Introduction

. . . A species of mental derangement, in which the patient raved continually of comets, deluges, volcanos and earthquakes; or talked of reclaiming the great wastes of the chaos, and converting them into a terraqueous and habitable globe. This unreal mockery, however, though it has endured long, and continued even to the present day, is now vanishing and melting into air. ([Playfair], 1811: 207–8)

John Playfair's dismissal of previous geological theories typifies the root-and-branch denial by most early nineteenth-century geologists of their scientific parentage. The path towards the true science of the Earth, they thought, would not merely be long and arduous. Rather, they needed to strike out completely afresh, on the new road of painstaking observation and disciplined induction. For earlier theories had been jerry-built on illegitimate intellectual foundations and vitiated by rampant armchair speculation. Above all, previous systems were no fit foundation for the fast-rising edifice of geological facts. As John Kidd, Professor of Chemistry at Oxford, concluded:

Considering then of how very small a portion even of the earth's surface we have at present any thing like an accurate knowledge; . . .considering also, that whoever has long accustomed himself to geological observations, will easily recollect that at different periods he has viewed the same phenomena with very different eyes; and that the history of geology shews the same thing to have happened to the most acute and accurate observers; that it often also happens that different persons are impressed differently at the same moment by the same phenomena; and lastly, that it is certain from the numerous and remarkably dissimilar systems of different philosophers, that nothing like probability of any high order has been yet attained in geological reasoning; from all these considerations we may at least be convinced, that the science of geology is at present so completely in its infancy as to render hopeless any attempt at successful generalization, and may

therefore be induced to persevere with patience in the accumulation of useful facts. (1815: 268–9)

George Bellas Greenough, first President of the Geological Society of London, capped Kidd's scepticism in his own *Critical examination* (1819). Abelard-like, he juxtaposed theories to spotlight their contradictions. But he also exposed how all 'facts' and categories were themselves theory-bound: 'The term Stratification is by no means unconnected with theory' (1819a: 23). Like Kidd, Greenough sought to wipe the slate clean with an intensive campaign of observation and data collection. In the Geological Society's Baconian crusade he was in the van.[1] So fervent was this new repugnance to 'speculation' that not only all hitherto-existing theories, but also the very activity of theorizing seemed ripe for the holocaust, now that geology had a

tendency in all its branches, to assume a character of strict experiment or observation, at the expense of all hypothesis, and even of moderate theoretical speculation. . .Matter of fact methods have lately been gaining ground in Geology, as in other sciences; hypotheses are now scarcely listened to; and even the well-organized theories which, a short time since, created so much controversy, receive in this day little attention or comment. ([Fitton], 1817a: 175, 177)

Denunciation of preceding theories was two-pronged. They were religiously motivated, and, therefore, distorted; and their science was merely speculative. The charge that geology had hitherto been in thrall to Moses reached its apogee in Charles Lyell's polemical historical introduction to his *Principles of geology* (1830, i), in which he scornfully dismissed 'the familiar association in the minds of philosophers, in the age of Newton, of questions in physics and divinity' (p. 36).[2] The upshot, Lyell lamented, was that the science's history over the last two centuries had been one of 'retardation as well as advance', so that his 'inquiry, therefore, . . .is singularly barren of instruction to him who searches for truths in physical science' (p. 30). As late as 1830 Lyell's 'mission' was to 'free the science from Moses' (Mrs Lyell, 1881, i: 268). The latter charge was no less pressed, that earlier geologists had shirked building up their explanations by sober induction from patient investigation, leaping instead to pretended causes filched from other disciplines (or, worse, from none at all). For the problem of relating past and present states of the Earth had

> fired the imagination of some speculative philosophers better versed in mathematics, astronomy, and geography, than in chymical or mineralogical knowledge, yet all desirous of tracing the origin of the globe and applying Geology to their several systems. Thus COSMOGONY was grafted on Geology. (Kirwan, 1799: v)

That geology was 'not to be confounded with cosmogony' became of course axiomatic (Lyell, 1830, i: 1).

Standard historiography has remained loyal to this assessment of the science's early history. It is amused by the cosmogonists, and almost ignores the fieldwork, of the seventeenth century. It then stigmatizes the bulk of the eighteenth century as a dark age, and attributes the real paternity of British geologists to heroes such as James Hutton, William Smith and Charles Lyell. Furthermore, it makes scant recognition of the profounder issue of the very formation of geology itself as a science.

