Introduction: writing about twentieth century geology

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In a classic paper by the late Yale historian of science, Derek De Solla Price (1965), based mainly on the study of citations in a single scientific research field, it was shown how citations in a developing research area have a strong 'immediacy effect'.[1] Citation was found to be at a maximum for papers about two-and-a-half years old, and the 'major work of a paper . . . [is] finished after 10 years', as judged by citations. There were, however, some 'classic' papers that continue to be cited over long periods of time, and review papers specifically discussing the earlier literature. There appears to be a need for such review papers after the publication of about thirty to forty research papers in a field. And the knowledge is synthesized in book form from time to time.

De Solla Price saw citations as the means whereby activities at the research front are linked to what has gone before. He wrote:

> [E]ach group of new papers is 'knitted' to a small select part of the existing scientific literature but connected rather weakly and randomly to a much greater part. Since only a small part of the earlier literature is knitted together by the new year's crop of papers, we may look upon this small part as a sort of growing tip or epidermal layer, an active research front.

He continued:

> The total research front has never . . . been a single row of knitting. It is, instead, divided by dropped stitches into quite small segments and strips . . . most of these strips correspond[ing] to the work of, at most, a few hundred men [*sic*] at any one time.

So we may imagine the research front of science being a multitude of partly interconnected fields, each growing like the shoot or branch of a plant. The research progress occurs at the 'tip' of each 'shoot', and its lower part consists largely of 'dead wood' – though not wholly dead as occasional reference back to classical papers continues. Obviously, the 'shoots' are loosely interconnected, as references may sometimes be from one research field to another.

I represent some of De Solla Price's findings diagrammatically in Fig. 1; and in this diagram I have also indicated what may be the range of interest of historians of science. It will be seen that while the scientists' interest in the earlier literature declines quite rapidly with time the historians' interest is focused on the earlier work and falls off towards the present.

It is an interesting question whether the study of the history of science generally, or geology in particular, is part of science. Some think it is, and in some cases they are obviously right. For example, old data are of importance in earthquake prediction or studies of geomagnetism. Field mappers may use old field-slips to help locate outcrops. Mining records are important to economic geologists. Palaeontologists need to know the early literature to avoid problems of synonymy. And so on.

On the other hand, one could hardly claim that study of, say, the work of Arthur Holmes is advancing any modern scientific research front. Historians of science usually have other motivations than the direct advancement of science. They are interested in the past 'for its own sake', the history of ideas, correct attributions of credit, understanding the philosophy and sociology of science, 'ancestor worship', and so on and so forth. Such historical work can be called

[1] In fact, the field selected by De Solla Price turned out to be an illusory one – the study of 'N-rays'. But the practitioners of the field were not aware at the time that they were investigating a spurious phenomenon. The field selected by Price for his analysis was well suited to his purpose as it had a clearly defined beginning; and its literature 'behaved' like that of other research programmes. That it had an ignominious end was not relevant to Price's findings. It is true, however, that some fields such as palaeontology make much greater use of early literature than do others such as geochemistry. Palaeontologists and stratigraphers have to observe the principle of priority of nomenclature and so are always involved with the early literature of their fields.

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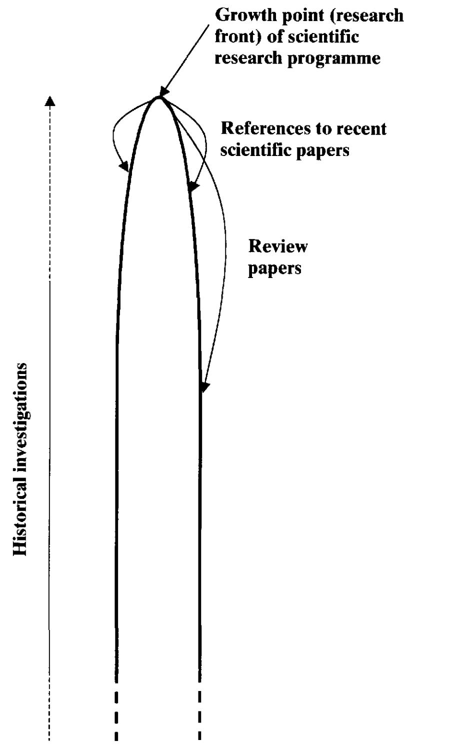

Fig. 1. Representation of the growth of a scientific sub-field, specialty, or research programme, based on the scientometric study of D. J. De Solla Price (1965), representing also the respective temporal interests of scientists and historians of science.

'metascientific'. It is different from what motivates scientists, as working scientists, to study the earlier stages of their fields of inquiry – to further the technical progress of science.

If we regard the study of the history of science as a 'metascientific' activity, then it too has some of the characteristics of a scientific research programme, as described by De Solla Price. But there are differences. The 'knitting' of, say, the history of geology literature into past work, via citations, tends to be more diffuse than is the case for scientific research programmes – though in some areas of the history of science (e.g. the study of Darwin or Lyell) there is a discernible 'research programme' with a developing research front not unlike that of a programme in science. In addition, if they are interested in recent science, historians of science have to scrutinize a target that does not remain fixed, as do the laws of the physical world, but expands indefinitely through time. However, most historians of science do not attend much to the very recent past. Such metascientific attention is the domain of the reviewer or the science journalist.

Studies of the history of geology were almost non-existent before the nineteenth century. Early contributions were 'part of' science (e.g. d'Archiac 1847–1860). Even Lyell's history (Lyell 1830–1833, **1**, pp. 5–74) served, for him, the polemical purpose of garnering support for his geo-philosophy. When studies of history of geology got going in a serious and professional way after the Second World War, most attention was given to the geoscience of the seventeenth, eighteenth, and nineteenth centuries (e.g. Gillispie 1956; Davies 1969; Ospovat 1971; Rudwick 1972; Porter 1977; Greene 1982). Such writings were different in character from the earlier efforts of scientist-historians (e.g. Geikie 1897; Zittel 1901; Woodward 1908). They were not necessarily concerned chiefly with the 'internal' history of science, and offered 'critical' historiography, attending in some cases to the social context of geology.

It was, of course, natural that historians should attend to earlier matters first. Remote events could be viewed with 'perspective' and without treading on the toes of people still alive. The foundations had to be established first, rather than the recent superstructure. Moreover, so far as the twentieth century is concerned, it is only just completed, so we can hardly expect to see much in the way of general synthetic overviews of twentieth century geology at the present juncture. Nevertheless, much more geology has been done in the twentieth century than in the whole of previous human history, and the task of trying to form an overview of it cannot be delayed long. So while the task of studying twentieth-century geology cannot be completed here and now, it can at least be started – or contributions made towards future syntheses.

