take place in China in the eighteenth century. It did not, however, have the social consequences that we assume a scientific revolution will have. The most obvious conclusion is that those assumptions are mistaken.[1]

[1] This essay incorporates my current views on a historical issue to which I have returned regularly for some years. No doubt my views on this topic will be different in another decade; all I mean to accomplish with these ephemeral reflections is to transmit the idea that the issue is worth thinking about, to suggest how one might think about it, and to point out that certain ways of thinking about it are so burdened by suspect assumptions that they do not encourage clear explanation. I have addressed one aspect or another in previous writings, to which the reader is referred for documentation: "Copernicus in China," *Studia Copernicana,* 1973, 6: 63-122; "Shen Kua" and "Wang Hsi-shan," in *Dictionary of Scientific Biography, s.v.,*; "Next Steps in Learning about Science from the Chinese Experience," *Proceedings,* XIVth International Congress of the History of Science (Tokyo and Kyoto, 19-27 August 1974), I, 10-18; Sivin (ed.), *Science and Technology in East Asia* (New York: Science History Publications, 1977), pp. xi-xxi; and "Chinesische Naturwissenschaft: Weber und Needham," in Wolfgang Schluchter (ed.), *Max Webers Studie fiber Konfuzianismus und Taoismus. Interpretation und Kritik* (München, 1982). Joseph Needham has also provided a summary of our conversations and correspondence on the "Scientific Revolution Problem," an interesting attempt to specify differences and similarities in our views, in *Science and Civi!isation in China,* vol. 5, part 2, *Spagyrical Discovery and Invention: Magisteries of Gold and Immortality* (Cambridge University Press, 1974), pp. xxii-xxvii. Here I set out my own somewhat different view of the divergences that accompany our very broad areas of agreement. In several points regarding the Scientific Revolution problem I have been anticipated by Wing-tsit Chan, "Neo-Confucianism and Chinese Scientific Thought," *Philosophy East and West,* 1957, 6: 309-332.

Anyone who has looked into the history of science, technology, and medicine in the last generation or so has been aware that all the great civilizations of the ancient world had their own sophisticated traditions. The Chinese traditions, because they are recorded so fully, and because they were more independent of European influence than the Islamic and Indian ones, are particularly fascinating if we want to compare how understanding of Nature varies in different cultural circumstances. Beginning in the 1920's, Chinese and Japanese historians explained what the Chinese knew and did. My English colleague Joseph Needham, in the 1950's, began calling their work to the attention of educated people in the West, and encouraging them to add to it. By now the study of China is one of the most flourishing fields in the history of science, with perhaps a thousand specialists in China, Japan, Europe, the United States, and elsewhere.[2]

When people become aware of what we have turned up, they usually begin wondering why the transition to modern science first happened where it did. In 1969 Joseph Needham gave the "Scientific Revolution problem" its classic formulation: "Why did modern science, the mathematization of hypotheses about Nature, with all its implications for advanced technology, take its meteoric rise only in the West at the time of Galileo?" "Why modern science had not developed in Chinese civilization … ?" He adds a second question that makes the larger problem more interesting: "why, between the first century B.C. and the fifteenth century A.D., Chinese civilization was much more efficient than occidental in applying human natural knowledge to practical human needs."[3]

In that millennium and a half, European civilization was first experiencing a slow general collapse and then even more slowly recovering from it. It is obvious that we ought to be looking at the Western end of Eurasia, not the Far Eastern end, to account for European inferiority in technology over a span of 1400 years. But there are still other doubts to be expressed in connection with this second question, with its claim of Chinese superiority over many centuries. The natural knowledge that was being applied to human needs was not what we usually call Chinese science.

I use "Scientific Revolution" to refer primarily to the transition in the exact sciences between Galileo and Laplace and its wider repercussions by 1800. This is one of several definitions in current use. I adopt it for the purpose of this essay not because it is the best possible definition, but because it is the one most commonly presupposed by Sinologists and laymen who set out to compare developments in China and the West. Needham's usage of the term "Scientific Revolution" is often, but not consistently, broader. No definition is better than a historiographic expedient. Lack of a consensus about the significance of the term has led some historians of science to reject its use altogether.

[2] [That was the case in 1982. By 2002, the number has probably doubled.]

[3] Needham, *The Grand Titration: Science and Society in East and West* (Toronto: University of Toronto Press, 1969), pp. 16 and 190. He first posed these questions in 1964.

Early technology did not succeed or fail according to how well it applied the insights of early science. On the whole, it was members of the small educated class in China who did science, and passed down their understanding in books. Technology was a matter of craft and manufacturing skills that artisans privately transmitted to their children and apprentices. Most such artisans could not read the scientists' books. They had to depend on their own practical and esthetic knowledge. What that knowledge was like we can only reconstruct from the artifacts they left and from the scattered written testimony of literate people. Literacy spread considerably outside the elite over the last several centuries, but this did not lead to the substantial use of books to teach craft skills.

Comparing all of the scientific and engineering activity of one civilization with all that of another in a single generalization conceals more than it reveals, since it is only in modern times that these various kinds of work became closely connected. It is true that between the end of the Roman period and 1400 or so, a Chinese visiting Europe would have found it in many respects technologically backward. On the other hand, there was probably not a great deal to choose between Chinese and European medical practice before about 1850 (knowledge of anatomy and physiology had hardly any therapeutic applications earlier). Mathematical astronomy in China by its last high point about 1300 never quite reached the general level of predictive accuracy that Ptolemy had mastered eleven hundred years earlier.

I don't need to dwell on comparisons of this kind. They tell us nothing at all about what we can expect to learn from one culture or the other. After all, no one is going to propose that we stop studying the European tradition of alchemy just because the Chinese alchemical literature is richer in chemical knowledge.[4] What matters is that we are now able to begin comparing several strong traditions of science and technology based on the ideas and social arrangements of different civilizations. All of them have to be studied if we want to understand the general relations through history and across the globe between science and culture, science and society, science and politics, science and individual consciousness. Without that comparative understanding we will remain trapped in our own parochial viewpoints.[5] Historians have more urgent work to do than trying to prove that every other culture was inferior to the one they specialize in.

