

Methods and Problems in the History of Ancient Science

The Greek Case

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THE AIM OF THIS PAPER is to set out, as briefly as possible, some of the chief methodological issues in the study of science in Greek antiquity. What are the main difficulties confronting such a study, what can it hope to achieve, what indeed is it a study of? It used to be assumed that one can straightforwardly pose problems to do with the "origins" of science and the meaning of "science" in antiquity. As will become apparent, I believe the questions have to be reformulated to be investigable.

I shall begin with two fundamental principles, one borrowed from the philosophy of science, the other from social anthropology. The first relates to the distinction between observation and theory, the second to that between actors' and observers' categories. Applying the latter to the Greek case, I shall then focus on one of its fundamental features, namely its pluralism. That in turn will provide a basis to tackle, finally, the problems of the validity of the question of origins and the usefulness of the category of science.

I

Modern philosophy of science has, for some time, insisted that the distinction between observation and theory is a relative, not an absolute one.¹ There is no set of data out there in reality to which we have unmediated access, that is to say data that can be described in theory-free terms and then used to check the theories suggested to explain them. The data the scientist has to work with are not "raw": their description itself presupposes a theoretical, that is a conceptual, framework. There is no such thing as a totally value-free, theory-free, account of the data.

This is not to abolish the distinction between observation and theory entirely, of course. Some descriptions are more, some less, theory-laden. To cite my favorite examples from Mary Hesse, we can contrast talk of "particle-pair annihilation" in a cloud chamber with talk of "two white streaks meeting and terminating at an angle."² Or again we can contrast the use of the term "epileptic fit"

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¹ See, e.g., Ian Hacking, *Representing and Intervening* (Cambridge: Cambridge Univ. Press, 1983).

² Mary Hesse, *The Structure of Scientific Inference* (Berkeley: Univ. California Press, 1974), p. 24.

with the type of description of loss of voice, foaming at the mouth, clenching of the teeth, and convulsive movements of the hands that we have in the Hippocratic treatise *On Sacred Disease*.

We can, as these examples show, retreat from greater theory-ladenness to lesser, but that retreat does not lead to an ultimate refuge of an entirely theory-free vocabulary in which raw sense-data can be set down innocently of all assumptions.

These points, developed in the philosophy of science, have their relevance and applicability also to the history of science. Any attempt to write the history of scientific inquiry at any period involves selection, judgment, a conception of the subject under review, and methodological presuppositions about the historical inquiry itself. The history of science can never be *purely* descriptive any more than the practice of science can. There will always be elements of evaluation in any account offered. The methodology of the historian may or may not be set out explicitly: but there will be methodological assumptions implicit in the account nevertheless. Since methodology is inevitable, it is better to be self-conscious about it.

As in the scientific case, this does not mean that the distinction between greater and lesser theory-ladenness is useless. It must, however, be perceived as a matter of degree. The remark that all history of science is evaluative does not mean that there is no check on our evaluations. That would mean that free rein could be given to prejudice. No: the fact that the facts are not raw data here either does not mean that they cannot be used to revise and modify theories and explanations, even though the facts themselves never come free from all theoretical preconceptions. My own point, that no fact is just a raw fact, applies, of course, to the remark I have just made, to my own use of "fact," for it clearly reflects a point of view, the one I am putting forward in this section.

None of the above points leads to extreme skeptical, subjectivist, or relativist conclusions, though they do counsel caution, in general and with regard to one's own preconceptions in particular. We certainly have to be wary about claims to have understood, let alone claims to have reached satisfactory explanations, in the history of science. As the history of our subject abundantly illustrates, any understanding achieved is at best only provisional, always revisable, often radically so. The conclusions we reach are certainly subjective in the sense that they reflect our preconceptions. But that does not mean that there is no way in which we can become critical of those preconceptions. Dialogue between historians of science is possible, even though no two historians will share exactly the same ideas either on substantive issues in the material they study or on the nature of the study itself.

Understanding another's point of view is difficult and can never be complete, for we must always be prepared to revise what we had thought we had understood. Yet it is not impossible to reach at least an approximate understanding, as the very possibility of such revision presupposes. I can never be sure that what you mean by the terms "star" or "sun" or "heart" or "blood" is precisely what I think you mean by them, in general or in particular collocations. But the principle of charity of interpretation allows and dictates that I adopt some notion of your understanding as a working hypothesis, open to revision as the dialogue between

us proceeds.³ A similar point applies across natural languages, where, to be sure, the divergences in the networks of approximately corresponding senses and references will be far greater—not that there is a single reality being mapped in different ways that could even in principle be set down in a language neutral as between different points of view.

