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The Potential of the Comparative History of Pre-Modern Science

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Argument

While undeniably more restricted in many respects than the history of modern science (both in that the evidence is lacunose and because the science itself is more elementary) the comparative inquiry into ancient investigations may, from another point of view, be said to be far richer. We can open up the question of why science should have taken such different forms in different ancient civilizations. How did the ancient investigators come to define the problems, and the methods to tackle them, in the ways they did? A history of pre-modern science provides access to ancient aims, values, and ideologies in a distinctive fashion, differing from other areas of ancient thought in that "reality" – as the ancients themselves understood it in different ways – could and did act as a check on their speculations.

In the last couple of decades or so the study of ancient science has been transformed. First the amount of primary material available has grown enormously. Way back in 1952 Neugebauer already complained of the vast quantities of cuneiform tablets that languished unpublished in the vaults of the British Museum and elsewhere (Neugebauer [1952] 1957, 59). Initiatives, such as the publication of the *State Archives of Assyria* series, have made a tremendous impact, including beyond the circles of specialists. The investigation of the Mesopotamian study of the heavens, opened up in the post-Neugebauer era by such scholars as Hunger, Parpola, Reiner, Pingree, has been further advanced more recently by Swerdlow, Rochberg, Brown, Robson. In China the excavation of tombs at Mawangdui, Zhangjiashan, and elsewhere has, as both Elisabeth Hsu and Karine Chemla here recall, brought to light mathematical, medical, and cosmological texts that have led to drastic revisions in our understanding of the background to the major Han classics in those fields, the *Jiuzhang suanshu*, the *Zhoubi suanjing*, the *Huangdi Neijing*, the *Huainanzi*, and so on.

In Greece, too, although the discovery of new texts has been on a smaller scale, Vivian Nutton here reminds us of the impact of recent scholarship on Greek medicine, where the translation of sources extant only in Arabic, and the collecting and editing of the scattered fragmentary remains of key medical theorists such as Herophilus

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and Diocles, have been especially important. In cosmology the Derveni papyrus has been analyzed by Gabor Betegh (Betegh 2004) and a newly discovered fragment of Empedocles has been the subject of heated controversy (Martin and Primavesi 1999). Greek and Roman epigraphic sources are now combed more systematically than they used to be, and, connected with that, more attention is paid to the evidence for the thought and practice of others besides those who belonged to the charmed circles of the literate elite. In the handbooks I published in the early 70s (Lloyd 1970, 1973) I commented on the lack of decent editions of primary materials and the inadequacy of the secondary literature (in those days indeed I had to make my own collections of Herophilus and Erasistratus). Those complaints apply far less in 2005.

The methodology and problematic of the study of ancient science have also changed, sometimes in tandem with changes in the history and philosophy of science more generally, sometimes independently of those. An earlier phase of scholarship was much preoccupied with the debate between internalist and externalist approaches. Now it is generally agreed that these are interdependent and that to pursue the one to the exclusion of the other would be crass. The importance of the controversies over the demarcation of science and pseudo-science, and of rationality and irrationality, has declined (though to be sure those issues have not disappeared completely). Few now share Neugebauer's totally dismissive attitude towards astrology, even though interpreting what "astrology" meant for its practitioners still raises challenging problems.

It is true that some of the impressive developments in the history of modern science have no counterpart in the study of antiquity. We are not concerned with the sociology of laboratory-based science, nor with the implications of the technology it depends on. Some modern specialists would argue that what we have to study does not count as "science" at all. Yet the ancients studied the heavens, the body, the causes of health and disease; they engaged in mathematical – both arithmetical and geometrical – investigations, both theoretical and practical; they described, classified, gave explanations, made predictions, sought, in short, to understand the world around them.