As an example of how studying the Chinese experience can suggest clues about the character of early science in general, let me dwell briefly on the case of Shen Kua (1031 -

[4] See in particular Needham, *Science and Civilisation in China*, vol 5, parts 2-4 (Cambridge University Press, 1974-1980).

[5] For a sample of such work, see G. E. R. Lloyd and Sivin, *The Way and the Word. Science and Medicine in Early China and Greece* (New Haven: Yale University Press, 2004).

1095), one of the most versatile figures in the history of Chinese science and engineering. Just to give a few examples, he is famous for the first discussion of magnetic declination and of printing with movable type, the only application of permutations in traditional Chinese mathematics, a proposal for daily records of the lunar and planetary positions, the first suggestion in East Asia of a purely solar calendar, an explanation of the process of land formation by both deposition of silt and erosion, and an important book on the theory and practice of medicine. In addition to his technical activities, his writing has to be consulted by every student of early Chinese archeology, music, art and literary criticism, economic theory, and diplomacy. He made his early reputation as a land reclamation expert. He was deeply involved as a high official in the 1060's in the most important political reform movement for some centuries.

Shen's combination of unlimited curiosity and involvement in the affairs of his time had a special interest for my own education. For some time, through a series of studies roaming through different historic periods and technical disciplines, I have been trying to piece together bits of answers to a large question that I find boundlessly interesting. How did Chinese scientists in traditional times explain to themselves what they were doing? In other words, what was their understanding of nature and of their relation to it as conscious individuals living in a society? How did the insights of the various sciences hang together to form this understanding? I had gradually formed a general idea of the sciences as defined in early China, but I couldn't see how their insights were combined to form that general understanding. It occurred to me that I might do well to study how the sciences fit together in the mind of a person who was involved in all of them. The obvious person to study was Shen Kua.

The pattern that emerged wasn't unexpected, but I had to take stock of it for the first time. One aspect was that there does not seem to have been a systematic connection between all the sciences in the minds of the people who did them. They were not integrated under the dominion of philosophy, as schools and universities integrated them in Europe and Islam. They had sciences but no science, no single conception or word for the overarching sum of all of them.

In Shen Kua's memoirs there is a classification called "regularities underlying the phenomena ." Under this heading he like many others grouped together physical and numerological aspects of astronomy, astrology, cosmology, and divination, which refract the pattern of physical reality in their various ways. A section called "technical skills " puts together medicine, engineering, and mathematics (including astronomical mathematics), because they share purely instrumental value. There they fit alongside architecture and games. His chapter on 'strange occurrences " sets out his thoughts on the origin of plant fossils, the first recorded description of a tornado in East Asia, an account of his experiment on the formation of rainbows, and similar gems, all sitting alongside unlikely hearsay and ephemeral curiosities.

You can see that what makes us think of Shen Kua as a scientist was widely scattered through his own scheme of human knowledge. That scheme cohered not on the level of science, but on a much more general level. In his writing, there are no clear boundaries between material that fits the modern conception of science and material that doesn't. That modern conception does not help us to understand what Shen Kua was getting at.

Shen Kua, in the second half of the eleventh century, made his turn on the stage of history at a time when a great upsurge in social mobility was broadening the group that ruled China. Many of these new men were interested in all sorts of practical affairs that well-born people in earlier times would have considered beneath them. Civil servants were expected to be competent and versatile, and might work their way to the highest posts of the empire as financial specialists. At that time the government was basing merit ratings of officials on the bottom line—quantitative measures of efficiency in collecting taxes, reclaiming land, and so on, instead of entirely on virtue, breeding, and orthodoxy, as had been the case earlier. At leisure too, this large group that Shen Kua belonged to was free to indulge curiosity—in an amateur way, of course—about anything in the universe, including technical matters that earlier were fit only for clerks or artisans.

Only after Shen's lifetime did this evolving amateur ideal settle on philosophy, the arts, and literature as the appropriate realms to be universal within, once again leaving the study of the earth and sky largely to the mere technicians. In the eleventh century Shen was only one of a number of polymaths whose scientific and technological interests, however amateur, all emerged in connection with their varied official responsibilities. The intellectual consistency of Shen's style in scientific thought seems to reflect only the consistency of his public career, in which that style was formed. What connected his research interests, in other words, were the remarkably diverse responsibilities and commitments of his civil service appointments.[6]

The astronomer in the court computing calendars to be issued in the emperor's name, the doctor curing sick people in whatever part of society he was born into, the

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[6] On other scientists pertinent to this point, in addition to frequent references in *Science and Civi!isation in China* see S. Miyasita, "Su Sung," III, 969-970 in Herbert Franke (ed.), *Sung Biographies* (3 vols., Wiesbaden, 1976); Teng Kuang-ming 鄧廣銘 & Wang Chen-to 王振鐸, "Su Sung 蘇頌," pp. 123-134 in Institute for the History of Science, Chinese Academy of Sciences (ed.), *Chung-kuo ku-tai k'o-hsueh-chia* " 中國古代科學家 (Ancient Chinese scientists, Beijing, 1959); and Wang Chin-kuang 王錦光, Sung-tai k'o-hsueh-chia Yen Su 宋代科學家嚴肅 (The Sung scientist Yen Su)" *Hang-chou ta-hsueh hsueh-pao* 杭州大學學報, 1979,3: 34-38.

alchemist pursuing archaic secrets in mountain haunts of legendary teachers, had no reason to relate their arts to each other. Philosophers were in no position to define a common discipline for all of them, as Aristotle and his successors had done in Europe, and so philosophers had practically no influence on the development of these special pursuits.

If anyone was going to seek out the common ground of the sciences in China, it was people like Shen Kua, who were mastering them all. But Shen put his own understanding together in ways that did not directly link the fields of Chinese science, and in ways that intimately associate what today would be considered scientific with what would be called grossly superstitious. That distinction simply gets in the way of understanding the way Shen Kua's thought was connected. Surely it is necessary to understand thought before one begins to label it.

