

# ANDREW LUGG

## OVERDETERMINED PROBLEMS IN SCIENCE

### 1

WHEN we look at the history of science, we can hardly avoid noticing recurring patterns in the way it evolves. Some advances rest on the successful exploitation of chance discoveries, some on the systematic testing of alternative covering theories, some on the elimination of anomalies, and so on. The purpose of this paper is to draw attention to a pattern of development, the significance of which has not been generally recognized. This pattern is characterized by an initial occurrence of what I shall call an overdetermined problem — i.e. a problem with no solution compatible with accepted belief and practice — followed by a resolution of the problem which relies on and exploits a change in what was previously taken as given.<sup>1</sup> In the early sections of the paper, I discuss the nature of overdetermined problems and argue that they actually occur in science; I then turn to the question of how such problems are solved; and finally, to a brief study of the bearing of overdetermination on some of the more popular theories of science.

### 2

Problems in science — for instance, the problem of developing a theory to cover certain specified data — never occur in a vacuum. They always arise against a background of relatively unproblematic belief and practice: we make progress by standing on the shoulders of others.<sup>2</sup> It is not, as many philosophers suggest, merely a matter of generating theories which cover the data; our theories must cover the data in a manner consistent with what we take to be well-established data, auxiliary hypotheses, theories, and methodology. But if so — if the interesting question is not how our theories are controlled by experience in the narrow sense of ‘past evidence’, but how they are controlled by experience in the wide sense of ‘all we think we’ve established’ — overdeter-

<sup>1</sup>The term ‘overdetermined’ is borrowed from mathematics. A simple example of an overdetermined mathematical problem is: solve  $x^2 + 2x + 1 = 7y$  given that  $x = 2$  and  $y = 1$ . I do not intend that ‘overdetermined’ be understood in the way it is sometimes understood, e.g. by L. Althusser, ‘Contradiction and Overdetermination’, *For Marx* (New York: Random House, 1969). This use of the word stems from Freud, who used it to refer to the phenomenon of a symptom being determined by a plurality of factors. On Freud’s use, but not mine, ‘overdetermined’ and ‘multiply determined’ are synonymous.

<sup>2</sup>This point has been stressed on a number of occasions by K. R. Popper. See, for instance his *Conjectures and Refutations* (New York: Harper and Row, 1962), Chps 4 and 10, and especially pp. 129 and 238.

ermination becomes a real possibility. Although all theories are undetermined by past evidence, it is not at all obvious that the same can be said when all well-established belief and practice is considered, *i.e.* when we consider past experience rather than past evidence. A sufficiently rich past experience could conspire to rule out all possible theories: it could happen that there is no methodologically sound theory, compatible with accepted theory and all reasonably held auxiliary assumptions, which covers the data.

Overdetermination of a theory by data within a framework is not the only type of overdetermination we can envisage. The same sort of situation can arise when we have a fact to explain but are unable to explain it for want of auxiliary hypotheses<sup>3</sup> or when we have a 'reducing' theory and a 'reduced' theory but are unable to execute the reduction for want of auxiliary theories. The crucial point is that in each case there is no guarantee of a solution compatible with the framework or setting within which it occurs.<sup>4</sup>

It is important to distinguish between overdetermined problems of the sort just characterized and what Max Planck calls 'phantom problems'.<sup>5</sup> An overdetermined problem may have many solutions; it just will not have any compatible with the setting within which it occurs. A phantom problem, on the other hand, such as the problem of designing a perpetual motion machine, of transmuting metals, or of characterizing the luminiferous ether, is so stated that it cannot possibly have any solution, whatever its setting. Phantom problems are always overdetermined but the converse is false.

### 3

It may be objected that overdetermination as characterized rests on a dubious distinction. In a recent paper, H. I. Brown has urged that problems and problem settings should never be considered as being independent, as having lives of their own; the problem setting should always be considered part of the problem.<sup>6</sup> But if so, the phenomenon I am concerned with here can never occur, there being no distinction to be drawn between overdetermined and phantom problems.

Brown's proposal is attractive: it eliminates the problem of separating what states a problem from what states its setting. However, it also entails that every change of setting produces a change of problem. This means that Ptolemy's

<sup>3</sup>H. Putman considers this sort of situation in his 'The "corroboration" of theories', *Philosophical Papers*, Vol. 1 (Cambridge: Cambridge University Press, 1975), Section 10.

<sup>4</sup>Problem settings should not be confused with either Popper's problem situations or his 'backgrounds'. For him, a situation comprises both a problem and a background and backgrounds are viewed as third-world items. See in this regard his *Objective Knowledge* (Oxford: Clarendon Press, 1972), 165.

<sup>5</sup>M. Planck, 'Phantom Problems in Science', *Scientific Autobiography* (New York: Philosophical Library, 1949). The examples which follow are Planck's.

<sup>6</sup>H. I. Brown, 'Problem Changes in Science and Philosophy' *Metaphilosophy*, 6 (1975), 177f.

problem was not Copernicus's — a view of the matter which conflicts to no obvious advantage with common sense. Moreover, we encounter a problem no less intractable than the one we started with: what is it that makes Ptolemy's problem similar to Copernicus's and not to, say, Darwin's?<sup>7</sup> For this reason, I intend to ignore the claims of 'radical problem variance' and proceed on the assumption that problems can persist through changes of setting.<sup>8</sup>

There is, to be sure, a certain amount of leeway in what we take a scientist's problem to be. It is not unreasonable, for instance, to see Ptolemy as proposing a solution to the problem of accounting for the motions of the planets, or the problem of accounting for these motions within an earth-centered framework in which all celestial motions are uniform and circular, or any of a number of other problems. Which we choose to regard as *the* problem Ptolemy was concerned with will naturally depend on what our interests are. However, it is worth noting here that there is a principle of charity for problems. In Ptolemy's case, for instance, charity suggests we see him as attempting to solve the problem of the motion of the planets, since if we do, his problem can be seen to be the same as that of Copernicus, Kepler, and Galileo, and a continuity of interest discerned where the scientists themselves saw it. On occasion, charity may even advise that we regard a scientist as having solved a problem quite different from the one he or she claimed to have solved. Kepler is perhaps a case in point.<sup>9</sup>

A second objection to the above account of overdetermination is that contrary to what was suggested, overdetermination can arise, if at all, only in spite of and never because of the demands of methodology. If 'methodology is independent of any particular assertion about the world, however trivial, however obvious, [if] it is supposed to provide a point of view from which all such assertions can be judged and examined',<sup>10</sup> we should expect overdetermination to occur rarely, if ever. It is after all relatively easy to find, for example, theories compatible with a given set of data and given auxiliary

<sup>7</sup>Brown recognizes this problem in *op. cit.* note 6, 184, where he says that when 'we compare the Ptolemaic and Copernican versions of the problem of the planets we have an example of what I will refer to as a "sameness/difference situation".' However, he does not attempt to solve the problem.