While undeniably more restricted in some respects than the study of modern science, the inquiry into ancient investigations is, from another point of view, far richer. Since the seventeenth century, scientific research carried out across the world has become homogenized. The differences in the interpretations of the key questions, and of the right way to go about tackling them, in most areas from fundamental particle physics to molecular biology, are nowadays quite minor. The student of pre-modern science, by contrast, is faced with a very considerable variety of styles of inquiry, of the ways, that is, in which ancient investigators defined the problems and in the manner in which they thought they should be resolved. Why this should be so – why "science" should have taken such different forms in different ancient societies and at different times – raises fundamental issues that constitute one of our most important challenges. We are led to open up questions to do with values as well as a whole raft of problems concerning the relationship between science and society. How far and in what circumstances

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was it possible for individuals or groups to question, undermine, and revise the basic presuppositions of the societies to which they belonged? In what ways were the studies of the heavens or of the human body aided and supported by state structures – or other institutions – or impeded by them? The problem that Kuhn focused on in one of his books (Kuhn 1977), namely the tension between innovation and tradition, recurs over and over again in the study of ancient science – not that Kuhn himself ever identified the opportunities that antiquity presents.

These questions can and must be answered by specialists who have to make the best use they can of the evidence available for particular fields in particular societies at particular junctures. But they also invite comparative analysis. In fact the temptation, in the past, has been for specialists to stay within their own domain of expertise and not consider its implications for a wider picture. Comparative history of science has, indeed, as I once put it (Lloyd 1997, 363), been considered something of a disaster area – and with reason, since much comparative work has been extremely superficial. The urge to arrive at global generalizations about whole civilizations, about whole civilizations across many centuries indeed, has repeatedly driven out careful detailed analysis and the recognition of heterogeneity.

Worse still, many comparisons were carried out simply to determine which society or group could be said to have got closest to modern science, or to have anticipated later results. The recurrent flaws in much Greek-Chinese comparative work in particular have stemmed from attempts to set them in polar contrast with one another. The proponents of the Greek miracle tended to dismiss all other traditions and styles of inquiry as inferior, as headed down blind alleys. In the process they generally underestimated, first, the heterogeneity within what we call Greek science, and secondly, the extent to which the Greeks too pursued programs that led nowhere. Even some of the acclaimed triumphs of Greek rationality, such as the development of axiomatic-deductive proof, had negative effects when that was adopted as a model in fields, such as medicine, to which it is not at all appropriate. Joseph Needham, who pioneered a far more favorable appreciation of Chinese work, became obsessed with the question of priorities, to the point where that distracted him from a close investigation of the fundamental intellectual and methodological underpinnings in different ancient fields of inquiry both in China and elsewhere.

Evaluation has repeatedly been the occasion for the display of rampant prejudice, and yet evaluation is inevitable. All science, and all history of science, presupposes a theoretical framework and is, to that extent, evaluative. But that does not mean we must inevitably fall prey to blatant self-delusion. We are, of course, the children of our age, the products of our formation or our deformation. But that does not mean we are totally in thrall to our background, our education, or the fashions of the group to which we currently belong. There is no need to fall out of the frying pan of the recognition of theory-ladenness into the fire of sheer determinism.

While it would be extravagant to attempt any general theory of evaluation, applicable to any type of investigation in any field, it seems possible to recommend, for the

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purposes I am concerned with here, what one might call a two-stage interactive model of evaluation (cf. Lloyd forthcoming). The first key step is to assess each ancient inquiry in its own terms, avoiding judging it by modern criteria let alone seeing it as directed to modern goals. In the process we can learn a good deal about what are, initially, very strange beliefs and modes of inquiry. That is where some of the interaction comes in, for we may have to revise our idea about what the investigation is an investigation of. This too is a point where we may have an advantage over the study of contemporary science or at least an opportunity that it lacks, provided, that is, that we are prepared to keep an open mind about what those beliefs meant to those who held them.

But then secondly, just because we start with their, actors', categories, rather than our, observers', ones does not mean that we should never bring to bear what we can nowadays claim to know in our accounts of ancient work. One prime example of this is Huang Yilong's study of the forgeries in ancient Chinese eclipse records (Huang 2001). We have, of course, to be sure of our texts and of the dates of the phenomena reported. But when those are secure, we find instances of eclipses that are recorded, but that never occurred, and conversely we can identify others that were in principle visible but that were ignored. In both cases the reasons are generally ideological, relating to the reputations of the rulers in power at the time. In the reigns of good ones, eclipses should not occur: while they would be expected to be particularly frequent when the rulers were debauched or corrupt. Yet once we have identified ideological factors influencing the records that were made, we should not lose sight of the fact that most of the reports in our sources were free from such influences: most indeed attained high levels both of accuracy and of precision (cf. Sivin 1995, II).

