BRITISH ACADEMY LECTURE

On the 'Origins' of Science

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I SHALL START WITH a naive question. Why is science not universal, that is found universally, in cultures ancient and modern, world-wide? The question might be thought to arise naturally from two assumptions, both of which have an obvious appeal, even though both are clearly problematic. I mean the assumption, first, that what's there for science to study—observe, predict, and explain—that is universal, that does not vary across cultures and times. Secondly, are not humans the same everywhere, so far as their basic cognitive equipment goes?

If we accept these two assumptions, then my starting-question looks as if it can hardly be avoided. But I said both assumptions are problematic. The first (the sense that what's there for science to study is universal) runs into difficulties associated with a certain simplistic realism. Of course, in a sense there is not anything 'out there' to which *unmediated* access is possible, for scientists or anyone else. All description is (as the jargon goes) more or less theory-laden. Quite a lot of familiar modern science is intensely theory-laden, presupposing lots of other science, to the point, on occasion, where it is very difficult for the non-specialist to follow what is being said. We can retreat, as has often been pointed out, from greater theory-ladenness to less. But there is no *ultimate* retreat to a *totally* theory-free language.

That is the difficulty for the first assumption. Yet there is still a reasonable sense to be attached to the idea that there were, for instance, certain lunar and solar eclipses that occurred that were visible at certain

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locations, at certain dates and times. Their visibility did not depend on their actually being observed, nor on whether whatever observers there were had any idea that corresponds at all closely to what we call an eclipse. Thus Tuckerman's Tables, retrodicting planetary, lunar, and solar positions from 600 BCE onwards, provide a welcome and on the whole reliable tool for the historian to use in evaluating ancient reports in the light of what was there for the ancients to see.[1]

The second of my two assumptions, that human cognitive equipment does not vary, is much trickier and in one sense plain false. Of course intelligence varies, whatever we mean by 'intelligence' and whether or not we like quantitative measurements of whatever we mean by it: I do not. Nor is that the only problem. Developmental psychology since Piaget (at least) has been hard at work studying children's, even infants', perceptions of space, time, causation, number, and much else, over the first decade or so of their lives. Yet the difficulty I have with much of the work done in this area is that it is suspiciously Western in orientation. Even some of the more sophisticated cross-cultural studies of Yoruba, Tzeltal, or Japanese subjects still tend to test *their* performance *against* those of American or English (it used to be Swiss) ones.[2] On many questions I do not think enough reliable evidence is yet available for us to make confident pronouncements on whether there *is* cross-cultural invariance in conceptual frameworks.

However, in one area at least we can be sure that certain cognitive equipment *is* universal, and that is in the matter of language acquisition. The capacity to acquire language is common, even though the natural languages actually acquired vary so dramatically. More than that, there is an impressive, linguists might say overwhelming, case for the thesis that, despite their surface differences, all languages exhibit a common deep structure—whatever reservations we may have about the working out of the Chomskyan programme in detail. Even so, we can be confident that we have more than just biology to cite, to confirm our common humanity.

So if my starting assumptions have *some* justification—what is there to study is universal, and what we have to study it with might be thought

[1] B. Tuckerman, *Planetary, Lunar and Solar Positions, 601 B.C. to A.D. 1* (Memoirs of the American Philosophical Society 56) (Philadelphia, 1962) and *Planetary, Lunar and Solar Positions, A.D. 2 to A.D. 1649 at 5 day and 10 day intervals* (Memoirs of the American Philosophical Society 59) (Philadelphia, 1964).

[2] An honourable exception should be made of the work of S. C. Levinson's group, Cognitive Anthropology Research Group at the Max Planck Institute for Psycholinguistics, Nijmegen.

so too—then why do we not all come up with the same results? What we actually find, in ancient and modern societies, is an almost incredible diversity of belief systems, not just in such areas as religion, or aesthetics, or morality (where there may be no good reason not to expect diversity) but also with regard to beliefs about the world. True, there is some variety in the natural environment encountered in different parts of the world, in the flora and fauna particularly. Some cultures are, others are not, familiar with oceans, or with snow: some see the northern constellations, others the southern and so on. But the ways in which explicit beliefs, about the classes of animals and plants, about the stars themselves, vary, go far beyond anything that can be put down to environmental diversity.