If we look for generalizations, we immediately remark the development of specialization, with the division of science into research programmes, such as those perceived by De Solla Price. Such specialization, accompanied by a growing divide between the humanities and the sciences, has long been deplored, at least from the 1950s, when C. P. Snow's essay on the 'two cultures' (Snow 1964) caused heads to shake in disapproval, and remedies for the supposed problem were sought – including the study of the history of science by students of the humanities. The philosopher Nicholas Maxwell (1980) deplored the supposed departure from enlightenment arising from specialization.

However, in one of the best books that I know on the *sociology* of science, the geologist and oceanographer Henry William Menard (1971) argued that the pressure towards specialization is irresistible. Influenced by De Solla Price (1961, 1963), he likened the development of science to that of a bean sprout, which eventually, however, inevitably loses growth and withers. The growth of science is like that of water lilies on a pool of finite size, following the pattern of the S-shaped 'logistic curve'. But this applies to specialisms or research programmes rather than science as a whole, which keeps 'alive' by constant divisions into new specialisms. Why does this specialization occur?

The 'explosive' nature of the growth of scientific literature is well known, and science itself has ways to try to cope with the problem, through the production of review papers, bibliographies, and text-books (and perhaps ultimately retrospective histories), and the storage of data in computers as well as libraries. How do people keep on top of it all? The answer, for most, is through specialization. There are new 'hot' fields, and old ones with slowing growth that are becoming ossified almost by virtue of their age and size. Menard considers the case of a new field. There may only be a handful of people in it, and a young person can get a handle on its literature relatively easily and advance to a position of influence when young. By contrast, for a person joining an old field it may take years to gain a commanding position, and all the 'positions and perquisites of academic, professional, and economic power are out of his [*sic*] reach for 20 to 40 years' (Menard 1971, p. 18).

Menard estimates that a person entering a really new field might become '*au courant*' with its literature in perhaps two months. For an 'average' field it might take three years. But someone entering a mature field might be faced with a literature of nearly 30,000 items! The newcomer may be near retirement before he or she has a grip on the literature. In any case, positions in an old field are very likely filled, keeping out new aspirants. Or, if the field is declining, vacancies that may occur are not filled by people in that field but by neighbouring predators. The trick, then, is to get into a new field, but not one that is a bad risk because of shaky foundations. Menard recommends that the optimum time to enter a field is at about its third period of doubling. Then the risks are at a minimum and opportunities at their maximum. However, if one has invested a lifetime's work in a research programme or in working according to some paradigm, and if one has, despite the problems of old research fields, made a successful career therein, then one may be exceedingly disinclined to abandon it and try something new.

Leaving aside such questions of career tactics, it can be seen that pressure towards specialization is intense, the concerns of the likes of Maxwell or Snow notwithstanding. By way of illustration, we see the field of ammonite studies in decline in the latter part of the twentieth century; and one of the authors of the papers in the present volume decided to leave it to all intents and purposes, to become an authority on the history of geology, particularly in the early nineteenth century. Such a career response is one way for a person to respond to changing circumstances. The commoner response is to seek to become an administrator, university teacher (as opposed to researcher), or go in for university politics. Becoming an historian seems to me a more attractive proposition – though one may be hard pressed to find the necessary funding!

Be that as it may, we should note that Menard regarded geology as somewhat moribund in the first half of the twentieth century. It had, so to speak, run out of puff: it was, as a whole, becoming a 'mature' or even 'elderly' science. During the nineteenth century (as, for example, was the case in the State Surveys in the US), it had been a rapidly expanding enterprize, with rather few bureaucratic accessories. There was a large and successful research programme, based on primary or reconnaissance surveys. But such work was limited to the Earth's surface rocks. There was little technology to explore within the Earth by geophysical methods, or (obviously) from without by aerial survey or space travel.

Further, much of the Earth was covered by oceans and inaccessible. Conditions within the Earth could not be simulated in the laboratory. In addition, the overarching framework of geological theory was (as it now appears) unsatisfactory in important respects. It embraced vertical movements as the prime type (though Charles Lapworth had demonstrated the importance of lateral movements in the NW Highlands of Scotland; earlier, geologists in Switzerland such as Albert Heim had done likewise with the idea of nappes; and in America James Hall and the brothers Henry and William Rogers had envisaged significant lateral movements). Besides, geological research was seriously impeded by the two world wars, though geologists contributed their services to both (Underwood & Guth 1998; Rose & Nathanail 2000). In Britain, an ill-advised reorganization of science education before the First World War tended to separate geology from biology, physics, and chemistry at the secondary level. The subject was not taught at elementary schools, and at university it was not seen as a relevant study for engineering students. According to Percy Boswell, in a Presidential Address to the Geological Society, 'while our science was suffering these reverses, the Geological Society stood magnificently and gerontically aloof' (Boswell 1941, p. xli)!

Menard distinguished fields of science that are in a steady state or decline, in transition, or in a state of real (perhaps super-exponential) growth. In the last case, the literature may double in as little as five years. Under such circumstances, papers are brief and published rapidly. Often communication by word of mouth or by pre-prints (or now by e-mail) is more important than by journal communication. The literature of 'hot' fields is not burdened with reviews, and citations are rather few in number. The field's practioners do not concern themselves unduly with bureaucratic or stylistic niceties. Bibliographic work is put aside. By contrast, in old fields many practitioners may have been diverted into administrative functions. Publication delays are considerable. The literature has copious bibliographies, and arcane terminological distinctions are devised, as, for example, in Marshall Kay's baroque taxonomy for different kinds of geosynclines (Kay 1963). In severe cases, papers spend more time discussing other papers than the subject matter of the fields. (Such a state of affairs is found hyperdeveloped in Classics, which has rather little new empirical nutriment.)

As is well known, geological sciences as a whole became re-invigorated in the 1960s and '70s through the plate tectonics revolution. This came about through the application of new technical methods (such as the use of computers in geology) and the partial fusion of two previously distinct fields: geology and oceanography. Submersibles and aeroplanes became useful tools in the progress of geology, complementing the hammer, microscope, field survey instruments, etc. One might say, with Darwin: '[h]ere then I [or, in the case now under discussion, geologists as a whole] had at last got a theory by which to work' (Darwin F. 1887, **1**, p. 83). Several authors (e.g. Hallam 1973) have, appropriately I think, seen the revolution as 'Kuhnian' in character (cf. Kuhn 1962), which implies in a way – at least according to the earlier exposition of Kuhn's views – a revolution in 'world-view'. In this case, it entailed a shift from seeing tectonic movements of the Earth's crust as primarily vertical to lateral also. (Of course, the movement of plumes – part of modern tectonic theory – is essentially vertical.)

