'simplicity' refers to phenomena, and in which such a concept plays a directive role in the experimental activity itself.

55. He uses the terms 'compound' and 'complicated' interchangeably as contrasting with 'simple'; see, for example, *ERE*, ii, 129 and 132 respectively.

56. For this term see, e.g., *ERE*, ii, 135.

57. Faraday to Ampère on 3 Sept 1822: *Correspondence* (ref. 17), i, 287, emphasis mine.

58. This is, of course, not to say that Faraday has no interest in such questions. But he decidedly defers them to another stage of the investigation.

59. Gooding introduces this distinction in *Experiment* (ref. 2), Sect. 3.5 and 3.6.

60. *Correspondence* (ref. 17), i, 222; emphasis mine. This quite characteristic passage is taken from the above-mentioned letter of 12 September 1821.

61. *ERE*, ii, 138, emphasis mine. For an analogous formulation, see again the letter of 12 September, *Correspondence* (ref. 17), i, 223.

62. Gooding, *Experiment* (ref. 2), 250, and *cf.* Nickles, *op. cit.* (ref. 1), 299.

63. I shall not deal here with the problem of the relation that Faraday finds between this ordered system of phenomena and theories. I discuss some aspects of this relation in F. Steinle, "Experiment, speculation and law: Faraday's analysis of Arago's wheel", *PSA 1994*, i, 293–303. For Faraday's general view of the role of speculations and theories, see Cantor, *Faraday* (ref. 16), ch. 8, in particular Sect. 8.3.

64. Studies of Faraday's investigation of electromagnetic induction in 1831/32 reveal a quite analogous procedure, although Faraday's terminology changes in some way; see Steinle, *op. cit.* (ref. 63). Even the examples given by Gooding in "Mathematics..." (ref. 10), 134–8, appear to bear strong resemblances to what I have called Faraday's establishing of chains of mutually related phenomena.

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PAST AND PRESENT KNOWLEDGES IN THE PRACTICE OF THE HISTORY OF SCIENCE

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1. INTRODUCTION

Consider three questions of method in the historiography of science, technology and medicine (STM):

1. If you are studying Victorian medical practice, could you usefully consult a doctor as to the likely effects of the 'cures' about which you are reading?

2. If you have no training in microbiology but are about to begin a study of germ theory *c.* 1900, could you benefit from a visit to your University's department of microbiology?

3. If you are trying to explain why Aristotle considered that mammalian hearts had three chambers, could you benefit from trying to repeat his observations, or from reading the reports of scientists who had done so?

It is my guess that twenty years ago, almost all historians of STM would have answered 'yes' to all three questions. Now, a significant minority would answer 'no' to all three; further, they would regard these negative answers as a touchstone of 'social constructionism' or of the 'new cultural history'.

I do not claim that these particular questions are hugely important to our practice as historians of STM, but I do believe the answers are diagnostic of significant differences within our community. Because many historians eschew formal discussion of historiographical issues, and especially of the philosophical issues which attach to history of STM, it is easy for groups of historians to assume the answers to questions such as these, and to talk past colleagues who adopt contrary positions. Some historians assume they must answer 'no' in order to promote the independence of history from science or to defend the recent advances in sociology of knowledge. To such historians, the answer 'yes' may look like a return to 'Whiggism' or 'presentism' and a sacrifice of the achievements of historical sociology of knowledge. But is this so?[1]

Perhaps we would do well to direct to our own writings the careful analysis we direct to scientific texts. We may then see that between the extremes of social constructivism (for example Woolgar[2]) and of inductive realism (as in much amateur history of science), there is a considerable middle ground, where sophisticated historians explain science as culture, without assuming that 'nature' plays no part in the construction. We could recognize that most methodological statements are programmatic or polemical and may have only tenuous links to historical practice, even to the historical practice of the polemicists. We may further recognize that even the methodological and philosophical literature is less polarized than is often suggested. It is in this conciliatory and integrative spirit that this essay is constructed.

First I shall list and analyse several reasons for answering 'no' to the questions with which I began. Then I shall develop a counter argument, by rehearsing some general considerations about the place of the present in the past and by applying them to the history of STM. In particular, I consider the role of present knowledge (and other anachronic knowledge) in elucidating historical observations. Thirdly, I shall briefly examine a series of philosophical, sociological and historiographical 'positions' to show that they are compatible with the model I am developing. In conclusion I shall return to the practical questions to suggest what one can learn (and what one should not learn) by answering 'yes'.

Why Answer 'No'

One might summarize the arguments as follows:

(a) The first task of an historian is to understand statements and actions in their own time, by the standards of that time. Our present knowledge (or any other anachronic knowledge) is irrelevant, indeed ....

(b) Present knowledge may be distorting because we may tend to regard present knowledge as 'the real', which does not require social explanation. Thus we may come to regard history as a record of incomplete knowledge or of error. In either case, history reduces to explaining defects. Thus we return to 'presentism', explaining the past in terms of the present. Further ....

(c) In the history of STM, we thereby perpetuate the subordination of historians to the authority of scientists. We become handmaidens to the practitioners of science, 'in-house' historians, when we ought to identify with the 'public interest'. So instead ....

(d) We ought to treat the STM of any period as we would any other part of that culture, so extending our links with other historians and dissolving the differences between the history of STM and other kinds of 'cultural' history.

Part of the appeal of these arguments stems from the non-judgemental relativism which appears to be involved: periods are to be understood in their own terms, the past is not be subordinated to the present; sets of ideas are to be understood in their own terms, and religion or popular beliefs are not to be

subordinated to science; disciplines have their own rules and aims, and historians (even those who choose to work on the history of STM) are not to be subordinated to scientists. These aims are indeed commendable, at least as first principles, but do they imply or necessitate arguments (a)–(d), or the answer 'no' to our three opening questions? We can begin the discussion by rehearsing some generally accepted conclusions about the relationship between history — any kind of history — and our present.

2. PAST, PRESENT AND EXPLANATIONS

While all historians would accept the need to understand the past in its own terms, they would also recognize, with R. G. Collingwood, that understanding Nelson cannot be a matter of being Nelson.[3] We live in the present and must speak to the present. Historians must explain Nelson and his circumstances, as anthropologists must explain the peoples amongst whom they have lived. And if historians are to speak to those who have not acquired native-competence in, say, naval warfare *c.* 1805, then they must be able to explain in the language of our present, as anthropologists who have not 'gone-native' must be able to speak to the understanding and concerns of the late twentieth century.