Contrary to these early nineteenth-century geologists from whom most historians have taken their cue, I wish to argue that attitudes towards the Earth and its investigation underwent great transformation in Britain between the mid-seventeenth and the early nineteenth century.[3] The labours of the seventeenth and eighteenth centuries provided the basis – material and conceptual – for the unquestioned flowering of geology in the nineteenth, not, as was claimed, an obstacle to be demolished before progress could be made.

My aim here, of course, is not primarily to redistribute glory, but rather to reconceptualize what was involved in the origins and growth of the science. For this transformation *is* the emergence of *geological* science in Britain, 'the making of the science of geology'. To some extent, this process was tangible, material and quantifiable: growing numbers of observers and observations, maps and monographs, collections and societies. But it was also broader and deeper. During this period Earth science was gaining a *social* base hitherto unknown. Being created, slowly and incoherently, was a highly complex institutional and intellectual fabric of devotees, facts, ideas, ambitions and controls, which mid-seventeenth-century pioneers like Hooke had been sorely aware were lacking (1705: 279), but which had fused into a distinctive, self-sustaining discipline by the early nineteenth.

But investigation of the Earth also underwent parallel *conceptual* change. There was continuous pressure to render the pursuit more 'scientific', while criteria of the 'scientific' themselves developed in course of time. Methods, techniques and standards were forged which were claimed to be more rigorous, philosophically sophisticated and appropriate to the object. Pragmatically speaking, these moves bore fruit. But to describe the investigation of the Earth as becoming more 'scientific' is also to imply that something like a coherent public discipline was being built up, with its own distinctively demarcated intellectual territory, construction of reality, and its own practices and practitioners.

My historical perspective, in other words, sees science as an integral but distinct part of the spectrum of man's intellectual and social activities. Human thought is like white light. When it passes through the prism of the outside world, it is fragmented into bands of colours, continuous but recognizably different. Science – like poetry or religion – is one of those colour bands. The historian must embrace the *historical* fact that natural sciences have shown a tendency, when contrasted with many other forms of discourse, to be descriptively *cumulative*, in the sense that new knowledge is successively acquired which can be used alongside other data. Science, likewise descriptively considered, has a tendency to be *progressive*, in the sense that inadequate theories have successively been discarded; *objective*, in that scientists have tended to operate within common traditions and shared public languages; and *rational*, in that in scientific culture, beliefs have been backed with validating reasons (Merton, 1967). Scientists – and, indeed, philosophers of science – have frequently, but fallaciously, taken for granted such claimed features of science as openness, progress, objectivity and impersonality, as unproblematically true. But this is to ignore the historical dimension. The historian's task, rather, is to see these very aspects of the culture of science as his *problem*. For to soft-pedal these historical features of the activity of science would shallowly conflate and reduce it to the history of ideas, of ideology, or the sociology of the professions. From the historian's point of view, the question of possible ontological reasons for natural science's distinctiveness as an activity is unanswerable and irrelevant. But he must not neglect science's

special historical characteristics (cf. Rudwick, 1975). For a comment on these historiographical presuppositions, cf. the Bibliographical Note, p. 237.

This poses the question: the science of *what*? At the beginning of my period, men saw the Earth through various heterogeneous traditions of discourse. By its end, some practised 'geology'. Changing ways of seeing are signposted by shifts in terminology – the coining of the word 'geology' and its derivatives.[4] This process indicates that the scientific questions which were asked about the Earth, and the conceptualization of the Earth itself, changed dramatically over a century and a half. To speak very baldly, investigating individual terrestrial products and features as isolated objects, perhaps within a philosophy of Creation, gave way to considering the Earth as a fully articulated, historically-related system of forces and materials. Static natural history gradually yielded to a dynamic 'directionalist' programme, concerned to relate the Earth's past and present, and to understand each in terms of the other. This shift was fundamental, marking the moment of the first historical science, with all its attendant functions and problems. William Whewell deftly acknowledged this in coining the term 'palaetiological' to define the historical sciences, of which geology was 'the representative' (1837, iii: 481).