<sup>8</sup>This 'assumption' is particularly natural in the case of long-lived problems such as the problem of the planets or that of the movement of the blood. In the case of the latter, it seems clear that Harvey and Galen were concerned with the same problem, in spite of their locating it within quite different frameworks. For just one indication of this see W. Harvey, *The Circulation of the Blood* (New York: Dutton, 1963), p. 55.

<sup>9</sup>K. R. Popper makes a similar point in *The Philosophy of Karl Popper*, P. A. Schilpp (ed.) (La Salle: Open Court, 1974), 107. The example is Popper's.

<sup>10</sup>P. K. Feyerabend, 'Philosophy of Science: A Subject with a Great Past', *Minnesota Studies in the Philosophy of Science*, V, R. H. Stuewer (ed.) (Minneapolis: University of Minnesota Press, 1970), p. 175. However, Feyerabend also recognizes the conception of methodology to be mentioned in a moment.

theories. The answer to this point is that there is a second and more important conception of methodology, as comprising a set of regulative principles. Some of these latter are of a very general nature (canons of correct procedure in experimentation and observation, criteria of acceptable explanations, 'metaphysical principles' having to do with, *e.g.* what the fundamental entities are — sensations, atoms, or whatever); some are much more specific (the principle of determinism, the principle of no-action-at-a-distance, principles having to do with the desirability of conformity with certain scientific theories, certain religious stories, and certain political doctrines).<sup>11</sup> All, however, drastically restrict what counts as an acceptable theory. As Buchdahl has emphasized, this idea — that extra-inductive criteria play a crucial role in the selection of scientific theories — has had a long history and has been championed by many distinguished philosophers and scientists.<sup>12</sup>

Without regulative principles of the sort just referred to, scientists would most of the time be at a loss to know what to do; without a point of view which discriminates between substantive claims there could be no such thing as scientific progress. By the same token, however, a point of view is liable to impede progress: our principles may guide us astray.

Regulative principles come into play at different times and in different contexts; they do not always have the same force and none can be relied on absolutely. Principles once valued highly are not any longer (*e.g.* the demand that our theories conform to the Biblical story); principles which are for us almost self-evident took centuries to establish (*e.g.* the demand that our theories conform to the deliverances of observation); and principles recognized by one generation but not by the next become favoured again later on (*e.g.* the principle of no-action-at-a-distance). Regulative methodology is not fixed nor is it apart from or above the rest of science. It forms a part of science and develops along with the rest of it.<sup>13</sup> Were this not so, every progress-impeding principle would constitute an insurmountable barrier, not merely a temporary setback.

<sup>11</sup>This second conception corresponds to what G. Buchdahl calls the 'achitectonic component' of scientific theorizing. In 'History of Science and Criteria of Choice', *Minnesota Studies in the Philosophy of Science*, V, R. H. Stuewer (ed.) (Minneapolis: University of Minnesota Press, 1970), Buchdahl characterizes this component very broadly as comprising, among other things, 'maxims of simplicity and economy: considerations of an esthetic nature; principles of continuity or discontinuity; linkages with general metaphysical notions as for instance 'the real does not change', 'nothing comes from nothing', 'the effect is equivalent to the cause'; or more generally, maxims like those of homogeneity, affinity (or the 'analogy of nature'), teleological or alternative preferred explanation schemas, and even theological conceptions' (p. 206). For ease of exposition in what follows I shall speak of scientific practice as though it is or can be expressed in terms of rules or principles. This need not be so, however. For a discussion of this point see T. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962), Ch. V, 'The Priority of Paradigms'.

<sup>12</sup>G. Buchdahl, *op. cit.* note 11, 201f; and 'Explanation and Gravity', *Changing Perspectives in the History of Science*, M. Teich and R. Young (eds.) (London: Heinemann, 1973), pp. 167–168.

<sup>13</sup>'Methodological fallibilism' of this sort is discussed by M. Black, 'The Definition of the Scientific Method', *Problems of Analysis* (Ithaca: Cornell University Press, 1954), 13f.

## 4

We are now in a position to ask whether overdetermined problems actually occur in science. I shall argue that they do. By way of a specific example I shall argue that the problem, which Louis Agassiz solved with his Ice-Age hypotheses, was overdetermined by what Agassiz and most other geologists took to be well-established.<sup>14</sup> My point is not that Agassiz was clearly aware that his problem was overdetermined, only that what he believed and practised, as a matter of fact, overdetermined what he took his problem to be.

Agassiz's problem is simply stated: to explain the occurrence and distribution of erratics (huge boulders far from their natural outcrops), polished and striated rocks, moraines (*débris* we now regard as having been transported and deposited by glaciers), *given* certain facts (*e.g.* that these phenomena actually occur), certain auxiliary hypotheses (*e.g.* hypotheses about the transporting action of glaciers), certain theories (*e.g.* theories about the history of the Earth), and certain methodological requirements (*e.g.* concerning the correct method in geology and its proper relationship to biology).

Now there is — to say the least — an enormous number of theories which can account for the occurrence of these phenomena and their distribution: as already noted no set of data uniquely fixes a theory. However, many, indeed most, of these theories run afoul of what Agassiz would take to be uncontroversial, *i.e.* they run afoul of the aforementioned setting. Thus, suppose it was suggested that the erratics had fallen from the clouds.<sup>15</sup> This would be quickly rejected: clouds are not the sorts of things that can transport or produce rocks; natural phenomena of the sort under study must be naturally caused; and so on. Indeed, once we reflect on the sorts of agents that early nineteenth century geology admitted, it becomes clear that far from there being an enormous number of theories to consider there are only a few.<sup>16</sup> Specifically, we seem forced to appeal to the action of water, floating ice, glaciers, or masses of silt and *débris*.<sup>17</sup>

<sup>14</sup>Agassiz first presented his views in a lecture to the *Société Helvétique des Sciences Naturelles*. His paper, now referred to as the *Discours de Neuchâtel*, is reproduced (in French) in J. Marcou, *Life, Letters and Works of Louis Agassiz* (New York: MacMillan, 1896) Vol. 1, 89f. For an English translation see L. Agassiz, *Studies on Glaciers*, translated by A. V. Carozzi (ed.) (New York: Hafner, 1967). Agassiz's discovery is discussed by Carozzi in the last mentioned work; by F. J. North, 'Centenary of the Glacial Theory', *Proceedings of the Geological Association*, 54 (1943); by R. J. Chorley, A. J. Dunn and R. P. Beckinsale, *The History of The Study of Landforms* (London: Methuen, 1964), Vol. 1, ch. 13; and by M. J. S. Rudwick, 'The Glacial Theory', *History of Science*, 8 (1969).