At this point two natural reactions suggest themselves. The first is to say that all that diversity of beliefs about the world is the direct outcome of cultural diversity. You should not expect uniformity in beliefs in that area any more than in, say, religion, since the operative factors are all cultural, in other words society-specific. The second is to make short shrift of my expectation that we should find science everywhere by denying that it is anywhere, before the twentieth century, or, maybe, the nineteenth, or maybe if you push it, the seventeenth. I shall come back to the problem of demarcation that that second reaction poses in due course; but first some comments on the first of my two reactions.

The strong version of that is that beliefs about the world are all culturally determined. Durkheim famously suggested that classification systems in general, including, for example, of animals and plants, reflect social structures. The key to all that diversity would lie in the diversity of human social arrangements, kinship systems, and the like. Why those social arrangements in turn vary is not the kind of question that can easily be framed, let alone answered. Attempts to find universal deep structures in kinship systems have not yielded much in the way of positive results.

But one problem with the extreme cultural determinist position is that it leaves no room for change. I do not mean as a reaction to influences from outside the society in question, but from within it. Identifying changes in beliefs about the world is certainly difficult. Some changes are very much in the eyes of the beholder, the historian or the anthropologist. Some are peripheral to the core of the world-view, which otherwise remains intact. Yet history provides some good examples where substantial elements of systems of beliefs about the world *have* undergone quite radical change. I shall be mentioning some examples, from Babylonia, China, and Greece, in a minute, and others could be given. But the lesson of those changes—an obvious one, maybe—is that there is a certain *plasticity* in the workings of the human mind. Even if most of what most people believe, on just about every subject, reflects how, when, and where they were brought up, not *all* of what *everyone* has always believed is *just* the result of the processes of their social incorporation. There is human inventiveness, in relation to ideas about the world as well as in other regards, even though that inventiveness has its limits and obeys its rules, however hard these may be to specify.

The second reaction I mentioned, that it is a mistake to talk of science at all other than in very modern times, is one with which I have a good deal of sympathy, although it too runs into difficulties. My sympathy stems from the fact that the science we are used to today is, in certain respects, very different from anything that went before. Many fields of research simply did not exist until a few decades ago. The institutions within which scientific work is conducted, the research laboratories, are of a complexity and sophistication that are without parallel in earlier centuries; and that makes a fundamental difference.

But there are two main difficulties for the hard line that has it that there was nothing that could count as science before, say, the late nineteenth century. The first is that the problems do not go away, though their focus may indeed shift. There is still the question of specifying the criteria of science, and that is not just a matter of how the term was used when it was finally coined—in that century.

But then the second difficulty is that, on any story, modern science draws on and uses earlier ideas, and the emphasis on the discontinuities of nineteenth- and twentieth-century work runs the risk of discounting some important continuities, in the studies that become astronomy, optics, harmonics, geology, anatomy, physiology, just to mention some of the more obvious subject-areas as we label them. The workings of the human body, and the pattern of the movements of the heavens, including for instance eclipse cycles, were the subject of concentrated investigations in a number of ancient civilisations already.

It would be absurd to represent what those earlier investigators were doing as 'inventing science': they did not know what was to come. They had their own aims, motives, methods, and assumptions. But if we are not content to treat science as we know it as something of a mystery, then it is precisely those earlier aims, motives, methods, and assumptions that we need to get to grips with. For many, the focus of interest would be the sixteenth or seventeenth centuries, what used to be called the 'scientific revolution' (though nowadays it is generally agreed that is more misleading than helpful as a characterisation of complex periods marked by continuities as well as change). But in this lecture I want to push back much further still, back indeed to those ancient investigations, not in the hope of *finding an* origin (that is the wrong question: as the data will bear out), but to identify the several, conflicting factors that contributed either to stimulating or inhibiting systematic inquiry. The strategic issue is, then, how did any such inquiry get going?

So to my case-studies, and it is fortunate that, for each of them I can draw on recent work by colleagues and collaborators. I shall use the comparison and contrast between the three cases to try to throw light on that strategic issue, as I have just called it. All three relate to the study of the heavens, and so, to the extent that the subject-matter is the same, facilitate those comparisons and contrasts.