I would have to say that I failed to find the internal unity of Chinese science that I was looking for in the mind of Shen Kua. By way of compensation, I did learn the importance of an issue that I hadn't paid enough attention to before, that is, the relations of the sciences to other kinds of knowledge.

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Now back to the Scientific Revolution problem. It is striking that this question—Why didn't the Chinese beat Europeans to the Scientific Revolution?—happens to be one of the few questions that people often ask publicly about why something didn't happen in history. It is analogous to the question of why your name did not appear on page 3 of today's newspaper. It belongs to an infinite set of questions that historians don't organize research programs around because they have no direct answers. They translate into questions about the rest of the world. The one that concerns us, for instance, translates into "in what circumstances did the Scientific Revolution take place in the seventeenth and eighteenth centuries in Western Europe?"

Why do people keep asking why the Scientific Revolution did not take place in China when they know enough not to waste time explaining why their names did not appear on page 3 of today's newspaper? Because the question encourages exploration of a fascinating topic and provides some order for thinking about it. It is, in other words, heuristic. Heuristic questions are useful at the beginning of an inquiry. As we comprehend enough to deal with complicated patterns, heuristic questions tend to grow murky, and finally to lose their interest compared with the emerging clarity of what did happen.

So much for heuristic questions in general. Why do we tend to take this one more seriously than the general run? Somehow the Scientific Revolution problem holds a special urgency.

That urgency is there, I suggest, because this problem relies on certain Western assumptions, shaky assumptions that we do not feel comfortable about questioning. Above all we usually assume that the Scientific Revolution is what everybody ought to have had. But it is not at all clear that that is what everybody wanted before it became, in recent times, an urgent matter of survival amidst violent change. This change resulted from, among other things, the Scientific Revolution that did take place. In fact we have made very little progress so far in understanding how Europeans originally came to want that revolution in one country after another, since the attention of historians has been concentrated on how it took place.

There is usually the equally sentimental assumption that civilizations which had the potential for a scientific revolution ought to have had the kind that took place in the West, that led to the sorts of institutional and social changes that appeared in the West.

These assumptions are usually linked to a faith that European civilization—above all in its current American form—was somehow in touch with reality in a way no other civilization could be, and that its great share of the world's wealth and power comes from some intrinsic fitness to inherit the earth that was there all along. Historical study does not suggest that Europe by 1600 had a concentration of intelligence, imagination, talent, or virtue that no other civilization could match. It does suggest that the privileged position of the West comes instead from a head start in the technological exploitation of nature and the political exploitation of societies not technologically equipped to defend themselves.

Finally there is the conviction among scientists that, since science has so quickly and thoroughly become international, it transcends European historical and philosophic biases, and is as universal, objective, and value-free as the Nature that it seeks to understand and manipulate.

What seems to be common sense in that last assumption (or in the self-conception that all the articles of faith I have mentioned are part of) does not stand up to thoughtful examination. Modern science is still too marked by the special circumstances of its development in Europe to be considered universal.

Chinese science got along without dichotomies between mind and body, objective and subjective, even wave and particle. In the West the first two were entrenched in scientific thought by the time of Plato. Galileo, Descartes, and others carried them into modern times to mark off the realm of physical science from the province of the soul, which was decidedly off limits to secular innovators like themselves. These distinctions let early modern scientists claim authority over the physical world on the ground that purely natural knowledge could not conflict with and therefore could not threaten the authority of established religion.

Science and religion have long since learned to coexist, but we are still living with these sharp distinctions between mind and body and so on. If they are European peculiarities, and perpetual sources of trouble at that, why hasn't modern science managed to rid itself of them? It is evidently not a simple matter to root them out. Until we do, there is something to be said for frankly admitting a certain parochialism in the foundations of science. The mathematical equations may be universal, but the allocation of human effort among the possibilities of natural knowledge is not.

Science and technology have spread throughout the world, but that has not made them universal, in the sense of transcending European patterns of thought. In one society after another the encounter between old and new ideas has been abortive, resolved by social change and political legislation. Traditional ideas are simply excluded (on the grounds that they are backward, superstitious, regressive, fit only for the lower classes, etc.) from the educational systems created to teach a new technical and managerial elite the values of technology alongside its theory and practice.

Modern technology is clearly more powerful than that of traditional societies; but to a larger extent than we generally realize, its strength emerges in application to needs and expectations that do not exist until it generates them.[7] True universality would require modern technology to coexist with and serve cultural diversity rather than standardizing it out of existence.

I am arguing that the notion of a universal and value-free modern science, which has somehow become independent of its social and historical origins, is wishful thinking. It is easy even for an intelligent reader to be led astray on this point. The narrow limits of the certainty from which this notion arises are never defined carefully by those who set out to explain science to non-scientists.

It would be foolish to deny that modern science has attained a verifiability, an internal consistency, a taxonomic grasp, a precision in accounting for physical phenomena, and an accuracy in prediction that no other kind of activity shares, and that lay far outside the grasp of early sciences. The rigor that makes these remarkable characteristics possible quickly disappears, however, once the formulation of a law or theory in mathematical equations, matrices of categories, or exactly defined technical concepts and models has been translated into the ordinary language and general discourse of a given culture. That translation into analogies and metaphors steeped in values must

[7] This point has been most persuasively argued in Langdon Winner, *Autonomous Technology: Technics-out-of-control as a Theme in Political Thought* (Cambridge, Mass., 1977).

precede all public discussion of science, and almost all philosophic discussion. It even precedes most reflection by scientists on fields outside their own disciplines.

Beyond the narrow, abstract realm in which exactitude is possible, values and subjective judgments come to bear on every activity situated within a society. There are, for instance, profound differences between the character of modern scientific activity in the contemporary People's Republic of China and United States. They reflect different predominant convictions about the relations between basic and applied science, the relation of both to general culture, the roles of scientists in defining research programs, procedures for planning and supporting individuals' research projects, expectations about the social aims to which scientific work will contribute, the organization and status of professional scientists, the connections of political ideas and scientific knowledge, and the division of national resources between science and other priorities, and between various scientific activities. That certain equations and models are invariant between the two societies is a factor in all these consensuses, but then so is the ubiquity of opposable thumbs. Despite the invariance, a given constellation of values will determine that certain laws and hypotheses can be developed further, and that others will be abandoned unless they are among the very few that individuals can explore at their private discretion and their own expense. The great disparity in Chinese and American definitions of psychology is only one particularly obvious example that affects the life and death of particular theories in one society or the other.