<sup>15</sup>This view was actually canvassed as late as 1925. See North *op. cit.* note 14, 7.

<sup>16</sup>That this is generally true will be readily appreciated by those who have had the opportunity of generating theories to cover data under the supervision of an expert. One soon learns that covering the data is only a small part of what is involved in developing a plausible theory.

<sup>17</sup>Here I assume that theories such as de Luc's — that the boulders and *débris* were shot out from the interior of the Earth during volcanic disturbances — were by the time of Agassiz too implausible for serious consideration. For de Luc's views, see F. J. North, *op. cit.* note 14, 7.

Agassiz certainly appreciated that only certain theories were at all worthy of consideration. Thus, in the *Discours de Neuchâtel*, the paper in which he introduced the Ice-Age hypothesis (cf note 14), we find him arguing first of all that no plausible alternative to glacial action can adequately cover the data. In the case of moraines, he argues that their form is quite different from that of levées, produced by floods and rock-slides; in the case of scratched and polished rocks, that neither water nor (very likely) water carrying ice can produce these effects; and in the case of erratics, that their position and order cannot be attributed to transportation by huge currents of water, by Lyell's floating ice-rafts, or by masses of silt and débris.<sup>18</sup> Moreover, as Agassiz also points out, it is unreasonable to see the three sorts of phenomena as being produced by different agents.<sup>19</sup> This means we have no other choice than to attribute their occurrence and distribution to glacial action.

However, here a problem arises: this last 'plausible alternative' also runs afoul of what Agassiz and most other scientists took to be given. To see this, consider first de Charpentier's view that the phenomena under discussion occur because in the geologically recent past the Alps were much higher and hence much colder than they are now.<sup>20</sup> If this were so, the Alpine glaciers, being more extended, could have produced the erratics, *striae*, etc. — at least in the Alpine regions. This view, which is in many respects a very attractive view, can be developed and supported in a number of ways. The greater height of the Alps in the past can be attributed — and was attributed by de Charpentier — to a catastrophic *époque de soulèvement* and the subsequent decrease in elevation to the settling down of the shattered rocks. However, as Agassiz points out, this explanation is contradicted by a 'very striking fact': 'the boulders in the Jura are generally less rounded and even much larger than those occurring in the moraines along the margins of present glaciers'.<sup>21</sup>

This observation does not refute the glacial theory itself, only one version of it. As Agassiz remarks there are two options worthy of study: de Charpentier's view that 'the most extended glaciers come down from the top of the Alps' and the quite distinct view that 'there was a time when ice formed naturally beyond the [lower valleys of Switzerland]'.<sup>22</sup> The problem here however is that the

<sup>18</sup>Agassiz, *op. cit.* note 14, xlii–lii and Agassiz's footnote 4. The following argument against the transportation of erratics by huge currents of water gives an idea of the sort of argument involved: 'How could streams of water, barely a few leagues long (I am talking here of the tributaries of the main valleys) have been able to maintain large boulders at more than a thousand feet elevation?' (p. li).

<sup>19</sup>Agassiz, *op. cit.* note 14, lii.

<sup>20</sup>De Charpentier's views are discussed by A. Carozzi, *op. cit.* note 14, xivf and by G. de Beer, 'Charpentier', *Dictionary of Scientific Biography*, C. C. Gillispie (ed.) (New York: Scribners, 1971), Vol. 3, pp. 210–211.

<sup>21</sup>Agassiz *op. cit.* note 14, 1. A similar argument was developed by Lyell in his 'Anniversary Address', *Proceedings of the Geological Society*, 2 (1836), 382–383. The relevant passage is reproduced in Chorley, Dunn and Beckinsale, *op. cit.* note 14, 201–202.

<sup>22</sup>Agassiz, *op. cit.* note 14, il.

second of these, the Ice-Age hypothesis, is inconsistent with the idea of a gradually cooling earth, a point de Charpentier well appreciated. As Rudwick has emphasized, de Charpentier attempted to avoid the difficulty by keeping the extension of the Alps to a minimum and by postulating the existence of an enormous *soulèvement*.<sup>23</sup>

These remarks do not prove that Agassiz's problem was overdetermined. They do however provide all that can reasonably be hoped for — strong evidence. Moreover, it is clear that Agassiz himself was aware that there is no theory which satisfactorily covers the data, given the generally accepted state of geological knowledge and methodological practice of the time. In discussions with de Charpentier in 1836 (*i.e.* before he had developed his own view) Agassiz would have become aware of the problems which beset 'non-glacial' theories, while the amount of time he devotes to the problem of a cooling Earth in the *Discours* shows that he appreciates that the Ice-Age idea has difficulties all of its own.<sup>24</sup> How Agassiz responded to this situation is something we shall discuss presently. First, I will examine the question of resolving overdetermined problems in general.

## 5

Any overdetermined problem which is not a phantom problem can be turned into a solvable problem by changing its setting. If we relinquish a part of the data, some of the auxiliary hypotheses, some of the background theory, or some of the principles governing what counts as good science, we can alleviate the overdetermination. We open the possibility of there being solutions and perhaps even reopen the question of the acceptability of solutions, previously ruled unsatisfactory. But this is not all. If it were, we could give up so much of the setting that almost anything would go. To solve an overdetermined problem it is not enough merely to change the setting and exploit this change to produce a solution. We must produce a solution which enhances or at least promises to enhance the state of our knowledge. In the normal course of events, it is unreasonable to give up a well-entrenched theory to open the way for an account of the odd bit of esoteric data when we have little or nothing to put in its place. We want to reduce our difficulties not to exacerbate them.

In some cases — cases of 'benign' overdetermination — solutions will be easy to come by. If the data are 'soft', the auxiliary hypotheses shaky, the background theories beset by more than the usual number of anomalies, or

<sup>23</sup>Rudwick, *op. cit.* note 14, 149.

<sup>24</sup>When Agassiz visited de Charpentier in 1836, he subscribed to Lyell's view that the erratic boulders had been transported to their present positions by ice rafts floating on lakes. Soon thereafter, de Charpentier convinced him that their origin was glacial. It is also likely that during this visit Agassiz became aware that the cooling-Earth hypothesis posed a problem for the glacial theory since de Charpentier had already commented on this difficulty in a paper published in 1835. For details, see Rudwick, *op. cit.* note 14, 144.

the guiding methodology in any way suspect, it will be readily apparent what part of the setting should be renounced and why. On the other hand, if the problem is 'severely' overdetermined, that is, if its setting is particularly well established, it will appear much more perplexing and, perhaps, even intractable. It is never easy to convince oneself, let alone anyone else, that reliance on the tried and trusted is misplaced or that something else is a much better bet. As a rule in such cases, small changes in the setting will be preferable since the less we have changed, the less we will have to replace and the less we have to replace, the more readily we will be able to find something to replace it with.