The next complication that must be mentioned relates to the transmission of ideas and practices between cultures that were in more or less regular contact with one another. Just what ideas and information the Greeks took from ancient Mesopotamia or from Egypt – and when they did so – are questions that are still far from having clear, let alone definitive, answers. The study of the transmission of ideas from Arabs to Chinese, from Chinese to Arabs, and from both to Western Europe, has really only just begun (see Abattouy, Renn, and Weinig 2001). There seems to me to be an enormous growth area here: in years to come, scholars may be expected to be in a much stronger position than we are to identify influences – and explain them. The question is not just what ideas and practices *could* have been transmitted, but which actually were – and what was made of them by those who received them, how indeed they were transformed, and sometimes deformed, as they were transmitted.

That takes us back, once again, to the fundamental issues of why inquiries took the form they did in the societies in question. What were the aims of "mathematics"? How indeed were the studies of numbers and of shapes defined and what practical or theoretical ends did they serve? To what extent was the study of the heavens geared to the regulation of the calendar or to the prediction of eclipses and what was the range of opinion about the causes and significance of the latter? When geometrical models were added to arithmetical ones in this field, what drove that ambition? Again in relation

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to problems associated with change, what ideas were entertained about causation or explanation? What modes of understanding were sought? What counted as a good account and by what criteria? We cannot do the history of ancient science without the history of ancient philosophy of science (see Netz 2003).

In each of the principal areas of inquiry for which we have evidence we must examine how investigations were initiated and how they were defined, why the methods used were thought to be the right ones, and how success and failure were judged – whether by outsiders or by insiders. What were the measure and form of the state's involvement in all of this? How far did those who wielded political power or other forms of authority determine the agenda, recruit and pay those who implemented it, and assess their results? Or how far could private individuals decide for themselves on how they set about their inquiries and who, then, were they endeavoring to persuade? What influence did the contexts of communicative exchange have and what impact did the technology of communication have on what could be and was communicated? What, in short, were the openings for criticism and innovation and who could embark on the discovery of what could be presented as new knowledge, and at what risk to themselves?

A historian of ancient science thus gains access to ancient world-views, values, and ideologies in a distinctive way. But our inquiry is not just about ideologies. The work of the ancient investigators – their explanations and especially their predictions – were subject to styles of verification that have no parallel in other areas of historical study. True, plenty of ad hoc explanations were dreamt up in antiquity – as they have been later – to excuse a prognosis that turned out false. Whether a prediction *had* been falsified was always a matter of interpretation. Yet in their ambitions to describe, explain, and understand the world around them ancient scientists constantly found reality – as they conceived it, and not just as we may – breaking in.

The problems I have sketched out can, as I said, only be tackled one at a time, in relation to one society, even one group, in one field and in one period: only then can we proceed to collate the results and build up a more general picture to be tested against new data. The papers in this collection exhibit a variety of approaches, some making more, some less, use of comparative materials. Some identify specific issues and tackle them in exemplary fashion with fascinating results. I observe that some of those that deal with Greek problems (Bob Sharples, John Vallance, Robert Wardy) mirror one feature of their subject-matter, namely that new interpretative hypotheses often emerge from scholarly disagreement. But Myles Burnyeat's reconstruction of pre-Aristotelian optics reminds us of what can be done with indirect argument when the primary evidence is lacking. Yet I would dare to hope that alongside continuing attacks on highly concrete specific issues, one principle will not be lost sight of as our subject develops. Insofar as it may be suggested that our ultimate target – or one of them – is the understanding of why the sciences developed in the different ways they did in different ancient societies, to answer that question certainly depends on an awareness of alternatives, not just theoretically possible ones that might be arrived at

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by abstract analysis, but the alternatives that the comparative historical record actually brings to light.

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