This general requirement has several specific aspects, some of which are particularly important for the history of STM.

(i) As is generally accepted, the problems with which historians deal arise, in some sense, in the present. At minimum, they arise from the present state of a scholarly tradition; more widely, they arise from present interests outside scholarly traditions. Present interest in the history of women is an obvious example.

(ii) Similarly, the conceptual apparatus available to historians will be (or include) that of the present. Feminist history uses concepts and values that were more or less unavailable to the historical subjects.

(iii) For these reasons, historians need to be aware that they cannot simply 'absorb' the categories of their historical subjects. They must be simultaneously aware of the present frameworks which they share with their audience. They indeed engage in a kind of translation, expressing actors' (-emic) understanding in the audience's (-etic) terms. More accurately, they create a wider, fuller, language, expanding -etic language where necessary, so that it can encompass and explain the -emic.

This linguistic model has been developed by Malcolm Crick, Charles Taylor and others; it seems to me central and very useful, not just as a technical explication of historiographical practice, but as expressing an ideal.[4] It illustrates why *verstehen* is important — why we need to understand the past in its own terms — and so does justice to a major thrust of modern historiography and its Wittgensteinian/Winchian rationale; but it also makes clear why historians, and social analysts more generally, need to be analytical about their own life-worlds and thus able to speak to the concerns of the audience. Between loyalty to the terms of the past and commitment to the problems of the present, there is no choice to be made. The question is which problems, whose problems?

At the one extreme, we might write only for fellow specialists in a particular field of history, but in so doing we risk becoming the equivalents of native informants talking to each other — knowledgeable certainly, but not translating that knowledge to any wider audience. We might do better to pursue wider agendas, whether theoretical or political, and/or to address present concerns of interest beyond the communities of historians. British historiography surely demonstrates the strength of that claim. Authors such as E. P. Thompson, Christopher Hill, Eric Hobsbawm and Sidney Pollard have demonstrated how political commitment and interest in the present can energize historical research which does full justice to the periods studied.

Historians of STM have but few such models from their recent past, and they may be particularly susceptible to arguments about period and purity. Because HSTM has often been subservient to scientists, technologists and physicians, both intellectually and in terms of academic politics, many historians of STM seek independent stature by cutting those links and aligning themselves with other kinds of historians. Thus by concentrating on one period they extend their linkages with fellow historians, and they distance themselves from HSTM predecessors whom they see less as fathers than as 'stepfathers' — the Sartons and Singers who seem now so hopelessly Whiggish and so uncritical of the scientific enterprise.

Perhaps this distancing is also responsible for the neglect of those generalists and theorists in history of STM who could continue to serve as 'ancestors'. Here one thinks of Henry Sigerist, Erwin Ackerknecht, George Rosen, Owsei Temkin in history of medicine; of Lewis Mumford in history of technology; of J. D. Bernal in history of science. Their works are known and respected, and yet, so often, recent historiography of STM is portrayed as a reaction against presentist internalism and dated from the 1960s. As historians of STM come to recognize the pressing need for new synthetic histories, at least for teaching undergraduates, they may perhaps reconnect with the general concerns of the inter-war generation (and with such earlier omniscients as J. T. Merz). With those general concerns must come an engagement with 'theory' perhaps as 'historical sociology' rather than just 'social history'; such theory is likely to include the present.

The Audiences for HSTM

If then we accept such a general prescription about the purpose and nature of historical work, what are the consequences for HSTM and especially for our question about the uses of present-day knowledge? We can now rehearse some of the general arguments for the particular case. Thus, in so far as the audience for history of science is technically literate (in present terms), the historian must know enough of modern science to link with that understanding. In as much as historians are to avoid naïve 'tabula-rasaism', they should analyse their own understanding of nature so it can be differentiated from that of their historical subjects (and maybe also from that of their technical audience). Thirdly, historians would presumably wish to know enough about the content, procedures and problems of present science that their historical investigations can speak to that present.

This prescription is a general ideal, and even those historians who accept it would no doubt wish to moderate its demands and to specify them in particular ways. Explaining history and thereby elucidating the present should be a matter of dialogue, in which historians engage with colleagues more directly concerned with present science or with policy issues. In such dialogue, the responsibility of the historian is less to the details of the present than to its overall qualities and dimensions, with how it compares or contrasts with that past which constitutes the historian's particular expertise. Historians of health policy may not understand all the intricacies of NHS reforms since 1974, but they should understand enough of such matters to see the logical and developmental relationships between these reforms and, say, the processes which led to the NHS Bill of 1946. Similarly, historians of physics may not need to be expert on the whole menagerie of novel particles, but they should be able to convey the differences and transitions between classical physics, relativity and nuclear physics.

At this point, those historians of science who are concerned with popular culture may wish to demur. They may accept the arguments for the inevitability of the present in the past, and for the wisdom of consciously engaging with the present, but they may not wish to engage with scientists, engineers or medical doctors, certainly not if 'engage' suggests any kind of identification. Rather, they would wish to identify with a public which they tend to perceive as victims of STM rather than beneficiaries.

The objection is surely valid. Historians must be free to make whatever political alliances they wish, provided they can defend them. Indeed, it is vitally important that historians of STM, who may be regarded as possessing rare and important critical skills and perspectives, should cover a range of political interests. Arguably, they have a responsibility to search for the public good, accepting all the problems so raised, rather than identifying with particular interests such as those of academic scientists (or academic historians). That some historians of science identify with movements critical of science is therefore to be welcomed. This may mean that they choose to concentrate on the social effects (and social causation) of science at a level which does not involve technical detail — so be it, provided the history is well argued.

If recent anti-science historiography is to be criticized, it should not be for 'bias' against science, but for attempting to short-circuit discussion of crucial issues, and so failing in the duty of academic specialists to elucidate issues of general importance. One can no longer equate science and human progress, but can one intelligently equate science and repression? Both science and repression are surely far too complicated and various. Do not historians of STM have a duty, if they engage with these issues, to indicate when, how and why certain aspects of STM may be regarded as liberating or repressive? What other class of scholars is so well placed to answer such questions?

Secondly, it seems to me, one cannot short-circuit the epistemological issues by invoking methodological principles about science being simply one form of belief-system, to be analysed like spiritualism, say, or witchcraft. Such methodological principles are useful, but they do not guarantee the results. Rather, they enable us to analyse differences between belief-systems as well as similarities, and that is another key task for historians of STM. In as much as technological power is an emergent and ever increasing characteristic of 'western STM', that system is marked out from other belief-systems in ways which would seem to be valued cross-culturally. In studies of cultural interactions, the extent to which western STM can be said to be chosen rather than imposed deserves proper critical analysis.