Governing these matters is our desire to obtain as complete and as consistent an account of the world as we can. If the overdetermination is benign, simply pointing out, for example, that a crucial piece of data is soft may be all that is needed to enhance the state of our knowledge. But if the overdetermination is severe much more may be called for. In such cases, we would like replacements for what has been lost, or failing this some assurance that replacements will be forthcoming in the not too distant future.

## 6

Agassiz's resolution of the overdetermined problem sketched in Section 4 follows the pattern just outlined: he modifies the problem and solves it in a way which can be viewed (and certainly was so viewed by Agassiz) as enhancing the overall state of geological knowledge. Let us look at his argument in detail.

Immediately after considering alternatives to a glacial account of erratics, *striae, etc.*, Agassiz remarks that 'the investigation of fossils has recently given some very unexpected results, particularly since it has taken a physiological aspect, that is, since we have perceived the existence of a progressive development among all the organized beings which lived on the earth, and since we have recognized epochs of renewal for all of them'.<sup>25</sup> Why is this important? Why should Agassiz turn to a discussion of fossils at so crucial a point in his argument? The answer, I suggest, is that progressivism — the doctrine that the organic world is marked by a succession of more and more advanced creations — makes plausible and is best suited to catastrophism — the doctrine that the inorganic world is marked by a succession of cataclysmic disturbances.<sup>26</sup> If there is indeed a 'progressive development among all organized beings', it is not unreasonable to think that the elimination of old species is brought about by some sort of catastrophe: a gradual change would allow time for migration. This lends weight to Agassiz's next claim:

<sup>25</sup>Agassiz, *op. cit.* note 14, il.

<sup>26</sup>The bearing of progressivism on catastrophism is discussed in detail by L. Eislely, *Darwin's Century* (New York: Doubleday, 1958), ch. IV and especially p. 110. The line of argument developed in the text receives additional support from Lyell's concern with flora and fauna. As Eislely emphasizes (p. 110) Lyell, unlike Hutton, had to provide a uniformitarian account of change in the organic as well as the inorganic world. In this respect, it is also interesting to note that Agassiz's co-worker at this time, K. Schimper, was a botanist.



'The idea of a uniform and constant decrease of the temperature of the earth, as presently accepted, is so contrary to any physiological concept, that it should be vehemently repelled in favor of a decrease of temperature in phases related to the development of organized beings which appeared and disappeared in turn at specific times, with the temperature remaining at a particular average value for a given epoch, and decreasing at fixed intervals'.<sup>27</sup>

Agassiz thus proposes a stepwise decline in the Earth's temperature: he suggests that a period of stable temperature is followed by a sharp drop, this by a rise in temperature to a value somewhat below that of the preceding epoch, another stable period, another sharp drop, and so on. The last of these drops in temperature was sufficient, according to this theory, to bring on an Ice-Age. Finally, to round out his view, Agassiz postulates a *soulèvement* of the Alps after the formation of the ice sheet.<sup>28</sup> This enables him to account for the fact that rounded pebbles and angular-shaped boulders are often found in Alpine regions in the same place: the boulders came to be where they are as a result of having slid down the tilted slopes whereas the pebbles were dragged into position by retreating glaciers.

Here we see Agassiz renouncing part of the setting in which the problem of the occurrence and distribution of erratics, *etc.* was set. He relinquishes the generally accepted hypothesis that the Earth has been cooling down uniformly. This opens the way for the idea that there was an Ice-Age in the geologically recent past. But, as Agassiz clearly realized, unless something was put in the place of the cooling-Earth hypothesis, the overall situation would seem worse, not better. This is why the paleontological results are so important. By appealing to these, Agassiz can skirt the major difficulty for the Ice-Age hypothesis. If there was a large scale extinction and renewal of species, as Cuvier's and Agassiz's biological studies indicated, it is reasonable to think that there were in the past catastrophic disturbances in the Earth's history.<sup>29</sup> But since the usual explanation at the time we are talking about involved huge currents of water, an agent incapable — according to Agassiz — of satisfactorily accounting for the existence of erratics, *etc.* we seem forced to recognize the existence of a continental ice-sheet. What other agent can be plausibly thought of as sufficiently devastating? Indeed when the Ice-Age hypothesis is taken in conjunction with the hypothesis that the Earth is cooler now than it once was, a large deviation from uniform cooling cannot be avoided. Thus, by linking the Ice-Age hypothesis with progressivism in the

<sup>27</sup>Agassiz, *op. cit.* note 14, liii.

<sup>28</sup>Agassiz, *op. cit.* note 14, iv. Note that this appeal to a *soulèvement* would not appear *ad hoc* to Agassiz. It figured in both de Charpentier's and Elie de Beaumont's theories and was at the time an idea in excellent standing. Cf. Rudwick, *op. cit.* note 14, 141.

<sup>29</sup>Of course, not everyone accepted catastrophism. But even those who did not recognized its force. Lyell, for instance, argues strenuously in his *Principles of Geology* (London: Murray, 1830–3) against drawing the conclusion mentioned in the text. Cf. especially Ch. 6–9 of Volume 1, but also Volumes 2 and 3. The magnitude of the problem is reflected in Lyell's appeal to the 'imperfections' of the fossil record.

organic world, Agassiz was able to make plausible the idea of a non-uniform but constant decrease in the Earth's temperature and develop a view that can reasonably be thought of as yielding an overall improvement in scientific knowledge.

The present account of Agassiz's discovery relies on two points that M. J. S. Rudwick has been at pains to establish: that Agassiz was concerned about the conflict between the Ice-Age and the cooling-Earth hypotheses and that the argument from paleontology plays a central role in Agassiz's theory.<sup>30</sup> But Rudwick does not locate these points within the kind of framework developed here. This is hardly surprising: his concern is with the historical record, not with the more abstract philosophical question of patterns of scientific change. It should also be noticed, however, that most other commentators see the issue in quite a different light: they see Agassiz's position as wildly speculative or at best ill-conceived. Thus, for J. Marcou the second half of Agassiz's paper was 'an error on his part' and the Ice-Age idea was the 'only rational and just conception' among a host of 'biological dreams and explanations'; for A. Carozzi, the *Discours* illustrates Agassiz's weakness for 'wild speculations on subjects about which he knew very little'; and for G. L. Davies, Agassiz 'threw all caution to the wind' and outlined 'what must be the most fantastic explanation of the glacial period ever offered'.<sup>31</sup> In response, it will perhaps suffice to note that if this viewpoint is correct it becomes difficult to understand why Agassiz's proposal was accorded so much serious and sophisticated critical study<sup>32</sup> or to explain his apparent command of the 'logic' of the situation when he notes that the Ice-Age hypothesis is forced on us as soon as we adopt the glacial account of erratics, *striae* and moraines.<sup>33</sup>

## 7

It is a striking fact that many scientific advances are made long after one would expect and are received with surprisingly vigorous hostility. What

<sup>30</sup>M. J. S. Rudwick, *op. cit.* note 14, especially pp. 148–150.