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Babylonian records for the study of the heavens are enormously rich and comprise three main groups of materials, a vast omen literature going back to the second millennium BCE, then the Letters and Reports by scribes to the Assyrian kings dating mostly from the mid-seventh century, and finally the Seleucid sources (from the late fourth century), the Astronomical Cuneiform Texts, Goal Year Texts, Almanacs, and so on, that incorporate, among other things, sophisticated arithmetical models for a variety of astronomical phenomena.[3] I am interested here particularly in the predictions in the second group of data, the study of which has been given a new impetus by the work of Francesca Rochberg and David Brown especially.[4]

As in some other areas of Babylonian inquiry (medicine, for instance), these predictions often take the form of conditionals. If so and so (the sign), then so and so (the outcome). We find many such already in the omen texts, such as the series known as Enūma Anu Enlil, put together some time between 1500 and 1200, but incorporating even earlier work.

[3] See especially O. Neugebauer and A. Sachs, *Mathematical Cuneiform Texts* (American Oriental Series, 29) (New Haven, 1945) and O. Neugebauer *A History of Ancient Mathematical Astronomy*, 3 vols. (Berlin, 1975).

[4] David Brown, *Mesopotamian Planetary Astronomy-Astrology* (Groningen, 2000). Among Francesca Rochberg's important studies are *Aspects of Babylonian Celestial Divination* (Archiv für Orientforschung, Beiheft, 22) (Horn, Austria, 1988) and two forthcoming papers, 'Scribes and scholars: the tupsar Enuma Anu Enlil' and 'Empiricism in Babylonian omen texts and the classification of Mesopotamian divination as science.'

One famous example is the Venus tablet, referring to the reign of Ammiṣaduqa around 1600, which contains empirical data about the appearances and disappearances of Venus recorded in conjunction with such ominous predictions.[5] Those predictions relate to such matters as the harvest prospering, the outbreak of hostilities or the formation of alliances: the political import is obvious. In other tablets we find such presages as: 'if Mars approaches the Scorpion: the city will be taken through a breach',[6] or 'if a star flares up from the West and enters the Yoke: there will be revolution'.[7]

But then from some time around the mid-seventh century there was a shift—as David Brown has insisted—both in what was being predicted and in the confidence and accuracy of some at least of the predictions. Many of the phenomena that had figured in the protases of the omen texts, the if-clauses, came to be rigorously classified and precisely predictable, that is not just in terms of an ideal pattern but including the deviations from such. These include (1) the length of the month as determined by successive visibilities of the new moon, (2) the phases of the planets, that is first and last visibilities, stationary points etc., and (3) lunar, and within limits solar, eclipses. Let us be clear about what is new in all of this. Changes in the height of the sun and in the length of daylight had, no doubt, always been recognised as conforming to certain general patterns, and so too the phases of the moon and the configurations of the constellations at different seasons of the year. But what happened in Babylonia—for the first time ever, so far as our extant evidence goes—was an appreciation of far more complex cycles.

The possibilities of determining, in advance, when a planet would become visible after a period of invisibility, or of saying when an eclipse of the moon or the sun would occur, or at least was possible, offered an altogether new scope for prognostication. Admittedly much remained beyond that scope. The scribes squabble not just about what could be predicted, but about what had in fact been observed. One writes: '[He who] wrote to the king, my lord, "the planet Venus is visible" . . . is a vile man, an ignoramus, a cheat! . . . Venus is [not] yet visible.'[8] But against that, they

[5] E. Reiner and D. Pingree, *Babylonian Planetary Omens, Part 1, the Venus Tablet of Ammiṣaduqa* (Bibliotheca Mesopotamica, 2, 1) (Malibu, 1975).

[6] E. Reiner and D. Pingree, *Babylonian Planetary Omens, Part 2, Enūma Anu Enlil Tablets 50–51* (Bibliotheca Mesopotamica, 2, 2) (Malibu, 1981), p. 41.

[7] H. Hunger and D. Pingree, *MUL.APIN. An Astronomical Compendium in Cuneiform* (Archiv für Orientforschung, Beiheft 24) (Horn, Austria, 1989), p. 115.