So long as there is variation of such magnitude in the balance between the cognitive, practical, normative, and social dimensions of science, such words as "international" and "universal" are out of place. When applied to the narrow, rigorous technical realm of scientific cognition alone, they constitute a modest claim indeed.

Nor can one accept uncritically the idea that modern science is in every essential respect European in its social and historical origins. To those familiar with the science of other cultures, any account of the early history of science is lopsided, and misleading on the most fundamental issues, if it restricts itself substantially to discoveries made and understandings worked out at the Western end of Eurasia; if it loses sight of the constant movement of ideas back and forth between civilizations from the New Stone Age to the present; if it does not adequately consider what Europeans had learned by 1600 about Islamic, Indian, and Chinese science; or if it ignores the impact of exotic technologies and materials on the experiences of Europeans.

Fallacies of Historical Reasoning

Growing awareness of the high level of science and technology in ancient China has led to cascades and avalanches of hypotheses from one scholar or another about factors that inhibited the evolution of modem science in China, or characteristics unique to the West that made possible or furthered a major scientific revolution.[8] These often incorporate elementary fallacies of historical reasoning that deserve notice.

For roughly two-thirds of a century, historians have argued that although Ch'ing

[8] The most significant early contributions to tltis literature, in order of their appearance, are Jen Hung-chün 任鴻雋, "Shuo Chung-kuo wu k'o-hsueh chih yuan-yin 說中國無科學之原因" (The reason for China's lack of science), *K'o-hsueh* 科學, 1915, *I:* 8-13; Yu-lan Fung, "Why China Has No Science - An Interpretation of the History and Consequences of Chinese Philosophy," *The International Journal of Ethics,* 1922, 32: 237-263; Homer H. Dubs, "The Failure of the Chinese to Produce Philosophical Systems," *T'oung Pao,* 1929,26: 96-109; Derk Bodde, "The Attitude toward Science and Scientific Method in Ancient China," *T'ien Hsia Monthly,* 1936, 2: 139-160; and Rhoads Murphey, "The Nondevelopment of Science in Traditional China," *Papers on China,* 1947, 1: 1-30 (for others see the bibliographies of *Science and Civi!isation in China,* esp. vol. II). Jen claims that science failed to develop in China after the Han period because of inattention to "the inductive method." Fung claims "it is because of the fact that the Chinese ideal prefers enjoyment to power that China has no need of science … (p. 261). Dubs refutes the silly prejudice that the character of the Chinese language made systematic thought impossible, but argues that "the result of the absence of mathematical systems was that the Chinese philosophers attacked the world piecemeal … by empirical rather than by rational methods" (p. 108). He has nothing whatever to say about Chinese scientists. Bodde considers attitudes toward science, and is aware of a few isolated scientific accomplishments despite his disregard for the technical literature, but suggests that the most important "retarding effect upon scientific innovation … has been the ideographic nature of the Chinese written language" (p. 158). Murphey, dependent upon Western-language sources and influenced by the stereotypes of F. S. C. Northrup, concludes "a naturalistic philosophy which might be called a reliance on the aesthetic continuum … clearly had no place for the inductive hypotheses necessary for science" (p. 15). Writings of this sort are full of acute observations, particularly about philosophic attitudes expressed in the early classics, but it is obvious that their authors failed to examine the literature of the Chinese scientific traditions. They may make a case that Lao-tzu or Hsun-tzu would have been mediocre biologists or mathematicians, but they do not help us account for the theoretical analyses, mathematical proofs, and programs of empirical discovery so profusely documented in the writings of those actually engaged in studies of nature.

Because of his knowledge of the Chinese sciences and the breadth of his hypotheses, Needham's is the earliest discussion of the Scientific Revolution problem that still commands attention, and is still the best. The most useful critiques of Needham's writings on this subject are, from Sinologists, Bodde, "Evidence for 'Laws of Nature' in Chinese Thought," *Harvard Journal of Asiatic Studies,* 1957 (pub!. *1959), 20:* 709-727, and "Chinese 'Laws of Nature'; A Reconsideration," ibid., 1979, 39: 139-155, Chan, and A. C. Graham, "China, Europe, and the Origins of Modem Science: Needham's The Grand Titration," pp. 45-69 in Shigeru Nakayama & Sivin (ed.), *Chinese Science. Explorations of an Ancient Tradition* (Cambridge, Mass., 1973); from a historian of science, Nakayama, "Joseph Needham, Organic Philosopher," ibid., pp. 23-43; from a philosopher, Robert S. Cohen, "The Problem of 19 (k)," *Journal of Chinese Philosophy,* 1973,1: 103-117; and from sociologists, Benjamin Nelson, "*Sciences* and Civilizations, 'East' and 'West.' Joseph Needham and Max Weber," *Boston Studies in the Philosophy of Science,* 1974,11: 445-493, and Sal Restivo, "Joseph Needham and the Comparative Sociology of Chinese and Modern Science," *Research in Sociology of Knowledge, Sciences and Art,* 1979, 2: 25-51. Kenneth G. Robinson sets to rest once and for all claims that literary Chinese is an inferior vehicle of science in "Literary Chinese as a Language for Science," in *Science and Civilisation in China,* ed. Joseph Needham, et al. (Cambridge University Press, 2004), volume 7, part 2, pp. 95-198.

dynasty thinkers took the world as observable, nominalistic fact, just as Francis Bacon (1561-1626) did, unlike him they did not develop a scientific methodology. Despite the positivist bias of such arguments, they did not even consider whether Bacon's scientific method has survived in the practice of contemporary science.[9] It was, in fact, largely Scholastic in its origins, concerned with taxonomies rather than theories of natural phenomena, and resolutely unconcerned with mathematical measurement. Of the major early modem attempts to define how physical science might fruitfully proceed it was probably the most sterile, in contrast to Bacon's very influential convictions about the organization and ideology of scientific activity.