<sup>31</sup>J. Marcou, *op. cit.* note 14, 110; A. Carozzi, *op. cit.* note 14, xviii; G. L. Davies, *The Earth in Decay* (Amsterdam: Elsevier, 1969), pp. 265–266. Davies also sees Agassiz's hypothesis as 'one of those sweeping generalizations in which he indulged in all too frequently' (p. 265). In the present regard, it is also interesting to bear in mind Marcou's report that 'Even de Charpentier was not gratified to see his glacial question mixed up with rather uncalled-for biological problems', *op. cit.* note 14, 110.

<sup>32</sup>The details are given in the next section.

<sup>33</sup>In a discussion with Murchison at a meeting of the Geological Society of London, Agassiz remarks that 'Mr. Murchison has objected to the glacial theory in the only way it could be objected to. He allows that the whole is granted as soon as you grant a little bit'. The relevant parts of the report of this meeting are reprinted in Chorley *et al.*, *op. cit.* note 14, 218–221. In addition, it is worth noting Rudwick's point (*op. cit.* note 14, 149) that Agassiz did not omit his paleontological 'speculations' in his *Etudes sur les Glaciers* (1840) although he could have done so without materially detracting from the argument of the book. Nevertheless, it should also be noted that according to Marcou, *op. cit.* note 14, 112, Agassiz continued to espouse his 'biological dreams' because 'it was so hard for him to admit a mistake'.

seems in retrospect to be the logical next step, the natural culmination of what has gone before, is rarely seen as such at the time. How can we explain this fact? One popular line of thought has it that prejudice and ‘blindness’ regularly prevents scientists from seeing things as they are.<sup>34</sup> However, this type of response is likely to appear particularly inadequate when the scientists involved are among the most brilliant and thoughtful of the time: such scientists are unlikely to form part of an extensive conspiracy of blindness and prejudice (see also below).

The present discussion suggests an alternative explanation for the delay and ambivalent reception of certain hypotheses. If a problem is overdetermined we should expect it to be more difficult to solve than an ordinary non-overdetermined problem. For, to solve an overdetermined problem, one first has to turn it into a solvable problem; one has to pick out a particular spot in the setting and mount an argument to show that its rejection leads or promises to lead to an overall improvement in what we believe. Moreover, not only are overdetermined problems difficult to solve, we are likely to be deflected from the correct solution: in the normal course of events, we will be unaware that our problem is overdetermined, so that the discovery of a conflict between the correct solution and accepted belief and practice is likely to bring about a premature end to its study. This latter will be particularly likely to happen if there is, as there often is, an alternative research programme which promises to provide the solution required.

We should also expect a less favourable reception to be accorded to solutions of overdetermined problems than to those of ordinary problems. If a problem setting is well-established, one will, quite correctly, be reluctant to give it up: the well-established, after all, rarely becomes well-established by chance. Furthermore, there is the added difficulty that in all but the most benign cases of overdetermination, the supporting argument is likely to be indirect, controversial, or sketchy — new viewpoints are rarely established over-night.

These points are illustrated by Agassiz’s discovery. As a number of commentators have noted, the formulation of the Ice-Age hypothesis seems in retrospect to have been surprisingly long in coming and its reception unnecessarily hostile.<sup>35</sup> We might reasonably expect the hypothesis to have been formulated soon after the glacial phenomena were observed and correctly interpreted. But this is not what happened. The phenomena were observed and seen as glacial many years before the *Discours* was written. Nathaniel Wraxall, for instance, reported the existence of erratics in his *Tour round the Baltic and*

<sup>34</sup>See, e.g. B. Barber, ‘Resistance to Scientific Discovery’, *Science*, 134 (1961), 596–602 and the discussion below.

<sup>35</sup>Davies, *op. cit.* note 31, Ch. 8; and B. Hansen, ‘The Early History of Glacial Theory in British Geology’, *Journal of Glaciology*, 9 (1970), 130. Rudwick’s main concern in *op. cit.* note 14 is to explain these observations.

through the Northern Countries of Europe, published in 1775, while B. F. Kuhn in 1787, Hutton in 1795, and Playfair in 1802, among others, canvassed the idea that the existence of erratics, *striae*, and moraines could be accounted for by the transporting action of glaciers.<sup>36</sup> Moreover, even the idea of vast sheets of ice was explicitly urged by A. Bernhardt some five years before Agassiz wrote his paper.<sup>37</sup>

Agassiz's hypothesis is today considered substantially correct. Although some of the details are wrong — it is now thought that the Alps were in place before, not after, the formation of huge masses of ice — the leading idea of an ice field covering a large part of Europe is no longer in question. Thus, it comes as something of a surprise when we learn that the reception afforded to Agassiz's hypothesis was, to put it mildly, somewhat ambivalent: von Buch and Elie de Beaumont reacted to the *Discours* with hostility even after Agassiz had pointed out 'visible and undisputable proofs' of glacial action to them on field trips; de Charpentier, who, of course, had no aversion to the glacial theory, found Agassiz's extension of his views quite unconvincing; and Lyell's view of the matter fluctuated from almost total agreement with Agassiz to opposition, and back again.<sup>38</sup>

One way to explain these facts is to say that 'Men shut their eyes to the meaning of the unquestionable fact'.<sup>39</sup> But this seems implausible. Geologists of the stature of von Buch, Elie de Beaumont, and Lyell were surely psychologically capable of recognizing a good thing when they saw it. A second, far

<sup>36</sup>For a discussion of the views of these and other pioneers of the glacial theory see North, *op. cit.* note 14, and Chorley, *op. cit.* note 14, 191f. Carozzi, *op. cit.* note 14, xiif and Davies, *op. cit.* note 31, 264f are also useful.

<sup>37</sup>The relevant sections of Bernhardt's paper appear in translation in *A Source Book in Geology*, K. F. Mather and S. L. Mason (eds.) (New York: Hafner, 1939), 327f.