My aim then is to explain what is meant by saying that investigation of the Earth became 'scientific' and 'geological'. I shall concentrate on the manner in which this occurred – that the science was *made*. This notion is in part suggested by Ravetz's historically fruitful conception of the scientist's work as a 'craft activity', which emphasizes the scientist as agent, not merely as thinker, without minimizing that what the scientist does, is intellectual:

1. Scientific inquiry is a craft.
2. The objects of this work are not natural things, but are intellectual constructs, studied through the investigation of problems.
3. The work is guided and controlled by methods which are mainly informal and tacit, rather than public and explicit.
4. The special character of achieved scientific knowledge is explained by the complex social processes of selection and transformation of the results of research. (1971: 71–2)

The man of science cannot breathe *in vacuo*. His work is to be

grasped in a social environment; though not primarily in respect of external 'influences', but mediated through the shared norms, goals, standards and practices of the community, however defined, within which he is working.

Science involves the generation of 'knowledge' by human selection. The historian will investigate the selection mechanisms, the resultant science, and how the former determine the latter. A proper historical view, therefore, of a particular science's configuration at any time will not home in on those scientific ideas with most affinity to present beliefs. It will present a comprehensive vista of a constellation of scientific theories – in harmony or in conflict – facts, suppositions, metaphysical and methodological surmises; facts and theories generally regarded as exploded; facts and theories yet to come into prominence and respectability, and which are (so to speak) candidates for recognition. And it will take account of the physical dimensions of the science – museums, laboratories, journals, experimental techniques and instruments. Deep historical research has invariably demonstrated the multiplicity, complexity and even incoherence of all those bids for knowledge which at any time make up the activity of a science. Such a 'truly historical approach to the history of science' would consider science as

> the ensemble of activities of the scientist in the pursuit of his goal of scientific observation and understanding. It includes the various influences that affect him significantly, perhaps unknown to himself, in this pursuit. It contains all the propositional formulations, both provisional and 'finished', with the reasonings *actually* followed (not just those ultimately reported). In short, . . .everything the scientist actually *does* that affects the scientific outcome in any way. (McMullin, 1970: 16)

At this level, then, the historian's task is to explain why some parts of this body of thought, data and speculation continue in currency, while others develop, or are transformed, or disappear, temporarily or permanently.

All sciences are *made*. They are fabrics constructed by human choice and work. Sciences hinge upon facts, but facts themselves are *facta*, factitious. There is, after all, nothing foregone and eternally right in investigating the Earth *geologically*. Geology is simply the way in which modern European civilization has opted to explore it.[5] Geology has been, indeed, in some respects more

conspicuously *made* than many other sciences. It is recent. It relies upon other, antecedent, sciences.[6] Furthermore, geology is exceptionally dependent upon deep social and institutional foundations. It needs a broad army of workers bestowing abundant data from diverse terrains. Hence, this book's aim is to show how something like a 'critical mass' of those interested in the Earth grew up, forging the necessary techniques, conceptions of Nature, critical standards, collections, publications and such like; none of which singly, but all of which together created the science, and launched it into self-sustained growth.

But to show how geology came to have a form and dynamic of its own is not to divorce it from the wider social context, which was precisely the condition of its existence as an organized science. Geology being a fabric which was made, we must understand its development in terms of the society which made it, or at least permitted its making. The Earth has been conceptualized differently in Europe from in China. So, equally, geology developed differently in Britain from in France, Germany, Italy or Scandinavia; differently in England from in Scotland or Ireland (cf. Merz, 1896–1914; Crosland, 1975).

Three final points may clarify my approach. Firstly, my subject is the transition from earlier beliefs about the Earth and ways of investigating it, to the science of geology as practised in the nineteenth century, a practice with many affinities to current geology, which produced much knowledge the truth of which nobody disputes. Salutary broadsides have lately been fired against Whiggish, teleological, history of science (cf. Thackray, 1970b). These timely warnings must serve as a *terminus a quo* for future history. But if historians take them too doggedly to heart, history of science will fastidiously reduce itself to the opposite vice, of timidly tackling each scientist, idea or paradigm in splendid isolation, and will lose its sense of the historically significant. Naturally the historian must initially understand men and ideas on their own terms (Skinner, 1969). But then he must also evaluate their function in larger historical movements which transcend the individual. The history of so-called cul-de-sacs should certainly be explored – Biblical science, the fate of alchemical mineralogy, or 'mere' fossil collecting. When it is, many may be found to be highways after all. But the central feature, hence the central historical

problem, of modern science has been its power unceasingly to expand – technically, intellectually, ideologically and socially. The historian must grasp the nettle, and probe the *historical* causes for the power of the scientific movement. To investigate the progress of science is not to hymn it, but it is to acknowledge the historian's urgent imperative to study the 'progressive' aspects of human life (Carr, 1961; Plumb, 1964).