Thirdly, one cannot properly neglect the connection between western STM and that tradition of scholarship which includes modern historiography. In as much as we regard such historiography as, in some sense and to some degree, a cumulative, rational, properly persuasive enterprise, capable of transcending in some ways the sectional interests of the producing nations, classes or cliques, so we must recognize the possibility for STM itself.

More generally, and to conclude this section of the argument, historians of STM may well be critics of modern science, but they undercut their own case and squander their expertise if they refuse to engage with STM sufficiently closely to discriminate between its various social and cognitive forms, or fail to give due account of the technical details and technological powers which have been STM's promise as well as its threat.

Explaining Past Science

So far I have applied to the historiography of STM certain arguments familiar from other aspects of historiography. I have argued that the present cannot be expunged from our account of the past; the present is the language of our audience and of ourselves as historians; it is the source and context of our historical problems. Such arguments apply to any kind of history, but would any kind of history prompt the question of procedure and expertise with which we began this essay — or is there something peculiar about HSTM, something beyond the discussion so far? Let's try the thought experiment. Would we ask a prison governor for his views on Benthamite panopticon principles? If so, what could we learn, for better or worse?

I wouldn't want a prison governor who had just come from prison-governor-

school and who would merely list for me the ways in which Bentham was wrong (or right). Presumably I could myself make that calculation by reading the textbook he had just read, should I wish to measure the distance between Benthamism and present dogma. Rather, I would want a governor with imagination and experience, familiar with several fashions in penology, and sceptical thereof. Such a man or woman might 'recognize' in personal or vicarious experience, some of the hopes of a Bentham, some of the effects that Benthamite moves may provoke, some of the consequences of such Benthamite moves. He would have the wit to know that the behaviour of prisoners may have varied between historical periods, and so might the behaviour and expectations of supervisors and reporters. But in drawing on his experience, are we not acknowledging that some aspects of behaviour may be relatively constant. Indeed, the dialogue of historian and governor, or indeed of historian and criminal, would seem to be in part a way of trying to measure the differences and the commonalities over time.

In general, it seems, we use real and vicarious or reported experience to construct historical reports as what we may call 'couplets', as the joint result of observer and observed. Is that not how we understand all reports, whether or not they are 'historical'? We hypothesize about what was 'out there' and what was 'in the head' of the reporter. It seems to me that we treat both aspects as dispositionals; both the objects and the 'observers' are understood as dispositions to act in certain ways; we interpret 'accounts of nature' as products of such coupled dispositionals. More accurately, we might speak of object-systems and of observer-systems so as to avoid the twin distortions of simple empiricism and psychologism. By so doing, we can take account of the Duhem-Quine thesis, recognizing the complexity of most scientific observations and the embedding of observers within complex social systems.

As Collingwood recognized half a century ago, scientists also work from 'historical' accounts, which are then winnowed, so as to make categorical statements about what is 'out there' and how it can be manipulated. In their scientific papers, and especially in note-books, they record actions, even aspirations. Such records are the material of history, and scientists themselves may write such history, to justify their own conclusions, or to demonstrate that alternative claims have to be explained in terms of peculiar procedures (errors or atypical material), rather than by the (general) nature of the world. But, for scientists such action-accounts are auxiliary; the primary product is a set of conclusions about the world. In history they are primary, one set of action accounts is compared with another; all of them are to be explained; first in terms of intentions, then in terms of the apparent responses of 'nature' to action.

But if all we have is a series of claims, perhaps discrepant, about the responses of nature, how are we to allot nature a role? Are we simply bound to accept that 'sorting' of accounts which science has achieved over time and which is therefore represented by its present claims? I see no reason to limit the historian in this way. In principle, he is to understand and explain all reports which seem to him relevant to a particular topic (and to explain away any of claimed relevance, where he rejects the claims). His resources are equally wide — all reports judged relevant, or indeed any reports which he may produce by 'experimental' work of his own (or scientist-collaborators). Part of his task is to explain the particular selection of reports made by present authorities; he risks circularity if that set is assumed as the standard.

This does not mean that all accounts are to be judged equally satisfactory; simply that they are all regarded as 'selections' from the totality of reports recognized by the historian as 'relevant'. The 'symmetry' principle here means that each set is judged in terms of the goals set, the area covered, and the degree of success in meeting the goals. In as much as the object of study are deemed constant, there is a sense in which report sets can be judged as if concurrent; they are about the same things. The differences are then to be explained in terms of historical (or synchronous) differences in aims and resources, including the technical and the cognitive. In principle such comparisons are independent of time, but in fact they are usually directional in time, because over several hundred years, Western societies have had the social machinery to cumulate both technical and cognitive resources — not necessarily by progressive elaborations, often by simple accumulation.

It follows that historians of science ought in principle to seek out all possible ways of seeing a given object, inventing their own, if they can. Thus they will provide the maximum number of comparators or controls when they come to ask why a given social group described in a given way what the historian takes to be the same sub-set of nature. One can then proceed by comparison, for example — why did Karl Pearson describe a continuum in the colours and shapes of peas where William Bateson saw distinct characteristics?[5] Have other people been able to see pea-characters in both ways? Can we see them both ways? What would we need to 'discount' in either case? Can we imagine how a set of peas could have supported these various construals? Do we have evidence from other times and places that further construals were made? If the peas were the same, can we imagine these construals also? That is how we begin to put 'nature' into our couplet — using whatever direct or vicarious experience we can get as to what construals were plausible.

Then we advance the dialectic. What preferences, say in terms of method, would explain the choices, including any to which we ourselves might tend? Within the methodological frames of the actors, what could explain and/or justify any discounting? Do we have evidence that such preferences were manifest? If so, how could we explain them in terms of social process? So we move between these two levels: an hypothesis about social causation might incline us to look for preferences which we would not otherwise have recognized. Evidence for preferences may lead us to see how peas could be so construed. But maybe we cannot find evidence of any differences in preferences between groups which reported differently. Maybe we then hypothesize that one had an 'atypical'

set of peas, and that possibility requires us to learn more about the total set of reports on peas, or maybe to breed 'freak' peas ourselves.