<sup>38</sup>For an interesting account of a field trip conducted by Agassiz in 1837 to show von Buch and Elie de Beaumont 'the evidence' see Carozzi, *op. cit.* note 14 xxf. The remark about 'visible and undisputable proofs' is Carozzi's, *ibid.*, xi. For de Charpentier's objections to the Ice-Age hypothesis, see his *Essai sur les glaciers et sur le terrain erratique du Bassin du Rhône* (Lausanne: Ducloux, 1841), 232–241. In 'On the Geological Evidence of the Former Existence of Glaciers in Forfarshire', *Proceedings of the Geological Society* 3 (1840), 337–345 and especially 338 and 345, Lyell shows himself strongly inclined towards the idea of a continental ice-sheet. However, soon thereafter he was again urging his own theory of transportation by ice-rafts and it was not until 1857 that he readopted the glacial theory and not until 1863 that the Ice-Age hypothesis again figured in what he was willing to admit. For his views in 1857 see his *Life, Letters, and Journals*, K. M. Lyell (ed.) (London: Murray, 1881), 249f; and for his views in 1863 see his *The Geological Evidences of the Antiquity of Man* (London: Murray, 1863), Ch. XIII. (Note that the glacial theory can be accepted without accepting the Ice-Age hypothesis. If this is not done, one is liable to see some scientists as supporting Agassiz when all they are doing is supporting de Charpentier and to see de Charpentier as the 'rightful scientific parent' of the Ice-Age idea. This latter is the view of G. de Beer, *op. cit.* note 20, 211.)

<sup>39</sup>A. Geike, *The Founders of Geology* (New York: Dover, 1962), 445. This is a reprint of the second (1905) edition of a work originally published in 1897. In a similar spirit, G. L. Davies *op. cit.* note 31, has suggested that most of those present at Agassiz's lecture to the Geological Society in 1840 'clearly found the idea of former British glaciers something of a strain on their imagination' (p. 287) and that the important British field geologist, J. B. Jukes, 'long remained blind to glacial phenomena' (p. 288).

more satisfying, approach is to see overdetermination at work. As long as the idea of a uniformly cooling Earth remained unchallenged, the Ice-Age theory would never seem worthy of sustained investigation; it would at best appear to be a serious alternative to views involving huge currents of water, ice-rafting, or a limited extension of present-day glaciers. Even this, however, may be somewhat too charitable. A more realistic appraisal would see Agassiz's theory as a poor substitute for diluvialism. At the time in question, diluvialism — according to which the main topographical features of the Earth were caused by the action of water — was a theory of outstanding explanatory power.<sup>40</sup> Indeed, prior to the development of a glacial theory of comparable power, it was almost inevitable that phenomena we now see as undermining diluvial theory would be seen as problems to be solved within its general framework.

As for the ambivalent reception to Agassiz's hypothesis, we can point to the fact that his argument in support of relinquishing the uniform cooling hypothesis is one few geologists could accept. Those partial to catastrophic events were for the most part committed to the idea of huge currents of water — diluvial theory coupled with Elie de Beaumont's theory of *époques de soulèvement* constituted a research programme with a great deal of life in it. On the other hand, those attracted to uniformitarianism were no more open to the view that the earth had once been cooler than they were to the view that it had once been hotter and least of all were they ready to countenance Agassiz's theory of sudden, violent changes.<sup>41</sup> Thus, right or wrong, the hypothesis would seem to many to be based on biological views which were irrelevant, false, or too tenuously related to the geological question. (It should also be noted here that Agassiz's view is inconsistent with a number of findings — e.g. the discovery of marine shells in 'glacial débris'; that his evidence is not unequivocal — e.g. the ice-rafting theory can be developed to account for all the phenomena Agassiz mentions; and that his view is open to 'technical' objections — e.g. de Charpentier's point that the Ice-Age could not have occurred *before* the uplifting of the Alps since the transported material follows the slopes of present-day valleys.<sup>42</sup> All of these factors, of course, further cloud an already murky affair.)

<sup>40</sup>Cf. Rudwick *op. cit.* note 14, 140–141. Better still, see the same author's 'Uniformity and Progression: Reflections on the Structure of Geological Theory in the Age of Lyell', *Perspectives in the History of Science and Technology*, D. H. Roller (ed.) (Norman: University of Oklahoma Press, 1971), 213f.

<sup>41</sup>'Strict' uniformitarians would object to any sort of cooling of the Earth because it runs counter to their view that save for small fluctuations the history of the earth is completely stable.

<sup>42</sup>Concerning the first of these points note Murchison's observation of 'the existence upon Moel Tryfare and the adjacent Welsh mountains of sea shells of existing species, at heights of 1500 and 1700 feet above the sea, where they are associated with mixed detritus of rocks transported from afar'. This is quoted in Chorley *et al.*, *op. cit.* note 14, 227. As an illustration of the second point, note that the theory of transportation by ice-raft can be developed to account for striation by postulating that the transporting icebergs occasionally run aground. (For details see Davies, *op. cit.*

In short, when Agassiz's discovery is seen as involving the resolution of an overdetermined problem we can give credit both to Agassiz's achievement and to the depth and subtlety of the opposition, something we are unable to do if we resort to a conspiracy of prejudice and misunderstanding.

## 8

Scientific developments involving overdetermined problems need not unfold in exactly the same way as Agassiz's discovery. The characterization of Sections 2 and 5 is a general one, not one keyed rigidly to Agassiz's hypothesis. In fact, certain intriguing aspects of overdetermination are obscured rather than illustrated by the Ice-Age idea. What I have in mind is the following.

First, notice that although Agassiz's problem was resolved in favour of a previously entertained solution — the Ice-Age hypothesis had been considered prior to 1837 — it is not essential that overdetermined problems be resolved in this way. The background setting could guide those concerned with the problem away from the viewpoint on which the solution depends. I believe that something of this sort happened in the case of Kekulé's discovery of the benzene ring. In the years prior to his discovery, the idea of a closed carbon chain was completely alien to Kekulé's conception of chemistry.<sup>43</sup>

A second point is that the severity of an overdetermined problem may be alleviated not all at once, but little by little. There may be progressive downgrading of certain 'well-established' beliefs and practices and the upgrading of others as a result of either internal or external developments.<sup>44</sup> In the extreme case, by the time the discovery is finally fully articulated the overdetermined character of a problem is so benign that its solution is accepted with few if

note 31, 297–298.) And on the third point, see de Charpentier *op. cit.* note 38, 232–241 and note that Lyell had argued against Elie de Beaumont's theory of *époques de soulèvement* in the first edition of the *Principles*, note 29, Vol. 3, Ch. XXIV.

<sup>43</sup>A full-scale demonstration that Kekulé's problem was overdetermined is, of course, beyond the scope of this paper. The argument turns on a number of points: that for Kekulé in the early years of the 1860s carbon atoms have a fixed valency, multiple bonding gives rise to saturation, and chemistry properly speaking is concerned only with 'transformation' and not 'constitutional' formulas. Moreover, his preferred nomenclature and even his graphic 'sausage' formulas incorporate no means of representing closed chains. With this setting it is reasonably easy to show that the problem of determining the correct formula for benzene is overdetermined. For a brief introduction to the intricacies of Kekulé's work see the extracts of his papers reprinted in *A Source Book in Chemistry 1400–1900*, H. M. Leicester and H. S. Klickstein (eds.) (Cambridge: Harvard University Press, 1952). Also see A. J. Ihde, *The Development of Modern Chemistry* (New York: Harper and Row, 1964), especially Chaps 8 and 12; J. R. Partington, *A History of Chemistry* (New York: MacMillan, 1964), Vol. 4, especially Chapter XVII; and N. W. Fisher, 'Kekulé and Organic Classification', *Ambix*, 21 (1974), 29–52.