[8] S. Parpola, *Letters from Assyrian and Babylonian Scholars* (State Archives of Assyria, vol. 10) (Helsinki, 1993), p. 54.

are confident over a range of phenomena including, for instance, the conditions of possibility of eclipses. 'As regards the watch of the sun about which the king, my lord, wrote to me, does the king, my lord, not know', another scribe writes, 'that it is hardly necessary?'[9] A clear difference opens up between a style of prediction that focuses on the good or bad fortune that will result *if* a celestial phenomenon occurs, on the one hand, and, on the other, one that predicts such celestial phenomena themselves.

The development of that latter possibility did not mean that the phenomena in question were no longer considered ominous. On the contrary, eclipses, in particular, were still considered inauspicious—not that they were thought to be causes of evil events to come, only signs of them. At the stage when the scribes were able to predict one or its possibility, they could and did warn the ruler, who set about diverting disaster from himself by the ritual of the substitute king (*namburbû*).[10] Some wretch who was considered dispensable was put on the throne, so that whatever mischance befell would happen to him, not to the real king, who was addressed meanwhile as 'the farmer'.

Let me now summarise the features I take to be important. (1) The study of the heavens was not undertaken for its own sake in Babylonia: rather, it was driven by a desire to acquire, somehow, advance knowledge of what was in store for the king or state. The work continued uninterrupted, indeed, throughout the political turmoils of the time, whoever it was who ruled Babylonia, the Babylonians, the Assyrians, even the Persians, and whether it was Babylonian scholars or Assyrian ones who did the work. Evidently, these studies were too important for whoever was in power to ignore or discontinue. (2) The individuals involved, the scribes or scholars, *ṭupšarru*, mainly, were based at court or in temples and reported directly to the kings. (3) The discovery of certain regularities in the phenomena did not lead to those phenomena no longer being considered ominous. (4) Nor was prediction subsequently restricted to the study of such regularities, since other much less predictable phenomena—storms, lightning, hail, for instance—also continued to be the subjects of attention. Success in some areas did not lead to a concentration purely on them.

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[9] S. Parpola, *Letters from Assyrian Scholars to the Kings Esarhaddon and Assurbanipal, Part 1 Texts* (Alter Orient und Altes Testament, 5, 1) (Neukirchen, 1970), p. 29, cf. *Part 2 Commentary and Appendices* (Alter Orient und Altes Testament, 5, 2) (Neukirchen, 1983), pp. 51–2.

[10] See J. Bottéro, *Mesopotamia: Writing, Reasoning and the Gods*, trans. Z. Bahrani and M. Van de Mieroop (Chicago, 1992), ch. 9.

Some of the same features recur in my second case-study, the investigation of the heavens in China (where I am engaged in an extensive collaboration with Nathan Sivin).[11] The Chinese distinguished between *li fa* and *tian wen*. The first is conventionally translated 'calendar studies', but it included other computational work as well, for example in connection with eclipses. The latter is the study of the 'patterns in the heavens', essentially qualitative in character, but including both cosmography and the interpretations of celestial phenomena thought to be ominous.

As in Babylonia, these studies were a matter of *state* importance, indeed of personal concern for the ruler (after the unification of China by the Qin in 221 BCE, the emperor). He was considered responsible not just for the welfare of the state, but for preserving harmony between heaven and earth. The so-called 'monthly ordinances', *yueling*, found in such late Warring States and early Han texts as the *Lüshi chunqiu* and *Huainanzi*, set out precisely what the ruler and the whole court have to do to ensure this harmony, the music to be played, the kind of food to be eaten, down to the colour of the dresses the court ladies should wear. The *yueling* texts end the account of each month with dire warnings as to what will happen—natural disasters and political ones—if the ritual is not followed to the last detail.

So it was evidently very important for the ruler to know, in the first instance, that the calendar was in good shape. But then the heavens needed to be scrutinised for *any* sign that might be thought to contain a message, for the ruler, his ministers, state policy, or whatever. That involved, potentially, a vast programme that was carried out, from Han times, in an Astronomical Bureau designated for the purpose. Of course, if (as explained) it was in the interest of the emperor to know everything that was happening in the heavens, it was also in the interests of the staff in the Astronomical Bureau that this imperial interest was maintained—for their jobs, after all, depended on it.