This pattern of thought, then, faults the Chinese for not developing a scientific method that later proved abortive in the West. The same habit shows up in many other forms. A well-known sociological study of astronomy in the last two centuries B.C. explains the failure to develop a "unified scientific system." One reason is that Chinese astronomers "were not interested in applied technical sciences, e.g., in developing theoretical tools which could be used to control the flight of a cannon shell or to direct ships safely across the sea."[10] So much for the first civilization to note the declination of the compass needle. So much for the astronomy of an era more than a millennium before the invention of the cannon. The same lack of interest is prominent in the impetus theoreticians from John Philoponus (fl. ca. 530) to Jean Buridan (ca. 1295-ca. 1358) and others of the School of Paris whose investigations furnished much of the basis for Galilean mechanics. How then did what is presented as a disastrous shortcoming in China fail to prevent in Italy—in fact, according to the conventional wisdom, help directly to bring about—the mathematical study of bodies in motion?

Considered generally, this fallacy amounts to claiming that if an important aspect of the European Scientific Revolution cannot be found in another civilization, the whole ensemble of fundamental changes could not have happened there. The flaw of reasoning that underlies it is the arbitrary assumption, never explicit, never discussed, that a given circumstance amounts to a necessary condition. It is almost invariably arbitrary because if we trace the prehistory of the actual Scientific Revolution backward far enough, in most cases we can find a point when the circumstance is absent in Europe. In

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[9] Jen, loc. cit.: Joseph R. Levenson, *Confucian China and its Modern Fate. The Problem of Intelleaual Continuity* (London, 1958), pp. 3-14; and David E. Mungello, "On the Significance of the Question 'Did China Have Science,'" *Philosophy East and West,* 1972, 22: 467-478, and my comments on this article in the same journal, 1973, 23: 413-416. Fung also refers to Bacon in connection with the Scientific Revolution problem, but there the issue is not method but, more pertinently, the relation of science and power (Fung does not specify what kind of power).

[10] Wolfram Eberhard, "The Political Function of Astronomy and Astronomers in Han China," pp. 33-70, 345-352 in *Chinese Thought and Institutions* (ed. John K. Fairbank; Chicago, 1957), p. 66.

that case, on what grounds can it be considered a necessary condition? In most cases one need not go back very far. That is why, despite their currency among Sinologists, in the past generation necessary conditions have practically disappeared from the armamentarium of discriminating historians of science.

The mirror image of this fallacy may be seen in an influential estimate of the *Chou i* 周易, the Book of Changes, as a deterrent to science. Here is the way Joseph Needham put it in 1956:

> while the five-element *(wu-hxing* 五行) and two-force (*yin-yang* 陰陽) theories were favourable rather than inimical to the development of scientific thought in China, the elaborated symbolic system of the Book of Changes was almost from the start a mischievous handicap. It tempted those who were interested in Nature to rest in explanations that were no explanations at all. The Book of Changes was a system for *pigeon-holing* novelty and then doing nothing more about it.

Nearly two decades later Ho Peng Yoke assured us that if Chinese scientists "were fully satisfied with an explanation they could find from the system of the *Book of Changes* they would go no further to look for mathematical formulations and experimental verifications of their scientific studies. Looking at the system of the *Book of Changes* in this light, one may regard it as one of the inhibiting factors in the development of scientific ideas in China."[11]

In these instances one is tempted to counter the arguments with matters of fact. Although Needham's extended discussion treats the Book of Changes predominantly as a static classificatory system of concepts, we find that natural philosophers most often used it to construct dynamic explanations of change. One also looks in vain for a habit among early Chinese scientists of constructing purely mathematical formulations and experimental verifications. If one cannot prove that this tendency was evolving steadily to a certain point, if there is no tangible evidence that without the Book of Changes they would have "gone further," there is no warrant for introducing from modern biology the metaphor of inhibition.[12]

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[11] Needham, *Science and Civilisation in China, II,* 336 and 340; Ho, "The System of the Book of Changes and Chinese Science," *Japanese Studies in the History of Science,* 1972, 11: 23-39. Attempts to explain scientific revolutions by lists of positive and negative factors abstracted from context have been criticized by Robert K. Merton in *Science, Technology and Society in Seventeenth-Century England* (New York, 1970), p. x.

[12] Although Needham has given considerable weight to the notion of inhibition, as one would expect of a first-rate biologist he is cautious about using it in relation to processes that he cannot prove were under way. Writers who draw on his work are not so discriminating. This point is easily demonstrated by examining the perceptive list of 29 "factors inhibiting the emergence of modern science in China and Western Europe" compiled from Needham's writing in Restivo (see note 8), pp. 46-47. In only four of these 29 does Needham actually invoke the concept of inhibition, and all are tautologous or too vague to challenge (e.g., "it is a matter for reflection how far Chinese algebra was inhibited from developments of

Exactly what does “inhibiting factor” mean in such contexts? Consider one of these often used to explain why China failed to beat Europe to the Scientific Revolution despite a putative early head start, namely the predominance of a scholar-bureaucrat class immersed in books, faced toward the past, and oriented toward human institutions rather than toward Nature as the matrix of the well-lived life. But in Europe at the onset of the Scientific Revolution we are faced with the predominance in the universities of Schoolmen and dons, immersed in books, faced toward the past, and oriented toward human institutions rather than toward Nature. They did not prevent the great changes that swept over Europe. It would take a more imaginative historian than myself to swear that those changes would have taken place sooner had Scholasticism never existed.

The confusion about “inhibiting factors” is no less a confusion when it has to do with ideas or techniques. One might just as well call Euclidean geometry an inhibiting factor for the development of non-Euclidean geometry, since so long as people were satisfied with it they didn’t move on to a new step. But can one argue that non-Euclidean geometry would have developed sooner without it? It is unfortunate to see the remarkably interesting technical language of the Book of Changes, so powerful in systematically relating broader ranges of human experience than modern science attempts to encompass, written off as an obstacle before anyone has taken the trouble to comprehend all of its dimensions.