<sup>44</sup>In this regard, it is important to keep in mind that science is a group effort and that scientists are never completely aware of their own or other's beliefs and manner of operation. For a useful discussion of large-scale systems of belief, see B. Mitchell, *The Justification of Religious Belief* (London: MacMillan, 1973), p. 133. Also important in the present regard is the point, recently stressed by P. K. Feyerabend, that on occasion events, not arguments, precipitate new standards. Cf. his *Against Method* (London: New Left Books, 1975), p. 25.

any dissenters. Again, Kekulé provides an example: during the early years of the 1860s there was a progressive down-grading of the conception of chemistry which Kekulé inherited from Gerhardt and a corresponding up-swing in the fortunes of structure theory.<sup>45</sup>

Third, a solution of an overdetermined problem may call for the renunciation of a central and substantial methodological principle. In many cases — for example, in the case of Agassiz — it will be possible to resolve an overdetermined problem by ‘merely’ relinquishing a well-established hypothesis. But this need not be so: as already remarked, Kekulé’s discovery called for no less than the renunciation of a particular conception of chemistry. Moreover, clearly, if the principle renounced is sufficiently central, *i.e.* sufficiently intimately related to other principles and theories, its renunciation may bring about far-reaching ‘revolutionary’ effects.<sup>46</sup>

Fourth, overdetermined problems may be ‘partially’ resolved: we can make progress by replacing one overdetermined problem by another. If the second problem setting appears more complete and more consistent than the first, it is reasonable to think an advance has been made, even though the new setting still overdetermines the problem. This suggests we would do better to concentrate on the notion of relative overdeterminedness than on the corresponding absolute notion.<sup>47</sup> Planck’s discovery of the quantum of action provides a fairly clear illustration of the point.<sup>48</sup> To solve the ‘problem of equilibrium

<sup>45</sup>For a discussion of the views of Gerhardt and the nature of the type theory to which Kekulé subscribed in the 1850’s, see the secondary sources mentioned in note 43. Among the many relevant ‘outside’ developments, we might note one in particular: Butlerov’s urging of the view that there can be only one formula for each compound. On this see, *e.g.* H. M. Leicester, ‘Kekulé, Butlerov, Markovnikov: Controversies on Chemical Structure from 1860 to 1870’, *Kekulé Centennial*, O. T. Benfey (ed.) (Washington: American Chemical Society, 1966), 15f.

<sup>46</sup>This was the case with Kekulé’s discovery. As is well-known, Kekulé’s work along with certain other less spectacular developments settled the problem of molecular constitution and opened the way for rapid progress in organic chemistry. For details see A. J. Ihde, *op. cit.* note 43, 304–343.

<sup>47</sup>These remarks highlight a generally overlooked theoretical problem: in what way can inconsistent sets of beliefs be rank-ordered with respect to their severity? This is important since as P. K. Feyerabend, *op. cit.* note 44, 65, has observed all theories are ‘in some trouble or other’. The whole of Chapter 5 of *Against Method* bears on this point. What needs to be developed is an account of why and when certain statements *cannot* be ‘inferred’ from an inconsistent system of beliefs and of the principles which govern our practice of rounding up inconsistencies and confining them to a particular part of the system.

<sup>48</sup>As in the case of Kekulé, a full-scale demonstration that Planck’s ‘problem of equilibrium between radiation and matter’ is overdetermined by what he believed true in 1900 is beyond the scope of this paper. However, that Planck’s problem was overdetermined will not seem surprising once it is remembered that its setting comprised the theories and techniques of classical mechanics, a rich and powerful — and hence exceedingly restrictive — body of knowledge. In particular, it is worth noting in the present regard that Rayleigh in a paper published in 1900 showed in effect that Planck’s radiator law is logically incompatible with the equipartition theory of statistical mechanics. On this, *cf.* M. Jammer, *The Conceptual Foundations of Quantum Mechanics* (New York: McGraw-Hill, 1966), 16f. Planck, however, did not realize this until much later. For details of Planck’s discovery see, besides Jammer, M. J. Klein, ‘Max Planck and the Beginnings of Quantum Mechanics’, *Archive for History of Exact Sciences*, 1 (1960–62); A. Hermann, *The Genesis of Quantum Theory* (1899–1913), C.W. Nash (trans.) (Cambridge: Cambridge, 1971); and M. Planck, *op. cit.* note 5, 35f.

between radiation and matter', Planck renounced the idea that energy had 'to be a continuously divisible quantity'.<sup>49</sup> But this did not settle the issue: the solution is only a 'partial solution'. For, as is well-known, Planck still clung to classical mechanics and thus to a setting which, being inconsistent, gives rise to a particularly straightforward form of overdetermination.<sup>50</sup>

Fifth and last, a solution of an overdetermined problem may bring with it unintended and unappreciated consequences. Not only will a scientist typically be unaware that he or she is confronting an overdetermined problem — scientists quite properly are not constantly examining and evaluating their 'framework principles' — he or she will be unaware of the repercussions any given solution will give rise to. These may turn out to be palatable; but then again they may not. This point is illustrated by Planck's attitude to his discovery: 'the most reluctant revolutionary of all time' formed part of his own opposition.<sup>51</sup>

## 9

Overdetermination of the sort I have been discussing has an important bearing on some of the more recent theories of science: many of them have no place for the pattern of development characteristic of overdetermined problems and their resolutions. I shall conclude by indicating why this is so.

Consider first the still popular view that the correct method in science is that of 'hypothesize and test'. As characterized by Hempel, this is the view that 'Scientific knowledge . . . is arrived at . . . by inventing hypotheses as tentative answers to a problem under study, and then subjecting these to empirical test'; as characterized by Popper, it is the view that 'We always find ourselves in a certain problem situation [and] the solution, always tentative, consists in a theory, a hypothesis, a conjecture. The various competing theories are compared and critically discussed in order to detect their shortcomings'.<sup>52</sup> Crucial to this view is the idea that there is no lack of solutions compatible with the evidence and that 'the initial proposal of hypothesis is a groping affair involving guesswork among sparse data'.<sup>53</sup> But this, of course, is totally at variance with the pattern of development characteristic of overdetermined problems and their resolutions. For, in the case of such problems, every

<sup>49</sup>M. Planck, *op. cit.* note 5, 84.