The transformation in theory associated with the plate tectonics revolution also led to significant changes in geology as a discipline. In many universities, departments were re-organized, involving fusion with, or incorporation of, studies in geophysics, and they were re-named as schools of 'Earth Science', or similar. In Australia, the changes occurred at about the same time as a notable expansion of prospecting and mining, and there was a 'boom' in geology as well as in mining shares. I am not sure whether that boom was linked to the plate tectonics revolution, but certainly geology began to be seen as an intellectually exciting, and (perhaps better) a lucrative field. There was a rush of students into the earth sciences, in parallel with the famous Poseidon Company (nickel) stock-market bubble. This story had an unhappy ending. The nickel market crashed and many geologists fell out of work or graduates failed to find jobs in the field in which they had trained. Thus the linkage of geology with the capitalist system may be remarked, though such links were nothing new in applied geology.

While important parts of geology became inextricably linked with physics, partly as a result of the plate tectonics revolution, it also became entwined in the latter part of the twentieth century with space science and aeronomy, so that we now find congresses in which the participants are partly earth scientists (seismologists, geomagneticians, tectonics specialists, etc.) and partly space scientists and space engineers (IAGA–IASPEI 2001), or even astronomers. The study of the Earth is now enriched by investigations of the Moon and planets. Geomagnetic studies (so important in the plate tectonics revolution) are linked to investigations of the Sun, the ionosphere, etc. Studies of movements of faults and plates are facilitated by the use of new techniques such as GPS, themselves made possible only by the work of artificial satellite engineers. Well before the end of the twentieth century, one of the leading journals for geologists was *Earth and Planetary Science Letters*. On the other hand, it should be emphasized that the effect of plate tectonic theory on the day-to-day activities of many geologists, particularly applied geologists, was often quite small.

In any case, much had gone on before the plate tectonics revolution actually occurred, both in theory and in technological development. Alfred Wegener (1915) and Alexander Du Toit (1937) had long before found much geological evidence for 'drift'. Arthur Holmes (1929) had upheld the idea of convection in the Earth's interior to account for 'drift'. Felix Vening Meinesz (1929 and other publications) had undertaken a series of underwater gravimetric investigations aboard a US submarine.

But mobilist theory was not generally accepted, meeting opposition in both dominant post-war powers: the US and the USSR. The reasons for the tardy acceptance of mobilist doctrine have been analyzed by Robert Muir Wood (1985) and Naomi Oreskes (1999).

Muir Wood suggests that Soviet scientists' opposition to new ideas was due to the conservative nature of society and the scientific community in the USSR, and the fact that Soviet scientists worked on a huge continental mass, had limited contacts with Western scientists, and lacked the oceanographic data available to the Americans. Oreskes argues that American opposition arose from several factors. First, American geology in the first half of the twentieth century had a certain style, exemplified by the grand collaborative effort of the US Coast and Geodetic Survey, begun in the nineteenth century, to determine the form of the geoid. For simpler calculation, this work assumed the Pratt (as opposed to the Airy) model for isostasy. A uniform global depth of isostatic compensation was assumed, and it appeared that the crust and mantle were generally in a state of isostatic equilibrium. Lateral movements, insofar as they occurred, were thought to be relatively small-scale, occurring in response to erosion of mountains and deposition of sediments in the oceans. The thinking was in accord with long-standing American ideas about the permanence of oceans and continental cratons, derived particularly from the work of James Dwight Dana. Americans such as Charles Schuchert and Bailey Willis attempted to account for faunal similarities across oceans by postulating various 'isthmian links'.

Second, there was the American delight in T. C. Chamberlin's (1897) 'method of multiple working hypotheses'. This was supposed to guard geologists against the uncritical adherence to grand theoretical systems, but in practice, according to Oreskes, it led to the overzealous collection of 'facts'. For William Bowie, the chief spokesperson on matters to do with isostasy, isostatic adjustment and balance was a 'fact', whereas continental drift was an 'interesting hypothesis'. Also, according to Oreskes, Lyellian uniformitarianism impeded acceptance of 'drift' theory. Schuchert believed that knowledge of present faunal distributions could not be applied to the past if there had been latitudinal changes in the positions of continents. It seemed to him that were this so, the present would no longer be the key to the past.

Such geological arguments may seem implausible, but the fact that they attracted favour can perhaps be explained by the hypothesis that geology was indeed in the doldrums before the plate tectonics revolution. Senior geologists were overly committed to an old paradigm and found it difficult to change their opinions. In the context of the 1960s, with the US as the dominant power in the West, it was unlikely that there could be a scientific revolution in geology unless the North Americans joined the revolutionaries. This they eventually did, with the work of J. Tuzo Wilson and the classic paper of Isacks, Oliver & Sykes (1968), in which it was shown, by seismological evidence, that there was movement along the fault planes postulated by theorists such as Wilson (1965*a*, *b*). But the transition was not easy.

The literature on the history of plate tectonics revolution is substantial, even if that on twentieth century geology as a whole is sparse. Besides the volumes by Hallam, Muir Wood, and Oreskes, one should mention particularly the earlier 'straight' account by Marvin (1973) and the later one by Le Grand (1988), which interprets the revolution in terms of the ideas of philosopher of science Larry Laudan rather than those of Kuhn. Henry Frankel (1978, 1979), by contrast, has seen the revolution through the eyes of the philosopher of science Imre Lakatos (which addresses the idea of competing research programmes, either 'progressive' or 'degenerating') than through those of Kuhn. For the oceanographical aspects, see Menard (1986) and Hsü (1992); and for the seismological aspects, see Oliver (1996). Geomagnetic issues are admirably treated by Glen (1982).