The first fallacy, then, confuses for a cause or necessary condition what merely describes an earlier state of a culture, or a culture’s way of doing something. In its complement, as can be seen by the examples just given, the absence of the subsequent state is confused with an inhibitor. One who commits this second fallacy is stopping growth that never took place. Both of the confusions I have described—blaming the earlier state for delaying the later state, and using the early absence of something modern to prove that modernity was unattainable later—confound continuity with stasis. They are bad history because they are bad reasoning.

I recur to the assumptions about ourselves that I have discussed earlier, for they are at the root of both these fallacies. They turn the history of world science into a saga of Europe’s success and everyone else’s failure, or at best inherently flawed and transitory success, until the advent of redemption through modernization.

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post-Renaissance type by its failure to produce a sign which would permit the setting up of equations in modern form,” *Science and Civilisation* in *China,* III, 115). In a half-dozen other places Needham uses wording which suggests inhibition, generally in a similarly vague way (e.g., Confucian rationalism and humanism as “fundamental tendencies which paradoxically helped the germs of science on the one hand and injured them on the other,” II, 12). In the remaining score of instances Restivo has read inhibition into statements about failure, lack, and inadequacy.

Joint use of the pair of fallacies makes it easy to prove that the European breakthrough is not simply a fact of history, but was inevitable since history began. Was the horse and buggy a necessary preliminary to the invention of the automobile, or did it delay that invention? Would the automobile have emerged sooner if the buggy had never been invented, so that people would have been dissatisfied with less adequate vehicles? If we find the horse and buggy in Europe, by fallacy 1 its absence in China made the invention of some analogue of the automobile impossible. If we find some analogue of the horse and buggy in China, we apply fallacy 2 and make it an inhibiting factor. Thus medieval European impetus theory, abstract and unconcerned with application, was a stage in the evolution of inertial guidance; if Chinese who thought about physical questions were equally uninterested in their application, inertial guidance could never have originated in East Asia.

This is an infallible formula for reading the strength and power of modern science into the historic past—but only the past of Europe. For the past of other civilizations the test is always anticipation of or approximation to some aspect of early European science, or modern science. Why does the science of early Europe not need to be tested? Because of the *assumption* that it and only it gave birth to the Scientific Revolution. Other civilizations shine only as they reflect the light of the European tradition. Or so the prophets of modernization suppose.

I claim, therefore, that the fallacies that so often accompany discussions of the Scientific Revolution problem reflect a set of disastrous assumptions that lie beneath the obvious "heuristic" interest and charm of such discourse. They are disastrous because they assure us there is no point in comprehending on their own terms the technical inquiries of non-Western cultures.[13] We now find these assumptions accepted not only in Europe but to some extent in every country in which the history of China is studied.

Why should intellectuals in a non-European country, which owes little of its culture before modern times to Western influence, accept this bias? That is perhaps inevitable, considering that modern education establishes itself (as it did originally in Europe) by teaching the rejection of the traditional past, or its demotion to a cultural exhibit that

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[13] Although Needham consciously assumes "that there is only one unitary science of Nature, approached more or less closely, built up more or less successfully and continuously, by various groups of mankind from time to time," he sees this as a reason to study, rather than to ignore, non-European traditions. See his discussion cited in note 1 above.

The sorts of scholars who affirm, without troubling themselves to peruse the Chinese scientific literature, that it could not possibly have any value (see notes 7 and 8) have recently provoked a reaction, equally uninformed, that claims European science was markedly inferior to that of China as recently as three hundred years ago. See John Gribbin, "Did Chinese Cosmology Anticipate Relativity?" *Nature,* 1975, 256: 619-620, and for a critical discussion, Sivin, "Chinese Cosmology," ibid., 1976, 259: 249.

may be of use for nurturing nationalism (and, in the era of mass-market foreign vacations, for enticing tourists). Since Japan has had over a century's experience with a modern educational system and the self-consciousness it produces, Nishijima Sadao's acute analysis does not come as a surprise:

> The 'static character' hypothesis holds that Chinese society lacked the capacity progressively to form a new era through its own efforts. This hypothesis was afforded particular emphasis by the viewpoint that the modernization of Chinese society was retarded. . . . Originally the 'static character' hypothesis, in company with that of 'Oriental despotism,' was advocated, in contrasts with Western European society, as a notion in polar opposition, for the sake of validating the self-consciousness that came into being with the formation of modern Western European society. That is, it was a postulate to serve as an element in the recognition of the value of modern Western European society. … In our country, when we deal with Chinese society from the point of view that makes the formation of the modern ego identical with the equal valuation of individuals in Western European civilization, we are led uncritically to use the 'static character' hypothesis. This has brought about our tendency to be controlled by the inverted logic that makes the goal of understanding Chinese society equivalent to grasping the origins or even the mechanism responsible for the persistence of its 'static character.'[14]

In a few words, anyone who begins by assuming that the paramount issue in the study of China is accounting for the inevitability of its backwardness is unlikely to question whether backwardness was inevitable, to ask whether there were not in her history prominent patterns of success from which we might learn, or to reexamine the assumptions about the modernized West that organize European history as a crescendo of success (with setbacks, to be sure, adding to the complexity and thus the charm of the crescendo), and that of other civilizations as a static picture of failure. Thus Nishijima states the convictions that justified and supported not only Japan's esteem for European civilization but also its political aspirations in East Asia before and during the Sino-Japanese War.