We learn what we need to learn to test our hypotheses (and to stimulate them?). Totality is a myth, of course, but we need not be restricted to actors' accounts, or even to accounts which were in principle available to our actors. We should be encouraged to look as widely as can be.[6]

Pedagogical Explanations

One way to clarify the issues may be to consider a variety of situations within which a particular passage of history of science may require explanation. Suppose, for example, that one were explaining the four-element theory to a modern chemistry student. One could explain it 'back from' post-Lavoisierian chemistry, as is often done; but this procedure, by running against the direction of historical change, and assuming the end point, is needlessly impoverishing. One could, perhaps, work from phenomena which the student knows or can imagine — for example observations on heating wood in a test-tube — and so construct the four-element theory by a kind of induction; but as we have discussed above, it is better then to include later categories in a comparative way, especially since the student will probably wish to relate what he now knows of four-element theories to what he already knows of later chemistry. If one is trying to make the history of chemistry 'live', then one proceeds in such ways — taking 'modern science' as one plausible way of dealing with phenomena, and as the putative category set of one's (technical) audience.[7]

It seems to me that when we explain four-element theory, we draw out from students several sets of experiences or imaginings, which we ask them to agree are plausibly attributable to 'nature'. We then try to show how and why historical actors 'saw' some of these features rather than others, concentrated on some rather than others, and constructed *x* rather than *y* therefrom or thereby. In such a model, any 'observation statements' which we accept as plausible comprise a compound set, a repertoire within which any actor's actual observations will form a subset. The construction of that subset and the meanings there construed become the explanans of the historical explanation. We note that such a model does not involve any hard or immovable difference between theoretical and observational terms, indeed, it would allow for various actors to place any such distinctions differently.

In some ways this is a conventionalist account in that claims about nature are always underdetermined. We do not wish to prioritize a particular account of nature, merely to show that several accounts have been really or potentially available, so the 'choice', conscious or otherwise, needs to be explained. But such an account also involves a metaphysical realism in as much as we understand 'observations' to refer to the same 'bit' of nature, and accept that certain changes in 'observations' are to be explained at the level of 'nature'.[8] To argue that 'nature' can be presented in many ways, is not to remove 'nature' from the explanations of 'science'. The argument that any results can be incorporated in any theory, does not mean that nature is irrelevant, because incorporation involves cognitive effort. Some results are incorporated easily; others may be assimilated only at the cost of structural changes more expensive than the construction of a new system. In such ways nature's resistance enters our accounts as a cause of a particular piece of science.

To some readers, this will seem obvious; I can only note that the obvious is frequently denied. Because we know that categories are socially constructed, that is, are dependent on language, and because we know they can (with effort) be changed, it is too easy to assume that 'truth' is a function only of language. As Joseph Rouse makes clear, the 'truth-or-falsity' availability for any statement does depend on our practices and languages, but within a given set of practices, whether a statement is true or false also depends on a 'nature' beyond language and practices.[9]

Perhaps we can again clarify these generalizations and link them to historiographical practice by taking an example — explaining the 'meaning' of earthquakes. We can freely admit that what we call earthquakes may have been 'explained' (perhaps even perceived) in many different ways by varied cultures. We may also accept that such phenomena may have gone unnoticed, or that our category 'earthquakes' may in some languages fall within a larger category within which it is not distinguishable; alternatively, our category could fall across category distinctions in other languages. We have no way of determining in advance what other people will make of the phenomena we call earthquakes. They are in that sense socially constructed. But does that mean that we can operate without nature here, or that anachronistic knowledge is bound to be irrelevant? Obviously not; much will depend on the question asked. If we ask why two social groups in Lisbon in 1755 read earthquakes differently, then social explanations may suffice. If we ask why they were all more interested in earthquakes in 1756 than in 1746, we may wish to draw on the widespread reports, from many cultural groups, perhaps including the evidence of later geologists etc., that in 1755 there was an earthquake in Lisbon of quite unprecedented violence. Evidence about such matters may well be available from such 'historical' sciences as meteorology or demography, even when it is not available in the 'archives'. *Annales* historians love tree-rings etc. as a means to the reconstruction of agricultural lives; it seems rather odd that some recent cultural historians seem to regard this kind of information as dangerous.

3. NATURE AND SOCIOLOGY OF SCIENCE

So far in this essay I have proceeded at a relatively practical level, addressing problems and attitudes which are current among historians of STM, trying to work out a general model of our historical practice. Obviously, I am much

indebted to analytical studies in philosophy of history and of science, and to studies by sociologists of science concerned with methods; but I have not yet conducted any systematic discussions of existing analytical positions.

Some such discussion should not be avoided, even by one who is not particularly equipped to conduct it. We need to review some of the methodological literature if only to establish one simple point: that the kind of pragmatic realism I am here suggesting can be broadly supported from the writings of several major authorities — philosophical analysts, sociologists of science, and, not least, key historians. Here we should recognize that several philosophers and sociologists of science who are strongly associated with the development of the sociology of scientific knowledge, should also be well known for their rejection of radical constructivism. The key figures here are Mary Hesse, David Bloor and Barry Barnes.

Mary Hesse, through her work on the Duhem-Quine thesis and underdetermination, and through her demonstrations that observations are necessarily theory-laden, did much to clear the paths of 'sociology of scientific knowledge'. She has been closely associated with the Edinburgh programme and has long sustained a concern with the historiography of science. But she has steadfastly denied the claims by Harry Collins and others that sociology of scientific knowledge requires that the natural world be treated as though it did not affect our perception of it. On this crucial point, Hesse, together with Bloor and Barnes, argues for a limited realism.[10]

Her argument, at root, is simple. We can all accept that knowledge is socially constructed in that different groups, with different skills and languages, will probably construe nature differently, and in that sense there is no non-social 'construal' against which actual knowledge can be checked. But this does not mean that 'nature' cannot properly enter our understanding of how 'nature' is constructed. That 'science' is underdetermined by nature does not mean that it is wholly determined by social factors; indeed, we can more profitably suppose that the outcome of scientific disputes are also underdetermined by 'social factors'. We can be reasonably sure that both kinds of factors will be involved — their relative importance is a matter for empirical investigation, the results of which are crucially important for our understanding of science. We should not prejudge the issues by methodological fiats.