Away from the plate tectonics revolution, there are biographies of a few notable individuals, such as Alfred Wegener (Schwarzbach 1986; Milanovsky 2000), Johannes Walther (Seibold 1992), and Arthur Holmes (Lewis 2000); and in connection with work on the study of the age of the Earth, and radiometric dating more generally, the volume of Dalrymple (1991) holds the field. There are useful collections of classic papers from the first half of the century edited by Mather (1967) and Cloud (1970). A set of essays on the history of sedimentology (Ginsburg 1973) is interesting for an essay by Roger Walker (1973), which proposes that the coming of the idea of turbidity currents (Kuenen & Migliorini 1950) constituted a scientific revolution of Kuhnian dimensions in sedimentology. A volume by Peter Westbroek (1991) takes one in the direction of the 'Gaia hypothesis', discussing, as the title *Life as a Geological Force* suggests, ways in which living organisms are involved in geological processes. It also contains material of an historical nature, such as discussion of Robert Garrels' ideas on the cycling of elements through the oceans, atmosphere, and lithosphere. A related topic – controversial over many years – has been that of eustasy, which takes one into the domain of sequence stratigraphy. A collection of papers edited by Robert Dott (1992) gives much useful detail, and includes an essay by one of the main protagonists in the eustasy debate, Peter Vail. There are various institutional histories (e.g., Eckel 1982; Bachl-Hofmann *et al.* 1999), but not much 'critical history' in this area. A two-volume encyclopedia edited by Gregory Good (1998) contains interesting essays on twentieth century geology.

One of the oldest geological topics has been the problem of the causes of the formation of mountains and ocean basins, and interest in the issue has been sustained through the twentieth century. Few have made a concerted effort to view the wood, as distinct from all the trees in the literature. However, in a collection of papers on geological controversies, mostly on sedimentological topics (Müller *et al.* 1991), the Turkish geologist and historian of geology Celâl Şengör (1991) gives one of his several accounts of his interpretation of the 'taxonomy' of the history of tectonic theories. He proposes a general model for the history of tectonics, there being, he suggests, two different tectonic *Leitbilder* (e.g., Şengör 1982, 1999). He drew the notion of *Leitbilder* from Wegmann (1958).

Şengör's 'Manichean' dichotomy of tectonic theorists proposes that two broad ways of thinking were established as far back as the eighteenth century (in the ideas of Hutton and Werner) and, in a sense, have been ongoing ever since. He further traces the philosophical (but obviously not the geological or tectonic) roots of the eighteenth century thinking back to the atomists and Aristotelians in Antiquity. In the nineteenth century, the two modes of interpretation were, he suggests, manifest in uniformitarian and catastrophist geologies respectively. Şengör (1991, p. 417) lays out his dichotomy as summarized in Table 1.

Table 1. *Classification of tectonic theorists, according to A. M. C. Şengör*

Atomists (e.g. Democritus)	Aristotle
Hutton	Werner
Lyell	Cuvier Élie de Beaumont
Suess	Dana Chamberlin
Wegener Argand	Kober Stille

Followers in the two traditions were, suggests Şengör (1982, 1991):

Wegener–Argand ('mobilism')	**Kober–Stille (episodic, world-wide orogenies)**
du Toit	Haug
Daly	Willis
Holmes	Schuchert
Salomon-Calvi	Bucher
Staub	Haarmann
Griggs	van Bemmelen
Ketin	Hans Cloos
[Wilson]	Kay
	Tatyayev
	Beloussov

Şengör sees the members of the Wegener–Argand school as tending to recognize irregularities in Nature and as being in accord with the falsificationist philosophy of science of Karl Popper – of which he strongly approves. By contrast, he regards the members of the Kober–Stille school as tending to look for and see regularities, both geometrical and temporal, in Nature. These two ways of looking at, or thinking about, the world can be seen in the ancient atomists and in the Artistotelians.

I am not aware that many have adopted Şengör's schema, one obvious reason being that today hardly anyone (or no anglophone) has the necessary knowledge of the early Continental and Russian tectonic literature to be able to evaluate his dichotomy satisfactorily. (Of course, even if one accepts Şengör's dichotomy of tectonic theorists one need not agree with his parallel division along methodological and metaphysical approaches or attitudes; and some may doubt that Lyell and Wegener should be situated in the same geological tradition.) Be this as it may, it is evident that Şengör offers a view of the history of twentieth century tectonics quite different from the 'before and after the plate tectonics revolution' account of most English language texts. It proposes a fresh pattern, to make sense of the 'bloomin-buzzin-confusion' of the tectonics literature. It is probably not a pattern that professional historians of ideas would find attractive, but it is undoubtedly an interesting schema; and to my knowledge no other author has tried to identify the common factors in the tectonic theories that have been proposed over the years. Şengör sees conceptual continuity, and Popperian piecemeal change, in the history of tectonics. By contrast, the Kuhnian 'anglophone' theorists such as Hallam have seen conceptual discontinuities.

It should be noted that Şengör's modern theoretical work is typically grounded in all the early literature relevant to his given theme. The same was true of the French geologist and historian of geology, François Ellenberger (1915–2000), but such levels of scholarship are becoming rarer. A recent study by Şengör (1998 for 1996) traces the lengthy history of the concept of the Tethys Ocean (a topic he was worrying about in the middle of the night when he was about ten!).

If tectonics is a major theme in, or branch of, geology, so too is petrology, but to date little has been written on the history of twentieth century petrology, experimental or otherwise. Davis Young (1998) has written a biography of the petrologist Norman Bowen, and Young's paper in the present collection is in a sense a digest of that book. Sergei Tomkeieff's (1983) posthumous *Dictionary of Petrology* contains valuable terminological information, with copious references to the early literature, and an older volume by Loewinson-Lessing (1954) is still useful. Yoder (1993) has published a set of 'annals' of petrology, which provides a chronological framework for a synthetic study of twentieth century igneous and metamorphic petrology. Such a volume will probably first appear from Davis Young's hand.

While the plate tectonics revolution stands out in most people's minds when thinking about the history of twentieth century geology, the re-emergence of 'catastrophism' has also been a noteworth phenomenon. It has chiefly taken the form of the theory – put forward with increasing confidence in the last quarter of the twentieth century – that impacts from extra-terrestrial bodies (bolides) have had a substantial influence on the Earth's geological history, especially in the realms of stratigraphy, palaeoclimatology, and evolutionary palaeontology (see e.g., Albritton 1989; Huggett 1989; Clube & Napier 1990). It has been an uphill task for 'bolide theorists' in that the very notion of extra-terrestrial contacts and attendant catastrophes smacks of nineteenth century 'catastrophism', or even earlier 'theories of the Earth' such as those of Buffon or Whiston. It runs counter to what geologists have long been taught: uniformitarianism and the virtue of the methodological principle that 'the present is the key to the past'. So 'neo-catastrophism' has perhaps had an even more complex history than that to do with the plate tectonics revolution in that there has been no swift and successful 'coup' or scientific revolution, but a long-drawn-out series of battles. Its proponents have had to produce and justify the empirical evidence, and also show that their theory is metaphysically or methodologically sound.