In the study of ancient scientific texts the problems are, of course, appreciably more severe than those of the attempted dialogue between contemporary historians. We cannot enter into dialogue with our authors. We cannot even directly check what they wrote. If the evidence in a dialogue between contemporaries is never fully in, the situation with regard to ancient evidence is immeasurably worse, not just in that it is incomplete, but also because what we have has been biased by the selective processes of transmission.

We must be clear, then, as a first principle, that the history of science is inevitably evaluative: it always presupposes a conceptual framework and a methodology. Our best hope, as historians, is to be as self-conscious and as self-critical as we can about these—not that we shall thereby escape being evaluative.

II

My second principle appeals to, and adapts, the social anthropologists' distinction between actors' and observers' categories.⁴ However, unlike in some social anthropology, that distinction must be subordinated to the philosophical principle I have just adumbrated. The anthropologist studying a foreign society—or indeed the sociologist studying our own—aims, so far as possible, to express the ideas, beliefs, practices of the society in question in the terms used by members of the society themselves—the actors. At the limit this would involve describing those ideas entirely in the actors' own natural language. But short of doing that, it is often necessary to keep many important terms untranslated, allowing their significance to emerge from examples of their use, though these have inevitably to be accompanied by interpretative glosses.

The aim of keeping as close as possible to the actors' own categories is twofold. Negatively, first, it helps to minimize the risks of assimilating alien ideas to our own, of assuming that the subjects studied have the same conceptual framework in mind that is suggested by the interpreters' own (observer) categories. Positively, second, it allows an alien network of meanings to be built up in its own terms and be seen for what it is, as alien.

Of course the moral of my first philosophical principle is that no anthropological attempt to state the ideas and beliefs of a foreign society in what they, the anthropologists, represent as the actors' own categories can ultimately succeed, and this for two reasons. First, the anthropologist is, after all, an observer, not an actor, at least not qua anthropologist. Second, we said, no observation statement is entirely theory-free.

³ Cf. Donald Davidson, *Essays on Actions and Events* (Oxford: Clarendon, 1980); and Davidson, *Inquiries into Truth and Interpretation* (Oxford: Clarendon, 1983).

⁴ See, e.g., the collection of essays edited by Robin Horton and Ruth Finnegan, *Modes of Thought* (London: Faber & Faber, 1973).

Similarly, no historians of ancient science can put themselves precisely in the position of ancient scientists. We could not do so, with Greece, even if we chose to write our histories in (imitation) ancient Greek. Yet as with the social anthropologist, so with the historian of science, it is clearly of cardinal importance to avoid, as far as possible, the assimilation of our subjects' ideas to our own modern conceptual categories. We have to recover, as fully as possible, the original context in which the ideas were put forward, the goals, assumptions, and methodologies of those who put them forward, and again the social and cultural institutions within which they worked and with which they interacted—in every case in their very considerable complexity. Although students of ancient societies cannot, in the strict sense, be ethnographers—we cannot interview our subjects—that should not prevent us from emulating, as far as our materials permit, the kind of dense—or, as Clifford Geertz calls it, “thick”—account that considers the data concerning belief systems against the whole background of the geographical, economic, technological, social, and political situation of the society under investigation.⁵ Doubtless it would be absurd to suppose that our aim should be to think like an ancient scientist. Yet we should and can attempt to investigate their reported ideas and beliefs in their contemporary context, in the light of what we can discover about what they thought they were doing, their motivations, and the social background of their work. We can thereby hope at least to avoid some of the grosser anachronisms that have bedeviled the subject from the uncritical application of modern scientific categories to ancient ideas. I shall have more to say on this topic in due course.

III

When we attempt to apply the two principles I have sketched out to the Greek case in particular, we must be fully aware, first, that the data we have to work with have already been extensively processed: by the selectivity of the evidence, mostly literary, that has survived, by the vagaries of transmission, by the pressures of constant reinterpretation. However, that does not prevent us from being able, second, to recover something of the original *pluralism* of their—actors'—views of the inquiries and activities they engaged in.