<sup>50</sup>See note 48.

<sup>51</sup>See L. Pearce Williams, 'Normal Science, Scientific Revolutions and the History of Science', *Criticism and the Growth of Knowledge*, I. Lakatos and A. Musgrave (eds.) (Cambridge: Cambridge University Press, 1970), p. 50. In his autobiography Planck remarks that his 'attempts to fit the elementary quantum of action somehow into classical theory continued for a number of years at the cost of a great deal of effort'. See *op. cit.* note 5, 44–45.

<sup>52</sup>C. G. Hempel, *Philosophy of Natural Science* (Englewood Cliffs: Prentice Hall, 1966), 17 and K. R. Popper, *op. cit.* note 9, 68.

<sup>53</sup>N. R. Hanson 'Is there a Logic of Scientific Discovery?', *Current Issues in the Philosophy of Science*, H. Feigl and G. Maxwell (eds.) (New York: Holt, Rinehart and Winston, 1961), p. 22.



conjecture runs afoul of something we wish to retain: Agassiz's problem was not one of selecting from among unrefuted competing hypotheses but from among 'refuted' ones.<sup>54</sup> Moreover, if we accept the method of hypothesis and test as the correct method in science, we are forced to see delays in the formulation of hypotheses and ambivalent receptions as matters for psychological study, as methodologically irrelevant. However, as we have seen (*cf.* Section 7) in some cases these phenomena are susceptible to philosophical scrutiny.<sup>55</sup>

Consider next Kuhn's influential account, according to which 'the shared paradigm [is] a fundamental unit for the student of scientific development'.<sup>56</sup> On this view, science is a discontinuous sequence of periods of 'normal' science, each of which is governed by a distinct paradigm. Thus, in Agassiz's case, as in any other, there are three possible ways of viewing his work, given Kuhn's framework: either he was operating in a pre-paradigm situation *or* he was engaged in normal science *or* his discovery brought about a paradigm change. However, the first of these is in conflict with Kuhn's claim that geology in the period under discussion was a paradigm-governed enterprise.<sup>57</sup> The second conflicts with the point made earlier: that Agassiz solved his problem by foregoing part of the network of commitments which gave direction to geology in 1830's.<sup>58</sup> Indeed what he advocated can be and has been seen as instituting a major shift in our understanding of the Earth.<sup>59</sup> And finally the third possibility conflicts with the following observations: there does not seem to be a paradigm shift between de Charpentier's and Agassiz's work; the shift — supposing there were one — does not appear to involve a break which can only be crossed by appeal to psychological or sociological mechanisms; and the main participants do not seem to be 'talking through one another' in the manner characteristic of Kuhnian paradigm shifts — von Buch, Elie de Beau-

<sup>54</sup>The second occurrence of 'refuted' here means of course, 'refuted by experience in the broad sense, *i.e.* including all well-established belief and practice'.

<sup>55</sup>C. G. Hempel *op. cit.* note 52, 16 and many others see Kekulé's discovery as a perfect illustration of the method of hypothesis and test. This is not unreasonable, since Kekulé himself saw it this way. Kekulé's account of the famous 'dream' in which the hexagonal formula was revealed is reproduced in W. G. Palmer, *A History of the Concept of Valency to 1930* (Cambridge: Cambridge University Press, 1965), pp. 63–64. However, we should bear in mind that when Kekulé analysed his discovery twenty-five years had gone by — plenty of time for drastic 'reconstruction'. More important, when we look at his papers on the benzene ring we notice that the actual discovery was an exceedingly complex event. In the period 1865–66, Kekulé wrote three papers on the subject, each of which was significantly different. Indeed, in only one of these are the positions of the double bonds indicated. See J. R. Partington, *op. cit.* note 43, 555.

<sup>56</sup>T. S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962), p. 11.

<sup>57</sup>T. S. Kuhn, *op. cit.* note 56, 10. For the argument that follows it is important to remember that for Kuhn a paradigm defines a field of science and brooks no rival. See *ibid.* 34.

<sup>58</sup>I take it that the cooling-Earth hypothesis was in the period under discussion part of the framework directing geological research. Whether or not one sees this as reasonable will depend on how one understands Kuhn's rather opaque notion of paradigm. See also Rudwick's discussion of geological paradigms in the age of Lyell, *op. cit.* note 40.

<sup>59</sup>This is the view of G. L. Davies, *op. cit.* note 31. See Ch. 8 and especially p. 263.

mont, and Lyell were as aware of the exact nature of Agassiz's proposal as he was aware of the exact nature of their objections.

It might be thought that we can improve the situation if we shift from Kuhn's picture of a single overarching paradigm to Lakatos's picture of multiple competing research programmes.<sup>60</sup> This would appear much more promising, since we can plausibly see Agassiz as working within a programme stemming from Perraudin and Venetz and possibly from Hutton and Playfair.<sup>61</sup> On this view, the 'land-ice' programme is in competition with other programmes: diluvialism, ice-rafting, *etc.* However, many problems remain. In particular does the 'hard-core' of the land-ice theory include the cooling-Earth hypothesis? If it does not, there can hardly be any objection on the part of de Charpentier or anyone else that Agassiz had stepped beyond the bounds of the programme. On the other hand — and more plausibly — if it does, Agassiz must be seen as instituting a new programme and another problem crops up. On Lakatos's account the relationship between old and new programmes is not open to philosophical scrutiny, but as we have seen the transition in Agassiz's case is not one susceptible only to a genetic account.<sup>62</sup>

These observations tell us that an adequate account of scientific change must recognize that beliefs and practices are often given up as other beliefs and practices are acquired and that this can happen during periods of relative calm as well as during times of great upheaval. Moreover, they show that over-determined problems, besides being of interest in their own right, provide an important test case for theories of scientific change.

*University of Ottawa*  
*Canada*

<sup>60</sup>I. Lakatos, 'Falsification and the Methodology of Scientific Research Programmes', *Criticism and the Growth of Knowledge*, I. Lakatos and A. Musgrave (eds.) (Cambridge: Cambridge University Press, 1970), pp. 91–196.

<sup>61</sup>*Cf.* A. Carozzi, who refers in *op. cit.* note 14, xiii, to 'the chain reaction of Perraudin to Venetz, to de Charpentier, to Agassiz'.

<sup>62</sup>The relationship between old and new Lakatosian research programmes is discussed by P. K. Feyerabend, 'Consolations for the Specialist', *Criticism and the Growth of Knowledge*, I. Lakatos and A. Musgrave (eds.) (Cambridge: Cambridge University Press, 1970), p. 215. For an extended discussion of the debate between Lakatos and Feyerabend see P. Quinn, 'Methodological Appraisal and Heuristic Advice', *Studies in History and Philosophy of Science*, 3 (1971).