The history of the shift of opinion on the question of neo-catastrophism has been complex in that it has involved different fields in geology (stratigraphy, palaeontology, geochemistry, planetary geology, mineralogy, etc.) with, broadly speaking, a debate between geologists chiefly involved with the life sciences and those associated more with the physical sciences. William Glen (1994) has edited an interesting collection, the papers of which examined the dynamics of the debate while still in progress – before the battle was over and one could see who had 'won'. Since the publication of that book, the conflict seems to have shifted in favour of the 'catastrophists', and recently, a neo-catastrophist, Charles Frankel (1999), has argued that the major subdivisions of the Cenozoic can all be matched with impacts, the 'smoking gun' for the K–T boundary being found at the Chicxulub Crater, by the edge of the Yucatán Peninsula, Mexico (as others had earlier suggested). The arguments of some stratigraphers and palaeontologists that the great change of flora and fauna at the end of the Cretaceous, including the demise of ammonites and dinosaurs, does not coincide in time with the layer of iridium-enriched sediment, thought by the bolide theorists to have been caused by some catastrophic impact, seems to have less appeal – at least to the public imagination – than the notion of an apocalyptic termination of the Cretaceous.

It is interesting that the nineteenth century (Cuvierian) catastrophists were looking to some such event to *explain* the discontinuities in the stratigraphic record; and it was discontinuities in the fossil record that made the establishment of stratigraphy by William Smith, Alcide d'Orbigny, Albert Oppel, and the like, possible. It is, therefore, a little ironic that, in the twentieth century, it has been chiefly biostratigraphers who have opposed the idea of extra-terrestrial impacts being responsible for fundamental features of the stratigraphic column. Be this as it may, the controversy is by no means over at the beginning of the twenty-first century. For example, one of the contributors to the present collection has recently co-authored a paper that argues with considerable cogency that the case for the Chicxulub event being responsible for the demise of the dinosaurs and other extinction events at about the end of the Cretaceous is anything but conclusive (Sarjeant & Currie 2001). It is, for example, not a little startling to read of the discovery of seemingly unreworked dinosaur egg remains (ornithoid theropod types) above the famous iridium horizon (Bajpai & Prasad 2000). It is not claimed that these fossils are Palaeocene, but it is suggested that the iridium layer does not mark the top of the Cretaceous (at least in India). It may well be some time, therefore, before Glen will be able to write a book recounting the closure of this controversy. The controversy may, in fact, eventually be resolved by some sort of compromise. Sarjeant and Currie certainly do not contest the occurrence of the Chicxulub impact event.

From what has been said above, it will be evident that any attempt to provide a synthetic overview of the history of twentieth century geology, as Zittel provided a summation of the geological endeavours of the nineteenth century, is at present premature. The story is infinitely more complex than that for the nineteenth century. The chapter of the twentieth century is only recently closed. Historians have not yet done the necessary analysis, which should precede the synthesis. A recent publication by Edward Young & Margaret Carruthers (2001) is interesting, however, in that it provides a kind of 'annals' or preliminary chronology of twentieth century geology – a 'year-by-year account of important advances since 1900'. The authors mention a deep 'crisis of identity' among those who study the Earth and the rocky bodies of the solar system. Even departmental names are 'doubtful'. The authors suggest that: '[i]n some quarters . . . the activities of scientists studying the Earth can no longer be described as belonging to a single discipline, and . . . just as it is rare to find the life sciences under a single roof in most universities today, so too will go the earth sciences'.

It is too soon to say whether the field of geology or earth sciences will eventually disappear as such, but it is true that it has been troubled, after the rush of adrenalin in the 1970s, by declining student interest, in some parts of the world at least. For example, in New South Wales, the decline in secondary-student enrolments in geology was so great that it appeared at one stage that the subject would vanish from the Higher School Certificate curriculum. The response was, in a sense, to 'disguise' geology in the clothing of 'environmental science'. This change was implemented in the late 1990s, and it is too soon at present to know whether it will prove effective in the long term from the point of view of those interested in the well-being of geology or the earth sciences, but I understand that enrolments have picked up. Clearly, students have been looking for a more 'holistic' approach to geoscience, and it is interesting therefore that in their 'annals' Young & Carruthers (2001) include a good deal of material on environmental issues and space science. For example, the publication of Rachel Carson's *Silent Spring* (1962) is seen as a milestone – along with Harry Hess's 'History of ocean basins' (Hess 1962). The authors' 'annals' of twentieth century earth science thus refer to issues traditionally categorized under the heads of geographic exploration (including satellite imaging), meteorology, environmental science, 'conservation' (such as the Rio summit of 1992), aeronomy, space science, etc.

Young & Carruthers' (2001) headings for the major branches of modern earth science are therefore interesting. They offer:

Understanding Earth's materials
Earth's deep interior
Geological time
Chemistry of Earth's near surface
Climate and global warming
Life on Earth
Plate tectonics
Beyond plate tectonics
Hazard assessment
Remote sensing
Planetary geology

These heads may strike the reader as somewhat whimsical, failing to cover the field adequately, or cutting the cake of geoscience inappropriately. They are, nonetheless, suggestive, and show the way the wind has begun to blow at the beginning of the twenty-first century. A register at the beginning of the twentieth century would surely have included stratigraphy or palaeontology as separate items. In the middle of the century, we would expected to see petrology, structural geology, and sedimentology in such a list. Now at the turn of the new century we remark the interest in the Earth, both 'inside and out'. To that extent, at least, the present collection of essays has common cause with the overview of twentieth century earth science sketched by Young & Curruthers. So far as I am concerned, it's not clear how geology could or would be geology if it were bereft of biostratigraphy. But perhaps that is to be the 'shape of things to come'.

When planning the Rio symposium we decided not to devote excessive attention to the history of plate tectonics. Despite the fact that the emergence of that theory has been the most important theoretical development in twentieth century geoscience (or at least it caused the greatest excitement in the earth science community), it has already been the object of substantial historical investigations, some of which are mentioned above. Nevertheless, the topic was unavoidable. So in the present collection we find that the papers by Lewis, Le Grand, Khain & Ryabukhin, and Barton deal with the question to a greater or lesser extent; and it appears in some of the other papers too.

Khain & Ryabukhin's paper should be of special interest. There has, I suggest, been a perception in 'the West', reinforced by Muir Wood's (1985) stimulating book, that Russian geology was reluctant to embrace plate tectonics. It is true that Russian geology adopted plate tectonics somewhat later than in the West, and one of her most influential geologists, Vladimir Beloussov, was antagonistic towards the theory, at least initially. However, Beloussov's opposition was not just 'perverse' or 'political'. His views were based on ideas developed by Nikolay Shatsky, based on seismic evidence for deep faults, apparently crossing the crust and upper mantle. In fact, as Khain and Ryabukhin reveal, there was intense discussion at Moscow State University, with, in effect, two contradictory theories being taught in the same institution. Khain was one of the main protagonists and actively promoted plate tectonic theory.