When we ask what categories and concepts the ancient Greeks themselves used of their own inquiries, we find quite a variety in play. The terms we conventionally translate “doctor” (*iatros*), “mathematician” (*mathēmatikos*), “philosopher” (*philosophos*), “physicist” or “natural philosopher” (*phusikos*, *phusiologos*), “musician” (*mousikos*), “architect” (*architektōn*), “engineer” (*mēchanikos*), all have points of contact with, but all diverge to a greater or lesser degree from, what those conventional renderings may suggest. Moreover, none of those terms picks out a single inquiry or activity about which the Greeks themselves had clear and unanimous views. Each was in antiquity already the subject of disagreement and dispute, with rival individuals or groups implicitly adopting or explicitly defending divergent conceptions.⁶

We may begin by taking two of what might seem to be among the least prob-

⁵ Clifford Geertz, *The Interpretation of Cultures* (New York: Basic Books, 1973).

⁶ See G. E. R. Lloyd, *The Revolutions of Wisdom* (Berkeley: Univ. California Press, 1987).

lematic terms, namely "doctor" and "mathematician." When we study Greek medicine we have first to rid ourselves of many standard modern assumptions, for example, concerning the differences between doctor and layperson. The institutions we associate with modern medicine—the hospitals, teaching schools, research laboratories—have no equivalents in Greek antiquity, where indeed there were no officially recognized qualifications that gave medical practitioners a legal right to engage in healing the sick.

Although we may talk loosely of "Greek medicine," we can never afford to forget that that term covers some five or six very different styles or traditions, and it follows from the point made in the last paragraph that *every* style or tradition competed with every other on more or less equal terms. The situation was quite different from that which obtains today between official practice and one or more varieties of so-called alternative medicine. There were, for instance, the root-cutters (*rizotomoi*) and the drug-sellers (*pharmakopōlai*), about whom we hear, for example, from Theophrastus. Then there were those called *maiai*, conventionally translated "midwives," though it is clear that the activities of the individuals in question were far from confined to what we call midwifery. Quite how far each of these three groups took for themselves or were given by others the title "doctor," *iatros*, is a delicate and disputed question. Thus none of the male writers represented in the extant Hippocratic treatises dating from the fifth and fourth centuries B.C. can bring himself to refer to his women associates as a doctor: the nearest we get to that is one text that speaks of "the healing woman," *hē iētreousa*, using the participle from the cognate verb. However, in the inscriptional evidence from the fourth century onward we have cases of women healers who are referred to as doctors, *iatros* or *iatreinē*, using the noun in either its masculine or feminine form.⁷

But in addition to these three diverse traditions there were also those who practiced medicine in the shrines of healing gods or heroes, such as Asclepius, establishments that from the fourth century B.C. onward were increasingly well endowed and that remained popular down to Galen's day, in the second century A.D., and beyond. Those healers may be distinguished, in turn, from others who also invoked the discourse of the divine or the demonic, the itinerant sellers of charms and purifications about whom we hear from Plato, for instance, as well as from some of our extant Hippocratic writers.⁸

Then there are those Hippocratic writers themselves, not that they are anything like a homogeneous group.⁹ They disagreed not just with the representatives of other traditions of healing, but also often among themselves, and not just about particular theories of disease or of treatment, but about the aims, status, and methods of medicine itself. Nor are these disagreements just a feature of the fifth and early fourth centuries B.C., for they too clearly continued down to Galen's day and beyond.

⁷ See G. E. R. Lloyd, *Science, Folklore, and Ideology* (Cambridge: Cambridge Univ. Press, 1983), esp. Pt. 2, Ch. 2. Cf. Ann Ellis Hanson, "The Medical Writers' Woman," in *Before Sexuality*, ed. David M. Halperin, John J. Winkler, and Froma I. Zeitlin (Princeton, N.J.: Princeton Univ. Press, 1990), pp. 309–338.

⁸ See, e.g., G. E. R. Lloyd, *Magic, Reason, and Experience* (Cambridge: Cambridge Univ. Press, 1979), Ch. 1.

⁹ See, e.g., Wesley D. Smith, *The Hippocratic Tradition* (Ithaca, N.Y.: Cornell Univ. Press, 1979).

Thus one fundamental dispute related to whether the medical practitioner should or should not base his medical practice on general physical and physiological theories about the constituents of the human body, about the nature and causes of such processes as digestion and respiration, and so on. Some Hellenistic medical practitioners took the view that the job of the doctor was simply to heal and that it is either impossible or useless or both to inquire into such questions as the physical constitution of the body.¹⁰ The two groups concerned, the so-called Empiricists and the Methodists, both rejected the use of dissection, that is of "anatomy" in the primary sense, that of "cutting open." To that others, including Galen, responded by insisting that dissection is important not just for theoretical reasons (for what it can teach you about physiological processes, for instance), but also for vital, practical ones. "If a man is ignorant of the position of a vital nerve, muscle, artery or important vein," he writes in *On Anatomical Procedures* 2.2, "he is more likely to be responsible for the death, than for the saving, of his patients."¹¹

To study ancient Greek medicine, then, we have to try, among other things, to recapture the divergent views held and expressed about the nature and aims of medicine itself. We have, as noticed already, to be aware that our evidence exhibits a very distinct bias, not so much as between one literate group and another (although that is sometimes the case), as between the literate groups on the one hand and those traditions that did not privilege the written word. But having said that, we must make the best use we can of the sources available to us, and even if much of the detail escapes us, they certainly make the general point about pluralism abundantly clear.