One more fallacy often appears in connection with the Scientific Revolution problem, when historians select the aspects of the European experience that are appropriate for comparison with other civilizations. I mean the fallacious assumption that one can make sense of the evolution of science by looking at intellectual factors alone, or socio-economic factors alone, according to preference. Some people think of science predomi-

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[14] Nishijima Sadao 西島定生, *Chūgoku keizaishi kenkyū* 中国経済史研究 (Studies in Chinese economic history; Tokyo, 1966), pp. 3-4. Nishijima's remarks are part of an effort to explain the slow development of studies in Chinese agriculture.

nantly as an intellectual quest after truths hidden in nature. They tend to think of China's failure to beat England to modern science as an failure of thought. Other people, who think of science as primarily a social or economic phenomenon, tend to see the defeat as a matter of Chinese social or economic backwardness. But neither of these exclusive approaches to explanation is adequate. The distinction between intellectual and social factors or between internal and external factors is not out there in the events we study, but in the mental habits and professional associations, in the division of labor, of historians.[15]

Dimensions of the Scientific Revolution

The Scientific Revolution and its consequences cut across the boundaries of historical specializations. Let me make this clear by defining its important dimensions.

To begin at the intellectual end, the Scientific Revolution was a transformation of our knowledge of the external world. It changed the questions we asked, the means we used to explore them, and the character of the answers. It established for the first time the dominion of number and measure over every physical phenomenon.

Ernst Gellner has pointed out a particular way in which the European Scientific Revolution is more than a leap to a new form of knowing. It is natural to assume that in science the crucial test has always been "is it true?" But earlier that was only one of several equally important questions: Is it beautiful? Is it conventional? Is it morally improving? Does it lead to perception of the Good? Does it conform to certain esthetic patterns that all truth must, as astronomers up to Kepler believed that celestial orbits must be compounded of perfect circular motions? In science the test of truth has displaced most of these and redefined the others. This demand for truth above all was an appeal to fact—fact that was in principle public, verifiable, morally neutral, that did not change with the social circumstances of the observer, that was immune from interference by magician, or god, or human need. But the new science did more than appeal to facts. It created facts of that kind for the first time, knowledge that had no value except truth value. That is an awesomely original creation. It took place in Europe between the time of Copernicus and Laplace and has spread across the world since.[16]

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[15] Lloyd & Sivin 2002 (see note 5), pp. 3, 234-238, uses this methodology. See also Sivin, "A Multi-dimensional Approach to Research on Ancient Science," forthcoming in *East Asian Science, Technology, and Medicine,* 2005, no. 23.

[16] Gellner, *Legitimation of Belief* (Cambridge, England: At the University Press, 1974), *passim.* The physicist-philosopher Robert Cohen makes the same point incidentally but a little more broadly when he speaks of "the Galilean turn" as a "rush toward dynamic, functionalized mathematics and abstract quality-stripped epistemology" (see note 7), p. 114.

The same leap was not taken in seventeenth-century China. People there considered the idea of objective knowledge without wisdom, without moral or esthetic significance, grotesque.

The Scientific Revolution in Europe also meant redefining the connections of natural philosophy (i.e., scientific thought) to other kinds of knowledge. It meant redefining man's orientation toward the past and the future. It meant redefining what authority should determine what uses may be made of knowledge. It meant redefining what knowledge of nature is socially desirable, and what socially undesirable. It meant redefining how knowledge ought to be related to human individuality and to the active relations of man and nature.

Galileo and his friends and successors could not have got round the authority of the Church on the strength of ideas alone. That message was conveyed to Galileo by the Congregation of the Index in 1616, and then with drastic finality when he was condemned in 1633. But he and his fellow spirits had begun constructing a new intellectual community outside the old establishment. A hundred years earlier there had been no organized alternative to the Church and its scholastic educational system; then even Galileo himself might have died an archbishop.[17] But in the Counter-Reformation the Church, threatened by Protestantism, became defensive and obsessed with thought control. It naturally became less attractive to the most talented and ambitious (and of course there was less room for those who were attracted). A variety of new careers was emerging. Among them the profession of scientist was being invented. This profession could not provide structures that paid salaries for specialist careers until about 1800.[18] Nevertheless, from the start it assumed for its amateurs, devotees, and enthusiasts, independent authority to formulate the laws of nature. Scientists took that authority away, in fact, from the Scholastics, for whom science could never be more than a collaborator of faith. Secular learning remade the universities and displaced other ancient institutions while over several centuries of evolution and revolution it formed a technical establishment.

This outline of the Scientific Revolution's many dimensions is meant to suggest how much we are likely to miss if we care only about social factors, or only about intellectual factors, as we survey the situation in China. Until recently, for instance, people con-

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[17] On the earlier position of the Church as a locus of careers open to talent see Alexander Murray, *Reason and Society* in *the Middle Ages* (Oxford, 1978), pp. 282-314.

[18] See, for instance, Arnold Thackray, "Natural Knowledge in Cultural Context: The Manchester Model," *The American Historical Review*, 1974,79: 672-709, esp. p. 692.

cerned with that topic, including myself, have overlooked a significant piece of the Chinese picture, which I will now consider.

Scientific Revolution in China

By conventional intellectual criteria, China had its own scientific revolution in the seventeenth century. This is a point of no small interest if we are meditating about why China couldn't have had one.

Western mathematics and mathematical astronomy were introduced to China beginning a little after 1600, in a form that before long would be obsolete in those parts of Europe where readers were permitted access to current knowledge. Several Chinese scholars quickly responded and began reshaping the way astronomy was done in China. They radically and permanently reoriented the sense of how one goes about comprehending the celestial motions. They changed the sense of which concepts, tools, and methods are centrally important, so that geometry and trigonometry largely replaced traditional numerical or algebraic procedures. Such issues as the absolute sense of rotation of a planet and its relative distance from the earth became important for the first time. Chinese astronomers came to believe for the first time that mathematical models can explain the phenomena as well as predict them. These changes amount to a conceptual revolution in astronomy.

That revolution did not generate the same pitch of tension as the one going on in Europe at the same time. It did not burst forth in as fundamental a reorientation of thought about Nature. It did not cast doubt on *all* the traditional ideas of what constitutes an astronomical problem. It did not narrow people's views of what meaning astronomical prediction can have for the ultimate understanding of Nature and of man's relation to it.