Their success on that score was unquestioned. The Bureau lasted for some 2000 years, down to the last imperial dynasty, the Qing. Their more purely astronomical performance was mixed. Among the more notable successes were first calendar regulation and determining more and more accurate lunar and solar eclipse cycles, and secondly discriminating between

[11] See, for example, N. Sivin, 'Cosmos and Computation in early Chinese Mathematical Astronomy' (originally 1969) in *Science in Ancient China: Researches and Reflections* (Aldershot, 1995), and G. E. R. Lloyd, *Adversaries and Authorities* (Cambridge, 1996), and G. E. R. Lloyd and N. Sivin, *The Way and the Word* forthcoming.

what was strictly predictable and what was not. Among the latter, Chinese records of novae, supernovae, and sunspots are the most complete we have down to the seventeenth century.[12] In the case of some of the mistakes that were made—when an eclipse that had been predicted did not occur, for example—that was sometimes excused (in the Tang) with the argument that the non-occurrence was a sign of the special virtue of the emperor. Such was his virtue—so it was claimed—that an eclipse that would otherwise have happened, did not. The incorrectness of the prediction was then not chalked up *against* the astronomers, but *for* the emperor.

Some of our sources indicate that a job in the Bureau was sometimes treated as a sinecure. The officers did not bother to carry out the regular observations, but recorded them on the basis of what had been predicted—which, of course, the observations were supposed to check. Yet when the calendar got out of step, there was the possibility, even in the long run the inevitability, of this being found out. Sometimes this happened as the result of the work of individuals outside the Bureau—though if they were successful in proposing reforms, they might well find themselves drafted into it. In the face of criticism, the tactics were to incorporate the critics, not to change the Bureau's structures. It was too important to abolish, and after all it produced notable results, both observational and, where eclipses were concerned, also theoretical, when predictions not just as to date and time, but also as to magnitude and duration, came to be within reach.

So what this brief survey of some early Chinese astronomy shows is first how the perceived political importance of the subject led, here, to sustained state support, and then both the advantages and the drawbacks of that, the continuous programme of work that could be undertaken, and the occasional stagnation of that programme when jobs came to be treated as sinecures.

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For my third case-study, I turn to Greece, where a first major difference relates to institutions. Unlike the Babylonian *ṭupšarru*, and the officers of

[12] See, for example, Xi Zezong and Po Shujen, 'Ancient oriental records of novae and supernovae', *Science*, 154 (1966), 597–603, D. H. Clark and F. R. Stephenson, *The Historical Supernovae* (Oxford, 1977).

the Chinese Astronomical Bureau, Greek students of the heavens did not usually work for kings and could not count on regular support from state institutions. The Alexandrian Museum, during the reigns of the first three Ptolemies, was something of an exception but the patronage it offered was trifling compared with the Chinese Bureau. The Greeks were, far more often, on their own, and so we have to ask what motivated their work, how they saw it and how it was seen by others. How indeed did they make a living? What effect did all of that have on their programme of research, if we can call it that?

I wish the answers could be given as crisply as I have just stated the questions. Although I am not one, usually, to risk generalisations, let me propose the following observations with regard to Greek astronomy when it gets going in the fourth century BCE. First, reputations depended on impressing not a ruler, but your contemporaries, not just fellow specialists but even the general public. Secondly, teaching was one of the main ways of earning a living, and that is connected, thirdly, with the institution of the public lecture or debate, the main vehicle both for building up your reputation and for attracting the fee-paying pupils you (partly) depended on for your livelihood. To that may be added, fourthly, but only from the Hellenistic period on, that you could earn quite a bit of money by casting horoscopes.

Correspondingly, the effects I see of the fourth-century Greek situation on the nature of the work done include, first, that there is a premium on originality: you were not going to impress a lecture audience very much by telling them what they knew already. Secondly, one tactic often used to get your own, new, ideas across was the demolition of everyone else's: that favours the highly critical scrutiny of foundations. Third, for your own part, you had to try to make your own position immune to such criticism: in this context, a rigorous notion of demonstration is developed and used in fourth-century Greek mathematics and related inquiries. More on that later.