It might be thought that some of the issues I have been referring to are specific to medicine, always a problematic and at best only partially scientific activity. One might suppose, for instance, that the nature and aims of mathematics, at least, were uncontroversial and agreed. Yet that is far from being the case.

There are two points here, one relating to the pluralism we can discern in what we recognize as mathematical inquiry in our terms, and the other to do with what the term *mathēmatikē* covered in Greek antiquity. Under the first head we should note that while mathematics in the Euclidean, that is to say axiomatic-deductive, style is well represented in our extant sources, that was far from being the only tradition of mathematical inquiry. There was also, for example, the so-called Heronic tradition of metrical geometry, characterized by its concern with the solution of problems of mensuration. Again there is the distinct tradition, recently explored by Wilbur Knorr in *The Ancient Tradition of Geometric Problems*, concerned with the investigation of such special problems as the squaring of the circle, the trisection of an angle, and the duplication of the cube.¹² Yet again there is a further distinct area of interest in the applications of mathematics to music theory.

Second, due attention has to be paid to the fact that the Greek term we con-

¹⁰ See, e.g., Michael Frede, *Essays in Ancient Philosophy* (Minneapolis: Univ. Minnesota Press, 1987), Chs. 12–15.

¹¹ Translation based on Galen, *On Anatomical Procedures*, trans. Charles Singer (New York/London: Oxford Univ. Press for the Wellcome Historical Medical Museum, 1956).

¹² Wilbur R. Knorr, *The Ancient Tradition of Geometric Problems* (Boston: Birkhäuser, 1986); and Knorr, *The Evolution of the Euclidean Elements* (Dordrecht: Reidel, 1975).

ventionally translate "mathematics" is far more general than that translation might suggest. *Mathēmatikē* is connected with the term *mathēma*, which can be used of any branch of learning, being derived from the verb *manthanein*, an entirely general term for "to learn." What we nowadays call mathematics is included, certainly, in the corresponding ancient term, though some of the styles of mathematics practiced today would scarcely be recognizable to ancient Greek investigators. But while they regularly included arithmetic and geometry under "mathematics," that term covered a good deal more. Thus when Ptolemy contrasts *mathēmatikē* first with theology and then with physics in the opening chapter of his theoretical astronomical treatise, the *Syntaxis* or *Almagest*, he includes the contents of that treatise under that term.

But it is not just that ancient *mathēmatikē* included some of what we might think of as the exact sciences. *Mathēmatikos*, "mathematician," is the word used not just for the person who engages in theoretical astronomy, but also for the astrologer. The distinction between two types of study is drawn by Ptolemy himself, for instance, in terms of the different types of prediction they attempt, although he thinks of both of these as predictions achieved by means of *astronomia* (*Tetrabiblos* 1.1). Thus one type of investigation aims, among other things, to build theoretical models that will enable the movements of the sun, moon, and planets to be predicted. But the other attempts predictions concerning events on earth from the study of planetary configurations, horoscopes, and the like. There are two ancient Greek terms, *astronomia* and *astrologia*, that can be used to draw the distinction that Ptolemy makes. Thus Sextus Empiricus contrasts that part of *astrologia* or *mathēmatikē* that is also called *astronomia* with the part relating to the casting of nativities (*genethliologia*) (*Against the Mathematicians* 5.1). Again there are two corresponding Latin terms, though as W. Hübner has recently shown their use diverges from the Greek.¹³ But far more often, in Greek and in Latin, the terms in question are employed indifferently and with no sense that *astronomia* and *astrologia* deal with distinct areas of study. Moreover, neither term regularly carries any pejorative undertones.