The point can be illustrated by an example discussed by Barry Barnes, a case which is close to the starting point of this present essay: can one legitimately explain historical differences between oceanographers by using later data on the differences between the oceans which were being studied?[11] In principle, if schools of oceanographers differed demonstrably in aims and interests and their results on the 'same ocean' varied in accordance with these interests, then we would regard the differences as explained, at least in part, by social factors. If, however, results correlated with the oceanic regions being studied, whatever the interests of the groups concerned, then we could reasonably conclude that the differences of knowledge were rooted in 'nature'. Indeed, such observations about 'history' would be the basis of our 'scientific' understanding of ocean differences.

It is perhaps unfortunate that these arguments against the exclusion of 'nature' have not (yet?) been set out with the same rigour and vigour as the argument of Barnes and Bloor against the exclusion of 'social factors' from our understanding of science. Such argumentation is becoming necessary, for the Edinburgh school has increasing reason to complain of being misunderstood — not just by social constructionists who assume their support, but by anti-social-constructionists who assume Edinburgh's complicity. Peter Galison, in his work on experiments in modern physics, glibly assumes that Barnes and Bloor deny a role to nature. Bloor has to remind him of elementary logic: to argue that more than 'nature' is involved is not to deny the involvement of nature.[12]

It is Hesse who has most systematically refuted extreme constructivism. She notes the failure to explain the enormous efforts which scientists make to 'wrestle with material objects', or to explain why such efforts may fail. "Is it conceivable that such problems should arise with sheer manipulation if all questions of replicability could be settled by social fiat without reference to the world?" She notes that Collins tends to concentrate on the replication of individual experiments:

> He neglects the point of the Duhemian conception of the holism of theory, which is not that all individual replications can be reinterpreted at will but that some can, while being constrained by others, and by the coherence of the whole theoretical framework. This coherence is a standard feature of 'ideal-type' science, and must therefore be accepted as a feature of the object understudy if and when sociologists claim to be investigating science as opposed to other types of symbolic systems. If some related activities, e.g. parapsychology, do not show this coherence or rigour of empirical testing demanded by 'normal' science one may conclude that they are not 'ideal-type' science — this is a sociological decision; one is bound to record and explain, but not to accept, actors' definitions of 'scientific activity'.[13]

That the constraints of nature have been so neglected in recent sociology of science may indeed be explained in part by the topics chosen for study. Contemporary disputes, still unresolved, can illustrate underdetermination by nature (and indeed by social factors), they cannot illustrate closure and hence miss part of the process of science. More generally, such studies of disputes are meant to demonstrate how investigators with access to the same sets of materials, observations and theory adopt different positions for reasons which must then be 'social'. Such studies, by their design, do not show how all the relevant positions may be constrained by the 'nature' commonly accepted. It is here that historical studies are playing a key role. By systematically exploring the mechanisms of closure in scientific disputes one may be able to measure the weight given to

evidence and the role of accumulating evidence in adjusting the balance between the poles of a dispute. We have no grounds for supposing such evidence is ever logically conclusive; we have good empirical evidence, especially from historical studies of geology, that 'natural factors' have sometimes proved crucial.[14]

In some cases one may be able to show that both positions came to be accepted by both parties in a kind of compromise — that they accept the relevant 'nature' as more 'plural' than either side had expected. The oceanography case, mentioned above, may be of this sort: oceanographers came to agree that the key difference lay between oceans, not between researchers. Or one may accept that a result was 'anomalous' — reproducible but only under conditions which came to be regarded as 'atypical'; an example would be the hay infusions in the Pasteur–Pouchet debate.[15] Since everyone maintained that brief boiling was sufficient to kill microbes, Pouchet argued that the subsequent appearance of live microbes in hay infusions was a demonstration of spontaneous generation; Pasteur attributed these results to sloppy technique. Later researchers argued that hay infusions contained spores which were not killed by boiling, so allowing a resolution which contained all the evidence. The accuracy of such a reconstruction is a matter for detailed historical investigation, but one should be aware that the plurality of 'nature' may be as significant as the 'plurality' of 'interests'. The balance is a matter for investigation, not fiat.

Indeed, one may argue that such investigations may become our best information not only about the importance of 'social factors', but about the reliability of beliefs about 'nature'. If Popper and Lakatos were right to believe that reliability is a matter of testing, and that testing requires alternative positions, then historians of science do indeed perform a critical function in showing the relative importance of 'social' and 'natural' factors in the establishment of particular positions. If apparently good arguments were ignored rather than met, one may conclude that the canons of method were flouted, either in this particular case, when the 'natural finding' is called into question, or more generally, in which case the canons of the actors may be judged defective or as different from those we had previously understood to apply to the science of that period. Paradoxically perhaps, where there were no disputes, historians will find it more difficult to perform this critical function.

That sociology of science may yet recover a due appreciation of 'natural context' is suggested by the latest master to attract a following in this field: Bruno Latour. His position on this issue is one entirely appropriate to the funniest of contemporary analysts. By an extra-ordinary extension of language he has formally introduced 'natural objects' into a tradition where their explanatory role was threatened. For sociological analysts who believed in people (or at least in texts) but not in 'objects', he has simply extended the range of the term 'actors' so as to include non-human 'actants'. To some more sober analysts, less affected by contructionism, this may seem rather like the malarial cure for a certain neurological condition — but it may work.[16]

It is probably not helpful, it may even be immoral, to erase the human–non-human distinction, as Latour does. But at this cost he achieves some benefit — he underlines the similarities, within interpretivist frames, between objects and people. Both can be recruited in support of particular scientific propositions, but only at a cost, which may in some cases be unpayable. It takes energy and ingenuity to line-up nature in a successful experiment; success is not arbitrary, non-human actants may be recruited for some positions much more easily than for others. This set of dispositions is indeed what we know of such actants; it is their nature. In principle, it is a historical summation, corresponding in a general way, to the model I developed above for explanation in the history of science.

4. OUR QUESTIONS ANSWERED?

In this last section of the essay, we return to the three practical questions with which I began. For each of them I would now wish to give a positive answer, using the model of historiography of science set out above. In addition, I hope to show how this positive answer can illuminate more general, but still practical, questions of historiography. Thus in considering the uses of a modern doctor for a medical historian, I shall open up that vexed question about 'cures' — did they 'work'? Should we indeed ask the question? From the discussion of the uses of microbiology, I shall consider the ways in which historians can best be 'informed' about non-human actors. Lastly, drawing on the Aristotle heart case, I shall consider the uses (and abuses) of 'historical reconstruction'.