The key fourth-century astronomer is Eudoxus, who can be used to illustrate both the dilemma that the Greek intellectuals found themselves in, in making a living, and some of the characteristics of the style of inquiry they promoted. We have some biographical information about him that comes, it is true, from a late source,[13] and so may not be too

[13] Diogenes Laertius, *Lives of Eminent Philosophers*, ed. R. D. Hicks (1925), Vol. 2, Book 8, chs. 86 ff.

reliable in detail, but that certainly gives us some clues about the situation in which he, like many other Greek intellectuals, found himself. He was born in Cnidos, where he was supported, in the first instance, by a doctor named Theomedon (not otherwise known as much of a patron). It was said he was Theomedon's lover. After a first trip to Athens and contact with the 'sophists' teaching here, and then further visits to Egypt, Cyzicus, the Propontis, and the Court of Mausolus, he returned to Athens. By then, however, he had acquired a large number of pupils, attracted (it would appear) not just by his work in astronomy and mathematics, but also, for example, in ethics.

We know that Eudoxus later became an associate of Plato and joined Plato's Academy (a private foundation, we must remember, that depended on the wealth of its members and then on the fees of its pupils for its finances). But he evidently originally made his reputation on what it would not be too anachronistic to call the lecture circuit. Like many of the so-called sophists (most of whom get such a bad press from Plato), he moved from city to city, and used the open, public, lecture as the chief means of attracting pupils.

That may not sound particularly interesting or important; but it surely becomes so when we think of the pressures that went with that way of making your reputation. Plato, of course, saw the whole thing very negatively. The sophists were just a bunch of irresponsible rhetoricians, teaching people to 'make the worse cause seem the better', doing so for money indeed, undermining the moral fabric of society. (If you find this reminiscent of the science wars debate, and the tiffs between the deconstructionists and their critics, you would not be the first to do so.)

Yet there were also positive repercussions of the hot-house atmosphere of open public debate, some direct, some indirect. I have mentioned the two fundamental points already. First there is the scrutiny of foundational assumptions. You went all out to probe your rivals' preconceptions, and, expecting similar treatment from them, you did your best to justify your own principles and your own methodology.

The second relates to a particular mode of justification that came to be demanded, not, it is true, by the most famous of the sophists whom Plato attacked, but rather by certain philosophers, on the one hand, and mathematicians, on the other. The objection that Plato himself brought against these sophists was that they dealt in the merely *persuasive*, whereas what he, Plato, required was *proof*, a style of reasoning that would secure certainty. Aristotle, in turn, defined strict demonstration as proceeding from self-evident primary premisses, via valid arguments, to incontrovertible conclusions. But while it was the philosophers who supplied the theory of such demonstrations, it was chiefly the mathematicians in the Euclidean tradition who exemplified it in practice, and the most notable of Euclid's mathematical predecessors was undoubtedly Eudoxus—even if the precise extent of his mathematical contributions to what we think of as Euclid's *Elements* is not now fully recoverable.

Yet if we turn back to what we know of his astronomy, the key feature that marks it out from the work of, say, the Babylonians is that he attempted geometrical models from which the movements of the planets, sun and moon could be derived and so explained. It is pretty clear that he fell some way short of giving a fully quantitative would-be demonstrative model. That was not to be achieved until Ptolemy in the second century CE. Yet that was pretty certainly already Eudoxus' aim.

The contrast with what we know of Babylonian astronomy is a double one. From the seventh century, the Babylonians were in a position to make some impressive predictions of certain planetary phases on the basis of observed periodicities, but they had no interest whatever in geometrical models, setting out the configurations of the planets and showing how their apparent irregularities could be seen as the product of a combination of regular motions. Much later than the Babylonians, in the fourth century, Eudoxus made at least a start at geometrization. But he and his immediate successors were in a far weaker position than their Babylonian counterparts had been in the matter of making good determinate predictions. That, frankly, was well beyond Eudoxus; and although it was not beyond Ptolemy, we have to remember that by then Greek astronomers could and did draw on Babylonian data in some quantity.