As with medicine and the term for "doctor," the point is not merely a question of semantics. The status and validity of the various kinds of studies of the heavenly bodies were as much topics of dispute as the goals and methods of healing. How far are predictions concerning events on earth possible on the basis of the study of the heavens? Again, how far are the theoretical models, based on epicycles and eccentrics and their combinations, credible? The former question is at the center of a heated debate where not just Ptolemy's *Tetrabiblos* but also Cicero's *De divinatione*, for instance, provides rich materials for the analysis of the arguments used *pro* and *contra*, while the second question is pressed strongly by Proclus among others. While we must certainly acknowledge that astrology was well established and widespread, it would clearly be grotesque to claim that it was unproblematic to the ancients themselves.¹⁴

¹³ W. Hübner, "Der Titel zum achten Buch des Martianus Capella," in *Vorträge des ersten Symposions des Bamberger Arbeitskreises "Antike Naturwissenschaft und ihre Rezeption"* (AKAN), ed. K. Döring and G. Wöhrle (Wiesbaden: Harrassowitz, 1990), pp. 65–86.

¹⁴ See, e.g., A. A. Long, "Astrology: Arguments Pro and Contra," in *Science and Speculation*, ed. Jonathan Barnes, Jacques Brunschwig, Myles Burnyeat, and Malcolm Schofield (Cambridge: Cambridge Univ. Press, 1982), pp. 165–192; and Nicholas Denyer, "The Case against Divination: An

The upshot of my argument so far in this section has been that both “medicine” and “mathematics” in the ancient Greek world diverge from their modern counterparts and that neither was a unitary investigation with an agreed goal, subject matter, and method. But what about the term that might seem to correspond more closely to what we mean by natural science? What about the “inquiry concerning nature,” *peri phuseōs historia*, the study associated especially, though not exclusively, with those known from the fourth century B.C. onward as *phusikoi* (physicists) or *phusiologoi* (in the primary sense of those who offer an account of nature)?

It is clear that from some time in the fifth century B.C. onward certain thinkers did begin explicitly to focus on the investigation of nature itself. Yet part of the context of this development is supplied by the concern shown by some of the individuals in question to mark out their style of inquiry from others belonging to traditional wisdom. The concept of *phusis*, nature, itself was used, among other things, for polemical purposes,¹⁵ that is to say to establish a contrast between the natural and the supernatural, the former the proper subject of inquiry, the latter the domain of superstitious belief. We can see this at work in the Hippocratic treatise *On Sacred Disease*, for example. There that author appeals to the concepts of nature and causation in order to *rule out* the assumptions and practices of his main rivals (the purifiers) on the grounds that they ignore the fundamental point that the sacred disease, like all other diseases, has a natural cause and can be treated with natural therapies.

Again nature could be, and was, invoked to contrast the domain of objective truth with that of conventional or culturally relative norms. In the fifth century B.C. *phusis* came to be placed in opposition to *nomos*, covering custom, convention, and law. Yet those who used that contrast were anything but unanimous as to how it was to be interpreted, whether in moral or political philosophy or in the study of nature. For some nature is the only domain of objective truth; but others insisted also on objectivity in the sphere of *nomos*. For some nature itself is value-free: but many others maintained that it was not, but was rather charged with values and indeed with moral significance, even though what that moral significance was was intensely disputed. Some argued that nature taught that might is right, while for others nature demonstrated the benevolence of the craftsmanlike force at work in cosmic creation.

The implications of these disputes for our understanding of the position of those who insisted on the importance of “the inquiry concerning nature” are clear. Although much of the research that passed by that name is indeed research into one or other of the fields that, for us, make up natural science, we should not take it that what it was to study nature was unproblematic or uncontroversial. On the contrary, the very insistence that there is a domain of nature there to be studied was a key move in the bid by new-style claimants to wisdom to outdo

Examination of Cicero's *De Divinatione*,” *Proceedings of the Cambridge Philological Society*, 1985, no. 211 (N.S., no. 31), pp. 1–10.

¹⁵ See G. E. R. Lloyd, “The Invention of Nature,” in *Methods and Problems in Greek Science* (Cambridge: Cambridge Univ. Press, 1991), Ch. 18.

their rivals from other traditions. However, just how that study is to be pursued, its goals and methodology, was hotly disputed among those who agreed, at least, that it was worth pursuing.¹⁶

IV

These points about the pluralism and controversiality of Greek inquiries have a moral for my fourth and final topic, the problems associated with any pursuit of the question of the origins of science. It is manifestly absurd to seek the origins of Greek medicine, or mathematics, as if they were homogeneous entities and as if those entities had historically datable beginnings. The same point can be extended well beyond Greece and generalized. A moment's reflection is enough to show how bizarre it would be to try to identify *an* origin of medicine or mathematics, as such, for every society employs some form of healing and has some use for number, even though the studies of social anthropologists as well as historians have made clear how diverse those forms and uses are and how varied are systems of numeration.