The Historian in Dialogue with the Doctor

There is a whole series of interconnected reasons why Victorian medicine, say, will pose a problem for medical doctors and historians whose knowledge of medicine is based in the present. Diseases (and the relevant biology of patients) seem to change over time in a way that the behaviour of sulphur, say, does not. So changes in 'nature' have to be added to the changes in techniques, focus and categories, in perception and in recording which separate us from Victorian medicine. Except perhaps in a few rare instances, no modern doctor is going to be able to 'rediagnose' cases securely or give 'prognoses' as he would for his own patients. Though this kind of rediagnosis is sometimes attempted, not least for famous patients, it has a rather poor name among medical historians because of the Whiggish over-confidence with which it is often accompanied. Serious dialogue, both sides will recognize, is a difficult and tentative procedure. What could be its gains, for the historian and indeed for modern medicine? Let's take the second question first.

Historical reports can be informative about patterns of disease to which we have no other access because they have changed meanwhile. The same may apply

to medical procedures no longer practised. But interpretation is difficult, for reasons which apply equally to conditions hypothesized as similar to the material of modern medicine. Indeed, we would be well advised to withhold credence from historical re-interpretation of lost diseases unless the skills of the re-interpreter are demonstrable on records of diseases which are not 'lost'. Scholars who wish to interpret case histories of epidemics, say, are well advised to familiarize themselves with records of 'everyday' complaints, where 'exotic' practices and categories are more likely than 'exotic' conditions.

Secondly, we may have reason to suppose that a condition is a persistent one, but that observations or clinical findings have been 'lost to science'. This happening is rare, but not impossible. History is best not seen as a scrap-heap, to be searched for discarded nuggets of wisdom, but this does sometimes work for those parts of history, such as history of STM, which deal in part with relatively unchanging aspects of nature. The unpopularity of this model among professional historians does not make the procedure logically impossible or unworthy. Indeed, the possibility is likely to be more attractive to professional historians if considered at the level of scientific theory or metaphysics, or maybe clinical practice. Implicit in much writing on 'scientific medicine' is a pervasive sympathy for 'holism' in medical theory, or for attention to patients' own accounts. Sometimes such sympathies are quite explicit, for example in Coulter's histories of medicine, which are written in support of homeopathy.[17] Though such sympathies may lead to distortion, they need not; in my opinion, they are to be encouraged. Let's find out what is to be said in favour (and against) the historical record of homeopathy. Scholarship requires attention to both sides of the arguments, but not that such arguments be kept out of historical practice. Historians are fairly asked 'what can we learn from the past' — especially where, as for STM, there are definable goals which seem to be shared by us and our historical actors.

If for argument we exclude 'lost conditions', lost 'data' or 'lost' theories, we shall be dealing with a set of observations in which Victorians saw one pattern (or set of patterns) and in which our doctor may see another. Our doctor may see how several of his categories are included in one Victorian grouping, or how a grouping which approximates one of his own categories may be sub-divided in a Victorian text, perhaps according to symptom-features which he has been taught to regard as of secondary importance. And this may be educational: the Victorian divisions may draw attention to features of illness, such as duration, extent of fever, or degree of discomfort, which may well be relevant in clinical management if not in pathological anatomy.

So, tentatively, historian and doctor (or historian/doctor) may suggest how a set of Victorian cases could be matched by a set of modern cases, under the conditions of observation which are hypothesized for the historical cases and thought-reconstructed for the modern. This reconstruction will be the more profitable the more the doctor is able to draw on experience technically comparable to Victorian medicine — medicine without lab-tests, say — or the more he is able to appreciate his own ignorance.

If a doctor can 'bring experience' to a historical record, then he or she may be able to help us with retrospective prognosis, especially where the later histories of the cases are absent from our records. Such a doctor may be able to guess how many patients would have 'got well' in the absence of any treatment, or how patterns of remission and recurrence would probably enter into judgement of remedies (and of practitioners). If, for example, many of the fevers seem to have the shape still recognized as malaria, then we may understand something (but not everything) about the popularity in some times and in some places of quinine.

So armed and so cautioned, we may wish to ask the further question — did the cures 'work'?

Medical historians have recently shied away from this question, even though it may well be the most important to the general public. The flight from presentism has seemed to require an agnosticism as to what really worked; historians will only tell you what was said to work. Here again, I believe, recent historians of medicine have been misled and thus have deserted an important public role. Paradoxically, these days, it is general historians, such as F. B. Smith on tuberculosis, who will try to answer the central question about sanatoria — were they of value to patients?[18]

If we accept that our reconstructions must include the objects studied, as well as observer-writer-text, then in the case of histories of clinical cases, we are committed to assessing, where possible, the changes in the potential for observations, including those due to changes in the health of the patients. For such judgements, we can also use (carefully) anachronic results, and results about results. We can, for example, use more recent data on the effect of placebos, or on spontaneous remission rates. In principle we could use data on variation between clinical observers faced with the same phenomena. In practice, we are likely to use only the outlines of such knowledge, because such data, prior to 1900 or even 1950, is rarely sophisticated. But, in principle, there is no reason for us to avoid the questions about efficacy.

Of course, efficacy, in these terms, could be unrecognized. We could in principle conclude that 'cure *x*' was indeed effective, that bodies improved, but were not seen to do so. That conclusion may be much rarer than the converse — that observers repeatedly claimed cures where we have no reason to believe that there was a physical effect common to the patient group concerned (or any identifiable set thereof). We have numerous ways of explaining 'false positives'. The point here is that in both cases we can in principle separate two questions: how did physical changes affect the possibilities of 'observations', and how were such observations made and construed?

Changes in patient bodies are not 'value neutral'. In as much as medicine is expected to cure (or prevent, or comfort) then our reconstruction of physical

changes will also involve assessment of 'success' or 'failure'. These may be ambiguous for all sort of reasons, but changes in value systems are probably *not* a major difficulty, at least in 'western' medicine over the last few centuries. Relativists are correct to emphasize the possibility of finding disparate, non-commensurable value systems, in which the goals of medicine have to be defined in terms of the particular way of life. But empirically, there would seem to be much that is trans-cultural about medicine. Though most cultures exhibit some circumstances in which pain is cherished, these are exceptional; in general, sufferers seek relief from pain; they hope that wounds will heal, that fevers will not prove fatal, etc. Even where the metaphysics of life and death have changed fundamentally — for example, the loss of belief in eternal life — medicine seems to have continued much the same as before, at least in Western Europe and North America. Medical historians should perhaps draw courage from such conclusions. Sensitivity to period and place does not preclude belief in a world of nature which we can help elucidate (at least by exhibiting the variety of plausible elucidations). Our construction of the historical is also a construction of the physical. And if the physical shows some constancies over time, so indeed do many human values. In as much as we share such values and such physical contexts, we can legitimately consider how much our historical actors, and our contemporaries, have contributed to the attainments of our common goals.