I cannot go into the complexities of later Greek developments, any more than I could the Babylonian ones, but it is worth mentioning very briefly yet another Greek development that reflects, I believe, that very Greek preoccupation with foundations. Neither Babylonian nor Chinese students of the heavens drew a contrast between what we should call astronomy and astrology. The Greeks certainly did, not in those precise terms, but in terms of a contrast between two models of prediction, one directed at predicting the movements of the heavenly bodies themselves (i.e. astronomy) and the other directed at predicting on their basis events on earth.[14]

[14] The classic text is Ptolemy, *Tetrabiblos*, trans F. E. Robbins (1940), Book 1, chs. 1–3.

I would view this as yet another result of those Greek concerns with foundational questions, with epistemology, with methodology. Sooner or later, in Greece, any inquiry was liable to challenge—to define itself and to justify its very viability. On the astronomy/astrology issue, all three positions were taken up. First, there were those who accepted the first, but dismissed the second (Cicero sets out many of the arguments in his *De Divinatione*). Secondly, probably the majority accepted both: Ptolemy did so, turning the demonstration/conjecture contrast to use, to line up astronomy on the one side, astrology on the other. Yet others, thirdly, among the Sceptics and Epicureans would have none of either inquiry, neither astrology—superstitious humbug—nor yet astronomy—hopelessly fanciful speculation.

There is obviously far more to Greek inquiries than can be hinted at in this brief sketch. But I hope I have said enough to allow certain salient points to emerge. The concern on the one hand with foundations and on the other with axiomatic-deductive demonstration are distinctive features of much (although certainly not all) Greek investigations. But they can be seen as, in part, a response to the situation in which most Greek intellectuals operated. Lacking state institutions that gave stable employment, they were in open and more or less continuous competition with one another, as teachers especially. The chief ‘institution’ (of a different kind) they learned to live with was the free-for-all of public debate, in which foundational assumptions came under intense scrutiny, and where many sought victory by means of rigorous demonstrations, securing (they hoped) incontrovertibility.

That may sound very promising. Yet the downside should not be forgotten (though it often is). To mention the three most important points: first, proof in the geometrical mode (*more geometrico*) became not so much a preoccupation as an obsession. In mathematics itself, as I think we can see from Archimedes’ *Method*, it inhibited the presentation of discoveries until rigorous proof was forthcoming, and outside mathematics it proved an implausible model on which to base reasoning in such areas as medicine (for where were there self-evident axioms there? Yet that did not stop Galen from trying).[15] Secondly, demonstrations were only as good as their premisses. The Greeks insisted on self-evident axioms, for

[15] See G. E. R. Lloyd, ‘Theories and practices of demonstration in Galen’, in *Rationality in Greek Thought*, eds. M. Frede and G. Striker (Oxford, 1996), ch. 10, with references to earlier scholarship.

starting from hypotheses or conjectures would not yield the incontrovertibility you aimed for. But even in mathematics self-evident axioms were in short supply. It was both a stroke of genius and a disaster, on Euclid's part, to make the parallel postulate a postulate. He evidently saw it could not be proved within the system; but he undoubtedly took it as no mere hypothesis, but an unquestionable truth. Its eventual denial, and the development of non-Euclidean geometries on its basis, would no doubt have left him stunned. Third, the radical scrutiny to which every premiss, argument, conclusion, might be subject, certainly undermined the formation of a consensus on substantive issues or even on methodology. By late antiquity, the non-delivery of what could be seen as stable results, in most areas of inquiry, led, in some quarters, to a deep pessimism about inquiry itself. But by then, in the West, many had in any case begun to turn elsewhere for inspiration and solace, namely to Christianity.

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We have come quite a way from my opening naive remarks about the uniformity of human cognitive capacities and the objectivity of what there is to study. My first move was to redefine the questions we need to focus on, not so much the 'origin' of science, as the factors that stimulated or inhibited systematic inquiry. What interests provided a stimulus, and whose interests were at stake?

Two main motive forces are detectable in early investigations of the heavens, the desire to look into the future (prediction) and the wish to understand (explanation)—except that in neither case will it do to stay at that level of generality. The study of the stars was partly motivated, in all three ancient societies I discussed, by the assumption that there were messages there to be deciphered, of importance for the state, the ruler, even private individuals. But if the Babylonians, in particular, began their investigations with one goal—to interpret omens—they found another one—the ability to predict certain heavenly phenomena themselves—amply fulfilled. The bid to predict, here, had some unpredictable consequences in the discovery of some rigorously predictable regularities.