Similarly with whatever we adopt as our working definition of science. The idea that science *as such* could be said to have a single beginning, whenever or wherever we choose to place that, is equally extravagant. As I put it in a recent study, "The intellectual outputs of ancient Egypt, Babylonia, India and China especially all have their contributions to make to our understanding of the origins of the scientific enterprise, and all pose their special problems of interpretation—as does the exploration of the crises of successive 'scientific revolutions' in later European history." "The origins of science . . . should certainly not be construed as a matter of a single historical event. Rather a whole range of issues would have to be considered, in any full account, in connection with widely separated historical periods, both long before and long after classical antiquity. . . . The difficulties that philosophers of science have encountered in specifying items either of content or of method, either of theory or of practice, as constitutive of science as such as a whole, should just strengthen our resolve in this regard."¹⁷

As those quotations indicate, it is not my view that all talk of origins and developments has to be ruled out. On the contrary, so far as ancient Greece goes, there are certainly determinate questions that can be investigated, even if we have always to be on our guard both against the too-easy associations with sudden change that go with the genetic model—the talk of origin as birth—and against teleology, the idea that earlier work was targeted at later results.

Some examples will illustrate the different types of problems that different topics pose. We can study, for instance, the history of dissection and vivisection as research methods in ancient Greece, where we can investigate, among other questions, what the first attempts to generalize the method (in Aristotle) owed to

¹⁶ See Lloyd, *Revolutions of Wisdom* (cit. n. 6).

¹⁷ G. E. R. Lloyd, *Demystifying Mentalities* (Cambridge: Cambridge Univ. Press, 1990), pp. 15–16, 28–29.

a general epistemology and theory of causation, and where we can explore the ongoing controversies about its use—where, as already mentioned, there was no general agreement about its usefulness and relevance.¹⁸

Again, we can study the Greek use of the pulse in diagnosis, how that came to be recognized and elaborated. We do not have the original texts of two of the main early theorists, Praxagoras and Herophilus, though we have reports of their work from Galen, among others, and his extensive treatises on the subject have survived. Once again the evidence available to us demonstrates that the technique was and remained controversial. Galen's own use of it, so he tells us himself, was criticized by some of his contemporaries as magic.¹⁹

In those two cases our main sources are the extensive extant medical texts and ancient histories of medicine. But if we take as a third example the use of trepanning, that clearly antedated our first Greek medical writings by centuries. So questions of origins there have to be explored through the archaeological record—of the actual skulls on which the operation has been performed—and the most we can hope for is to establish very broad limits for how far back the practice can be traced. What the medical writings of the classical period add is that this technique too was controversial and some of its hazards recognized.

Similarly in physics or astronomy, we have extensive materials—in Aristotle's *On the Heavens*, in Archimedes' *Sand-Reckoner*, in Ptolemy's *Syntaxis*, among other works—that throw light on the pre- and post-Aristotelian debates on the shape of the earth, its position in space, and whether it is at rest and if so why. Clearly none of these sources is neutral on the issues, and none is exactly a reliable witness to the ideas and arguments of those who, for one reason or another, denied that the earth is at rest at the center of the universe.

Again we can go some way, even though not as far as we should like, to trace the history of the astronomical observations that Greeks carried out themselves, the systems of chronology used, and the gradual development of coordinate systems replacing very rough and ready techniques of location in relation to prominent stars or constellations. Here is an important case where the question of what the Greeks knew of Babylonian work, at different periods, is as important as the study of their own observational methods and results.

We can study, as well, one outcome of careful and sustained observational work: Hipparchus's discovery of precession. Here too we have to rely on a later source, the account we are given by Ptolemy of how Hipparchus tried various hypotheses before concluding that it was in relation to the sphere of the fixed stars as a whole, not just those near the ecliptic, that this motion occurred. Moreover, this is yet another example where what we might think of as a concrete result was the subject of ancient dispute. Although Ptolemy certainly accepted precession himself, it was thereafter often ignored and sometimes flatly denied, as, for example, by the fifth-century philosopher Proclus—and this despite the fact that the evidence for it increased with the passing centuries.

These examples are mostly ones that may seem familiar enough to anyone coming to the ancient world with conventional twentieth-century ideas about

¹⁸ See Mario Vegetti, *Il coltello e lo stilo* (Milan: Il Saggiatore, 1979); cf. G. E. R. Lloyd, "Alcmaeon and the Early History of Dissection," in *Methods and Problems in Greek Science* (cit. n. 15), Ch. 8.