Will Labs Corrupt Historians?

The second question concerns the utility of scientific education for historians of science. Not, I would stress, whether such education, in a formal way, is a necessary prerequisite for writing competent history of science, still less whether it is a sufficient condition; there are good historians of medical science who 'gave up' science at age 16 or less; and certainly the 'scientific' knowledge manifest in some amateur chronicles of science does not prevent them being inadequate as history. As we discussed earlier, knowledge of 'how things really are' may lead to hopelessly presentist historiography, usually because it is combined with a naïve empiricism which assumes that things being 'how they are' is a sufficient condition for their being so regarded. I fully accept that one can learn the science from one's historical sources, and that there is merit and interest in such an approach. Even so, I would wish to argue that anachronic knowledge can be useful — not just for reasons of audience, or as a means for the historian to recognize his own irreducible anachronisms, but because anachronic knowledge may help us reconstruct the object-observer couplet which I have outlined as central to our task as historians.

One could, for example, learn from a microbiology class how certain kinds of materials may have behaved in given historical circumstances, what they 'offered' to investigators, how such 'nature' resisted (or co-operated) with the investigations carried out. Such knowledge may be more direct than that discussed in the previous section, in as much as we are now concerned with the historian's own experience in science. (On the other hand, it is less direct and less particular than that gained from the accurate historical reconstructions which I shall discuss in the next section.)

Broadly, I would argue, one can hope that the scientific education of a historian would help produce the qualities which a historian would value in a technical advisor — scientific imagination and humility based on practical experience, rather than the dogmatism of textbooks. The student needs to know that experiments sometimes produce odd results and that the experimenters may have to stop asking why and get on with experiments that 'work'. Such insights are best gained from one's own practical work and especially from reasonably open-ended 'research' projects. Such experience will also teach much about investigators and skills, about socialization and the social functioning of laboratories.

Obviously, such experience can be used for better or worse. If the historian transfers the experience uncritically to the historical reconstruction of material, methods and social relations which are importantly different from those which he or she has directly encountered, then the experience will be worse than useless. But if the historian is sufficiently self-conscious to measure out the differences involved in such reconstructions, then we can learn thereby, both in the imaginative reconstruction of past scenarios and in specifying the 'modern' feature of present experience. Arguments against this procedure are, one suspects, variants of the naïve 'inductivism' which is generally used by empiricist historians to try to keep the present out of the past. In our historical reconstruction of, say, a Victorian laboratory, we are bound to draw on our own experiences and expectations — better, therefore, that we do so as explicitly as possible.

But for much, if not all, of the history of science, non-laboratory experience could be equally or more valuable. Historians should know about kitchens and cooking, about theatres and spectacular shows, about secret clubs and magical rites. They should go to church and confessional more often. Bookish as we are, we need not limit ourselves to reading about spectacles or rites. If we are suitably 'controlled', we should perhaps seek out the experiences, especially if we can take good note of the variety of responses which such situations may involve or induce.[19]

The argument becomes one for range and variety in the training of historians. Like Charles Lyell in his advocacy of actualist geology, I am arguing for a lively present which can lend its liveliness to the past (without uniformitarian presumptions). The seeking of such experience will of course also familarize the would-be historian with part of the potential audience, in this way too enhancing the dialogue between past and present and so increasing the interest and utility of historical writing.

Aristotle and Dead Dogs

Finally, we come to the question of Aristotle's hearts, and more generally the question of historical reconstruction of experiments.

As in the previous cases, the chief use of the anachronic knowledge is to allow reconstruction of the object of the actors' investigations. I do not for a moment deny that Aristotle may have tended to see the heart as three-chambered. For reasons of cultural tradition/training, to fit with other aspects of his biology, because there was a market for three-ish theories, he may well have tended to do so, and the possibility needs to be explored. Given the usual construal of lower vertebrate hearts as three-chambered, and Aristotle's concern with animals generally rather than man in particular, the theoretical advantages of his recording three-chambered mammalian hearts may need no further explanation.[20]

If one is not a realist, one could perhaps stop there: Aristotle's mammalian hearts were three-chambered. But if we also ask why Aristotle recorded three chambers when later traditions, including ours, have recorded four, we are in a position to make good use of T. H. Huxley's Victorian observation on strangled dogs. Huxley adduced evidence that Aristotle had killed dogs by strangling and that under such conditions, the chamber at the base of the great veins (usually called the right auricle) can appear to be continuous with the great veins, so it might not be counted as a separate chamber of the heart.[21]

My point is not that Huxley was correct in his reinterpretation, but that the information he adduces is in principle useful to the historian. We do not need to read him as showing that hearts are 'really' four-chambered, and that Aristotle was wrong. I would rather say that he shows us the conditions under which it is relatively easy to record three chambers and those under which one records four. We are led to understand why four is the usual construal, but also why Aristotle probably had good empirical support for his three-ish generalization.

The case is a nice illustration of my general argument. To simply record Aristotle's observations tells us nothing; to pretend we can record three-hearts or four-hearts with no explanation of the difference seems perverse. By discovering more than is commonly known now about anatomical procedures and the effects of strangulation, we can show how the studied objects contributed to a result which is puzzling for as long as the objects have no voice and are presumed to speak as hearts are now commonly understood.

Hence to the general problem of reconstruction — a method which has been out of favour in HSTM but which seems to be enjoying a come-back, stimulated by the concerns with practice which have dominated much recent sociology of science.

Reconstructions were found objectionable because they were often interpreted wrongly. Enthusiasts for reconstructions, such as those involving authentic period microscopes/preparations, seemed to assume that one had only to recreate the object. More sophisticated historians knew one had to recreate the mind-set etc. of the observer; that the same 'object' could be seen in many ways. Hence, by the familiar logical mistake, their misconclusion that the 'objects' were effectively irrelevant; at least that reconstructions were more trouble than they were worth.