Secondly, it is not the point of this book – nor is there room – to biographize individual geologists or to expound their discoveries and theories. The aim is rather to present an inevitably programmatic interpretation of how the science of geology was made. The subject matter is necessarily dynamic, and elusive – though, I hope, not illusory. The argument cannot be proven by secure weight of documentation as in chronicling a controversy or reconstructing a life-history. In its nature, my case rests upon the general plausibility of the interpretative pattern for a lengthy time period. Though, like a moving picture, composed of many stills, it is to be judged by the animation. For the scenario to cohere, it must cover a century and a half, which inevitably means that many individuals and their work, and innumerable problems, have only walk-on parts. I hope partly to fill these gaps by publishing in the near future a number of articles covering in detail topics barely touched on here (see p. 9).

Lastly, what are the broader goals of this study? It is meant as an illustration of how a science is created, developed and maintained; how urgent natural problems change over time; and, perhaps most important of all, how complex a fabric is the activity of scientific investigation, although, iceberg-like, only the tip ever appears on the public surface. It is offered as a contribution to grasping some central problems in the relationship of science to Western culture, and the emergence of modern patterns of thought. Why did natural science expand so remorselessly, as a vocation and belief system, throughout this period? How should we interpret its powerful, equivocal role in the forming of modern civilization? I hope this study of the constituting of the Earth as an object of scientific study throws some light on these issues.

I should be fortunate if this work helped to persuade geologists – and scientists in general – that the history of science is much more than a tale of anticipations and precursors, founding fathers

and who discovered what first. I should be delighted if it helped historians in general to see that the history of science is continuous with their own economic, social, political and intellectual history, though with certain distinctive historical features. Not least, of course, I hope it might offer historians, sociologists and philosophers of science some food for thought.

Note on 1980 reprint

Though this is not the place to attempt a bibliography of works in the history of British geology which have appeared since this book was completed, it might be useful to list here some further publications of mine which provide fuller evidence for the arguments set out in this book, and which develop them further: 'George Hoggart Toulmin's theory of man and the Earth in the light of the development of British geology', *Annals of science*, xxxv (1978), 339–52; 'Philosophy and politics of a geologist: G. H. Toulmin (1754–1817)', *Journal of the history of ideas*, xxxix (1978), 435–50; 'Gentlemen and geology: The emergence of a scientific career, 1660–1920', *Historical journal*, xxi (1978), 809–36; 'Creation and credence: the career of theories of the Earth in Britain, 1660–1815', to appear in S. Shapin & B. Barnes (eds.), *Natural order* (Sage Publications, 1979); 'Dr John Woodward; "A droll sort of philosopher" ', *Geological magazine* (forthcoming). The collection of essays edited by L. J. Jordanova and myself, *Images of the Earth* (Chalfont St Giles, 1979), also contains several major papers on themes covered in this book, in particular those by Hugh Torrens and Paul Weindling.

I

Orientations: *c.* 1660–1710

The Renaissance geocosm

Beliefs about the Earth – its history, form, purposes and future – have always been central to European culture, religion and science. Discourse on the Earth was common in Renaissance Britain, deriving largely from Classical sources, passed down by Medieval scholars, and ranging from concrete fact to cosmological wisdom. Proper ideas about the Earth mattered in a largely rural society, whose ideology was grounded upon cosmic order, whose understanding of man hinged upon macrocosm–microcosm analogies, and whose sacred book began with its Genesis (Humboldt, 1845–62; Glacken, 1967). Such common-sense knowledge was socially broadcast in sermons, school-books, compendia and folklore, reverberating through imagery, symbolism and metaphor (Duncan, 1954; V. I. Harris, 1966; Williamson, 1935). Much of it carried over into the latter part of the seventeenth century, being debated, modified, accepted or rejected, and serving as the fruitful nub of future problems.

But in light of later developments, certain features stand out. Little empirical investigation of the Earth was conducted in Britain up to the mid-seventeenth century (Raven, 1947: 328). Most data derived from Continental sources. Knowledge of the Earth was generally located within other bodies of discourse (e.g. meteorology, or Biblical exegesis) rather than being organized as a science in its own right. What empirical study there was centred upon particular products, such as gems, rather than upon the Earth itself. Furthermore the diverse traditions of discourse about the Earth now to be discussed were conducted largely in mutual isolation.