¹⁹ See Lloyd, *Revolutions of Wisdom* (cit. n. 6).

science. But others help rather to remind us how alien some Greek theories and inquiries were. This is true, for instance, of the widespread idea that the exacerbations and remissions of diseases are governed by patterns of what were known as critical days. Yet here, too, there was no general agreement, let alone unanimity, about *which* are the critical days or how their sequences are arranged. Some favored combinations of either odd or even days, counting from the onset of the disease; others adopted other patterns; and many recognized the great variety of experience in observed exacerbations and remissions, and some of these commented explicitly on the difficulty of establishing exact periodicities.

A second example would be provided by the common, but also contested, medical and physiological idea that the womb wanders around all over the inside of the female body. This is one among many instances that lead the historian to examine deep-seated assumptions about gender difference in ancient Greece and the place of women in Greek society, in addition to the medical and physiological aspects of this nexus of beliefs.²⁰

Yet a third example would be the belief in the harmony of the spheres, an idea at the intersection of acoustic theory, of astronomy, and of cosmology, where beliefs about the structure of the universe and about the place of humans in it are at stake, as also the whole question of the intelligibility of the cosmos.

These are just a few of the many questions that the historian of Greek antiquity can tackle. Any selection of themes or topics has, of course, to be justified in terms of the particular aspects of the overall subject that they can illuminate; but it must be acknowledged that a *completely* comprehensive account is an unattainable ideal. Scholars will have their own ideas as to the areas that deserve particular attention, and some that were scarcely explored at all a mere twenty years ago have begun to produce interesting results. There is every reason, in fact, for us to be as pluralist in *our* approaches as the ancient Greek theorists we are studying were in *their* views of their own inquiries.

One group of problems that seems to me particularly well worth investigating relates to the hard-hitting ancient epistemological and methodological debates that I have mentioned several times already. This is for three reasons especially. First, those debates are one of the chief contexts where the Greek pluralism I have spoken of manifests itself. The divergences of approach adopted by different groups of ancient Greek theorists are often expressed in these debates, the chief forum for the explicit justifications offered for rival ideas concerning the proper aims and methods of inquiry. Second, these debates may be rather a distinctive, or at least a distinctively prominent, feature of ancient Greek science, and on that score worthy of attention. Third, some of the ideas put forward, notably the competing models of science as demonstrative, deductive, and explanatory, or alternatively as empirical and conjectural, were particularly influential at different stages in the subsequent development of European science.

Let me elaborate those last two points and introduce some cautionary remarks, and first concerning influences. The added interest that some ancient Greek methodological disputes gain from the way that some of the ideas in question

²⁰ See the essays collected in Averil Cameron and Amelie Kuhrt, eds., *Images of Women in Antiquity* (Detroit: Wayne State Univ. Press, 1983); and Lesley Dean-Jones, "Menstrual Bleeding according to the Hippocratics and Aristotle," *Transactions of the American Philological Association*, 1989, 119:177-192.

were later taken up is, it is true, a very mixed blessing. Of course, in the Renaissance battles between the Ancients and the Moderns, the Greeks themselves were often badly misrepresented on both sides, whether cast in the role of heroes or as villains. However, that does not mean that it is not worth investigating the ancient forms of those disputes and how they originated. On the contrary, the attempt to get the record straight concerning the ancient models themselves is all the more important as a necessary preliminary to the study of those later battles.

As regards statements of Greek distinctiveness, they always run the risk of being mistaken for, or confused with, claims to Greek superiority. As historians, however, we should be resolute in insisting both that *every* culture should be studied in its own terms and that, to understand *any* culture, we have, so far as possible, to adopt a comparativist, and diachronic, perspective. Certainly to try to understand any of the complex and varied products of Greek speculative inquiry we need to do more than just study the Greeks.

To be sure, plenty of explanatory hypotheses have been suggested that aim to identify the significant factors in Greek culture or society that may have contributed to, or even have been responsible for, some of those products. Economic, technological, geographical, political, social, and cultural considerations all need to be reviewed, as well as factors internal to the intellectual inquiries themselves. But in evaluating those hypotheses the comparativist approach seems to me to be essential. How else can we hope to assess what contribution the spread of literacy, notably the use of an alphabetic system of writing, may have made? How else evaluate what difference it made that the Greeks were aware of their non-Greek neighbors, from whom indeed they borrowed many of their ideas? Or again, how else to judge what differences it made that the Greeks developed the political and legal institutions of the city-state—where many citizens of the classical period (and not just in the democracies) gained extensive direct experience not just of political debate in general, but of certain styles of adversarial argument in particular? I have attempted some such evaluations myself in several publications, but it is not that I claim that they represent a definitive resolution of the issues.²¹ Quite the reverse, since they were offered to further, not to close, discussion of the problems. Moreover, it is not that I hold that the issues I claim to be important are the *only* ones worth pursuing in Greek antiquity, just that they are *among* those that are.