The tectonics theorist Khain is, of course, writing about the events of the 1970s from the perspective of the 'winning' side; and it might be said that, having lived longest, he now has the opportunity to write the history the way it appeared to him. Be this as may, there was evidently no monolithic anti-mobilist theory in Russia in the 1970s, and by the end of that decade immense efforts were being made to apply plate tectonics within the Russian domains, as is evident, for example, from the arduous work undertaken in the Urals (Zonenshain *et al.* 1984). Incidentally, it may be mentioned that geological theory at Moscow State University remains 'un-monolithic' to this day, as I understand, with some classes teaching expanding (or pulsating)-Earth theory, while the majority offer standard plate tectonic doctrine. The Russian paper also refers to some theoretical notions not well known in the West.

Some of the contributors to the present volume are scientists interested in the history of geology; some are historians of geology. Homer **Le Grand** is one of the latter. His paper utilizes oral history, providing some reminiscences about the extension of plate tectonic theory to 'terrane theory'. It is well that such reminiscences be captured for posterity. Le Grand, of course, has been an observer of events, rather than a participant.

The same may be said of the historian Cathy **Barton**. Her paper is partly based on interviews with Marie Tharp, well known for her contributions to the mapping of the ocean floors – a necessary empirical first step towards the plate tectonics revolution. There is currently considerable interest in the part played by women in science, and it is sometimes said that women have had a hard time in 'getting on' in geology. Barton's paper shows that Tharp was not much hindered because of her gender; but she had the advantage of working at a time when there were vacancies in civilian science due to the Second World War; and she also had Bruce Heezen's patronage. Interestingly, though Heezen and Tharp's work (or that like it) was, I think, necessary for the emergence of plate tectonics, it was not sufficient, for they adopted the now-rejected expanding-Earth theory.[2] Barton suggests that they were the geological equivalents of Tycho Brahe in the Copernican Revolution. They provided essential empirical information, but for them it led to what is (according to the present consensus) an erroneous theory.

Cherry **Lewis**, known among geologists for her work on fission-track estimates of the 'lost overburden' of some of the older rocks in Britain, has for some time been studying the work of Arthur Holmes, on whom she has published a biography (Lewis 2000). Lewis's paper raises the problem of the age of the Earth, which was for many years a major issue in geology and beyond, but was eventually solved in principle by Holmes, regarded by some as the outstanding geologist of the twentieth century. He was also one of those who accepted mobilist doctrines well before the plate tectonics revolution proper, and he advocated (but did not originate) the idea of a convectional mechanism for continental movement that still stands in essence.

Readers picking up this book will immediately notice its famous cover illustration, and the title. Two of the papers (those of Good and Marvin) deal respectively with the Earth's interior and with entities external to the Earth. Thus we are taken into the realms of geophysics and astronomy – where geology overlaps with physics and with planetary science (or even cosmology).

Ursula **Marvin**, geologist, meteoritics expert,

[2] But Ursula Marvin (pers. comm., 25 Sept. 2001) informs me that she heard Heezen say at a meeting in 1966 that some calculations he had made suggested that the Earth expansion required just to account for the opening of the Atlantic was unreasonably large. Heezen is generally regarded as an 'expansionist' but the matter perhaps deserves closer historical scrutiny.

and authority on the history of meteoritics, takes the reader into the world of outer space and what it can tell us about the geology of our Earth. As discussed above, one of the main trends in twentieth century earth science has been the extent to which it has been integrated with planetary science (and aeronomy). Marvin's paper is a perhaps unlikely, but also a good, place to start this collection of essays. Meteorites provide some of the most useful empirical evidence we have about ways in which the Earth may have formed. Also, the study of craters on the Moon and elsewhere has thrown light on terrestrial impacts, and their possible role in the history of life on Earth, which, as mentioned above, has been hotly debated over the last twenty years or so by 'astrogeologists' and traditional palaeontologists and stratigraphers (see e.g. the paper by Torrens in this volume).

Marvin takes us through the story of the efforts to find meteorites and discover whence they came, particularly those that seem to have come from the Moon and from Mars. I was particularly struck by two points she made in her Rio paper. She remarked that the 'vision' of our Earth, seen from space and depicted on the cover of this book, had a substantial impact on the way we now think about the Earth; and the 'vision' did wonders for the 'holistic' environmentalist movement. This is the planet where we live, which we can now 'see' as a whole from the outside; and this is where we shall likely perish as a species if we do not act sensibly as its stewards. Marvin also observed that the summary geological time-chart, which delegates received in their conference-bags at Rio (REPSOL: YPF 2000), listed the lunar names for the epochs of the Hadean Period (Cryptic, Basin Groups 1–9, Nectarian, and Early Imbrian) obtained by mapping of the Moon, which preserves a stratigraphic record that is keyed to dated samples reaching back to that time. Direct stratigraphic evidence on Earth for those remote times has long since been lost, so insofar as we have a 'stratigraphy' for the very early Earth it is inferred from entities *outside* our planet.

Incidentally, though the present collection does not have a paper specifically devoted to the question of bolide impacts and their implications for Earth history, Marvin addresses some aspects of the question, even though she does not discuss it in detail. (It was treated by her in a previous Special Publication: Marvin 1999).

The historian of geology, Gregory **Good**, takes us *inside* the Earth. He is interested in the changes that have taken place through the twentieth century in studies of the Earth's magnetic properties. The early work developed from the many observations of its magnetic field that go back to the beginnings of geomagnetic investigation. By the first half of the twentieth century, the subject had progressed well beyond Baconian (or Humboldtian) data-collecting, and attempts were made to develop theories about the causes of the existence of, and changes in, the Earth's magnetic field. This work, Good argues, lay within the domain of 'terrestrial magnetism'. It was related to problems in navigation, for example, rather than geological theories *per se*. But as time passed, more information became available about the Earth's interior and it became possible to produce theories about the origin of the Earth's field and its changes. After palaeomagnetic studies' substantial contributions to the plate tectonics revolution, much attention is now bestowed on palaeomagnetics, as geologists seek evidence about former positions of the poles in reconstructing the geological histories of different parts of the Earth (a matter also intimately related to terrane theory). Good argues that the very nature of geomagnetics has changed; and he holds that the view of earlier work has become distorted because it is seen through the lens of the later.