We have to qualify the point about the search for understanding too. Everyone no doubt always wants to understand. But normally in most traditional societies you make do with ready-made understanding. You do not normally question—not radically, not rudely at least—whatever you are told, about the sun going round the earth and why it disappears at night. But the particular enthusiasm for questioning and overturning traditional ideas that we find in Greece—and the proliferation of alternative explanations, including some highly counterintuitive ones, such as that the earth goes round the sun—owe much, I argued, to the particular situation in which Greek intellectuals worked. That questioning was one way you made a name for yourself and earned a living as a lecturer or a teacher.

But if I am right about the *varying* factors at work in the course of the development of sustained investigations of the heavens in these three ancient societies, then that in turn suggests that some of the more popular, or at least widely diffused, ideas that have been suggested are pretty seriously flawed. Many attempts to account for what has been dubbed the Great Divide (the gulf that separates prescientific cultures from ourselves) suffer from oversimplifying either the explanandum (what *was* that Divide) or the explanatory factors invoked to account for it. It is not just the period of the so-called scientific revolution that has often been dealt with superficially, but also antiquity.

For Farrington and others, the key difference was secularisation, leaving the gods out. But while that may do for some early Greek natural philosophy, it discounts the fact that when the Babylonians studied the stars (as they did with such success) they considered them gods—as indeed many Greeks did too.

Then, for Popper and others, criticism was the key, possible only in an 'open society'. Yet China was never, in that sense, open. But that did not prevent the Chinese from doing sustained observational and theoretical work, in astronomy and in other fields.

Joseph Needham, who did more than anyone to insist on those Chinese achievements, thought of the divergent ancient traditions as rivers all running to the single sea (of modern, ecumenical, science, that is). But while that allows for pluralism, it makes it all seem too easy. It does not allow for the fact that many ancient inquiries came to be seen, by the ancients themselves, as just dead ends.

At a more basic level, for sure, we can identify some common factors that may be considered necessary conditions for the emergence of the kinds of study we have discussed.[16] All three of our ancient societies possessed economies, technologies, and political institutions of a certain degree of complexity. All three had advanced levels of literacy among

[16] These issues have been aired by J. Goody, *The Domestication of the Savage Mind* (Cambridge, 1977), cf. G. E. R. Lloyd, *Magic Reason and Experience* (Cambridge, 1979), ch. 4.

some of their members at least, even while the forms of writing used vary so widely. Obviously, for sustained and systematic inquiry, and not just in astronomy, you need to be able to preserve, draw on, and transmit the work of each generation, and that means a sufficiently robust technology of communication and social institutions to exploit it. Yet invoking such general factors as these does not help with the specificities of the nature of the inquiries that were undertaken—our explanandum.

The three case studies I have sketched out suggest first that science developed very differently in Babylonia, China, and Greece, both in the nature of the investigations undertaken, and in terms of the social and intellectual institutions within which the investigators worked. And in some cases there appear to be connections (between the work and the institutions), not that I am proposing a determinist thesis, that the institutions determined the outcome, as if every individual was similarly affected by them. That clearly would be extravagant.

But then the second point that emerges is the *tension* between different factors that may all be thought to have had some part to play, a tension that serves to underline that there was no one factor that *just* favoured development. The advantages and disadvantages of each system are, in a striking way, the mirror images of one another. On the one hand, state support, the creation of institutions such as the Chinese Astronomical Bureau, carried enormous advantages, offering stable employment for a very considerable staff of specially trained investigators. Yet such institutions could also inhibit innovation—state interests determined the agenda—and they ran the risk of ossification.

On the other hand, without such institutions individuals were far more free to choose their own research programme—and yet have no secure job. The rivalries that went with such insecurity in Greece contributed to the radical scrutiny of assumptions, but just as surely inhibited the formation of a consensus, the sense of the advantage of a joint endeavour of individuals united behind an agreed research programme. For all the impressiveness of Greek intellectual brilliance, for continuity of sustained effort in the observations of the heavens the Chinese won hands down.