So I turn, finally and in conclusion, to the question posed at the beginning, of the appropriateness or otherwise of the category of “science” itself in our historical studies of the ancient world. That this concept runs certain risks and suffers from certain drawbacks is easy to see. To start with, it is our category—an observer’s, not an actor’s one—and I dare say that is true not just so far as the ancient Greeks are concerned. Second, there are obvious dangers of anachronism as soon as we start judging ancient work by the criteria of modern science, let alone start looking, whiggishly, for anticipations of later results. Should we, then, abandon all such talk of science entirely, and limit ourselves to discussion of those broad ancient categories of “medicine,” “mathematics,” “the inquiry concerning nature,” and so on that we have spoken about? Or should we be content

²¹ See, most recently, Lloyd, *Demystifying Mentalities* (cit. n. 17).

with other, even broader, ancient categories, such as “wisdom” (*sophia*) or even “knowledge” (*epistēmē* or Latin *scientia*)?

Both of those solutions suffer, in turn, from their own disadvantages. The problem, if we limit ourselves to “medicine,” “mathematics,” and so on, is that studies so defined may not take sufficient account of the interactions between these fields. Yet the rivalries I have spoken of were a matter of conflict not just *within* medicine (for instance) but *between* doctors (of certain types) and philosophers, and even involved the attempts of some doctors to emulate mathematical methods. Three methodological developments that seem particularly important—the recognition of the value and importance of empirical research, the development of the notion of axiomatic-deductive proof, and the application of mathematics to physical explanations—all are inter- or cross-disciplinary, the first spanning “medicine” and “the inquiry concerning nature,” the second both those two and mathematics, the third mathematics and “the inquiry concerning nature.”²²

Conversely, the use of such an all-embracing category as “wisdom” or “knowledge” runs the risk of including too much. We are not historians of poetry (often the field, par excellence, of *sophia* in ancient Greece), nor of history (in our sense of the study of historical events), nor yet of political philosophy. We are not dealing with ancient views of just *any* phenomena, nor with *any* ancient explanations, not even with just those explanations that can, in some sense, be described as systematic. The types of ancient knowledge we are concerned with are related directly or (in the case of mathematics) indirectly to natural phenomena, even though, as I have insisted, what counted as “nature” was itself a subject of ancient dispute.

Neither modern nor even ancient categories will do precisely for all our purposes. I have argued that in general ancient ones are preferable to modern, but also that it would be absurd to think that we can use the ancient ones entirely to replace our own. Any search for an entirely neutral language in which to report and discuss ancient ideas is, in any case, bound ultimately to fail. But that does not mean that we cannot discuss those ideas at all. Rather, it means that whatever terms we use must be treated as provisional and revisable.

In this sense the consideration of the convergences and the divergences between ancient Greek ideals, methods, and practices and later European ones can be used, judiciously, to throw light on the former—as well as on the latter. So too can comparisons with other ancient societies, where those between ancient Greek and classical Chinese inquiries seem to me particularly fruitful. It is not that we should try to judge either by the criteria of the other. It would be foolish to ask why the Greeks had no concept of *Qi* (氣) or the Chinese no exact equivalent to *stoicheia* (elements)—for they spoke rather of phases, *xing* (行). It is rather that the exploration of markedly different traditions of intellectual inquiries in contrasting social and cultural circumstances can serve as a reminder of the parochialism of each—or at least of their culturally specific characteristics.

Thus, as I have claimed, it is largely by the comparative method that we can

²² These points are documented in Lloyd, *Magic, Reason, and Experience* (cit. n. 8); and Lloyd, *Revolutions of Wisdom* (cit. n. 6).

reach clearer ideas both of the explananda and of possible explanations. Our goal, as historians of Greek inquiries, is to reach as accurate an understanding of them as we can—in all their complexities and controversiality. It goes without saying that any hypotheses, on that score, that we offer have to be tested against the evidence available, and both the limitations of the latter and the tentativeness of the former have duly to be acknowledged. But the very fact that the complexities and the controversiality are now much more generally appreciated than used to be the case can be taken as an indication that our understanding of what it means to talk about the *science* of the ancient Greeks can indeed make some progress.