The paper by Richard **Howarth**, is authored by someone who assisted in the development of the use of the computer in geological studies. He has also made much use of statistical analyses for the purpose of geological research. It might not be obvious that there is a coherent field of 'mathematical geology', but in this paper and in his other historical publications Howarth has demonstrated the coherence of the field as a branch of geology appropriate to historical investigation (e.g. Howarth 1999). He has also been much interested in the use of figures such as 'rose diagrams' or stereograms in geological analysis, and for understanding geological ideas and making them comprehensible to others (cf. Rudwick 1976). Such representations did not begin *ex nihilo* in the twentieth century, though they are characteristic of the work of that period.

As mentioned, there has long been a dearth of studies in the history of petrology, perhaps the most basic of the geosciences, yet neglected by historians of science, especially for the twentieth century. For this reason I am gratified that the present collection contains four petrological papers. The field is, of course, enormous, and we cannot expect an author to cover the whole in a paper such as might fit into the present collection. In the contribution of Eugen and (his wife) Ilse **Seibold** we are provided with a straightforward survey of twentieth century sedimentological writings, extending into sedimentary petrology. It identifies the main themes in the field, and provides an entree to its vast literature. It will be particularly useful in that, written by German authors, it is not focused on English-language writings (this may well become appropriate for the twenty-first century, but it is not so for the twentieth century), but discusses English, French, German, and Russian publications. I am particularly grateful to Professor Eugen Seibold for completing this work in a year when he had to undergo an eye operation. He has been President of the International Union of Geological Sciences and participated in voyages undertaken for the purpose of ocean-floor surveys. Ilse Seibold is a foraminifera specialist and author of a book on Johannes Walther (1992).

As to igneous petrology, one of the most important topics for the twentieth century has been the problem of understanding the changes that occur during magma crystallization. Amongst those who worked on this topic, one of the most important figures was Norman Bowen. He came from the research institution where arguably the most important work in experimental petrology was done, at least in the first half of the twentieth century: the Geophysical Laboratory of the Carnegie Institution, Washington. The petrologist and historian of petrology Davis **Young** argues that this particular institution provided the ideal framework for Bowen's work in igneous petrology, most of it experimentally based, utilizing the apparatus for the study of rocks and rock melts at high temperatures and pressures available at the geophysical laboratory. The issue of what happens when melts cool and differentiate is fundamental to igneous petrology. For Bowen, it was essentially a laboratory problem, but his work led to fundamental progress in the understanding of rocks as they are present in the field, as discussed, for example, in the classic work of Wager & Brown (1968).

Eventually Bowen's work (in conjunction with Orville Frank Tuttle) led to a resolution of one of the great debates of twentieth century geology: the battle between the 'migmatists' and the 'magmatists' regarding the origin of granite, Tuttle & Bowen (1958) declaring in favour of the latter (see Read 1957). Consideration of this topic leads us into the intricacies of metamorphic petrology, discussed by Jacques **Touret** and Timo **Nijland**. The authors have undertaken the massive task of 'picking the eyes' out of twentieth century metamorphic petrology, to which field they have themselves contributed, having worked together in Scandinavia. The history of metamorphic geology still requires detailed analysis, but the Touret and Nijland paper should serve as a starting-point for all future studies. Like several other essays in the present collection, the authors have found it necessary to trace the roots of twentieth century debates in earlier ways of thinking – in this case even back to the eighteenth century. They also travel as far afield as the work of Miyashiro in Japan. Regretfully, this is the only paper in the collection that attends to ideas developed in the Far East.

Studies of metamorphic petrology are naturally associated with Scandinavian geology, for metamorphic rocks are particularly well exposed in the Baltic Shield, where they have led to new ideas about their production. In the essay by the historian of geosciences, Bernhard **Fritscher**, we look more closely at one of the Scandinavians mentioned in the Touret and Nijland paper: Victor Goldschmidt. He was a petrologist but is chiefly associated with geochemistry, being one of that discipline's founders, especially through his *Geochemistry* (Goldschmidt 1958). He also listed the abundances of elements in the solar system, on the basis of analyses of meteorites. So he too was interested in the Earth 'inside and out'. Here, however, Fritscher focuses on the application of the phase-rule to petrology, and debates about the development of petrology based on fundamental chemical principles – as opposed to the approach via fieldwork and the study of thin-sections favoured by British petrologists like Alfred Harker. Fritscher sees important differences between British and Continental workers and offers some socio-political explanation for the differences.

One of the points made *en passant* by Touret and Nijland is that they find metamorphic petrology in decline (at least in The Netherlands, admittedly a country lacking metamorphic rocks), with posts in the field disappearing, whereas it was formerly a leading area of research. This decline – matched in their country in some other fields such as mineralogy – may reflect changes in public concerns, such as a heightened awareness of environmental problems or dislike of fields regarded as being associated with mining and mineral exploration. It meshes with the broad shifts in emphasis in the second half of the twentieth century that were discussed above, but, I suggest, the current contraction of the field in some parts of the world should not be taken to imply that metamorphic petrology is shrinking for want of interesting and important problems. Indeed, new instruments used in well-funded institutions such as Edinburgh University are being used for exciting work on space material, oil-field metamorphism studies, and so on.

Be that as may, metamorphic petrology is not the only branch of geology whose fortunes have changed in the twentieth century. Hugh **Torrens** is (or formerly was) an ammonite specialist and stratigrapher, but now chiefly studies the history of geology in relation to technology. He too has seen his field contract during the span of his career, so that whereas biostratigraphy was once king it is now being 'squeezed' by specialties such as magnetostratigraphy or sequence stratigraphy. When presenting his Rio paper, Torrens sought (at my request!) to do the impossible, namely discuss stratigraphy as a whole during the twentieth century. In his revised version, he has focused on the question of precision and the extent to which measurements of time by various stratigraphic criteria are more or less precise, and well founded. He takes his starting-point in the nineteenth century, considering the work of the American Henry Shaler Williams and the English stratigrapher, Sydney Savory Buckman. They showed how fossils allowed the correlation of different rock units in different localities and how different thicknesses and types of rock can represent equal amounts of time. Particular lithologies may cross time-lines. For Torrens, the notion of correlation implies determination, or knowledge, of time. And the question he addresses in his paper is what measures are available for the determination of time, so that stratigraphy can make increasingly precise determination of time-intervals.