In fact, a key positive feature of physical reconstruction is indeed the trouble they involve, and the likelihood of getting 'results' other than those recorded by the original experimenters. That fact used to mean that the initial observers were uncommonly persistent and/or gifted. Now it also means 'interpretive flexibility' — it underlines, practically, the Duhem-Quine message. But that is only half the story. That experiments are difficult to replicate does not make them arbitrary. That craft rules are involved enlarges our understanding of science, but in as much as these rules are not arbitrary or specific-theory dependent, and in as much as they may correspond to rules for practice outside science, our knowing of them can enlarge our appreciation of the possibility of consensus over results, within and outside particular scientific communities.[22]

Again, in as much as results are partly-determined by 'nature', reconstruction can aid us in the estimation of that part. Only by 'practical' knowledge (which in some cases may be vicarious, or by imaginative extension) can we understand the possibilities of construing *x* as *a* or as *b*. Only by some such experience can we learn to describe observer-object couples in such ways as will do justice to the limited flexibility on each side. We need this kind of experience of the 'object' if we are to find terms in which to describe the encounter of different observers with similar 'bits' of nature.

Indeed, in physical reconstructions, historians act out the dual role which is central to the model presented in this essay. One could, therefore, end this essay by fantasizing a reconstruction of observer as well as object, in which the historian acted like a theatre director: alter the object a little; turn it round so the actor sees the other side of it. Point things out to the actor, teach him eighteenth-century chemistry. Now can he tell you where the phlogiston is? Can he be relied upon to come up with the observations recorded in the eighteenth-century texts? Maybe we should try such reconstructions — perhaps in schools, perhaps using expert systems on computers. To my mind, such experiments would demonstrate rather than deny the rich complexity of explorations and explanations which characterize our historical discipline.

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1. This essay, mostly drafted in 1990–91, owes much to the methodological pluralism of my past and present Manchester colleagues. I thank them all, but especially David Edgerton, Jon Harwood and Steve Sturdy for comment on the text, Joan Mottram for assistance and Jeff Hughes for additional references. For comments and information I am also grateful to David Bloor, Jon Hodge and Roy Porter.
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5. D. McKenzie and B. Barnes, "Scientific judgement: The biometry–Mendelism controversy", in B. Barnes and S. Shapin (eds), *Natural order: Historical studies of scientific culture* (London and Beverly Hills, 1979).
6. As Jonathan Hodge explained to me in the early 1970s, using present experience to explain the past need not make the past dull, it may be an incentive to explore the liveliness of the present. He was, of course, discussing the 'actualism' of Charles Lyell in geology. See M. J. S. Hodge, "Darwin and the laws of the animate part of the terrestrial system (1835–7): On the Lyellian origins of his zoonomical explanatory programme", *Studies in the history of biology*, vi (1982), 1–106, esp. pp. 6–13.
7. See M. Shortland and A. Warwick (eds), *Teaching the history of science* (Oxford, 1989), especially the article by Steven Pumfrey.
8. This kind of realism is defended by Joseph Rouse, *Knowledge and power: Towards a political philosophy of science* (Ithaca and London, 1987); it underlies much of the work of the Edinburgh School — see, for example Barnes's exchange with Woolgar, ref. 2 above. Also see Thomas Nickles, "Good science as bad history: From the order of knowing to the order of being", in Ernan McMullin (ed.), *The social dimensions of science* (Notre Dame, 1992), 85–129. In my opinion, some such limited (scientific) realism underlies most of the studies in the social history of scientific knowledge accomplished over the last quarter century. I here exclude the constructivist heuristic of Harry Collins, the reflexive school, radical ethnomethodology, and the hylozooical monism of Bruno Latour and his co-actants, which are briefly discussed towards the end of this essay.
9. Rouse, *op. cit.* (ref. 8), esp. p. 164.
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12. P. Galison, *How experiments end* (Chicago, 1987), 10; D. Bloor, review of Galison's *How experiments end*, *Social studies of science*, xxi (1991), 186–9. Galison's mistake reveals a common tendency to attribute to all sociology of scientific knowledge the metaphysical idiosyncracies of the Bath school and its descendants.
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21. See A. Platt, "Aristotle on the heart", in C. Singer (ed.), *Studies in the history and method of science* (2 vols, Oxford, 1921), ii, 520–32, and T. H. Huxley, "On certain errrors respecting the heart attributed to Aristotle", *Nature*, xxi (1880), 1–5. I owe these references to L. G. Wilson, see his article on Aristotle in the *Dictionary of scientific biography*, i, 266–7.
22. On some of the difficulties of replication, and for a wonderful evocation of the changing "natural history" of physics labs, see R. G. Stansfield, "Could we repeat it?", in J. Roche (ed.), *Physicists look back: Studies in the history of physics* (Bristol, 1990), 88–110.

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COMMITMENTS AND STYLES OF EUROPEAN SCIENTIFIC THINKING

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Plato's Pythagorean friend, the mathematician Archytas of Tarentum, wrote of his predecessors and contemporaries in the fourth century B.C.: "Mathematicians seem to me to have an excellent discernment, and it is in no way strange that they should think correctly concerning the nature of particulars. For since they have passed excellent judgement on the nature of the whole, they were likely to have an excellent view of separate things." When we speak today of natural science, we mean a specific style of rationality created within European culture, a specific philosophical vision at once of knowledge and of the object of that knowledge, a vision at once of nature and of natural science, a vision explored and controlled exclusively by argument and evidence. I shall offer an analytical interpretation of the history of this European style. To understand the present, and no doubt to foresee the future, we must take a long and a comparative historical view. European rationality is a style of thinking that may be traced to the ancient Greek commitment to this mode of control and decision, as distinct from custom, edict, authority, revelation or some other practice. It was the Greek habit to make their style of rationality explicit, in the manner of Socrates, and in this manner European scientific thinking was initiated, by the ancient Greek philosophers, mathematicians and medical men, in their search for principles at once of nature, including human nature, and of argument itself. The Greeks introduced an exclusive form of rationality based on two fundamental ideas: universal, self-consistent and discoverable natural causality and, matching this, formal proof. From these two ideas came the essential style of Western philosophy, mathematics and natural science: the conception of a rational system, generated by the identification of problems as distinct from doctrines, the selective vision of the soluble, and the criteria of what counts as a solution, both in the particular and in general systems of theoretical explanation.

The Greeks invented a style of rationality not only effectively competent to solve problems, in which an essential criterion for accepting a general system was that it could incorporate the solution of the particular; each problem generated an open-ended proliferation of further problems into an expanding diversity of cognate subject-matters. In this sense they invented the notion of a scientific problem as distinct from a doctrine. They invented likewise the conception of