The most striking long-range outcome of the encounter with European science, in fact, was a revival of traditional Chinese astronomy, a rediscovery of forgotten methods, that were studied once again in combination with the new ideas and that supported what might be called a new classicism. Rather than replacing traditional values, the new values implicit in the foreign astronomical writings were used to renovate traditional values.[19]

[19] Elman, *From Philosophy to Philology: Social and Intellectual Aspects of Change in Late Imperial China* (Cambridge: Harvard University Press, 1984).

Why didn't this conceptual revolution have the social consequences that historians of Western science have encouraged us to expect? The old and new astronomy were not in antagonistic competition, once the Chinese acknowledged that the European techniques yielded much more reliable predictions. By the mid seventeenth century European civilization had had no appreciable political or social impact, and astronomy was making its way on its own merits.

One is tempted to see the later process by which Western astronomy became rooted in China as the last major face-to-face encounter of non-Western and European science in world history. By the eighteenth century modern science was crossing national boundaries on the coattails of Empire, and competition between sciences, literatures, religions, etc., on the basis of their particular merits had become a thing of the past. Even in the mid seventeenth century, despite the high drama of eclipse prediction contests in the late Ming court, the fact remains that the triumph of European computational techniques came about not through a consensus of great minds but by an imperial decision to hand over operational control of the Astronomical Bureau to Jesuit missionaries.

The foreign techniques, powerful though they were, offered Chinese students no alternative route to security and fame, and the civil service examination system left no room for one. The only astronomers who could respond to the Jesuits' writings were members of the old intellectual elite. They were bound to evaluate innovations in the light of established ideals that they felt an individual responsibility to strengthen and pass on to the next generation.

Revolutions in science as well as in politics take place at the margins of society, but the people who made the one in seventeenth-century China were firmly attached to the dominant values of their culture.[20] At the time there were no students of astronomy motivated to cast off traditional values. There were no groups of intellectuals alienated enough to follow ideas where they led even if the society around them fell apart.

The most influential first-generation champions of Western astronomy were men of the lower Yangtze region who lived through the Manchu invasion of the 1640's. They adopted the traditional role of the loyalist who would not serve a new dynasty, particularly what they saw as a non-Chinese dynasty. Having refused to strive for conventional careers in a society that in their view *had* fallen apart, they were motivated to spend their lives studying and teaching the new mathematics and astronomy while

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[20] See "Wang Hsi-shan," pp. 159 and 164, and for further details, Sivin, "Wang Hsi-shan," *Dictionary of Ming Biography, s.v.*

they used them to master the neglected techniques of their own tradition.[21] They rejected the Ch'ing present not for a modernist future but to keep alive the lost cause of the Ming for one more generation. Wang Hsi-shan even avoided using the Ch'ing dating system. Despite his superb critical acumen he was the opposite of Descartes, for whom every ancient institution had to justify itself by the new criterion of clear and distinct ideas or be considered a dead relic.

If then we seek in China those for whom science was not a means to conservative ends, those for whom a proven fact outweighed values that had evolved for thousands of years, we do not find them until the late nineteenth century. Then it was people with little or no stake in the old society who became the first modern scientists. By that time foreigners exempt from Chinese law and backed by gunboats could do what they wanted in China. They constructed new institutions and new career lines that let them attract and educate talented young people who had no other prospects. We can no longer talk about the encounter of the old and new astronomy. Social and political change had left nothing for the old to do. It became rare as time passed for modern scientists to be aware that their country had had its own scientific tradition. Only in the last couple of generations has that awareness became general.

Conclusion

My frustrations in trying to make sense of science in China arise partly because of the many levels of human activity that have to be encompassed over such a great sweep of time and human experience. They arise partly because the European Scientific Revolution seems to call for an understanding in greater breadth and depth than its historians have insisted upon. Once we keep in mind the many dimensions of scientific change and their complex relations, it becomes less surprising that the Scientific Revolution took place only when and where it did. The process increasingly comes to resemble any historic evolution, which is always the sum of human decisions and acts, some arbitrary, many wrongheaded—in other words, muddling through. We do not need to appeal to fate, determinism, teleology, cultural superiority, an inexorably unfolding inner logic, or the hidden operation of some World Spirit.

Looking at these two scientific revolutions—the one we think we know so well in Europe, and the one that wasn't what we expect it to be in seventeenth-century China—suggests that we have a great deal to learn about the specific circumstances of each,

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[21] Elman 1984 (see note 19). Another signal contribution on the influence of astronomy is John Henderson, *The Development and Declne of Chinese Cosmology* (New York, 1984).

seen in all its dimensions, before we are ready to tell the world why it couldn't have happened in other times or places.

I believe that the breakthroughs coming up in the study of Chinese science will be of another kind altogether. They will have to do with understanding in depth and in an integral way the circumstances of people who did science and technology: how their technical ideas related to the rest of their thought; what the scientific communities were—that is, who formed a consensus that certain phenomena were problematic, and that certain kinds of answers were legitimate; how those communities were related to the rest of society; how they were supported; how the responsibility of men of knowledge to their colleagues in science was reconciled with their responsibility to society; and what larger ends the sciences served, that kept their laws conformable to the laws of Chinese painting and to the basic principles of moral conduct.[22]

These are issues about which we understand very little with respect to China or to Europe. It will take much further study and reflection on both sides before the comparative history of science is ready to take off. My prognostication is that by that time we will no longer be asking why the transition to modern science did not first take place in China.

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[22] The language in which I pose these questions is more or less that of the sociology of knowledge. It is interesting that A. C. Crombie has phrased a very similar set of topics, also intended to provide an integrated view of the Scientific Revolution problem in terms familiar to intellectual historians and thus to the majority of historians of science: "conceptions of nature and of science, of scientific inquiry and scientific explanation, of the identity of natural science within an intellectual culture, and the intellectual commitments and expectations that affect attitudes to innovation and change" ("Science and the Arts in the Renaissance: The Search for Truth and Certainty," *History of Science,* 18: 233-246, especially pp. 234-235).

Appendix

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Brush Talks from Dream Brook

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There are a total of 507 jottings in Brush Talks from Dream Brook, or 609 including its two sequels. For a classification of the book's contents according to field of knowledge treated, see Needham, *Science and Civilisation in China*, I, 136.