Torrens argues that biostratigraphy, where changes of fossil types are able to be calibrated by radiometric determinations (cf. Holmes's work), still provides the best way for stratigraphers to proceed, and in consequence he deplores the loss of 'ammonite lore', for example, that has begun to afflict stratigraphy. Torrens also has, with others, doubts about the efficacy of sequence stratigraphy, fearing that it may be prone to arguing in circles; for the 'packets' of sediments identified by seismic investigation are not always dated (calibrated) by palaeontological methods. However, he does not actually deal with sequence stratigraphy, its extensive successful use in (say) the oil industry notwithstanding. Rather, he discusses the question of the chronological precision of the events claimed to be associated with the impacts of meteorites.

In considering potential papers for the present collection, it was evidently impossible to have one that covered the whole of palaeontology, which would have been as unrealistic as a paper that might cover stratigraphy as a whole. So for palaeontology I invited William **Sarjeant** to write a paper on the history of one of his numerous fields of interest (e.g., palynology, ichnology, bibliography, writing novels, folk singing, . . .): namely palynology. He responded with enthusiasm but in so doing he found it necessary and appropriate to trace the historical roots of the field, so that, with its worldwide coverage, and considering the several branches of palynology, his paper starts before and does not reach the end of the twentieth century. Yet, as Sarjeant remarks, palynology has grown from 'a scientific backwater into a mainstream of research'. For example, in my own recent investigations of the history of geology in the English Lake District, I have been forcibly struck by the significance of acritarchs for making progress in the understanding of the stratigraphy of rocks such as the Skiddaw Slates, which have few macrofossils. To a significant extent, it has been acritarchs that have promoted major revisions in structural understanding, helping, for example, to reveal the presence of olistostrome structures in the Lakes. Palynology is also making major contributions to palaeoclimatology and Quaternary geology, not to mention the oil industry.

Palynologists (and palaeontologists more generally) are much concerned to inter-relate their knowledge of fossils by having knowledge of the literature – which may sometimes be published in disconcertingly obscure places. Sarjeant's paper does not pretend to offer a guide to the literature of palynology as a whole, even to his approximate closing date of the 1970s. He says he is writing a 'short history'. Nevertheless, his bibliography is massive, and should be of considerable value to palynologists, or to 'outsiders' who may become involved in the field from time to time. Sarjeant's paper is partly autobiographical, for he has himself played his part in twentieth century palynology. It is pleasing to have his own account of some of his contributions, and his recollections of encounters with colleagues. Whether the interest in matters bibliographical is a sign of the 'old age' of a discipline, as Menard's arguments might lead one to imagine, I leave others to figure out. Naturally, palynology has extended its influences considerably, subsequent to Professor Sarjeant's self-imposed cut-off date of 1970.

Microfossils are, of course, never likely to 'run out', but it is not obvious that the same holds true for macrofossils in a small country like Britain, where collectors from schoolchildren to professors have long been active. To what extent should collecting be open to all, and what regulations (if any) should apply to collecting and conservation? This became an acute problem in Britain and some other countries in the late twentieth century. Ideas on the matter – and the appropriate regulations – have varied considerably. The problem is treated historically, largely for Britain but also with reference to America, by the museologist Simon **Knell**. In his paper, we encounter the cultural, social, and political framework within which geology operates. Through his study of the recent history of collecting, Knell examines the issue of geology's changing social context, thereby showing the way that science operates in practice. He is interested in the public perception of geology and the way geology presents itself to the world, as well as its 'internal' workings.

There is no simple answer to the question: to have or not to have unregulated collection? But questions that have no simple answers are always worth asking. Knell concerns himself with fossils, but what he says applies equally to mineral collection and conservation, or even rocks. I think his paper sufficiently reveals the nature of the question, which is part of the much broader problem of the conservation of objects, whether they be buildings, archives, ... or the environment as a whole. Knell focuses on geological collecting in one country in the late twentieth century. But his paper raises larger issues; and so far as geology is concerned it may prompt questions about policies in countries where problems of collection and conservation are not yet as acute as in Britain. It may be, as Touret and Nijland suggest, that metamorphic petrology is now in 'retreat'. But Knell's kinds of questions will necessarily become more acute in the twenty-first century and beyond. They link the present selection of papers with the trends towards the increasing interest on the part of earth scientists in environmental issues and conservation issues, previously noted.

It may also be mentioned that Knell's paper signals important changes that have occurred in the very nature of science, as a whole, towards the end of the twentieth century. When De Solla Price wrote, his 'growing-shoot' analogy was perhaps more apt than it is today. In the 1960s, the advancing fronts for different geological research programmes could be approximated by the metaphor of more or less discrete growing shoots – extending towards the light chiefly in the favourable environments of university departments, research institutes, or national government-funded geological surveys. But things became substantially different in the second half of the century. Tax-sourced funding declined. Problems came to be addressed, not just in the context of research programmes but in the context of particular technical applications or goals, which are diffused through society. Problems like the extraction of oil from beneath the North Sea could not be solved by expertise within a single discipline. We have, then, what has been called 'transdisciplinarity' (Gibbons *et al.* 1997). For science in this 'mode' (so-called 'Mode 2'), results are communicated, not primarily by publicly accessible journals, but by 'internal' reports and personal contacts. Knowledge may move with the practitioners as they transfer to new problems when old ones are solved. New kinds of sites for the production of knowledge emerge – in consultancies, think-tanks, industrial laboratories, etc. – side by side the traditional ones to be found in universities and research institutes. Funding is garnered from numerous different sources, according to what may be available and the nature of the problems in hand.

Concomitantly, the network of interested parties increases: we may find natural scientists, social scientists, lawyers, business people, engineers – a heterogeneous mix – all involved in developing solutions to problems. Those who are involved may find themselves embroiled in politics and have to be increasingly aware of the social, political, and economic implications of what they are doing. They must take account of the values and interests of groups normally regarded as outside the system of science and technology: solutions to problems have to be socially, politically, and economically acceptable. The fact that this came to be so increasingly in the late twentieth century is illustrated by Knell's paper. The science he discusses does not grow like a free shoot in a hot-house (or ivory tower). It has to interact with all the forces of the society in which it finds itself and negotiate its activities accordingly. It is, in consequence, a rather different *kind* of science from that which De Solla Price analyzed three decades earlier ('Mode 1') – which was based on the study of a scientific field from the first half of the century.

Regretfully, the present collection can only scratch the surface of the history of twentieth century geology. *How* and *why* the changes to science referred to in the preceding paragraph came about are problems too large to be entered into here. But, as said, analysis must precede synthesis. So without claiming to have achieved a synthesis, it is hoped nevertheless that the present collection will prove useful to those who may subsequently tackle the heroic task of furnishing an historical synthesis of twentieth century geology, earth science, planetary science, environmental science, conservation, ...

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